Matters arising

Evidence from a statewide vaccination RCT shows the limits of nudges

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ARISING FROM H. Dai et al. *Nature* https://doi.org/10.1038/s41586-021-03843-2 (2021)

Simple messages derived from behavioural science have increased the uptake of the seasonal flu vaccine[1–5], and early studies from the coronavirus disease 2019 (COVID-19) vaccine rollout have found that this strategy works for recently eligible older adults[6] and healthcare workers[7]. However, it is unknown whether messaging on its own will encourage vaccination against COVID-19 among reluctant populations. In a randomized controlled trial (RCT) five to eight weeks after all adults in the study population (n = 142,428) were eligible for vaccination, we find that the best-performing nudge in previous studies[2,6] and seven additional messages—stressing vaccines' safety, efficacy, minimization of bad outcomes, accessibility (free, no identification required), protection of recipients' families or widespread adoption—had no detectable effect among people who had not been vaccinated according to state records. This suggests an important boundary condition for nudges that is consistent with a recent result from late in the flu season[8]. Public health authorities should consider simple messages to encourage vaccination at key inflection points (for example, rollout of paediatric COVID-19 vaccines and full Food and Drug Administration approval for adults), but may see diminishing returns if using them to encourage the more hesitant.

After a strong initial push, the rate of COVID-19 vaccinations declined in the USA. Efforts to encourage vaccination have run the gamut from free doughnuts and marijuana to million-dollar lotteries and rare experiences such as driving at a superspeedway. Recently, Dai et al.[6] reported promising results from an RCT evaluating another tactic—sending people short messages informed by behavioural science. The appeal of this approach is clear: it is cheap and minimally invasive. It is also well supported by convergent evidence: email messages increased COVID-19 vaccination appointment sign-ups among healthcare workers[7], and SMS[1–3], mail[4] and email[5] messages have increased seasonal flu vaccinations. Moreover, it has garnered considerable media attention[9], with pieces advocating it in *The Washington Post*, *Fortune*, *The Guardian*, *U.S. News & World Report* and this journal[10]. Policymakers also took note, as several states implemented SMS campaigns[9].

The Dai et al. study was conducted early in the COVID-19 vaccine rollout with recently eligible older adults. Although the results show the potential of nudges, it is unknown whether short messages can change motivations in the population that did not get vaccinated immediately. Indeed, Dai et. al. distinguish burden reduction (helping people to follow through on pre-existing intentions) from demand creation (changing intentions), and numerous reviews find limited and mixed evidence on what drives demand[11–14].

To test whether these findings generalize beyond the initial stages of COVID-19 vaccination, we evaluated the efficacy of text messages sent by the Rhode Island Department of Health (RIDOH) to increase uptake in May and June 2021. The messages included the best-performing 'ownership' language from Dai et al. and a related flu study[2]. This language was supplemented in most conditions with information about safety, efficacy or access, for example. This study offers a strong test of direct messaging because recipients were unvaccinated five to eight weeks after becoming eligible. It is also a realistic test of what a government can and, more importantly, cannot do (for example, craft messages containing false claims and send excessive communications).

RIDOH maintains separate databases of individuals who have been vaccinated and tested for COVID-19. Our study population is the difference of these lists (tested but not yet vaccinated) matched through a series of quasi-identifiers and excluding people under 18 when tested (final n = 142,428; see Extended Data Fig. 1 for randomization scheme). The primary outcome was vaccination by the end of the measurement period: 25 May 2021 to 21 June 2021 (one week after the last day of messaging). At time of launch, all Rhode Islanders over 16 had been eligible to get vaccinated since 19 April 2021, and free, walk-in availability was widespread. The study was deemed exempt by RIDOH's institutional review board. The sample size was dictated by policy goals, as all eligible individuals received messages. A previous study[2] with more conditions and a sample size similar to our first iteration detected meaningful effects.

We created eight messages (Extended Data Table 1, Supplementary Information section 1) on the basis of behavioural science research on COVID-19 health behaviours and other vaccination contexts. All included ownership language ('a vaccine is waiting for you')[2,6], a sentiment also appearing in a standalone condition. Other conditions further emphasized safety, access, minimal likelihood of bad outcomes, reduced risk to one's family, social norms or some combination. All included a link to a state-run page providing vaccination options.

Individuals were assigned to receive one of eight messages or no message (control group). We randomly divided the population into three consecutive iterations of 40,000, 39,709 or 78,394, and then into roughly equal groups per day within those weeks. Within these strata, individuals were assigned to receive one of eight messages or no message (control group).

To maximize overall vaccinations, in iterations 2 and 3 we used an adaptive design such that the likelihood of assignment to any given message was determined by message performance in the previous iteration, with an ε-bounded Thompson sampler adjusting the probability of assignment to condition over time (Supplementary Information section 2).

This study is a block-randomized experiment. All analyses (preregistration: https://osf.io/pkhae) use either the Cochran–Mantel–

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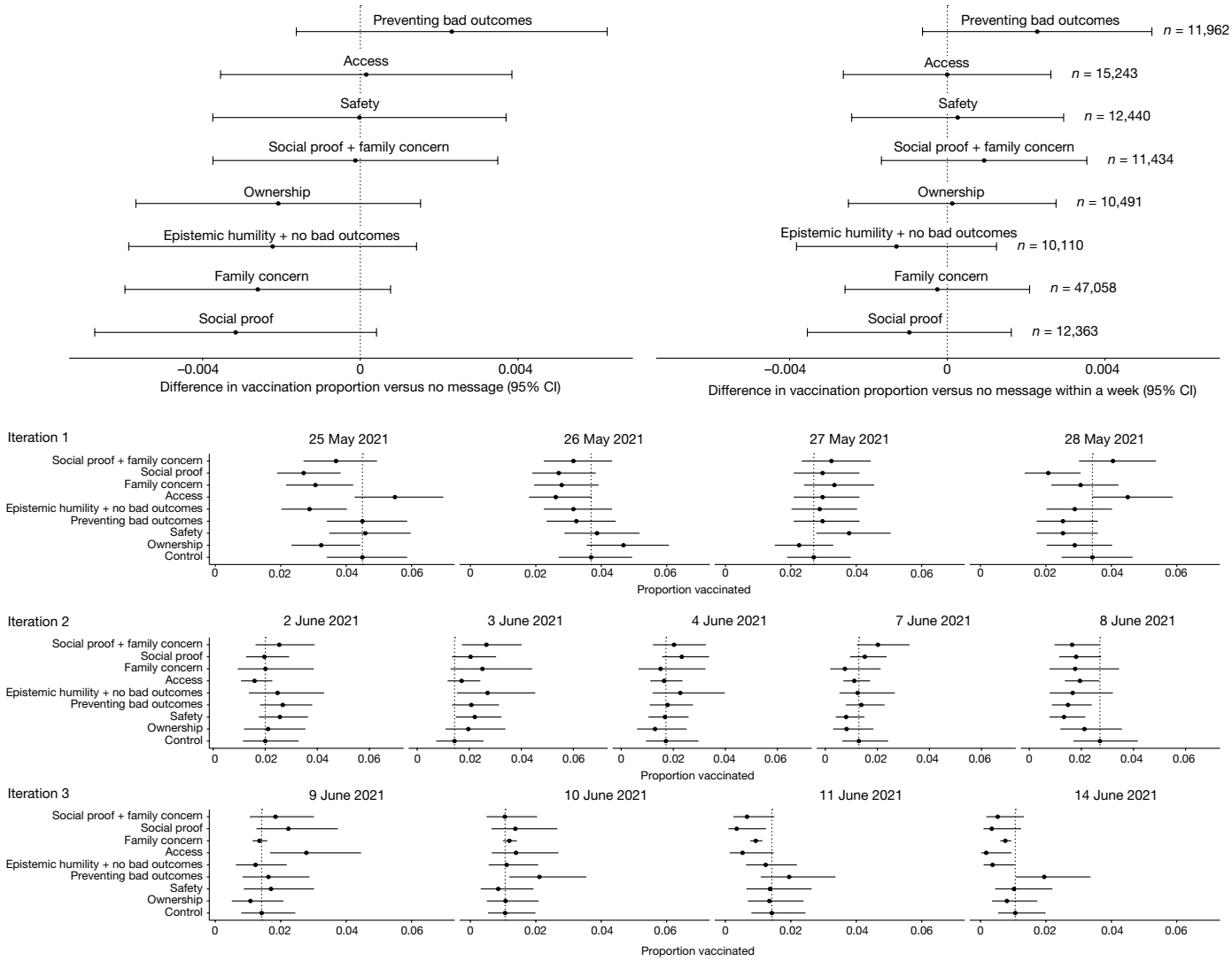

Fig. 1 | Average treatment effects for the eight experimental conditions overall and proportions vaccinated by day. Top left, the differences in the proportion vaccinated by the end of the study between each message condition and the control or 'no message' condition (2% of the control condition was vaccinated within the study period). Top right, the differences in the proportion vaccinated within a week of message sending (1% of the control condition was vaccinated within a week of message sending). The total control condition participation was 11,327. The total size of each arm is shown on the right. All point estimates with 95% confidence intervals (CIs). No adjustment was made for multiple testing as no test cast doubt on the null of no difference. Bottom, proportions vaccinated by 22 June 2021 in each message by the date messages were sent. The grey vertical line shows the proportion vaccinated in the control condition. The 95% confidence intervals for small proportions come from the binomial ensemble method of ref. [17].

