

NBER WORKING PAPER SERIES

THE EFFECTIVENESS OF DIGITAL INTERVENTIONS ON  
COVID-19 ATTITUDES AND BELIEFS

Susan Athey  
Kristen Grabarz  
Michael Luca  
Nils C. Wernerfelt

Working Paper 30273  
<http://www.nber.org/papers/w30273>

NATIONAL BUREAU OF ECONOMIC RESEARCH  
1050 Massachusetts Avenue  
Cambridge, MA 02138  
July 2022, Revised October 2022

We thank CARE US, Direct Relief, and the Kaiser Family Foundation for granting us permission to use their ads. We also thank Kang-Xing Jin and the health group at Meta Platforms for driving the creation of these public health interventions in the first place. Author listing is alphabetical, and all participated significantly in the design, analysis, and writing. S.A. acknowledges funding from the Golub Capital Social Impact Lab at Stanford Graduate School of Business. Funding for this research was provided in part by Harvard Business School. Funding to hire K.G. as a part-time contractor to work on this project came from Meta Platforms. Competing Interests: N.W. is an employee of Meta Platforms and owns stock. K.G. is a part-time contractor through PRO Unlimited, a contracting agency used by Meta Platforms. S.A. received funding from Meta Platforms for research projects related to public health, and she previously provided consulting services to Meta. M.L. declares no competing interests. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2022 by Susan Athey, Kristen Grabarz, Michael Luca, and Nils C. Wernerfelt. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

The Effectiveness of Digital Interventions on COVID-19 Attitudes and Beliefs  
Susan Athey, Kristen Grabarz, Michael Luca, and Nils C. Wernerfelt  
NBER Working Paper No. 30273  
July 2022, Revised October 2022  
JEL No. D0,I0,M0

### **ABSTRACT**

Public health organizations increasingly use social media advertising campaigns in pursuit of public health goals. In this paper, we evaluate the impact of about \$40 million of social media advertisements that were run and experimentally tested on Facebook and Instagram, aimed at increasing COVID-19 vaccination rates in the first year of the vaccine roll-out. The 819 randomized experiments in our sample were run by 174 different public health organizations and collectively reached 2.1 billion individuals in 15 languages. We find that these campaigns are, on average, effective at influencing self-reported beliefs - shifting opinions close to 1% at baseline with a cost per influenced person of about \$3.41. Combining this result with an estimate of the relationship between survey outcomes and vaccination rates derived from observational data yields an estimated cost per additional vaccination of about \$5.68. There is further evidence that campaigns are especially effective at influencing users' knowledge of how to get vaccines. Our results represent, to the best of our knowledge, the largest set of online public health interventions analyzed to date.

Susan Athey  
Graduate School of Business  
Stanford University  
655 Knight Way  
Stanford, CA 94305  
and NBER  
athey@stanford.edu

Kristen Grabarz  
Harvard Business School  
Soldiers Field  
Boston, MA 02163  
grabarzkristen@gmail.com

Michael Luca  
Harvard Business School  
Soldiers Field  
Boston, MA 02163  
and NBER  
mluca@hbs.edu

Nils C. Wernerfelt  
Meta Platforms  
1 Hacker Way  
Menlo Park, CA 94025  
nilsw@fb.com

# Introduction

Throughout the rapidly evolving COVID-19 pandemic, policymakers and public health agencies needed to communicate with citizens about mitigation measures ranging from mask wearing and social distancing to vaccines. Advertising on social media emerged as a popular channel to quickly reach large numbers of people, and has been used by public health organizations in nearly every country both to convey information and influence behavior. An understanding of the expected impact of these campaigns is important as such organizations continue to engage in interventions as the pandemic unfolds. Assessing these campaigns is further valuable as digital public health interventions become increasingly used to address broader health-related outcomes.

To speak to these questions, this paper aims to evaluate the impact of social media advertisements on a variety of COVID-19-related outcomes. Analyzing advertising campaigns run on Facebook and Instagram by 174 public health organizations around the world, we investigate three main questions. First, what effect did these social media advertising campaigns have? Second, how cost effective were they? Third, which types of outcomes have the campaigns been most effective at influencing?

The campaigns in our sample were run between December 2020 and November 2021, reached users in nearly every country, and in aggregate consist of \$39.4 million dollars of advertising spending. They include a wide range of public health organizations that span major multinational nonprofits, public health ministries, and local non-governmental organizations.<sup>1</sup> Importantly, our data set contains the near universe of relevant experiments that were run on Facebook and Instagram over this period.<sup>2</sup> This feature of our sample allows us to draw conclusions that are not vulnerable to selection biases that commonly arise in meta-analyses, such as publication bias, whereby experiments with positive outcomes are more likely to be included in the sample. To our knowledge, the data set we analyze is the largest set of online public health interventions studied to date.

The data have two key features relevant for our analysis. First, we have data from a large number of the campaigns that conducted experiments where exposure to the ads was randomized at the user level, allowing us to assess the causal effect of each campaign. This is especially important in the context of online advertisements, where selection bias is a significant obstacle in non-experimental data [27, 23, 12, 42]. Randomized experiments have become more common in online advertising, and companies (including Meta) have developed standardized experimentation tools to facilitate testing. The campaigns we analyze all used these tools to conduct experiments.

Second, we are able to combine the experiments with user-level survey data for a subset of users. The surveys ask a variety of questions - namely, a user's willingness to get a COVID-19 vaccine, belief in the importance of vaccination, belief in vaccine effectiveness, belief in vaccine

---

<sup>1</sup>The identities of the individual advertisers are not included in this article to protect their confidentiality.

<sup>2</sup>We discuss the nature of these experiments in more detail below and in the Supplemental Material (SM). We are able to identify all campaigns that made use of the platform's standardized "Brand Lift Study" infrastructure as defined later, and asked one or more questions about COVID-19 vaccines. The vast majority of experiments identified this way are included in our analysis; the only ones that were excluded were a small number of experiments that chose to ask customized COVID-related questions that do not fall into the seven types that we study. We note a primary alternative approach for advertisers to conduct randomized experiments is to randomize advertising exposure by geographical region and then compare aggregate outcomes, measured separately, across geographies. We are aware of a handful of such experiments that were run during the time window we consider; these are not included in our analysis because we do not have access to the details of these experiments.

safety, whether the advertiser is a trustworthy source of COVID-19 information, how knowledgeable the user feels about how to get the vaccines, and whether they think vaccines are socially acceptable.<sup>3</sup> While not all advertisers asked all questions, the survey questions were largely standardized across campaigns, facilitating comparisons. As is common practice with such experiments, we classify responses into a binary outcome according to whether respondents were engaging in the public health behavior of interest (for instance, intending to get the vaccine) or had the relevant public health information (for instance, knowing where to get the vaccine). Looking across all studies, we can then see whether interventions had an impact on the binary outcome of interest.<sup>4</sup> This approach is similar in spirit to the approach taken by other papers that aggregate a set of experiments with distinct outcomes.<sup>5</sup>

Overall, our combined findings suggest that these campaigns were effective at influencing people’s attitudes and beliefs about the vaccine. We find an average increase in the fraction of positive responses of 0.55 percentage points ( $p = 2e-13$ ) across all experiments, with a baseline 55.7% positive rate. While this point estimate is small on a per person basis, the reach of the campaigns implies that even under conservative assumptions, around 11.6 million individuals were influenced by these campaigns alone, at a cost of about \$3.41 per incremental person. Translating this estimate into a cost for incremental vaccinations requires additional assumptions and data; the survey outcome can be considered a “surrogate” for the outcome of interest, vaccination [5].<sup>6</sup> In the SM, we present the results of an analysis where we use data from the U.S. to estimate the correlation between country-level vaccination rates and county-level survey responses, finding a correlation of .6 (standard error .0174) in a sample of 2,710 counties with more than 20 survey responses. Using this estimate together with the result of our meta-analysis implies that the cost an incremental vaccine is \$5.68. These estimates suggest that campaigns may be an easily scalable intervention that can in aggregate shape the public health outcomes of a large number of citizens.<sup>7</sup>

These results can be broadly compared to those from other initiatives aimed at influencing vaccination decisions.<sup>8</sup> [7] and [45] estimate a \$68 and \$49 cost per incremental COVID vacci-

---

<sup>3</sup>In this paper, we will refer to these questions shorthand as Willingness, Importance, Effectiveness, Safety, Trustworthy Source, Knowledgeable, and Social Norms, respectively.

<sup>4</sup>The wording of each question, possible responses, and how answers were classified are all listed in the SM.

<sup>5</sup>For instance, [22] aggregates a variety of distinct binary outcomes, corresponding to whether an action was taken or not, from a large set of behavioral experiments. Examples include whether or not someone filled out a government form or whether or not someone paid a fine. Two other studies that similarly use meta-analytic methods to combine different treatments and outcomes are [10, 31].

<sup>6</sup>One required assumption is that the effect of the treatment is fully captured by the survey question; since the treatment is unlikely to have a negative effect on vaccination, a violation of this assumption would likely lead to a conservative estimate. A second requirement is that the treatment does not directly change the relationship between the survey outcome and vaccination. This requirement could be violated if treatment induced individuals to respond positively to the survey, e.g. in an attempt to please the experimenting organization. This problem is unlikely because the survey is given at a separate time and format than the advertising exposure, and is not associated with the public health organization. However, since we cannot link treatment to changes in vaccination status, we cannot directly assess whether a change in survey responses due to treatment would lead to a change in vaccination rates that matches the observed cross-sectional correlation. This requirement would fail if, for example, the effect of advertising was very short-lived or if there are barriers to vaccinations among individuals close to the margin of getting vaccinated that are not reflected in the average relationship between survey responses and vaccination decisions.