Haenszel (CMH) test for 9 (condition) × 2 (outcome) × 13 (day) strata tables or a block-specific weighting, which provides unbiased estimates of intent-to-treat effects and randomization-justified variance calculations.

No SMS message did substantially better or worse than the control whether vaccination rates were measured one week after the messages were sent or at the end of the study period. Figure 1 illustrates the small size of these differences: the largest positive difference was 0.002 for the 'preventing bad outcomes' condition (that is, 2% of control and 2.2% of 'preventing bad outcomes' were vaccinated). Furthermore, we see no evidence of differences in vaccination rates (however measured) between the control and an aggregated 'any message' condition (estimated difference in proportions vaccinated −0.001, 95% confidence interval (CI) −0.004 to 0.001, CMH test, $P = 0.27$), nor between the arms taken all together (CMH test for 9 × 2 × 13 table, $P = 0.12$). For demographics, see Extended Data Table 2; for additional analyses see Supplementary Information sections 3–6.

We find no evidence that a strategy found effective early in the vaccine rollout[6,7] increased COVID-19 vaccination among people who remained unvaccinated five or more weeks after becoming eligible. Public health officials—especially those avoiding or legally barred from mandates—may turn to this strategy to increase vaccination rates among the less enthusiastic but will probably see minimal impact. Dai et al. highlighted a promising, valuable and low-cost tool that can help to increase vaccinations; although our result does not contradict theirs, it does bound the reach of such approaches, a possibility one of their co-authors contemplated elsewhere[10].

One limitation of our study is that the initial recipient list may contain some vaccinated people. Rhode Island residents could get tested at home but vaccinated out of state, and certain sites (for example, Veterans Affairs hospitals) do not need to report individual-level records to the state. Base rates may be inaccurate because of this and other sources of noise (Supplementary Information section 6), although this would not mask treatment effects, as message assignment was

random. Another limitation is that race and ethnicity information is incomplete (Extended Data Table 2).

The study by Dai et al. differed from ours in several ways, including population age (mean age 70 versus 39), message source (recipients' health network versus a state agency), sign-up ease (recipients being directed to a sign-up system versus a page providing vaccination options) and vaccination context (appointments were scarce in February 2021 but abundant by May 2021). Although these factors could account for the different outcomes, flu vaccine findings suggest otherwise: similar interventions have shown success among younger populations[1], when issued by the state[15], and using inconvenient media (mailed letters[4]), and flu vaccines are comparatively easy to procure. One feature that Dai et al. and many flu vaccine studies do share is that they were conducted early in their respective campaigns, whereas ours was not. Notably, a study of older adults found increased uptake of flu vaccines due to postcard messages in October but not November, December or January[8]. Taken together, this suggests that nudges help early in vaccination campaigns, but the efficacy decays. Another COVID-19 study recently made public provides further support[16].

Although we cannot identify the mechanism(s) responsible for decaying efficacy of nudges, the possibilities include novelty effects early on, oversaturation effects later on, different types of hesitancy (logistical barriers versus objections to vaccines), and, especially for COVID-19, increasingly polarized discourse, divergent social norms and differential vaccine knowledge. Future work in public health communication should distinguish these mechanisms to better implement message campaigns. It may also be that short messages effectively encourage those somewhat inclined to vaccinate but cannot move those less inclined, regardless of timing, and with time, the former group shrinks. Despite our null result, nudges may serve foreseeable public health needs (for example, vaccinating children under 5 or promoting boosters) if timed correctly. Indeed, we know of no studies showing reduced vaccinations owing to message campaigns, so they carry little potential harm. However, their ability to move the more reluctant may be limited.

Reporting summary

Further information on experimental design is available in the Nature Research Reporting Summary linked to this paper.

Data availability

The data analysed in this paper were provided by the Rhode Island Department of Health and contains protected health information. To protect privacy, we cannot publicly post individual-level data. Qualified researchers with a valuable research question and relevant approvals including ethical approval can request access to the de-identified data about this trial from the corresponding author. A formal contract will be signed and an independent data protection agency should oversee the sharing process to ensure the safety of the data. Lightly aggregated data that support most of the analyses in this paper can be found at https://github.com/thepolicylab/COVID-SMSExperiment. Some demographic analyses rely on publicly available data from the United States Census Bureau, the United States Department of Housing and Urban Development, the Rhode Island Geographical Information System and the Rhode Island Board of Elections. Copies of these data and, where appropriate, the code that gathered the data are available at https://github.com/thepolicylab/COVID-SMSExperiment.

Code availability

The code to replicate the analyses and figures in the paper and the Extended Data is available at https://github.com/thepolicylab/COVID-SMSExperiment.

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Author contributions N.R., M.S., D.G., J.B., A.T., K.H.W. and D.Y. conceived of and designed the study. Z.O. and K.H.W. oversaw data collection. J.B., K.H.W., D.G. and N.R. conducted the analysis. J.B., K.H.W., D.G., N.R. and M.S. interpreted the data. All authors contributed to the manuscript.

Competing interests The authors declare no competing interests.

Additional information
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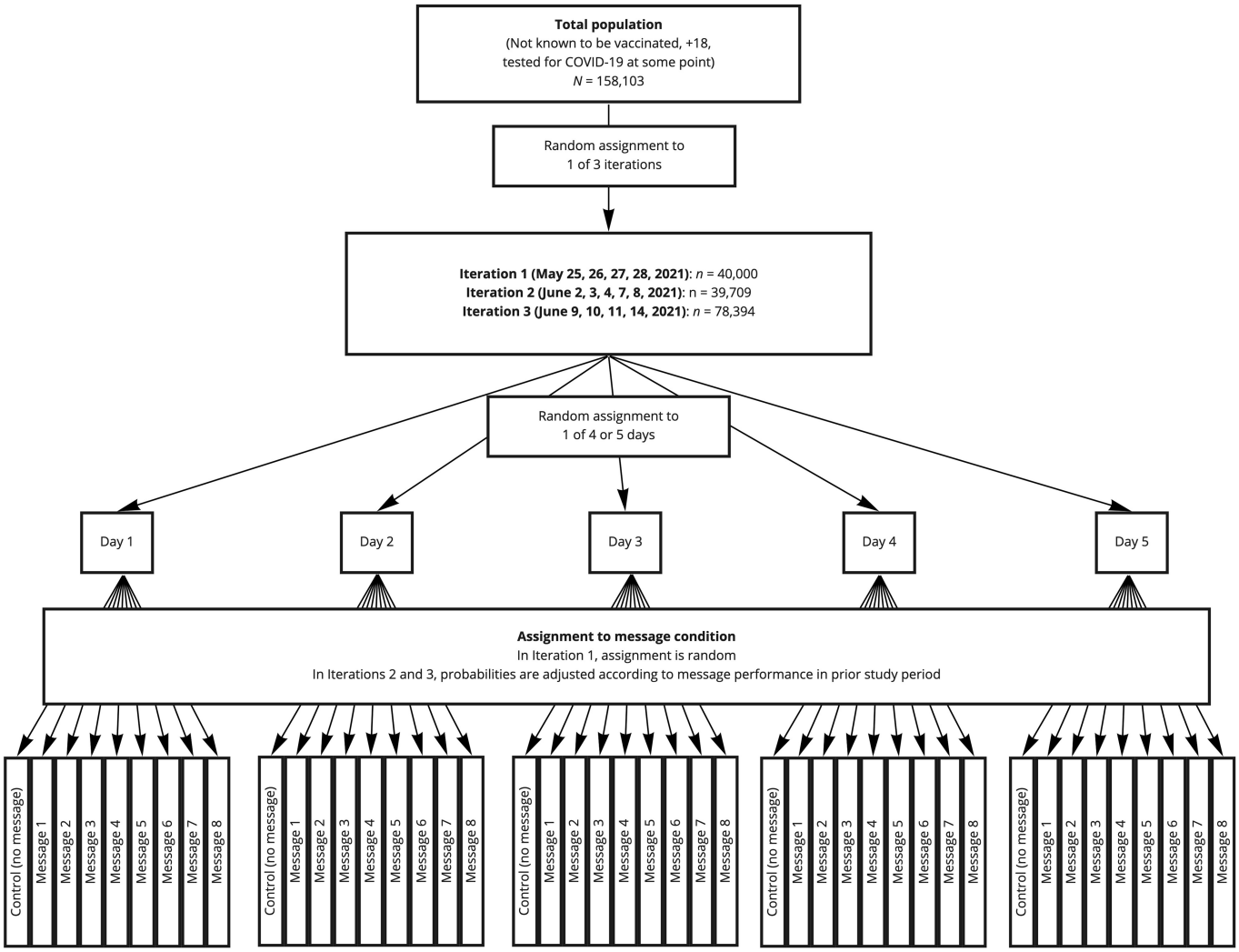