<sup>7</sup>This scalability includes not just reaching more people but also reaching individuals who may be hard or costly to reach via other means. This may be particularly important for some subpopulations where there is evidence of a disproportionate impact of the pandemic [2].

<sup>8</sup>A separate branch of literature has evaluated impacts of interventions on other COVID-related behaviors. For

nation in Ohio from the Vax-a-Million lottery (though see [48, 46]); in a separate study, [17] find a cost per incremental vaccination on the order of \$400; and [35] estimate costs of \$88-\$380 for incremental flu vaccinations in seniors in the US. [36] finds a much lower cost, specifically \$1 per incremental COVID vaccination from a location-randomized YouTube advertising experiment in select counties in the US. [43] explores methods of effective communication for influencing health outcomes, including through randomized experiments run on Meta. Since our data does not include information about actual vaccination decisions, it is hard to directly contrast our estimates to those. However, such estimates highlight how challenging it can be to influence health behaviors, and the potential value of identifying low cost, scalable interventions. When considering such interventions to influence vaccine uptake, there has been much advocacy for behaviorally-informed promotions [9, 47]. The campaigns we analyze broadly fit into this category.

Our findings also connect to the literature on health nudges, which similarly tends to focus on low cost, scalable interventions. Much of this literature has focused on text-based interventions, which have shown potential across a number of domains, ranging from flu appointments to court appearances [40, 16, 24]. Specifically in the context of COVID-19, [21] sent participants in California text-based reminders to make vaccination salient and easy to remember. They find that reminders sent one day and eight days after notification of vaccine eligibility increased vaccination rates by 3.57 percentage points and 1.06 percentage points, respectively. [6] analyze the effect of a video message randomly distributed via SMS to millions of individuals in West Bengal, India; they find substantial effects on both the treated individuals as well as non-treated community members on a broad range of COVID-related outcomes. One advantage of text-based interventions is that they may be more salient and thus have a larger effect relative to our effects. On the other hand, advertising campaigns do not require gathering phone numbers and may thus be more easily scaled to a large population.<sup>9</sup>

Our results can be related to prior large-scale meta-analyses of online advertising. The literature has highlighted major challenges due to low statistical power [37]. Meta-analysis is a natural way to address this challenge, but it requires access to data from the experiments of many advertisers and also creates challenges comparing effectiveness across heterogeneous advertising objectives. We are aware of only three other meta-analyses of digital advertisements that have comparable scale to our study.<sup>10</sup> First, [33] used internal data from Google’s display advertising platform to study the effect of digital advertisements on website visits for 432 digital advertising campaigns, finding effects of 8% of baseline website visits. Second, [25] analyzed 2,892 experiments carried out by a brand research firm, each using a similar survey methodology and sample size to the experiments considered here, finding an effect of about 10% of the baseline on survey responses concerning intention to purchase. Third, [26] analyze more than 600 advertising experiments on

---

example, [19] use smartphone data from 10 million devices and find large effects of stay-at-home orders on both movement and transmission rates.

<sup>9</sup>A related vein of literature has focused on identifying mechanisms for effective communication around COVID that could then be implemented at scale. For example, holding a wide range of factors constant, [3] vary characteristics of the messenger and signal content in a video infomercial and find evidence of substantial heterogeneity in effectiveness only by shifting those attributes. Similarly, [34] find evidence that prosocial framings are important for shifting COVID-related outcomes across a range of experiments. Insights from studies such as these two could help inform both digital and non-digital interventions.

<sup>10</sup>[33] provides an overview of the literature, including related meta-analyses in other forms of advertising such as online search advertising and television; see also [8] who analyze 54 mobile advertising campaigns, and [30] who study social ads across 74 products.

Facebook, comparing the estimated effects for different measures along what is referred to as a “funnel” or a customer’s journey to a final action of interest. They find effects of 28%, 19%, and 6% for measured outcomes that capture consumer behavior at the top, middle, and lower parts of the funnel. Our paper is the first we are aware of to conduct a large-scale meta-analysis of online public health campaigns across multiple outcomes. Our estimated effects (about 1% over baseline) are substantially smaller than the effects found in these studies, suggesting that it is more difficult to change attitudes and beliefs about vaccination than it is to increase more standard advertising outcomes. Similar to [26], we find smaller effects for outcome measures that are closer to the ultimate outcome of interest, vaccination.

Building on our main results, we next look across the different survey outcomes to see which ones are most impacted by the campaigns. We find significant effects on Knowledge, Safety, Social Norms, and Importance (all have  $p < 0.001$ ), while no significant effects on Willingness, Effectiveness, or Trustworthy Source. There were individual campaigns that were able to significantly move these last three metrics, but we could not detect an overall average effect. Finally, we find evidence that the campaigns may have been particularly effective at shifting users’ knowledge around the vaccines. Knowledge has the largest treatment effect point estimate (1.23 percentage points,  $p = 5e-7$ ), and it is significantly higher than nearly all the other coefficients.<sup>11</sup> This suggests that, on average, the digital advertisements in our sample may have been a particularly cost-effective channel for information dissemination, or that information is easier to retain. In interpreting these comparisons, it is important to recall that different campaigns were designed for different purposes, and some campaigns may have conducted experiments on both primary and secondary outcomes, so that smaller impacts might be expected for secondary outcomes. For example, a campaign focused on providing information might have evaluated its impact on Knowledge but also on Willingness. If Knowledge was less commonly included as a secondary outcome, it might be expected that the measured impact would be higher. In the SM, we investigate this hypothesis by manually classifying advertisements as to which outcome is the best match as a primary outcome, and we find similar patterns.<sup>12</sup>

Overall, our results suggest that social media advertising campaigns can be an important component of public health initiatives. Over the course of the past two years, health oriented organizations have engaged in a wide range of tactics in an effort to shift attitudes and behaviors<sup>13</sup>; key challenges with many of these include scalability, measurement, and generalizability. Digital advertising can help overcome these challenges. However, the small per person impact highlights that these campaigns are best thought of as part of a broader set of strategies. To this end, our paper complements the growing literature on designing effective public health interventions on COVID.

The rest of our paper is organized as follows. The Data section describes our sample, the outcome variables, and the approach we use to analyze the campaigns; the Results section presents

---

<sup>11</sup>In a two sided t-test, the coefficient for Knowledge is significantly greater than that for Effectiveness ( $p = 0.038$ ), Importance ( $p = 0.003$ ), Safety ( $p = 0.034$ ), Trustworthy Source ( $p = 0.002$ ), Willingness ( $p = 0.020$ ), and our overall estimate ( $p = 0.008$ ), including when dropping Knowledge studies ( $p = 0.006$ ). It is not significantly different from our estimate for Social Norms, though it is close in a one sided test ( $p = 0.109$ ).

<sup>12</sup>Our manually labeled sample is smaller, but we see that the point estimate for Knowledge is again significantly larger than that for Importance, Safety, and Willingness; it is non significantly different from that for Effectiveness and Social Norms.

<sup>13</sup>See <https://www.nga.org/center/publications/covid-19-vaccine-incentives/> for a list of different incentives offered in the U.S. alone, ranging from Girl Scout cookies to laps on a NASCAR track.

findings in greater detail; and the Conclusion summarizes. Additional analyses are provided in the Supplementary Materials (SM).

## Data

### Overview

We analyze a set of 819 randomized experiments that were conducted between December, 2020 and November, 2021. The experiments in our sample are derived from 376 distinct advertising campaigns and 174 organizations. There are often multiple experiments associated within a single ad campaign, where each experiment corresponds to a specific survey outcome. For example, an advertiser may take one campaign and run three separate experiments that measure the impact of the campaign on Willingness, Importance, and Effectiveness. The average campaign in our data ran slightly more than two experiments; in other words, advertisers measured the impact of their campaigns on an average of about two outcome metrics each.<sup>14</sup>

The studies were all conducted using Meta’s infrastructure for conducting advertising effectiveness experiments across Facebook and Instagram; through the rest of this paper, we refer to this infrastructure as “the platform.” We focus specifically on experiments that measure the extent to which advertisements affect individuals’ attitude or beliefs as measured by survey questions.<sup>15</sup> We note that not all advertisers run these experiments, so our results are underestimates of the total impact of digital advertising interventions on Facebook and Instagram.<sup>16</sup>

We limit the set of experiments to those measuring outcomes in one of seven categories mentioned earlier (see SM for details).<sup>17</sup> Though not an exhaustive set of COVID-19 ad experiments, these seven categories were selected because they are the most prevalent across COVID-19 vaccine related experiments. We use all studies that asked these questions and, following the platform’s policy<sup>18</sup>, restrict to users aged 18 and older.

The campaigns we study total \$39.4 million in ad spend, with a reach of 2.1 billion unique users translated across 15 languages. The average campaign cost more than \$100,000 and reached nearly 13 million people; these were substantive efforts, but importantly also not beyond the budget of

---

<sup>14</sup>The platform normally caps the number of questions per campaign at three.

<sup>15</sup>These experiments are known as ‘Brand Lift Studies’ in advertiser-facing documentation. There are many companies that offer Brand Lift experiments to advertisers, each with slightly different implementations and methodologies. These studies are commonly used to measure effects on outcomes such as ad recall, brand sentiment, or intent to purchase, but have become popular during the pandemic to also look at health related outcomes that may not be observable in log data.

<sup>16</sup>Meta imposes minimum budgets to run one of these studies that vary across country; for example, in the U.S. it is currently \$30,000, which is more than many advertisers’ budgets.

<sup>17</sup>While the platform proposed standardized questions to the advertisers, they did have autonomy to adjust the language for the questions if they wished, so that we see some heterogeneity of questions asked within the seven categories we study. In our data there are no instances where the same campaign ran multiple experiments that each asked the exact same survey question. However, 38 campaigns ran multiple experiments measuring the same outcome variable using distinct questions. For example, a campaign may have run two separate experiments that both measured its impact on Knowledge, with different survey questions, such as *Do you know your order of priority to get the COVID-19 vaccine?* and *Do you know where to go to get a COVID-19 vaccine for yourself?* Omitting these campaigns yields no material shift in our results, and in the SM we conduct additional analyses that factor in this within-campaign heterogeneity.

<sup>18</sup>See <https://www.facebook.com/business/help/2396060560411130>.

what many public health organizations could conceivably spend on similar campaigns in the future. Table 1 provides summary statistics.

Table 1. Summary statistics by outcome metric.

Category	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
# Experiments	64	237	94	218	107	59	40	819
# Unique Campaigns	48	234	73	218	107	59	40	376
# Unique Organizations	32	109	57	100	50	30	17	174
Earliest Campaign Start	2021-02-17	2020-12-08	2021-02-19	2020-12-15	2021-02-03	2021-02-24	2020-12-08	2020-12-08
Latest Campaign End	2021-11-15	2021-11-12	2021-11-03	2021-11-15	2021-11-14	2021-10-21	2021-06-15	2021-11-15
Avg. # People Reached	17,932,855 (3,216,101)	12,833,435 (1,353,134)	15,341,751 (2,957,774)	11,959,068 (1,746,021)	12,497,147 (2,396,430)	9,510,847 (1,971,257)	27,297,621 (6,773,069)	12,913,047 (1,225,276)
Avg. # Survey Resp per Experiment	1,753.53 (151.09)	2,130.65 (90.27)	1,734.60 (128.96)	1,931.67 (90.81)	1,510.58 (120.24)	1,762.17 (148.21)	1,429.00 (155.72)	1,860.94 (45.75)
Avg. Campaign Cost	\$136,807 (\$41,543)	\$65,487 (\$6,376)	\$122,796 (\$36,000)	\$132,491 (\$26,694)	\$63,556 (\$13,409)	\$104,177 (\$24,353)	\$392,678 (\$142,627)	\$105,183 (\$16,872)
# Experiments Rejecting No Effect (0.1)	7	23	22	29	14	3	1	99
# Experiments Rejecting No Effect (0.05)	5	16	17	20	8	3	1	70
Implied False Discovery Rate (0.1)	0.914	1.000	0.427	0.752	0.764	1.000	1.000	0.827
Implied False Discovery Rate (.05)	0.640	0.741	0.276	0.545	0.669	0.983	1.000	0.585
# FDR Survivor Experiments (10% FDR)	2	14	5	5	1	2	0	27

Note: Standard errors in parentheses. # Experiments Rejecting No Effect (0.1) references the number of studies that are significant at the 0.1 level in a two-tailed t-test against the null of no treatment effect. Implied False Discovery Rate (0.1) estimates the false discovery rate if we accepted all experiments that were significant at the 0.1 level, as  $FDR(0.1) = (0.1 * n_e) / (n_{rej})$ , where 0.1 is the level of significance,  $n_e$  is the number of experiments, and  $n_{rej}$  is the number of experiments rejecting no effect at 0.1. # FDR Survivor Experiments is the number of experiments determined via the Benjamini-Hochberg algorithm to survive a false discovery rate of 10% [11].

Although the campaigns we study reached billions of users, as we can see in Table 1, we observe a much smaller number of survey responses. The platform provides the experimentation service to enable advertisers to estimate the incremental impact of their campaigns on survey-based outcomes; however, the number of responses per experiment is limited by the platform. The limits are presumably motivated by the fact that users may only be willing to engage in a small number of surveys, and the user experience may be negatively impacted by too many surveys. Thus, the platform caps the number of respondents per study to balance the tradeoff between statistical power and user experience.<sup>19</sup>

In total, our dataset incorporates 1.5 million total responses across all experiments. Per experiment, the number of responses ranges from 300 to 4507, split across test and control groups. This does not give us too much power for each individual experiment. If an experiment of average sample size (1861 in our sample) and average baseline positive response rate (55%) were to be analyzed using a difference in means between treatment and control, the minimum detectable

<sup>19</sup>See the SM for more details on how these experiments are implemented.



effect size with 80% power and a 10% significance level would be about 0.06, close to 10% of the baseline. The average effects we find below are an order of magnitude lower than that.<sup>20</sup> Indeed, we see only 99 out of the 819 experiments rejected the null at the 10% level, just slightly greater than the 10% of experiments that would be expected if there were no treatment effects.

Only 27 experiments on their own are included in a set of experiments determined to have a 10% false discovery rate using the Benjamini-Hochberg procedure [11]. We also report the implied false discovery rate for the set of experiments that are individually significant at the 5% level, and we repeat this for the 10% level. This exercise is motivated by the idea that an organization might choose to further scale a campaign after seeing a statistically significant impact. We see that if organizations used the 5% threshold for scaling, for the Knowledge outcome the false discovery rate would be about 1/4, while for the Importance outcome, the rate would be about 3/4. Although such a false discovery rate might not be problematic, as it is unlikely the campaigns would be harmful and in aggregate these campaigns would be cost-effective, our findings as a whole suggest that we are not well powered to well identify individually effective campaigns. This motivates the approach we pursue in this paper of conducting a meta-analysis of hundreds of experiments together rather than seeking to identify individual campaigns with positive effects.

In the SM, we provide graphs of the CDFs of p-values both overall and for each metric, where we can see that overall and particularly for the Knowledge outcome, the CDFs of p-values depart from the uniform distribution that would be expected if there were no effects.

## Survey Questions

We provide more detail on the questions, their possible responses, and their coding in the SM. In Table 2 we provide text from our standardized survey questions as a reference.

Table 2. Overview of Survey Questions

Question Category	Wording
Importance	How important do you feel a vaccine is to prevent the spread of COVID-19?
Safety	How safe do you think a COVID-19 vaccine is for people like you?
Willingness	How likely are you to get vaccinated for COVID-19 when the vaccine is available to you?
Effectiveness	How effective do you think the COVID-19 vaccination is in preventing COVID-19?
Knowledge	Do you know where people in your local community can go to get a COVID-19 vaccine?
Social Norms	When you think of most people whose opinion you value, how much would they approve of people getting a COVID-19 vaccine?
Trustworthy Source	Do you agree or disagree that [advertiser name] is a trustworthy source of COVID-19 vaccine facts and information?

## Validity of Survey-Based Outcomes

We now turn to consider the validity of the survey-based outcome measures, and in particular the extent to which they (or changes in them) do not capture changes in beliefs or knowledge. Privacy

<sup>20</sup>In response to an early draft of this manuscript, the platform started granting more exceptions to the normal response caps on these experiments in order to deliver better powered results.

and legal constraints prevent advertisers from asking about or measuring some ultimate quantities of interest on platform (e.g., health or vaccination status), but the self-reported measures may still be meaningful.<sup>21</sup> Here, we discuss two categories of potential concerns about these measures.

A first category of concerns relates to whether the survey outcomes as entered in the platform reflect beliefs and behaviors in the physical world. There are several considerations. Following established practice for social media brand campaigns, survey outcomes are a primary outcome that public health organizations have been using to evaluate their campaigns. Campaigns start, stop, and change based on the results of these experiments, dictating how entire ad budgets of COVID interventions are spent. Hence, it is important to understand how these outcomes have responded to campaigns to date, and to add to the understanding of public health organizations whose individual experiments to date have been under-powered to detect small effects.

Relatedly, a common goal of public health organizations is to simply shift attitudes and beliefs. Akin to traditional advertisers where campaigns may target different levels of the conversion funnel, many of these advertisers are aiming to move awareness or basic beliefs, and may invest in complementary tactics to change behavior once beliefs have been influenced. To the extent that the implementation details and survey responses provide insight about awareness and beliefs, the campaign experiment outcomes are informative.

Finally, for social media campaigns in general, there is evidence that responses to platform surveys correlate reasonably well with behaviors of interest. [41] find an  $R^2$  of 0.83 in a regression of country-level vaccine uptake on self-reported vaccine status collected from a survey on Facebook. And [4] find correlation between survey metrics on Facebook and off-platform COVID-19 cases (see [14] and [44] for further discussions and caveats). [1] finds a high degree of correlation between characteristics of businesses that Facebook users self-report to own and offline statistics from the US Census. Although the contexts and analyses from these studies are different, together they suggest that there does appear to be informative signal in social media survey outcomes.

In the SM we explore the relationship between survey response positivity and county-level vaccine takeup and find that the two are strongly correlated. We use these findings to extrapolate an estimated cost of each additional vaccination.

A second category of concerns about survey results relates to whether the differences in survey outcomes between treated and control groups can be interpreted as the causal effect of the advertisements [27]. One issue is that there can be systematic differences between the treatment and the control group due to the implementation of the randomized advertising experiment. Details are provided in the SM, but in short, randomization of assignment to ads takes place just before an ad was intended to be shown to a user, so that whether a user sees an ad is random within the experiment. However, after seeing an ad, whether a user in the treatment group is subsequently shown a survey depends on an additional factor that is not present for the control group. In particular, if after randomization into the treatment group, the platform intends to show the user an ad but the user scrolls past it or does not scroll to it at all, the user will not be sent a survey. This is done to only capture survey responses from users in the treatment group who actually saw the ad, but to the extent to which this behavior is correlated with the survey outcome, it could lead to confounding and thus biased estimates of treatment effects.

---

<sup>21</sup>[15] ran a location-randomized experiment where different regions were targeted with ad campaigns on Facebook. This experimental design allowed measurement and detection of significant effects on off platform outcomes (namely, travel and actual COVID cases). This result, though from a single experiment, demonstrates the potential for relevant offline effects from similar digital ads.