Extended Data Fig. 1 | Randomization scheme and sample. RIDOH maintains separate databases of (a) individuals who have been vaccinated and (b) individuals who have been tested for COVID-19. Vaccination data comes from medical providers and pharmacies receiving vaccines supplied by the State of Rhode Island, who are required to participate in the Rhode Island Child and Adult Immunization Registry (RICAIR) through electronic data reporting. Immunization records can be accessed by an individual's medical provider or by authorized RIDOH users conducting public health surveillance activities including linking vaccination records with the state's COVID-19 testing or case databases to verify information collected during case investigation. COVID-19 testing data (b) is reported to the state through the National Electronic Disease Surveillance System (NEDSS). Our study population is the difference of lists (a) and (b); the resulting database contained 162,504 unique entries. The study ended one day early after RIDOH received complaints about excessive communication. It is unclear how many complaints were received and how many were specifically about this study; other concurrent outreach efforts included SMS messages about COVID-19 testing and phone calls to older adults encouraging vaccination. Nevertheless, leadership halted all such communications out of concern that people would block crucial emergency messages. The final N for the study is 142,428. A small subset of the initial population ($N \approx 800$) had chosen Spanish as their preferred language on testing sign-up forms. While we had initially planned to send this group messages translated into Spanish, an unresolved encoding problem prevented Spanish characters from displaying properly on some cell phones. The project team decided to reintroduce these individuals into the general study population for Iteration 3.

Extended Data Table 1 | Messages used in the RCT and rationales

Condition	Text	FK	Rationale
1. Ownership (baseline prompt)	A COVID-19 vaccine is available for you.	3	At the time of the study, only one messaging RCT that measured vaccination against COVID-19 had been publicly reported.[6] The most effective message in this large trial was conceptually similar to the best performers in two large RCTs measuring vaccination against seasonal flu.[2,18] The core idea is to confer a sense of ownership by informing recipients that vaccines have become available *for them* and may now be claimed. Given the unusually strong evidence for this strategy and the need for a concluding prompt in each message, we used ownership language in all treatment conditions. A baseline condition included only this message. We note as well that survey data indicated that Black and Latinx people were less likely to know that vaccination was free.[19] We therefore emphasized that vaccines are free in all conditions.
2. Safety	More than 150 million people across the nation from diverse backgrounds got the COVID-19 vaccines. They are very safe. A vaccine is available for you.	3.5	Vaccine safety is a perennial concern among those reluctant to vaccinate against COVID-19[20-25] and other diseases.[26,27] Although the long-term effects of COVID-19 vaccines were unknown, the sheer volume of participants in the clinical trials casted doubt on the likelihood of hidden dangers. This kind of information had been shown to be effective in a COVID-related survey experiment[28] and a public opinion poll.[29]
3. Pros of vaccination (implicit choice): preventing bad outcomes	Vaccines are extremely effective at preventing bad COVID-19 outcomes. A vaccine is available for you.	5.4	On one theory of health behavior, people unvaccinated against COVID-19 but considering it weigh the pros and cons of the decision.[30] While an extensive list of advantages and disadvantages was impossible in a short SMS message, one pro was conspicuous: vaccines demonstrably reduce severe disease and hospitalization to near zero. A message emphasizing this was effective in a public opinion poll that did not randomize message assignment.[29] Given that some groups may be reluctant to get vaccinated because they feel their freedom is threatened,[31] a potentially helpful feature of highlighting pros of COVID-19 vaccines is the (true) implication that the message recipient is making a choice.
4. Epistemic humility + pros of vaccination (implicit choice): preventing bad outcomes	We don't fully understand why some people with no medical conditions have bad COVID-19 outcomes. But we do know that vaccines mostly prevent these outcomes. A vaccine is available for you.	5.3	Choosing to get vaccinated requires trust in a community of scientific experts.[32-36] But science is not infallible, and scientists can be wrong. We extended the pros condition to include an acknowledgment of the uncertainty inherent in science. Groups concerned about safety and liberty may be more convinced by claims that COVID-19 vaccines minimize hospitalization when they are coupled with an acknowledgment that other aspects of the disease are not well understood.
5. Access	A free COVID-19 vaccine is available for you at CVS, Walgreens, or Stop & Shop and sites across the state. You don't need an appointment, insurance or other documents.	5.6	Some groups may not get vaccinated because of logistical or structural barriers rather than reluctance. Some of these barriers had been reduced by recent developments, such as the eligibility of all adults and the availability of no-appointment vaccines at CVS or other pharmacies. But people (especially Black and Latinx individuals) may not have realized that they were eligible to be vaccinated or that vaccination is free.[19] In a related finding, an RCT showed increased vaccine uptake due to logistical facilitation (messages that included a map indicating the location of vaccination centers).[5]
6. Family concern	Keep your family safe. A COVID-19 vaccine is available for you.	2.8	Some studies had suggested that appeals to the wellbeing of people's families are superior to appeals to the wellbeing of their communities[37,38] or to no message at all[28,39] when encouraging COVID-related health behaviors including vaccine information search. This framing had also been a conspicuous element in Rhode Island's emergency communications during the pandemic, so we tested its utility in promoting vaccine uptake.
7. Social proof	Over 600,000 Rhode Islanders have already been vaccinated against COVID-19. A vaccine is available for you.	5.7	Our own survey work in Rhode Island and numerous studies nationally[40-42] showed a strong relationship between beliefs about others' COVID-related health behaviors and reports of one's own. This relationship extends to vaccination and is seen in many other health behaviors.[43-45] Some studies had found effects of messages emphasizing the behaviors of others,[46,47] while others had not.[48] We tested the effectiveness of such messages by reporting the (true) number of Rhode Islanders that had been vaccinated to date.
8. Social proof + family concern	Keep your family safe and join the 600,000 Rhode Islanders who have already been vaccinated against COVID-19. A vaccine is available for you.	5.8	One concern with the above strategy is that it could impart a *lack* of necessity to getting vaccinated for recipients unworried about the risks of COVID-19. We therefore coupled the latter message with the emphasis on the wellbeing of people's families.

All messages were preceded by "A message from the Rhode Island Department of Health:" and concluded with "Click here for all the ways to claim your free dose: health.ri.gov/[address unique to the message condition]." Rationales are based on refs. [2,5,6,18–48]. FK, Flesch–Kincaid readability score.

Extended Data Table 2 | Demographics for study population

Age	
Mean	SD
39.14	16.94

	Gender		
	Female	Male	Unknown
count	65310	62446	14700
%	45.85%	43.84%	10.32%

	Race/ethnicity					
	Unknown	White	Hispanic	Black	Other	Declined
count	63810	50286	12157	5659	4867	5677
%	44.79%	35.30%	8.53%	3.97%	3.42%	3.99%

Demographic information was entered by individuals or medical technicians at the time of COVID-19 testing and was voluntary. Thus, this information is incomplete, with missing race and ethnicity values for 45% of individuals and missing gender for 10%. We report the demographics that are known as a partial look at the characteristics of the group.

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☐	☒ A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☐	☒ For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted *Give P values as exact values whenever suitable.*
☒	☐ For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
☒	☐ For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☒	☐ Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	The database of not-yet-vaccinated Rhode Islanders who had previously taken a COVID test was provided to The Policy Lab by the Rhode Island Department of Health (RIDOH) based on its administrative data systems. The outcomes, vaccinations, were also provided by RIDOH using its own administrative systems. The randomization was conducted using python. Code available in https://github.com/thepolicylab/COVID-SMSExperiment. Each day of the study the randomized id numbers were provided to RIDOH who used the Code Red SMS messaging system to send the text messages. Some descriptive analyses rely on publicly available data from the US Census Bureau and Department of Housing and Urban Development, Rhode Island GIS. Some data from the Board of Elections of Rhode Island required scraping from their website. The code to pull and aggregate this data is also available at https://github.com/thepolicylab/COVID-SMSExperiment.
Data analysis	Code for data analysis is available in the public github repository: https://github.com/thepolicylab/COVID-SMSExperiment

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio guidelines for submitting code & software for further information.