In addition, even if the set of users who were sent surveys was perfectly randomized within each experiment, there still is the potential for differential survey response between the treatment and the control group. This might occur if individuals influenced by the ads were more likely to respond to the survey. We address both of these issues in the SM, where we show that along several observable dimensions, the treatment and control group are similar. We further address these concerns by adjusting for several observable characteristics of individual respondents in our analysis, as described below.

A final concern is that the population answering the surveys differs from the overall target population along unobservables correlated with our outcome variable. For example, certain age groups may be more likely to answer the surveys. Post-stratifying our results by age and gender (as we do) is an industry standard approach to address this concern; in addition, in the SM we also compare observables from a post-stratified sample with those from the target population and find reasonable overlap.

## Results

We now turn to our main results, which we break into three categories. First, we describe for each survey outcome, the average effect across all experiments that focused on that outcome. Second, we combine the treatment effect estimates with data about the number of unique people who received advertisements as well as the total cost of the campaigns to estimate how many people have been influenced and what the cost per influenced person is. Finally, we discuss characteristics of successful campaigns.

### Meta-Analysis of Experiments

We begin by analyzing each experiment separately using the following weighted linear model:

$$response_i = (\tilde{X}_i' \beta + \beta_0) W_i + \tilde{X}_i' \gamma + \varepsilon_i \quad (1)$$

where  $response_i$  is an indicator for whether individual  $i$  gave a positive response,  $\tilde{X}_i$  is a matrix of de-meaned controls that could potentially be related to outcomes (age bucket, gender, expected click through rate, and expected conversion rate), and  $W_i$  is a dummy variable denoting whether  $i$  was in the treatment or control group. The expected click through and conversion rates are platform-generated estimates of how likely a user is to click on and complete the survey; age buckets are 18-24, 25-34, 35-44, 45-54, 55-64, and 65+. We de-mean and interact our covariates with the treatment indicator so that  $\hat{\beta}_0$  remains an unbiased and consistent estimate of the average treatment effect even in the presence of heterogeneous treatment effects by our covariates [32].

In our regression, each response is weighted with post-stratification weights by age bucket and gender within the treatment and control group, such that both arms of each experiment are representative of the population reached by the relevant campaign. That is, we obtain the proportion of users in each age bracket and gender group reached by the campaign associated with a given experiment and divide it by the proportion of responses to obtain the weight for each response.<sup>22</sup> We post-stratify by these two variables as age and gender are basic demographics that advertisers

---

<sup>22</sup>To reduce variance, they are trimmed to an upper bound of 3 and a lower bound of 0.3 to decrease the influence of outlying observations. Rerunning without any trimming yields no material difference in the results.

are both frequently interested in and where heterogeneous effects are often observed.<sup>23</sup> In the SM we explore robustness to different weighting schema and find no material difference in the results.

This procedure is based off the one that Meta uses to analyze results from these survey-based experiments for advertisers. Repeating this approach for each of the experiments in our dataset, we are left with 819 estimates of average treatment effects and standard errors. The next step in the analysis is to combine these point estimates into estimated effects by outcome metric and to generate an overall, combined estimate, following standard meta-analytic methods [13, 28].

Specifically, to generate the average effect for each outcome and overall, we combine the respective experiments using inverse variance weighting (Table 2). This approach estimates a single, homogeneous effect per category while minimizing variance. We present this approach due to its simplicity and the fact that there is not evidence of heterogeneity across all outcomes; in the SM we report several alternative specifications that allow for greater heterogeneity across experiments and outcomes and find very similar results.

Table 3. Meta-analysis of experiments by outcome.

Category	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0012 (0.0027)	0.0010 (0.0042)	0.0055*** (0.0008)
p-value	0.114	0.0004	5e-7	8e-5	0.0006	0.639	0.807	2e-13
Cost per Influenced Person	\$2.43	\$2.41	\$0.77	\$3.20	\$1.00	\$11.79	\$17.14	\$3.41
Baseline Positive Response Rate	0.505	0.672	0.575	0.501	0.556	0.365	0.517	0.557
Treatment Effect as % of Baseline	0.89%	0.64%	2.14%	1.24%	1.46%	0.33%	0.19%	0.99%
<b>Power Calculations (Approximate)</b>								
<i>Minimum Detectable Effect</i>	0.0071	0.0030	0.0061	0.0039	0.0059	0.0066	0.0104	0.0019
<i>Power to Detect Given Effect Size</i>	0.474	0.973	1.000	0.989	0.962	0.120	0.081	1.00
<i># Experiments Needed for 80% Power</i>	159	115	23	87	57	1,660	4,133	94
<i># Survey Resp. per Exp for 80% Power</i>	4,349	1,036	426	770	801	49,567	147,635	214

Note: Standard errors in parentheses. For each column, we consider the set of associated experiments and calculate the inverse-variance weighted average treatment effect (row 1). The Baseline % Positive Response is an unweighted mean across all the relevant experiments; calculating it using fixed or random effects models changes the numbers only slightly. The Cost per Influenced Person for each subset is calculated using the spend and number of unique people reached across all campaigns in the relevant subset. Finally, we include power calculations based on the standard error of the treatment effects (abstracting away from heterogeneity across experiments). Power to detect a given effect size is calculated at the  $\alpha = .1$  level; for the last two rows, we want to convey how power could be improved by either increasing the number of experiments or the number of surveys per experiment. For those calculations we ask how much of either we would need holding the other fixed to have 80% power to detect the given estimated treatment effect. See SM for details.

Several comments are salient to the interpretation of these results. First, this dataset is very broad. Past efforts to understand what has and has not worked with shifting behaviors around COVID have often by necessity studied a small number of treatments at modest scale or been

<sup>23</sup>At the experiment level, we observe many significant positive and negative coefficients on our age and gender interaction terms; rerunning our meta-analyses on these coefficients yields insignificant average effects, however.

one off ex post analyses. External validity with such studies is frequently a concern and potentially helps explain why studies to date have found conflicting results (e.g., [18, 17, 46, 48, 45] on effects of financial incentives on vaccination rates). In contrast, here, pooling hundreds of studies from a broadly representative population, we find a positive and statistically significant average main effect. The evidence may not be conclusive yet on what kinds of behavioral nudges work, but this is evidence that these digital advertising campaigns can help move the needle on COVID-related attitudes.

Second, consider results about the specific outcomes. We find that Importance, Knowledge, Safety, and Social Norms showed highly statistically significant effects. In contrast, we do not detect an effect for Effectiveness, Trustworthy Source, or Willingness. (Though Effectiveness is close to marginally significant in our main specification above.)

We note that these last three metrics, particularly Effectiveness and Willingness, are arguably lower in the vaccine conversion funnel (that is, they are better proxies for a final desired action) than the first four metrics. A stylized fact from advertising is that such “lower-funnel” behaviors often see smaller effect sizes than more upper level outcomes, and are generally challenging to study as they may be influenced by a variety of unmeasured factors. While our finding thus accords with this intuition, a limitation of our study is that even with a very large sample size, we are not powered to generate more precise estimates of these averages (see the final rows of Table 2).

Third, we note that there is evidence the estimated lift for Knowledge (1.23pp) is significantly higher than our other metrics. Our estimate for Knowledge is significantly greater than that for Effectiveness ( $p = 0.038$ ), Importance ( $p = 0.003$ ), Safety ( $p = 0.034$ ), Trustworthy Source ( $p = 0.002$ ), Willingness ( $p = 0.020$ ), and our overall estimate ( $p = 0.008$ ), including when dropping the Knowledge experiments ( $p = 0.006$ ). It is nearly significantly greater than Social Norms in a one sided t-test ( $p = 0.109$ ). We note that Knowledge is a distinct outcome here in that the other outcomes relate more to persuasion, whereas Knowledge focuses simply on conveying information. These results suggest that social media campaigns may be particularly attractive for public health organizations interested in the latter.<sup>24,25</sup>

Finally, we note that in the SM we explore different specifications and find broadly similar results. In addition, the standard output provided to advertisers on Meta comes from a Hierarchical Bayesian Model; we chose a frequentist approach due to its simplicity, but in the SM we show robustness to using a similar Bayesian approach.

**Number of influenced people, cost per influenced person.** Conditional on our results, how many people were influenced by these campaigns, and how cost effective were they? As aforementioned, the survey data only comes from a subset of the overall users who saw the ads; to calculate the number of influenced people, we follow the common industry practice and scale the point estimate of the treatment effect from each experiment by the size of the overall population that saw the campaign. In our case, since we have data across many advertisers and some users were shown ads from multiple campaigns, to generate a (conservative) estimate of the number of influenced people per campaign, we can treat these collective campaigns as effectively one large one. Specifically,

---

<sup>24</sup>In thinking about the effects for Knowledge as well as the other metrics, a relevant data point is the baseline positive response rate for Trustworthy Source (36.5%). While this question was only asked in a subset of campaigns it is still revealing that the effects we are seeing are despite a relatively low user trust level with some advertisers. This suggests that health organizations with strong brand values may be well positioned to see particularly large effects, a topic we leave to future research.

<sup>25</sup>See also our additional analysis in the SM with manually labeled ads.

we combine the total spend, total unique reach, and our estimate of the average treatment effect to generate a back of the envelope estimate of the number of people who were influenced by this combined effort.

Doing this calculation, we estimate that about 11.6 million people were influenced by these campaigns. To be clear, by ‘influenced’ we mean shifted self-reported beliefs to a positive outcome; this does not capture people who, for example, moved along the intensive margin of these categories.<sup>26</sup>

Conditional on estimates of how many people were influenced, how much does it cost to influence someone? For this, we divide the number of influenced people by the ad spend, again as is typical in the industry. From Table 2 we can see the average cost per influenced person was \$3.41.