Data

Policy information about availability of data

All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A description of any restrictions on data availability
- For clinical datasets or third party data, please ensure that the statement adheres to our policy

The aggregated datasets generated during and/or analysed for the main findings of the current study are available at https://github.com/thepolicylab/COVID-SMSExperiment. Some of the exploratory analyses used ZIP code level data. To protect privacy, we cannot publicly post individual-level data or ZIP code level data. Qualified researchers with a valuable research question and relevant approvals including ethical approval can request access to the de-identified data about this trial from the corresponding author. A formal contract will be signed and an independent data protection agency should oversee the sharing process to ensure the safety of the data.

Demographic data for some of our exploratory analyses came from the US Census Bureau, the US Department of Housing and Urban Development, the Rhode Island Geographical Information System, and the Rhode Island Board of Elections. Copies of this data, and where necessary the code that retrieved the data, are available at https://github.com/thepolicylab/COVID-SMSExperiment.

Field-specific reporting

Please select the one below that is the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

☐ Life sciences ☒ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see nature.com/documents/nr-reporting-summary-flat.pdf

Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	This is a quantitative randomized control trial.
Research sample	The Rhode Island Department of Health (RIDOH) maintains separate databases of individuals who have been vaccinated and tested for COVID-19. Our study population is the difference of these lists (tested but not yet vaccinated) matched through a series of quasi-identifiers and excluding people under 18 when tested (final N = 142,428).
Sampling strategy	We used this entire database. We did not sample.
Data collection	Data were collected by RIDOH using their administrative data systems: vaccinations (the outcome of the analysis) are reported to RIDOH from almost all providers in the state (some providers, such as Veterans Affairs hospitals, are not required to provide data). Exploratory supplementary analyses using ZIP code level census data used publicly available data on those geographic units merged onto the main database.
Timing	May 25, 2021 through June 21, 2021
Data exclusions	No data were excluded from the analysis. Note that the process of merging outcomes ("vaccinated or not") onto the main dataset involved some matching (using three letters of first names, last names, and phone numbers where available). So, some duplicated observations were excluded en route to a clean set of data with one row per participant.
Non-participation	If anyone declined SMS messages from the Rhode Island Department of Health, it occurred before the start of this study. The vaccination outcome information was also collected by RIDOH as a part of its legal obligations to the state.
Randomization	The Policy Lab randomly assigned 8 active messages and 1 control message to subjects with probabilities that varied by iteration (roughly, week of the study), and also by day within iteration (with the same probabilities). Goal of the study was both to 1) learn which messages were more effective so that RIDOH might use them to help overcome vaccine hesitancy at a low cost and 2) increase overall vaccination. Thus we combined a fixed probability randomization within block (day of the study) with an partially adaptive randomization (for weeks 2 and 3). Randomization occurred using python and the adaptive algorithm withheld a proportion ε of the pool for each week for fixed, equal probability randomization and (1 - ε) for adaptive randomization using the Thompson sampling approach. In weeks 1 and 2, ε = 0.25. For week 3, ε = 0.33. See https://github.com/thepolicylab/COVID-SMSExperiment for the code used for randomization, data cleaning and merging, and analysis.

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involved in the study
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology and archaeology
☒	☐ Animals and other organisms
☐	☒ Human research participants
☐	☒ Clinical data
☒	☐ Dual use research of concern

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Human research participants

Policy information about studies involving human research participants

Population characteristics	The Rhode Island Department of Health (RIDOH) maintains separate databases of individuals who have been vaccinated and tested for COVID-19. Our study population is the difference of these lists (tested but not yet vaccinated) matched through a series of quasi-identifiers and excluding people under 18 when tested (final N = 142,428).
Recruitment	RIDOH maintains this database as a part of its COVID surveillance duties.
Ethics oversight	The study was deemed exempt by RIDOH's IRB.

Note that full information on the approval of the study protocol must also be provided in the manuscript.

Clinical data

Policy information about clinical studies

All manuscripts should comply with the ICMJE guidelines for publication of clinical research and a completed CONSORT checklist must be included with all submissions.

Clinical trial registration	https://osf.io/pkhae/
Study protocol	https://osf.io/pkhae/
Data collection	The setting is Rhode Island, USA. Time period is May 25, 2021 through June 21, 2021.
Outcomes	The outcomes were vaccination by end of the study period OR vaccination within a week of being assigned an SMS message.

nature portfolio

https://doi.org/10.1038/s41586-022-04526-2

Supplementary information

Evidence from a statewide vaccination RCT shows the limits of nudges

In the format provided by the authors and unedited

Supplementary Information

SI.1. Message creation

Despite intense research interest in facilitators of and factors associated with vaccine uptake over the last several decades, many systematic reviews find a lack of extensive evidence linking messaging interventions to actual vaccination.[1] If tried and true strategies for promoting seasonal flu, H1N1, HPV, or childhood vaccines had emerged from the literature, we might simply adapt them to the context of COVID-19 despite the pandemic's unique features (e.g., its unprecedented reach or unusual degree of politicization). Since they do not, we have conducted a two-step review process for generating candidate messages (see Figure S1). First, we searched for individual message test studies and judged the strength and relevance of the evidence using the following criteria, with the second in each pair indicating better evidence than the first: (i) measured outcome: self-reported/projected behavior (e.g., intention to get vaccinated) versus actual behavior (measured vaccine uptake); (ii) context: seasonal flu/HPV/other vaccination context versus COVID-19; (iii) random message assignment: no versus yes. Studies that did not meet at least one of the preferred criteria are not included. Most studies were conducted in the United States. Second, we considered the survey work on various demographic groups' concerns about COVID-19 vaccination (which are not uniform) to ensure that some messages in the set conceivably address these concerns. This process generated eight candidate messages, described below and listed in Table 1. The content of the messages was discussed with and approved by RIDOH's communications team and medical directors. Slight changes to wording were necessary to keep readability scores at a desirable level.

Although there is a lack of extensive evidence on the efficacy of particular message strategies, we note that a number of studies find increased intended[2] and actual[3] vaccine uptake due to *any* message at all relative to control (no message) conditions. No studies to our

---

[1] "We were surprised to find that few randomized trials have successfully changed what people think and feel about vaccines, and those few that succeeded were minimally effective in increasing uptake" (Brewer et al., 2018). "Given the paucity of information on effective strategies to address vaccine hesitancy, when interventions are implemented, planning a rigorous evaluation of their impact on vaccine hesitancy/vaccine acceptance will be essential" (Dubé et al. 2015). "...given the complexity of vaccine hesitancy and the limited evidence available on how it can be addressed, identified strategies should be carefully tailored according to the target population, their reasons for hesitancy, and the specific context" (Jarrett et al., 2015). "More high quality research is needed to demonstrate the effects of messaging interventions on actual vaccine uptake" (Lawes-Wickwar et al., 2021). "Overall, there is a lack of good-quality primary studies [on risk messages], and existing interventions are suboptimal" (Parsons et al., 2018).

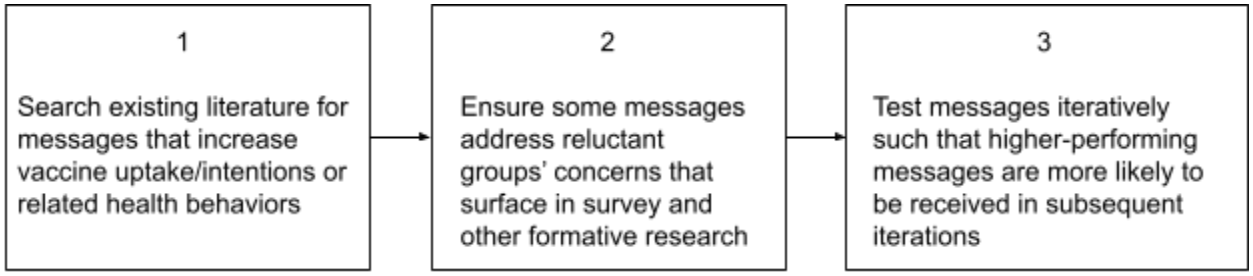
[2] Argote et al., 2021. Messaging interventions that increase COVID-19 vaccine willingness in Latin America.