To understand how cost effectiveness translates to real-world public health outcomes, in the SM we explore the relationship between survey positivity rate and vaccine series completion rate at the county level in the US. Across survey outcomes, we find that for each additional positive survey response, we would expect to see about 0.6 increase in vaccination takeup in the CDC data. Applying this to our average cost per influenced person from Table 2, this implies an estimated cost per additional vaccine of about \$5.68.

While we are hesitant to extrapolate substantially outside our sample, these magnitudes suggest running even a few million dollars’ worth of additional campaigns could achieve relatively large shifts in the baseline fraction of outcome variables.

## Conclusion

Over the course of the pandemic, public health agencies increasingly leveraged social media advertising to pursue public health goals. Our results show that public health interventions via digital advertising are an effective medium for changing important self-reported beliefs and attitudes around COVID-19. Combining with non-experimental data on vaccination rates, our results suggest that these campaigns were a cost effective approach to increasing rates as well. The cost-effectiveness and scale of these campaigns can make them appealing to a broad range of organizations around the world. The use of social media advertising more broadly has the potential to aid in the pursuit of other health policy goals, ranging from childhood vaccination to hand washing).

---

<sup>26</sup>In the SM we explore a less conservative way of estimating the number of influenced people (and thus the cost per influenced person).

## References

- [1] Georgij Alekseev et al. *The effects of COVID-19 on Us small businesses: Evidence from owners, managers, and employees*. Tech. rep. National Bureau of Economic Research, 2020.
- [2] Marcella Alsan, Amitabh Chandra, and Kosali Simon. “The great unequalizer: initial health effects of COVID-19 in the United States”. In: *Journal of Economic Perspectives* 35.3 (2021), pp. 25–46.
- [3] Marcella Alsan and Sarah Eichmeyer. *Experimental Evidence on the Effectiveness of Non-Experts for Improving Vaccine Demand*. National Bureau of Economic Research, 2021.
- [4] Christina M Astley et al. “Global monitoring of the impact of covid-19 pandemic through online surveys sampled from the facebook user base”. In: *medRxiv* (2021).
- [5] Susan Athey et al. *The surrogate index: Combining short-term proxies to estimate long-term treatment effects more rapidly and precisely*. Tech. rep. National Bureau of Economic Research, 2019.
- [6] Abhijit Banerjee et al. *Messages on COVID-19 prevention in India increased symptoms reporting and adherence to preventive behaviors among 25 million recipients with similar effects on non-recipient members of their communities*. Tech. rep. National Bureau of Economic Research, 2020.
- [7] Andrew Barber and Jeremy West. “Conditional cash lotteries increase COVID-19 vaccination rates”. In: *Journal of Health Economics* (2021), p. 102578.
- [8] Yakov Bart, Andrew T Stephen, and Miklos Sarvary. “Which products are best suited to mobile advertising? A field study of mobile display advertising effects on consumer attitudes and intentions”. In: *Journal of Marketing Research* 51.3 (2014), pp. 270–285.
- [9] Jay J Van Bavel et al. “Using social and behavioural science to support COVID-19 pandemic response”. In: *Nature human behaviour* 4.5 (2020), pp. 460–471.
- [10] Shlomo Benartzi et al. “Should governments invest more in nudging?” In: *Psychological Science* 28.8 (2017), pp. 1041–1055.
- [11] Yoav Benjamini and Daniel Yekutieli. “The control of the false discovery rate in multiple testing under dependency”. In: *Annals of Statistics* (2001), pp. 1165–1188.
- [12] Thomas Blake, Chris Nosko, and Steven Tadelis. “Consumer heterogeneity and paid search effectiveness: A large-scale field experiment”. In: *Econometrica* 83.1 (2015), pp. 155–174.
- [13] Michael Borenstein et al. *Introduction to meta-analysis*. John Wiley & Sons, 2021.
- [14] Valerie C Bradley et al. “Are We There Yet? Big Data Significantly Overestimates COVID-19 Vaccination in the US”. In: *arXiv preprint arXiv:2106.05818* (2021).
- [15] Emily Breza et al. “Effects of a large-scale social media advertising campaign on holiday travel and COVID-19 infections: a cluster randomized controlled trial”. In: *Nature medicine* 27.9 (2021), pp. 1622–1628.
- [16] Alison Bутtenheim et al. “Effects of Ownership Text Message Wording and Reminders on Receipt of an Influenza Vaccination: A Randomized Clinical Trial”. In: *JAMA Network Open* 5.2 (2022).

- [17] Pol Campos-Mercade et al. “Monetary incentives increase COVID-19 vaccinations”. In: *Science* 374.6569 (2021), pp. 879–882.
- [18] Tom Chang et al. *Financial Incentives and Other Nudges Do Not Increase COVID-19 Vaccinations among the Vaccine Hesitant*. Tech. rep. National Bureau of Economic Research, 2021.
- [19] M Keith Chen et al. “Causal estimation of stay-at-home orders on SARS-CoV-2 transmission”. In: *arXiv preprint arXiv:2005.05469* (2020).
- [20] William G Cochran. “Some methods for strengthening the common  $\chi^2$  tests”. In: *Biometrics* 10.4 (1954), pp. 417–451.
- [21] Hengchen Dai et al. “Behavioural nudges increase COVID-19 vaccinations”. In: *Nature* 597.7876 (2021), pp. 404–409.
- [22] Stefano DellaVigna and Elizabeth Linos. “RCTs to scale: Comprehensive evidence from two nudge units”. In: *Econometrica* 90.1 (2022), pp. 81–116.
- [23] Dean Eckles, Brett R Gordon, and Garrett A Johnson. “Field studies of psychologically targeted ads face threats to internal validity”. In: *Proceedings of the National Academy of Sciences* 115.23 (2018), E5254–E5255.
- [24] Alissa Fishbane, Aurelie Ouss, and Anuj K Shah. “Behavioral nudges reduce failure to appear for court”. In: *Science* 370.6517 (2020), eabb6591.
- [25] Avi Goldfarb and Catherine E Tucker. “Privacy regulation and online advertising”. In: *Management science* 57.1 (2011), pp. 57–71.
- [26] Brett R Gordon, Robert Moakler, and Florian Zettelmeyer. “Close Enough? A Large-Scale Exploration of Non-Experimental Approaches to Advertising Measurement”. In: *arXiv preprint arXiv:2201.07055* (2022).
- [27] Brett R Gordon et al. “A comparison of approaches to advertising measurement: Evidence from big field experiments at Facebook”. In: *Marketing Science* 38.2 (2019), pp. 193–225.
- [28] Larry V Hedges and Ingram Olkin. *Statistical methods for meta-analysis*. Academic press, 2014.
- [29] Julian PT Higgins et al. *Cochrane handbook for systematic reviews of interventions*. John Wiley & Sons, 2019.
- [30] Shan Huang et al. “Social advertising effectiveness across products: A large-scale field experiment”. In: *Marketing Science* 39.6 (2020), pp. 1142–1165.
- [31] Dennis Hummel and Alexander Maedche. “How effective is nudging? A quantitative review on the effect sizes and limits of empirical nudging studies”. In: *Journal of Behavioral and Experimental Economics* 80 (2019), pp. 47–58.
- [32] Guido W Imbens and Donald B Rubin. *Causal inference in statistics, social, and biomedical sciences*. Cambridge University Press, 2015.
- [33] Garrett Johnson, Randall A Lewis, and Elmar Nubbemeyer. “The online display ad effectiveness funnel & carryover: Lessons from 432 field experiments”. In: *Available at SSRN 2701578* (2017).



- [34] Jillian J Jordan, Erez Yoeli, and David G Rand. “Don’t get it or don’t spread it: Comparing self-interested versus prosocial motivations for COVID-19 prevention behaviors”. In: *Scientific reports* 11.1 (2021), pp. 1–17.
- [35] James W Krieger et al. “Increasing influenza and pneumococcal immunization rates: a randomized controlled study of a senior center–based intervention”. In: *American journal of preventive medicine* 18.2 (2000), pp. 123–131.
- [36] Brad Larsen et al. *How to Increase Vaccine Uptake Using Trump: Evidence from a Large-Scale Ad Experiment on YouTube*. Tech. rep. Unpublished, 2022.
- [37] Randall A Lewis and Justin M Rao. “The unfavorable economics of measuring the returns to advertising”. In: *The Quarterly Journal of Economics* 130.4 (2015), pp. 1941–1973.
- [38] Charles E McCulloch and John M Neuhaus. “Misspecifying the shape of a random effects distribution: why getting it wrong may not matter”. In: *Statistical science* 26.3 (2011), pp. 388–402.
- [39] Rachael Meager. “Understanding the average impact of microcredit expansions: A bayesian hierarchical analysis of seven randomized experiments”. In: *American Economic Journal: Applied Economics* 11.1 (2019), pp. 57–91.
- [40] Katherine L Milkman et al. “A megastudy of text-based nudges encouraging patients to get vaccinated at an upcoming doctor’s appointment”. In: *Proceedings of the National Academy of Sciences* 118.20 (2021).
- [41] Alex Moehring et al. “Surfacing norms to increase vaccine acceptance”. In: *Available at SSRN 3782082* (2021).
- [42] Sarah Moshary, Bradley T Shapiro, and Jihong Song. “How and when to use the political cycle to identify advertising effects”. In: *Marketing Science* 40.2 (2021), pp. 283–304.
- [43] World Health Organization. *Communication for Health in the WHO Western Pacific Region*. 2021.
- [44] Alex Reinhart and Ryan Tibshirani. “Big data, big problems: Responding to” Are we there yet?“. In: *arXiv preprint arXiv:2109.00680* (2021).
- [45] Neil KR Sehgal. “Impact of Vax-A-Million lottery on COVID-19 vaccination rates in Ohio”. In: *The American Journal of Medicine* (2021).
- [46] Harsha Thirumurthy et al. “Association between statewide financial incentive programs and COVID-19 vaccination rates”. In: *Available at SSRN* (2021).
- [47] Kevin G Volpp, George Loewenstein, and Alison M Buttenheim. “Behaviorally Informed Strategies for a National COVID-19 Vaccine Promotion Program”. In: *JAMA* 352.2 (2021), pp. 125–126.
- [48] Allan J Walkey, Anica Law, and Nicholas A Bosch. “Lottery-based incentive in Ohio and COVID-19 vaccination rates”. In: *JAMA* 326.8 (2021), pp. 766–767.
- [49] Witold Wiecek and Rachael Meager. *baggr: Bayesian Aggregate Treatment Effects*. R package version 0.6.18. 2022. URL: <https://CRAN.R-project.org/package=baggr>.