[3] Esteban-Vasallo et al., 2019. Effect of mobile phone text messaging for improving the uptake of influenza vaccination in patients with rare diseases. Lee et al., 2020. Large-scale influenza vaccination promotion on a mobile app platform: A randomized controlled trial. Regan et al., 2017. Randomized controlled trial of text message reminders for increasing influenza vaccination. Yokum et al., 2018. Letters designed with behavioural science increase influenza vaccination in Medicare beneficiaries. But see OES, 2021. Low-cost interventions to increase vaccination uptake.

knowledge show depressed rates of vaccination due to message reminders. Thus, while some messages may be more effective than others, there was no indication that any would have negative effects on vaccine uptake.

For the complete list of messages used, see Extended Data Table 1.

Figure S1. Message selection process.



SI.2. Message reweighting (adaptive design)

In assigning individual treatments, we employ the following strategy. First, for each week w, we draw N_w individuals from our remaining pool uniformly at random. For each of these N_w people, with probability ϵ_w, we assign them uniformly at random to one of our nine messaging arms. With probability $(1 - \epsilon_w)$, we utilize a Thompson Sampler to assign them one of the nine messaging arms. For this experiment, we took $\epsilon_1 = 1$ (since no information is available at time 1), $\epsilon_2 = 0.25$, and $\epsilon_3 = 0.33$.

Specifically, before each assignment time t, we consider the m_{at} people who had been assigned to arm a by that point. Of those m_{at} individuals, let s_{at} be the number of individuals assigned to arm a who by time t had received a vaccine or scheduled an appointment for a vaccine according to RIDOH's records. Let $f_{at} = m_{at} - s_{at}$ be the number who had not. Let $p_{at} \sim Beta(\alpha + s_{at}, \beta + f_{at})$ be the posterior distribution at time t of the true probability p_a that an individual enrolled in arm a will receive a vaccine. Here we take a $Beta(\alpha, \beta)$ prior distribution for p_a with a hyperprior $\pi(\alpha, \beta) \propto (\alpha + \beta)^{-2.5}$.

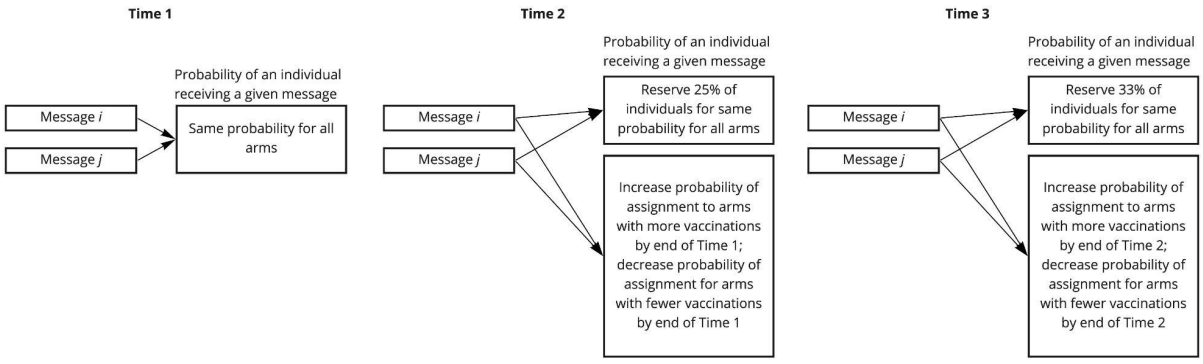
Then if an individual i is to be assigned by the Thompson sampler at time t, we draw realizations q_{ati} from each of the distributions p_{at} and assign individual i to $a_i = argmax_a\, q_{ati}$.

This strategy, which Caria et al. (2020) call Tempered Thompson Sampling, though we prefer the term ϵ-bounded Thompson Sampling, allows us to interpolate between gathering maximum information about each arm (uniform assignment to each arm) and maximizing expected reward at the cost of not gathering information about some arms (Thompson sampling).

Intuitively, the process resembles a Bayesian reasoner: if more people in the message 1 group than in the control group engage in the desired behavior during the first iteration, then during the second iteration the probability that anyone receives message 1 will increase slightly, thus making the design responsive to incoming evidence.

We note that while our ultimate outcome was *vaccination*, our Thompson Sampler utilized sign-ups as well. Some noise entered into this list, due to the deduplication process employed in matching individual testing records. This led to approximately 39% of those phone numbers which had a record of either a vaccination or a sign-up after the first week of the study to reappear in the eligible population after the second week of the study. However, a χ^2 test indicates that this noise was not significantly different between arms ($p = 0.83$). This error limits interpretation of the results to do with adaptation (see SI.4 EQ6) but not those relevant to the primary research question of the study, which is answered through average treatment effects by message condition regardless of the likelihood that a given individual would receive a particular message.

Figure S2. Overview of ϵ-bounded Thompson sampler.



SI.3. Additional pre-registered analyses.

These analyses were registered at https://osf.io/pkhae/. All asymptotic tests were paired with permutation based tests. All results were the same so only asymptotic tests reported here. Below we use the R statistical analysis language and within it the `cmh_test()` function from the coin package (which enables both asymptotic and permutation based randomization inferences) and `lm_robust()` from the `estimatr` package (which produces randomization justified HC2 standard errors by default). Code for all analyses, including permutation tests, is available at http://github.com/thepolicylab/thepolicylab/COVID-SMSExperiment.

RQ0: Is there any effect of condition assignment?

```
rq0_asym <- cmh_test(vaccinatedF ~ messageF | date_sentF, data = wrkdat,
distribution = asymptotic())
rq0_asym
    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by
     messageF (message_0, message_1, message_2, message_3, message_4,
message_5, message_6, message_7, message_8)
     stratified by date_sentF
chi-squared = 13, df = 8, p-value = 0.1
pvalue(rq0_asym)
[1] 0.1153
```

RQ1: Is there an effect of receiving a message as opposed to not receiving a message?

```
rq1_asym <- cmh_test(vaccinatedF ~ not_controlF | date_sentF, data = wrkdat,
distribution = asymptotic())
rq1_asym
    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 1.2, df = 1, p-value = 0.3
pvalue(rq1_asym)
[1] 0.2663
rq1_est <- difference_in_means(vaccinated ~ not_controlF, blocks =
date_sentF, data = wrkdat)
rq1_est
Design:  Blocked
             Estimate Std. Error t value Pr(>|t|)  CI Lower CI Upper     DF
not_controlF -0.00147   0.001363  -1.079   0.2806 -0.004141 0.001201 142402
```

RQ2: Does any given message differ from control (focal tests)?

```
rq2_est <- lm_robust(vaccinated ~ messageF, weights = IPW_weight_multarm,
data = wrkdat)
rq2_est
                     Estimate Std. Error  t value  Pr(>|t|)  CI Lower  CI Upper
DF
(Intercept)        0.02060861   0.001312 15.71145 1.406e-55  0.018038 0.0231795
142419
messageFmessage_1 -0.00208199   0.001842 -1.13002 2.585e-01 -0.005693 0.0015291
142419
messageFmessage_2 -0.00002266   0.001898 -0.01194 9.905e-01 -0.003742 0.0036968
142419
messageFmessage_3  0.00232052   0.002012  1.15339 2.488e-01 -0.001623 0.0062638
142419
messageFmessage_4 -0.00222375   0.001863 -1.19395 2.325e-01 -0.005874 0.0014267
142419
messageFmessage_5  0.00015034   0.001886  0.07972 9.365e-01 -0.003546 0.0038467
142419
messageFmessage_6 -0.00260175   0.001719 -1.51333 1.302e-01 -0.005971 0.0007679
142419
messageFmessage_7 -0.00316289   0.001824 -1.73444 8.284e-02 -0.006737 0.0004113
142419
messageFmessage_8 -0.00012584   0.001844 -0.06826 9.456e-01 -0.003739 0.0034876
142419
## Adding the fixed effects estimates (biased, but more precise/statistically
powerful) No substantive difference. This was not what we pre-registered: we
pre-registered using the unbiased block-size weighting approach above. We
report using the above analysis but present the fixed effects FYI.
rq2_fe_est <- lm_robust(vaccinated ~ messageF, fixed_effects = ~date_sentF,
data = wrkdat)
rq2_fe_est
                      Estimate Std. Error t value Pr(>|t|)  CI Lower  CI Upper     DF
messageFmessage_1 -0.0022322   0.002007 -1.1125  0.26595 -0.006165 0.0017006 142407
messageFmessage_2 -0.0002051   0.001966 -0.1044  0.91688 -0.004058 0.0036474 142407
messageFmessage_3  0.0005308   0.002000  0.2654  0.79071 -0.003390 0.0044514 142407
messageFmessage_4 -0.0029133   0.002013 -1.4469  0.14792 -0.006860 0.0010330 142407
messageFmessage_5 -0.0006247   0.001874 -0.3334  0.73881 -0.004297 0.0030474 142407
messageFmessage_6 -0.0027722   0.001567 -1.7690  0.07689 -0.005844 0.0002992 142407
messageFmessage_7 -0.0034071   0.001902 -1.7915  0.07321 -0.007135 0.0003204 142407
messageFmessage_8  0.0004312   0.002011  0.2144  0.83020 -0.003510 0.0043723 142407
## Verifying the p-values above with a permutation based cmh test for each
message versus control
test_msgs <- function(msg1, msg2) {
  ## msg1 and msg2 are strings indicating message assignment in messageF
  effect_test <- cmh_test(vaccinatedF ~ messageF | date_sentF,
    data = wrkdat,
    subset = wrkdat$messageF %in% c(msg1, msg2),
    distribution = approximate(nresample = 10000, parallel = "multicore",
ncpu = 6)
  )
  return(pvalue(effect_test)[1])
```

```
}
message_test_ps <- sapply(levels(wrkdat$messageF)[-1], function(msg) {
  test_msgs("message_0", msg)
})

message_test_ps
message_1 message_2 message_3 message_4 message_5 message_6 message_7 message_8
   0.2778    1.0000    0.7040    0.1866    0.9335    0.1025    0.0502    0.8985
```

We specified that we would report adjusted p-values, although it is not necessary since we are not reporting any discoveries. We did the adjustment and show it in the Github repository but do not report them here.