## **Acknowledgments**

We thank thank Kang-Xing Jin and the health group at Meta Platforms for driving the creation of these public health interventions in the first place. S.A. acknowledges funding from the Golub Capital Social Impact Lab at Stanford Graduate School of Business.

## **Funding**

Funding to hire K.G. as a part-time contractor to work on this project came from Meta Platforms.

## **Author Contributions**

Author listing is alphabetical, and all participated significantly in the design, analysis, and writing.

## **Competing Interests**

N.W. is an employee of Meta Platforms and owns stock. K.G. is a part-time contractor through PRO Unlimited, a contracting agency used by Meta Platforms. S.A. received funding from Meta Platforms for research projects related to public health, and she previously provided consulting services to Meta. M.L. declares no competing interests.

# Supplementary Materials for

## Effects of Digital Interventions on COVID-19 Attitudes and Beliefs

**Susan Athey,<sup>1</sup> Kristen Grabarz,<sup>2</sup> Michael Luca,<sup>3</sup> Nils Wernerfelt<sup>4</sup>**

<sup>1</sup>Stanford Graduate School of Business, 655 Knight Way, Stanford, CA 94305 USA  
([athey@stanford.edu](mailto:athey@stanford.edu))

<sup>2</sup>Harvard Business School, Soldiers Field, Boston, MA 02163 USA ([grabarz@hbs.edu](mailto:grabarz@hbs.edu))

<sup>3</sup>Harvard Business School, Soldiers Field, Boston, MA 02163 USA ([mluca@hbs.edu](mailto:mluca@hbs.edu))

<sup>4</sup>Meta Platforms, One Hacker Way Menlo Park, CA 94025 USA ([nilsw@fb.com](mailto:nilsw@fb.com))

### Supplementary Text

This section has eight parts: (i) we provide an overview of the advertising experimentation infrastructure at Meta and how our studies were implemented; (ii) we run various robustness checks on our main analyses; (iii) we provide graphs of the CDFs of the  $p$ -values overall and by metric; (iv) we explore a less conservative way of calculating the cost-effectiveness of our campaigns; (v) we document characteristics of survey respondents across groups to provide insight into any possible bias; (vi) we rerun our analysis on a subsample of manually labeled ads to analyze differences in effectiveness by campaign characteristics; (vii) we explore the relationship between survey responses and real-world vaccination uptake using county-level vaccine data from the Centers for Disease Control; and (viii) we list the questions asked, possible responses, and bucketing into positive responses and not.

### Overview of Advertising Campaign Experiments at Meta

“Lift” studies are the core tool advertisers use to assess the impact of their campaigns on Meta. Within such studies, “Brand Lift Studies” are those which focus on survey-based outcomes and are the ones we focus on in this paper. Meta has an external-facing self-service tool for Brand Lift Studies, though large enough advertisers have access to a liaison within Meta who can set up more sophisticated studies (e.g., ones that have multiple treatment arms, custom questions, and varying holdout sizes). Despite this heterogeneity in how advertisers interact with the platform, the technical implementation of the lift studies is standardized. This greatly facilitates analysis across studies.

Each time such a campaign experiment is created, all users are hashed into treatment and control for that campaign. When an auction is run for a specific user, if an ad from the study in question wins the auction and the user is in the treatment group, the ad is sent to the user, but if the user is in the control group, the ad is held back. Both treated and control users are eligible to be sent a poll 4-48 hours after the corresponding (potential) exposure event. Given a total desired number of responses, surveys are sent to users spaced out over the course of the campaign, where each user may only be polled once per study. Natural variation in response rates means responses

are not always uniform across time within the course of the campaign, but in practice the variation tends to be minimal.

A complication here is even if Meta’s servers send a user an ad, that does not necessarily guarantee she will see it. The physical layout of the delivery surfaces themselves may mean, for example, that a user may be sent an ad but they may not scroll down far enough to see it, or may scroll too fast. In the experimental setup, only users in the treatment group who saw an ad are eligible to receive surveys. This avoids sending surveys to users assigned to test but who did not see the ad, which would capture an ITT advertisers arguably may care less about. In contrast, any user in the control group where the treatment ad won the auction and was held out from being sent is eligible to receive a survey, whether or not they saw the slot where the ad would have been. This ensures that no user who is sent a survey in the control group was exposed the test ad (at least on their account), but it does create a slight difference in the survey-eligible populations across treatment and control. For example, users who scroll quickly may be more likely to be sent surveys if they are in the control. In practice, the differences between the two populations has proven slight – our results, as do those reported out to advertisers, contain controls for activity bias which is by far the largest source of differential exposure across groups, and even unconditional on that, internal analyses have shown that the two populations do not significantly differ along a wide range of observables. For completeness, however, we perform an additional check later in the SM.

Conditional on this data generating process, we have the data on respondents, their demographic characters, and those of the target population that we use in our main analyses. We note this is the same raw data that Meta analyzes and reports out to advertisers, though as aforementioned, we do our own, custom analysis.

## **Robustness of Results to Different Specifications and Weighting Schemes**

Below we report on different specifications for our main results. We run three broad categories of alternative analyses: (i) we vary the controls, weighting, and linear specification in our experiment-level analysis; (ii) we allow for heterogeneity in our treatment effect estimates across experiments (‘random effects’ meta-analysis methods); and (iii) we explore how our results differ under a Bayesian analysis. We present the respective results in turn. Unless otherwise noted, conditional on experiment-level results, estimates are combined using inverse variance weighting to generate the main point estimates.

### **Varying the controls, weights, and linearity**

Recall our main specification:

$$response_i = (\tilde{X}_i' \beta + \beta_0) W_i + \tilde{X}_i' \gamma + \varepsilon_i \quad (2)$$

with variables described as in the main text. In particular, within  $\tilde{X}_i$  we have platform-generated controls for expected click through and expected completion rates of the survey. One may be concerned that these are rather black box and may be correlated with our treatment variable in a way that may affect inference. Hence, we rerun our main results dropping these platform-generated controls.

Second, in our main specification, the individual responses are weighted based on their age and gender category and the frequency of those buckets in the campaign’s target population, such that

the treatment and control groups are both representative of the campaign demographics in each experiment. To explore the sensitivity of our results to this weighting, below we rerun our baseline model with different weights (only gender and only age).

Finally, while we estimate the above using OLS, logistic regressions are commonly used in these settings (e.g., due to their bounded range), so we report results out using that as well. Specifically, for each experiment we estimate the average marginal effect of our treatment and then combine those across experiments. Across all these specifications, which are reported in Table S1, our results are broadly similar – or stronger.

Table S1. Varying the controls, weights, and linearity.

	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
<b>Main results</b>								
Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0012 (0.0027)	0.0010 (0.0042)	0.0055*** (0.0008)
p-value	0.114	0.0004	5e-7	8e-5	0.0006	0.639	0.807	2e-13
<b>Dropping platform-generated controls</b>								
Treatment Coefficient	0.0006 (0.0034)	0.0032* (0.0015)	0.0124*** (0.0033)	0.0044* (0.0017)	0.0076** (0.0028)	0.0031 (0.0030)	-0.0005 (0.0054)	0.0044*** (0.0007)
p-value	0.858	0.030	2e-4	0.011	0.007	0.303	0.921	2e-9
<b>Post stratification weighting by age bracket only</b>								
Treatment Coefficient	0.0034 (0.0038)	0.0041** (0.0016)	0.0112* (0.0044)	0.0065*** (0.0018)	0.0083** (0.0028)	0.0028 (0.0031)	0.0012 (0.0042)	0.0057*** (0.0008)
p-value	0.364	0.008	0.011	0.0003	0.003	0.367	0.782	7e-14
<b>Post stratification weighting by gender only</b>								
Treatment Coefficient	0.0050 (0.0035)	0.0045** (0.0015)	0.0119** (0.0042)	0.0065*** (0.0017)	0.0079** (0.0027)	0.0039 (0.0032)	0.0017 (0.0040)	0.0060*** (0.0007)
p-value	0.147	0.002	0.005	0.0002	0.004	0.224	0.683	2e-16
<b>Logistic regression</b>								
Average Marginal Effect	0.0025 (0.0030)	0.0056*** (0.0013)	0.0141*** (0.0026)	0.0052*** (0.0017)	0.0089*** (0.0026)	0.0061* (0.0032)	0.0048 (0.0057)	0.0064*** (0.0008)
p-value	0.395	3e-5	4e-8	0.002	0.001	0.060	0.406	6e-15

Note: Standard errors in parentheses.

## Incorporating heterogeneous effects across experiments

Conditional on our experiment-level results, the analysis in the main text combines them together using inverse variance weighting. This assumes a homogeneous treatment effect across all pooled studies (a ‘fixed effects’ meta-analysis). In practice, however, we may think treatment effects may vary substantially across campaigns or advertisers, motivating an analysis that estimates a distribution of treatment effects (a ‘random effects’ meta-analysis). We report the results in the main text given their simplicity and the fact that for some metrics the heterogeneity does not seem very substantial [29]. Further, incorporating distributions of effect sizes, as we do below, the results do not change substantively.

In particular, we run two separate analyses below, which are both reported in Table S2. First, we do a standard random effects meta-analysis within each outcome. Specifically, for experiment  $i$  that asks question  $q$  with observed effect size  $\hat{\beta}_{qi}$  and observed standard error  $\sigma_{qi}$ , we allow the true effect size,  $\beta_{iq}$  to vary around a true, grand average effect for that question,  $\bar{\beta}_q$ , with variance  $\tau_q^2$ . As is common in such models, we parametrize the true treatment effect for the experiment,  $\beta_{iq}$ , to be drawn from a normal distribution  $N(\bar{\beta}_q, \tau_q^2)$  and then the observed treatment effect  $\hat{\beta}_{qi}$  to be drawn from a normal  $N(\beta_{iq}, \sigma_{iq}^2)$ . We do not want to lean on this too heavily, but we note past work has found such Gaussian assumptions in random effects meta-analyses can perform reasonably well even if the true data are non-Gaussian [38]. We fit this via maximum likelihood for each outcome separately, reporting the results for the grand mean and its corresponding prediction interval below. For the Overall column, we repeat this procedure but across all experiments.

Second, given the same campaign is often involved with multiple experiments, we can allow correlation within effects for each campaign. This is mainly an issue for estimating the Overall effect, though there are campaigns that did ask slight variants of the same question (as discussed in the main text). This constitutes a three-level random effects meta-analysis where we allow a distribution over campaign effects, a distribution within each campaign across questions, and then a final distribution over observed experiment effects conditional on the true treatment effect. We parametrize this similarly as three nested normals: for a campaign  $i$  and question  $q$ , the distribution of true campaign effects,  $\beta_i$ , is  $N(\bar{\beta}, \tau_1^2)$ ; the distribution over question true effects,  $\beta_{iq}$ , within each campaign is given by  $N(\beta_i, \tau_2^2)$ ; and the distribution of realized experiment treatment effects,  $\hat{\beta}_{iq}$ , is  $N(\beta_{iq}, \sigma_{iq}^2)$ . We again fit this via maximum likelihood for each outcome separately and then for Overall.

Finally, we do not report it as it is only for the Overall outcome, but we also ran a three-level random effects model with a distribution over questions, a distribution over experiments within each question, and then a distribution over realized effects. This yields an estimate of 0.56pp, which is again significant from zero ( $p < 0.0001$ ).

Below we also include estimates of Cochran’s  $Q$  [20], a common statistic in meta-analyses to test for heterogeneity across experiments. Cochran’s  $Q$  is a weighted sum of squares across experiments – specifically, it is defined as  $Q = \sum_k (\hat{\theta}_k - \hat{\theta})^2 / \hat{\sigma}_k^2$  where the summation is taken over all experiments in question,  $\hat{\theta}_k$  is the estimated treatment effect in study  $k$ ,  $\hat{\theta}$  is the estimated treatment effect of the studies derived via a fixed effects model, and  $\hat{\sigma}_k^2$  is the estimated variance of the treatment effect estimate in study  $k$ . Note that intuitively higher  $Q$  values mean there is greater dispersion in treatment effect estimates around the pooled estimate, evidence of heterogeneity. Cochran’s  $Q$  follows a Chi-squared distribution, with low p-values meaning a rejection of the null of homogeneity. While suggestive, this test in isolation should not be used to determine

between fixed and random effects meta-analyses (see [29] for more details).

Table S2. Random effects models.

	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
<b>Main results</b>								
Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0012 (0.0027)	0.0010 (0.0042)	0.0055*** (0.0008)
p-value	0.114	0.0004	5e-7	8e-5	0.0006	0.639	0.807	2e-13
<b>Random effects within each outcome</b>								
Treatment Coefficient	0.0033 (0.0038)	0.0040*** (0.0015)	0.0106** (0.0044)	0.0065*** (0.0018)	0.0077*** (0.0028)	0.0030 (0.0032)	0.0010 (0.0042)	0.0056*** (0.0010)
p-value	0.382	0.009	0.015	0.0003	0.006	0.337	0.806	9e-9
Cochran's <i>Q</i> ( <i>p</i> -value)	96 (0.004)	424 (<1e-4)	232 (<1e-4)	412 (<1e-4)	167 (<1e-4)	113 (<1e-4)	39 (0.468)	1497 (<1e-4)
<b>Three-level random effects</b>								
Treatment Coefficient	0.0027 (0.0040)	0.0043*** (0.0012)	0.0118** (0.0049)	0.0062*** (0.0016)	0.0077*** (0.0028)	0.0030 (0.0032)	0.0010 (0.0042)	0.0052*** (0.0011)
p-value	0.493	0.0004	0.015	<1e-4	0.006	0.337	0.807	<1e-4
Cochran's <i>Q</i>	96 (0.004)	424 (<1e-4)	232 (<1e-4)	412 (<1e-4)	167 (<1e-4)	113 (<1e-4)	39 (0.468)	1516 (<1e-4)

Note: Standard errors in parentheses.

## Bayesian Analysis

Finally, we use a Bayesian approach to rerun one of our earlier specifications as a sanity check. Bayesian meta-analyses have become popular recently for a number of reasons (e.g., [39, 29]), but a general advantage in random effects settings is an ability to directly model the uncertainty in the between study variance. Hence, below we run a Bayesian version of the first random effects model we outlined above to see if there is any evidence the Bayesian approach produces substantively different estimates.

Specifically, we assume a hierarchical likelihood as follows. For all experiments  $i$ , with estimated treatment effect  $\hat{\beta}_i$  and estimated standard error  $\hat{\sigma}_i$ , we assume  $\hat{\beta}_i \sim N(\beta_i, \hat{\sigma}_i^2)$ , where  $\beta_i \sim N(\beta, \tau^2)$ . Following the defaults of [49], we specify weak priors of  $\beta \sim N(0, 100)$  and  $\tau \sim U[0, 10\bar{\sigma}]$  where  $\bar{\sigma}$  is simplistically defined as the standard deviation of the estimated  $\hat{\beta}_i$ 's. Given this set up, we then run the Bayesian model for all of the experiments that correspond to each outcome as well as all of them collectively for the Overall estimate. In Table S3 we report estimates of the hypermeans and corresponding uncertainty intervals; we can see that compared to the above, there is reassuringly little difference in the estimates.

Table S3. Bayesian analysis.

	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
<b>Main results</b>								
Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0012 (0.0027)	0.0010 (0.0042)	0.0055*** (0.0008)
p-value	0.114	0.0004	5e-7	8e-5	0.0006	0.639	0.807	2e-13
<b>Bayesian Analysis.</b>								
Hypermean Estimate	0.0032 [-0.0048, 0.0108]	0.0040** [0.0010, 0.0071]	0.0106** [0.0015, 0.0190]	0.0065** [0.0030, 0.0101]	0.0077** [0.0023, 0.0132]	0.0030 [-0.0032 0.0100]	0.0012 [-0.0078, 0.0103]	0.0056** [0.0037, 0.0075]

Note: 95% uncertainty intervals in parentheses. Two asterisks denotes 0 is not contained in the 95% uncertainty interval.



### CDFs of p-values

To further explore evidence on the potential rate of false positives in our study, in Figures S1 and S2 we plot CDFs of the p-values across studies, both overall and for each metric. For both the overall distribution of p-values and the four questions for which we detected a significant effect, we can see there is more density at p-values less than 0.05 compared to the null of no effect (uniform distribution of p-values). Values less than 0.05 are highlighted in red.