RQ3: Does epistemic humility help?

Message 4 vs. 3 (CMH test, difference of proportions estimator). Only very small differences between those two arms.

```
rq3_est <- difference_in_means(vaccinated ~ messageF, blocks = date_sent,
data = wrkdat, subset = wrkdat$messageF %in% c("message_3", "message_4"))
rq3_est
Design:  Blocked
                  Estimate Std. Error t value Pr(>|t|) CI Lower  CI Upper    DF
messageFmessage_4 -0.003194   0.002074   -1.54   0.1235 -0.00726 0.0008707 22046
rq3_test <- test_msgs("message_3", "message_4")
rq3_test
[1] 0.1083
```

RQ5: How do social proof and appeals to the family interact?

We will test the overall hypothesis of no difference between 6 (family concern), 7 (social proof), and 8 (family concern + social proof). If we reject this, we test 6 versus 8 and 7 versus 8.

```
rq5_overall <- cmh_test(vaccinatedF ~ messageF | date_sentF, data = wrkdat,
subset = wrkdat$messageF %in% c("message_6", "message_7", "message_8"))
rq5_overall

    Asymptotic Generalized Cochran-Mantel-Haenszel Test

data:  vaccinatedF by
     messageF (message_6, message_7, message_8)
     stratified by date_sentF
chi-squared = 5.1, df = 2, p-value = 0.08
```

RQ6: Did adaptive randomization increase vaccinations over fixed randomization?

We also will report the effect of using adaptive randomization versus fixed randomization on total vaccinations –- since we withheld 25% of each of the three weeks experimental pools for

fixed randomization and adapted the other 100 – 25%. Our aim in this study was to (1) learn about which messages worked best but also (2) increase vaccination. The fixed randomization maximized statistical power to detect effects whereas the adaptive randomization increased the numbers of people exposed to more effective messages.

```
rq6_est <- difference_in_means(vaccinated ~ is_chosen_from_uniform, blocks =
date_sentF, data = wrkdat, subset = wrkdat$date_sent >= "2021-06-02")
rq6_est
Design:  Blocked
                       Estimate Std. Error t value Pr(>|t|)   CI Lower CI Upper
DF
is_chosen_from_uniform 0.0009958  0.0008211   1.213   0.2252 -0.0006135 0.002605
102410
rq6_cmh_perm <- cmh_test(vaccinatedF ~ factor(is_chosen_from_uniform) |
date_sentF, data = wrkdat, subset = wrkdat$date_sent >= "2021-06-02",
distribution = approximate(nresample = 10000, parallel = "multicore", ncpus =
6))
rq6_cmh_perm
    Approximative Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by
     factor(is_chosen_from_uniform) (FALSE, TRUE)
     stratified by date_sentF
chi-squared = 1.5, p-value = 0.2
```

SI.4. Exploratory analyses, pre-registered

EQ1: Do explicit appeals to the safety of vaccines increase responses in areas with higher proportions of Black or Latinx people? Message 2 vs. control

We cannot detect any simple linear differential effect of pct black or latinx on the message 2 versus control comparison.versus control comparison.

```
wrkdat3_eq1 <- wrkdat3 %>%
  filter(messageF %in% c("message_0", "message_2") & zcta != "00000") %>%
  droplevels()

make_weights <- function(dat) {
  block_m_each <- with(dat, table(date_sentF, messageF, exclude = c()))
  block_prob_each <- block_m_each / rowSums(block_m_each)
  declared_randomization <- declare_ra(blocks = dat$date_sentF, block_m_each
= block_m_each, conditions = sort(unique(dat$messageF)))
  IPW_weight <- 1 / obtain_condition_probabilities(declaration =
declared_randomization, assignment = dat$messageF)
  stopifnot(all.equal(sort(unique(1 / IPW_weight)),
sort(unique(block_prob_each))))
  return(IPW_weight)
}

wrkdat3_eq1$IPW_eq1 <- make_weights(wrkdat3_eq1)
eq1_blk_estA <- lm_robust(vaccinated ~ messageF * pct_any_blk, data =
wrkdat3_eq1, weights = IPW_eq1)
```

```
eq1_blk_estA
                            Estimate Std. Error   t value  Pr(>|t|)  CI Lower CI Upper    DF
(Intercept)                 0.0188928   0.002016  9.370867 7.829e-21  0.014941 0.022845 23245
messageFmessage_2          -0.0002165   0.002783 -0.077775 9.380e-01 -0.005672 0.005239 23245
pct_any_blk                 0.0538385   0.018998  2.833943 4.602e-03  0.016602 0.091075 23245
messageFmessage_2:pct_any_blk -0.0002156   0.026120 -0.008253 9.934e-01 -0.051413 0.050982 23245
```

```
eq1_lat_estA <- lm_robust(vaccinated ~ messageF * pct_hisp, data =
wrkdat3_eq1, weights = IPW_eq1)
```

```
eq1_lat_estA
                           Estimate Std. Error t value  Pr(>|t|)  CI Lower CI Upper    DF
(Intercept)                0.018012   0.001849  9.7392 2.264e-22  0.014387 0.021637 23245
messageFmessage_2          0.001080   0.002534  0.4262 6.700e-01 -0.003887 0.006048 23245
pct_hisp                   0.034689   0.009438  3.6756 2.378e-04  0.016191 0.053187 23245
messageFmessage_2:pct_hisp -0.007924   0.012534 -0.6322 5.272e-01 -0.032492 0.016643 23245
```

EQ2: Does the implication of choice through emphasis on a conspicuous advantage increase responses in areas with higher proportions of Republican people? Message 3 vs. control

No detectable difference in effects.

```
wrkdat3_eq2 <- wrkdat3 %>%
  filter(messageF %in% c("message_0", "message_3") & zcta != "00000") %>%
  droplevels()
wrkdat3_eq2$IPW_eq2 <- make_weights(wrkdat3_eq2)

eq2_gop_estA <- lm_robust(vaccinated ~ messageF * pct_gop, data =
wrkdat3_eq2, weights = IPW_eq2)
eq2_gop_estA
                       Estimate Std. Error t value  Pr(>|t|) CI Lower  CI Upper    DF
(Intercept)            0.033399   0.00480  6.9582 3.541e-12  0.02399  0.042808 22768
messageFmessage_3     -0.002249   0.00661 -0.3402 7.337e-01 -0.01520  0.010707 22768
pct_gop               -0.027623   0.01238 -2.2321 2.562e-02 -0.05188 -0.003366 22768
messageFmessage_3:pct_gop  0.009509   0.01705  0.5576 5.771e-01 -0.02392  0.042935 22768
```

EQ3: Do explicit appeals to ease of access increase responses in areas with higher proportions of Black or Latinx people? Message 5 vs. control

No detectable differences. Magnitude of moderation is not small given this phenomenon (on order of 1 or 2 pts, but negative).

```
wrkdat3_eq3 <- wrkdat3 %>%
  filter(messageF %in% c("message_0", "message_5") & zcta != "00000") %>%
  droplevels()
wrkdat3_eq3$IPW_eq3 <- make_weights(wrkdat3_eq3)

eq3_blk_estA <- lm_robust(vaccinated ~ messageF * pct_any_blk, data =
wrkdat3_eq3, weights = IPW_eq3)
eq3_blk_estA
                           Estimate Std. Error t value  Pr(>|t|) CI Lower CI Upper    DF
(Intercept)                0.018567   0.002043  9.0861 1.098e-19  0.01456 0.022572 25930
messageFmessage_5          0.001946   0.002743  0.7095 4.780e-01 -0.00343 0.007322 25930
pct_any_blk                0.051307   0.019179  2.6752 7.473e-03  0.01372 0.088898 25930
messageFmessage_5:pct_any_blk -0.026276   0.025041 -1.0493 2.940e-01 -0.07536 0.022805 25930
eq3_lat_estA <- lm_robust(vaccinated ~ messageF * pct_hisp, data =
wrkdat3_eq3, weights = IPW_eq3)

eq3_lat_estA
                         Estimate Std. Error t value  Pr(>|t|)  CI Lower CI Upper    DF
(Intercept)              0.017433   0.001874  9.3041 1.455e-20  0.013760  0.02111 25930
messageFmessage_5        0.001536   0.002512  0.6114 5.410e-01 -0.003388  0.00646 25930
pct_hisp                 0.034860   0.009734  3.5814 3.424e-04  0.015782  0.05394 25930
messageFmessage_5:pct_hisp -0.011863   0.012585 -0.9426 3.459e-01 -0.036531  0.01280 25930
```

EQ4: Does epistemic humility increase responses in areas with higher proportions of either Black or Latinx people or Republican people? Message 4 versus 3

No detectable differences in effect.

```
wrkdat3_eq4 <- wrkdat3 %>%
  filter(messageF %in% c("message_3", "message_4") & zcta != "00000") %>%
  droplevels()

wrkdat3_eq4$IPW_eq4 <- make_weights(wrkdat3_eq4)

eq4_gop_estA <- lm_robust(vaccinated ~ messageF * pct_gop, data =
wrkdat3_eq4, weights = IPW_eq4)
eq4_gop_estA
                      Estimate Std. Error t value  Pr(>|t|) CI Lower CI Upper    DF
(Intercept)           0.031424   0.004622  6.7992 1.079e-11  0.02237 0.040483 21573
messageFmessage_4    -0.002244   0.006677 -0.3360 7.369e-01 -0.01533 0.010844 21573
pct_gop              -0.018053   0.011944 -1.5115 1.307e-01 -0.04146 0.005358 21573
messageFmessage_4:pct_gop -0.003277   0.017226 -0.1902 8.491e-01 -0.03704 0.030487 21573
eq4_blk_estA <- lm_robust(vaccinated ~ messageF * pct_any_blk, data =
wrkdat3_eq4, weights = IPW_eq4)
eq4_blk_estA
                          Estimate Std. Error  t value  Pr(>|t|)  CI Lower CI Upper    DF
(Intercept)               0.019125   0.002084  9.17906 4.730e-20  0.015041  0.02321 21573
messageFmessage_4        -0.003302   0.002909 -1.13507 2.564e-01 -0.009003  0.00240 21573
pct_any_blk               0.067888   0.020681  3.28267 1.030e-03  0.027352  0.10842 21573
messageFmessage_4:pct_any_blk -0.001599   0.028836 -0.05547 9.558e-01 -0.058120  0.05492 21573
eq4_lat_estA <- lm_robust(vaccinated ~ messageF * pct_hisp, data =
wrkdat3_eq4, weights = IPW_eq4)
eq4_lat_estA
                       Estimate Std. Error t value  Pr(>|t|)  CI Lower CI Upper    DF
(Intercept)            0.018922   0.001878 10.0774 7.851e-24  0.015242 0.022603 21573
messageFmessage_4     -0.002755   0.002664 -1.0345 3.009e-01 -0.007976 0.002465 21573
pct_hisp               0.038470   0.009790  3.9294 8.542e-05  0.019280 0.057660 21573
messageFmessage_4:pct_hisp -0.004235   0.013966 -0.3032 7.617e-01 -0.031610 0.023140 21573
```

EQ5: Is there a day-of-week effect? Proportions of vaccinations collapsed across all messages by day.

Since the randomization to message occurred **within a given date** and we have relatively few weeks, it is difficult to disentangle day of week effects from date effects. So, we only present descriptive information here.

```
table(weekdays(wrkdat3$date_sent))

   Friday    Monday  Thursday   Tuesday Wednesday
    33626     23621     33619     17940     33622
wrkdat3$weekday_sent <- weekdays(wrkdat3$date_sent)

wrkdat3_weekday <- wrkdat3 %>%
  group_by(weekday_sent) %>%
  summarize(
```

```
    prop_vac = mean(vaccinated),
    prop_vac_in_week = mean(vac_in_week), nweek = n()
  )

wrkdat3_weekday
# A tibble: 5 x 4
  weekday_sent prop_vac prop_vac_in_week nweek
  <chr>           <dbl>            <dbl> <int>
1 Friday         0.0181          0.00803 33626
2 Monday         0.00923         0.00703 23621
3 Thursday       0.0195          0.00943 33619
4 Tuesday        0.0294          0.0109  17940
5 Wednesday      0.0218          0.0102  33622
```

EQ6: Is there an iteration effect? Some people were randomly assigned to have 3 weeks to schedule a vaccination and others only 1 week before the study ended. We explore whether there is a difference here.

The following table shows that we have no strong arguments against the claim that our messages were the same as control in regards either vaccination at all or vaccination within a week, regardless of whether the messages were sent in the first, second, or third weeks of the study. We present raw p-values here because this is exploratory work and do not adjust because we have so few small p-values: for example, we are not interpreting the effect of message_7 in week 1 or the effect of message_3 in week 3 below as discoveries.

```
test_msgs2 <- function(msg1, msg2, the_iteration, thefmla = vaccinatedF ~
messageF | date_sentF) {
  ## msg1 and msg2 are strings indicating message assignment in messageF
  effect_test <- cmh_test(thefmla,
    data = wrkdat3,
    subset = wrkdat3$messageF %in% c(msg1, msg2) & wrkdat3$iteration ==
the_iteration,
    distribution = asymptotic()
# no difference when we used permutation tests, for speed switching to
asymptotic tests
# approximate(nresample = 10000, parallel = "multicore", ncpu = 6)
  )
  return(pvalue(effect_test)[1])
}

msg_by_iteration <- as_tibble(expand.grid(iteration = 1:3, messageF =
levels(wrkdat3$messageF)[-1], stringsAsFactors = FALSE))

msg_by_iteration <- msg_by_iteration %>%
  rowwise() %>%
  mutate(p_vs_ctrl = test_msgs2("message_0", messageF, iteration)) %>%
  arrange(iteration, messageF)
```

```
msg_by_iteration <- msg_by_iteration %>%
  rowwise() %>%
  mutate(p_vac_week_vs_ctrl = test_msgs2("message_0", messageF, iteration,
thefmla = vac_in_weekF ~ messageF | date_sentF))

msg_by_iteration <- msg_by_iteration %>% mutate(p_vac_week_vs_ctrl =
ifelse(p_vac_week_vs_ctrl == p_vs_ctrl, NA, p_vac_week_vs_ctrl))
print(msg_by_iteration, n = 100)
# A tibble: 24 x 4
# Rowwise:
   iteration messageF  p_vs_ctrl p_vac_week_vs_ctrl
       <int> <chr>         <dbl>              <dbl>
 1         1 message_1   0.414              0.624
 2         1 message_2   0.777              0.776
 3         1 message_3   0.485              0.554
 4         1 message_4   0.0950             0.151
 5         1 message_5   0.432              0.999
 6         1 message_6   0.174              0.368
 7         1 message_7   0.00849            0.185
 8         1 message_8   0.911              0.458
 9         2 message_1   0.601              0.188
10         2 message_2   0.685              0.655
11         2 message_3   0.870              0.127
12         2 message_4   0.510              0.215
13         2 message_5   0.369              0.357
14         2 message_6   0.741              0.250
15         2 message_7   0.732              0.298
16         2 message_8   0.289              0.0548
17         3 message_1   0.541              0.731
18         3 message_2   0.988              0.893
19         3 message_3   0.0394             0.127
20         3 message_4   0.321              0.203
21         3 message_5   0.944              0.680
22         3 message_6   0.318              0.517
23         3 message_7   0.567              0.523
24         3 message_8   0.405              0.469
```

Nor is there strong evidence that “any message” was better than control, even when we assess the relationships for each iteration separately:

```
rq8_iteration1_test <- cmh_test(vaccinatedF ~ not_controlF | date_sentF, data
= wrkdat3, subset = wrkdat3$iteration == 1)
rq8_iteration1_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 1, df = 1, p-value = 0.3
rq8_iteration2_test <- cmh_test(vaccinatedF ~ not_controlF | date_sentF, data
= wrkdat3, subset = wrkdat3$iteration == 2)
```

```
rq8_iteration2_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 0.0055, df = 1, p-value = 0.9
rq8_iteration3_test <- cmh_test(vaccinatedF ~ not_controlF | date_sentF, data
= wrkdat3, subset = wrkdat3$iteration == 3)
rq8_iteration3_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vaccinatedF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 0.57, df = 1, p-value = 0.5
## Also looking at vaccinations within a week for the first iteration
rq9_iteration1_test <- cmh_test(vac_in_weekF ~ not_controlF | date_sentF,
data = wrkdat3, subset = wrkdat3$iteration == 1)
rq9_iteration1_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vac_in_weekF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 0.36, df = 1, p-value = 0.6
rq9_iteration2_test <- cmh_test(vac_in_weekF ~ not_controlF | date_sentF,
data = wrkdat3, subset = wrkdat3$iteration == 2)
rq9_iteration2_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test
data:  vac_in_weekF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 1.9, df = 1, p-value = 0.2
rq9_iteration3_test <- cmh_test(vac_in_weekF ~ not_controlF | date_sentF,
data = wrkdat3, subset = wrkdat3$iteration == 3)
rq9_iteration3_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test=
data:  vac_in_weekF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 0.34, df = 1, p-value = 0.6
```

SI.5. Exploratory Analyses, not pre-registered

Effects on vaccination within a week

The experiment ran during a time of national campaigns in favor of vaccination. The control group in our experiment would have been exposed to this, and thus, might have gotten vaccinated for reasons other than a nudge from a text message.

No strong evidence that people were likely to be vaccinated within a week in "any message" versus control or versus any given message.

```
rq7_test <- cmh_test(vac_in_weekF ~ not_controlF | date_sentF, data = wrkdat)
rq7_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test

data:  vac_in_weekF by not_controlF (0, 1)
     stratified by date_sentF
chi-squared = 0.00041, df = 1, p-value = 1
rq7a_test <- cmh_test(vac_in_weekF ~ messageF | date_sentF, data = wrkdat)
rq7a_test

    Asymptotic Generalized Cochran-Mantel-Haenszel Test

data:  vac_in_weekF by
     messageF (message_0, message_1, message_2, message_3, message_4,
message_5, message_6, message_7, message_8)
     stratified by date_sentF
chi-squared = 8.7, df = 8, p-value = 0.4
rq7a_est <- lm_robust(vac_in_week ~ messageF, weights = IPW_weight_multarm,
data = wrkdat)
rq7a_est
                    Estimate Std. Error   t value  Pr(>|t|)   CI Lower CI Upper     DF
(Intercept)        0.009127748  0.0009266  9.851260 6.884e-23  0.0073117 0.010944 142419
messageFmessage_1  0.000124368  0.0013438  0.092548 9.263e-01 -0.0025095 0.002758 142419
messageFmessage_2  0.000262444  0.0013718  0.191314 8.483e-01 -0.0024263 0.002951 142419
messageFmessage_3  0.002280338  0.0014842  1.536381 1.244e-01 -0.0006287 0.005189 142419
messageFmessage_4 -0.001289688  0.0012946 -0.996239 3.191e-01 -0.0038270 0.001248 142419
messageFmessage_5 -0.000005737  0.0013428 -0.004273 9.966e-01 -0.0026376 0.002626 142419
messageFmessage_6 -0.000254321  0.0011945 -0.212902 8.314e-01 -0.0025956 0.002087 142419
messageFmessage_7 -0.000963766  0.0013199 -0.730162 4.653e-01 -0.0035508 0.001623 142419
messageFmessage_8  0.000933308  0.0013295  0.702014 4.827e-01 -0.0016724 0.003539 142419
```

SI.6. Sources of noise in the dataset

As with any study drawing on government data, noise in our initial dataset is inevitable. Dai and colleagues independently estimated base rates of vaccination in Rhode Island during the study period adjusted for the randomization scheme used here and found a rate of 5.67%, almost three times the rate in our data. This raises two questions: (1) Could the discrepancy reflect some systematic difference between our population and the true population of vaccine-hesitant people that undermines our interpretation of the null result? And (2) could the discrepancy reflect a problem with the initial dataset such that the null is explained by insufficient power to detect a true effect?

SI.6.1. Does the base rate discrepancy suggest underlying characteristics of our population that undermine our interpretation?

A possible explanation for the discrepancy is that noise inherent in our dataset depressed the observed base rate. Almost certainly, some phone numbers were entered erroneously by participants or testing site staff, while others reflected individuals tested in Rhode Island but vaccinated elsewhere. Providence, the largest city, has a transient student population, and many Rhode Islanders work in neighboring states, both of which could affect where an individual got vaccinated. These factors would effectively increase the initial sample size of our study by making it appear that we had more phone numbers of unvaccinated residents than we actually did, thus decreasing our observed base rate by increasing our denominator.

We also note that some people share phone numbers, especially those in group quarters such as nursing homes. Only the first person vaccinated with a particular phone number would count in our numerator (if no one with that phone number had been vaccinated by the start of our study) or denominator. Of course, people would not receive text messages at all if they provided a land line—an issue we return to in section SI.6.2.

Another possibility is that design features contributed to the discrepancy. As far as we could tell, Dai et al.'s estimate did not remove people under 18, a population that was excluded from our study but could and did get vaccinated during the study period. To the extent that minors were helping to drive the state's topline numbers but were not in our study, our base rate would be lower than the state's; our own calculations suggest this was the case to a small degree. Moreover, as noted in the original manuscript, some vaccination sites like Veterans hospitals report aggregate numbers but not individual vaccination information to the state, which would contribute to topline vaccination rates without being matched to an individual. Despite these sources of uncertainty in our initial dataset—a problem for most research using administrative data—the entries were randomly assigned. So invalid phone numbers or people who got vaccinated out of state were no more or less likely to appear in treatment or control conditions. Moreover, we do not see how our population of ostensibly vaccine-hesitant individuals is

confounded by omitting adolescents[4], veterans,[5] or VA hospital staff[6], since these groups have been less—or at best, equally—likely to get vaccinated relative to the general population. If anything, their exclusion from our population should push the result toward seeing a treatment effect rather than not.

In sum, while there are several, non-exclusive, possibilities as to why our base rate is lower than Dai and colleagues' estimate, none seemingly account for the observed lack of message effects and therefore threaten our interpretation that the critical time window for nudges had passed.

SI.6.2. If the base rate discrepancy were due to invalid entries, would it change the result?

Random assignment aside, we might ask whether invalid entries and/or artificial inflation of sample size meant the true population size did not provide sufficient power to detect an effect. Let us consider the worst-case scenario where two-thirds of our initial dataset was invalid.

In this scenario, the number of outcomes coded as non-vaccinations would have decreased by two-thirds in each arm. Excluding these theoretical bad entries would increase the base rate while decreasing sample size. Although Milkman et al.[7] were sufficiently powered to find a ~2% increase in flu vaccinations due to the message we used as a base text with a sample size (N = 47,306) roughly equivalent to one-third of ours (142,428/3 = 47,476), we nonetheless thought it appropriate to examine the issue further with an example. Consider that we recorded 300 vaccinations in the "Access" message condition and about 265 vaccinations in the control condition during the study period. Now we may imagine an experiment with this outcome and roughly 15,000 people in the treatment condition and 15,000 in the control condition. Without blocking or other complex design features, this would yield an effect estimate of about .002 = (300/15000) – (265/15000) and p = .14 using Neyman-style randomization inference from the R command lm_robust() (see https://github.com/thepolicylab/COVID-SMSExperiment for code and analysis). What would we see if instead of 15,000 total observations, we had only one-third that number? We would increase our effect size (300/5000) – (265/5000) = .007, but the loss of sample size would not appreciably improve our statistical power (p = .13). In order for the reduction of our denominator to make an increased effect size that is statistically significant, we would need to reduce the sample to 600 people in each arm (300 / 600) – (265 / 600) = .06, p = .04. Although we have noted several likely sources of noise above, it is extremely unlikely that 96% (1 – 600/15000 = .96) of our initial entries were invalid due to these sources. Even if

[4] Scherer et al. (2021). Acceptability of adolescent COVID-19 vaccination among adolescents and parents of adolescents — United States, April 15–23, 2021. *MMWR*.

[5] Thorpe et al. (2021). Communicating about COVID-19 vaccine development and safety. *medRχiv*.

[6] Meyer et al. (2021). Trends in health care worker intentions to receive a COVID-19 vaccine and reasons for hesitancy. *JAMA*.

[7] Milkman et al. (2021). A megastudy of text-based nudges encouraging patients to get vaccinated at an upcoming doctor's appointment. *PNAS*.

two-thirds of our entries were invalid, which we also consider unlikely, a true effect would not be masked by insufficient power.