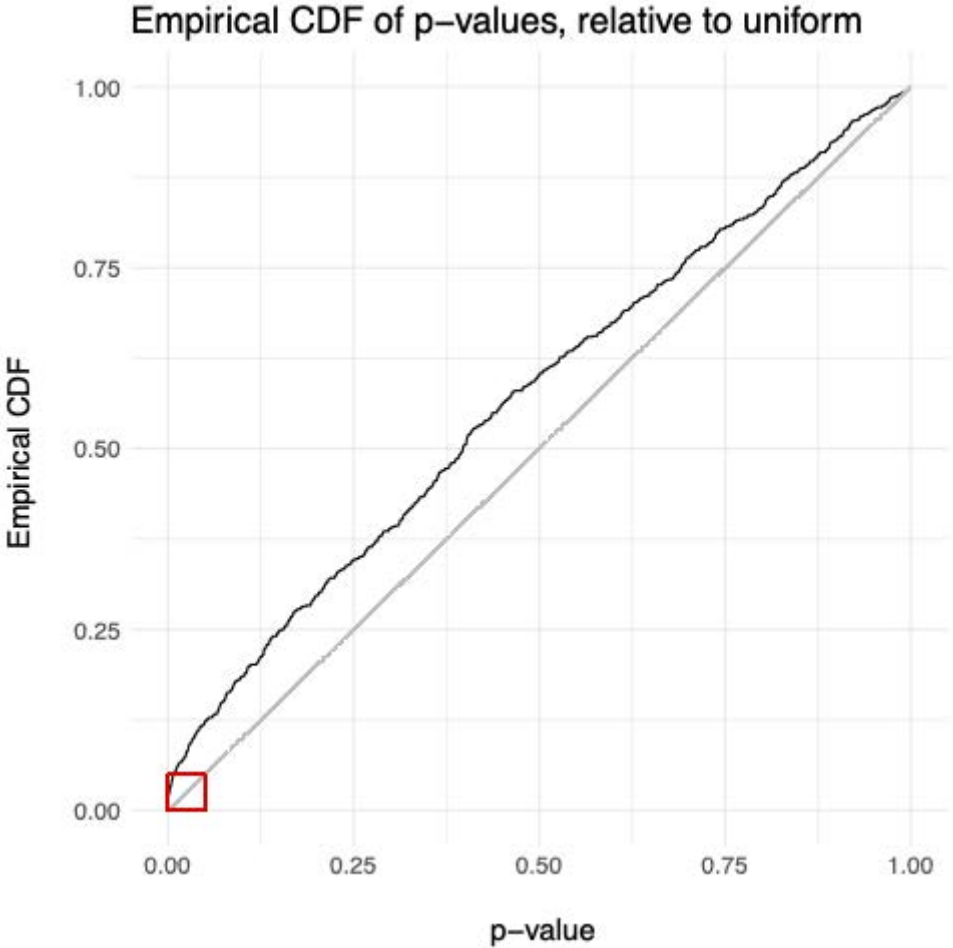
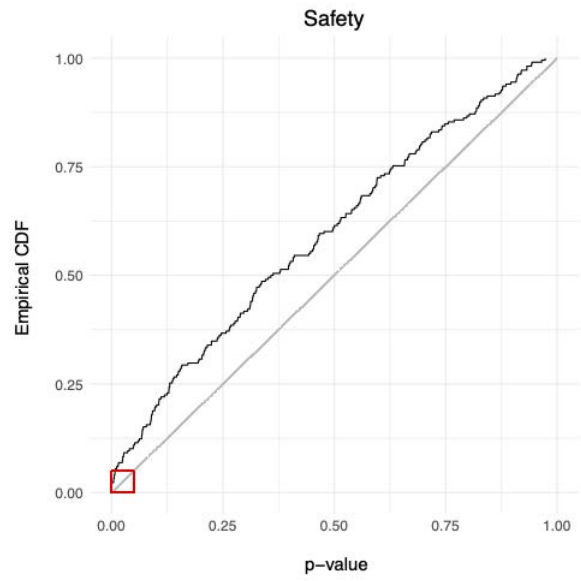
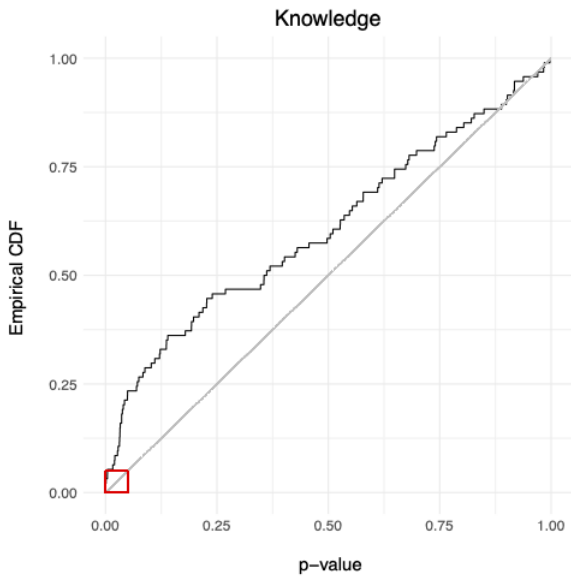
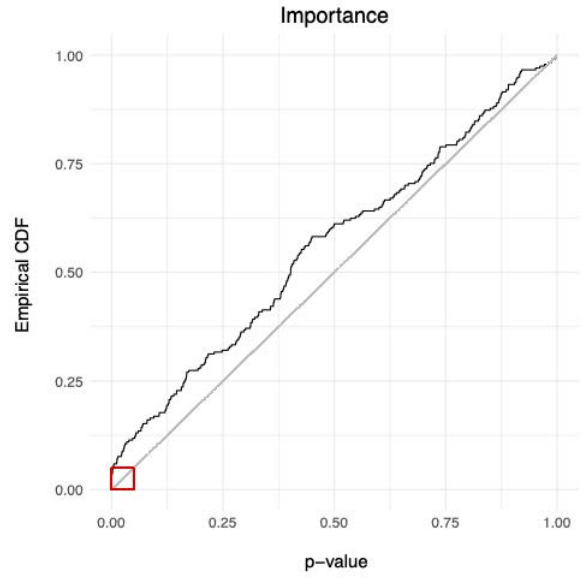
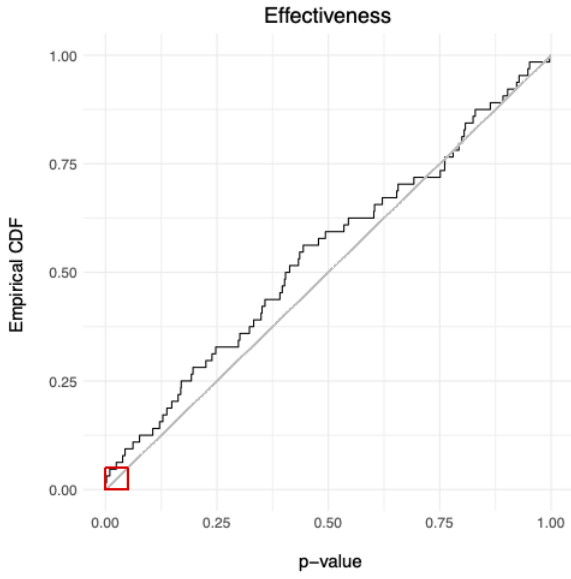


Figure S1. Empirical CDF of treatment effect p-values across experiments.



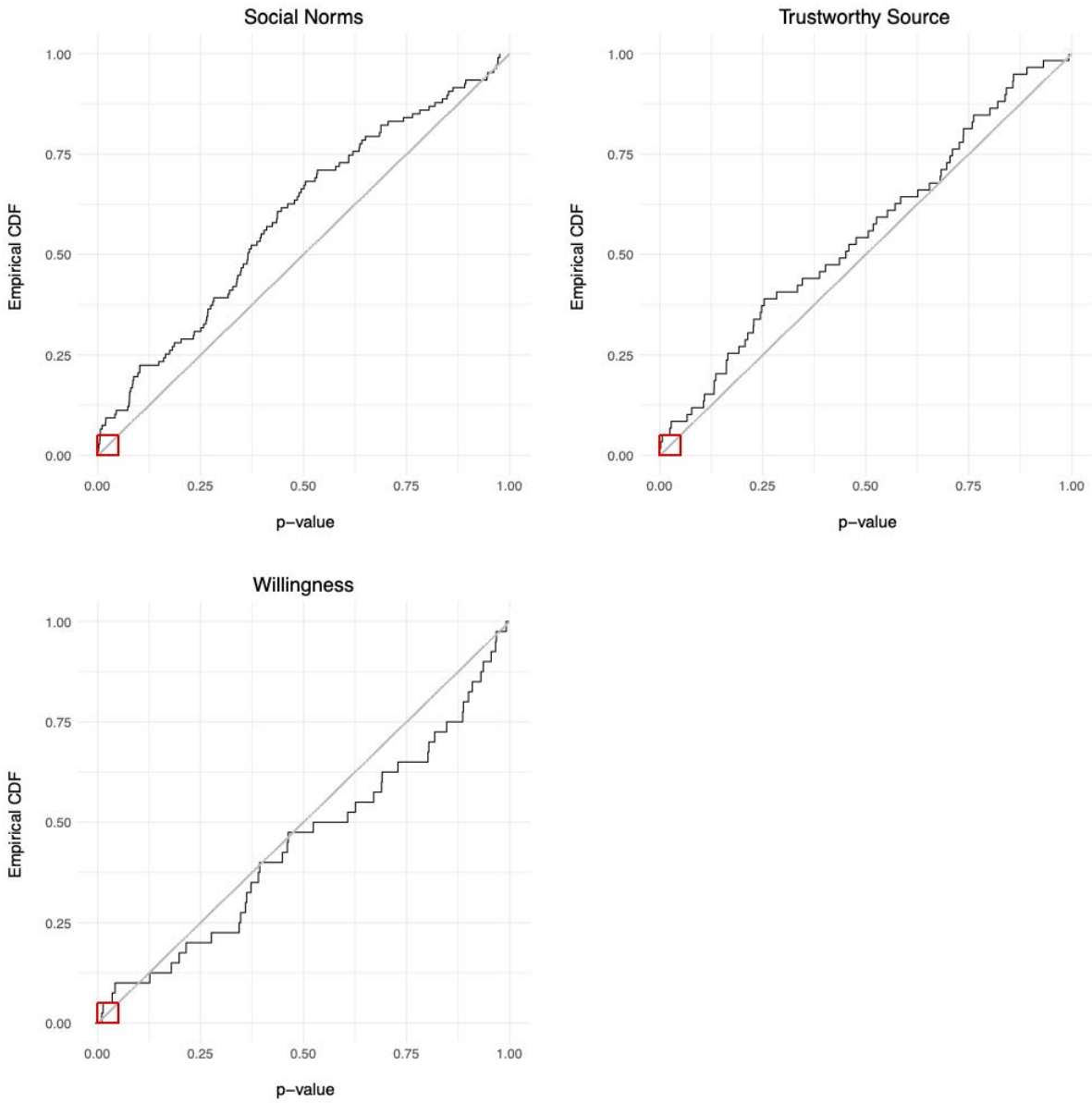


Figure S2. Empirical CDFs of treatment effect p-values across experiments, by outcome. Note how the four questions we observe significant effects for (Importance, Knowledge, Safety, Social Norms) each have CDFs with substantial mass above the uniform distribution for  $p < 0.05$ .

## Alternative Estimates of Cost Effectiveness

There are multiple ways one could think about trying to quantify an aggregate effect across these campaigns in terms of the total number of influenced people and the cost per influenced person. Our preferred approach, as detailed in the main text, is to treat the set of campaigns effectively as one aggregated campaign. Specifically, we can look at the total number of unique users who were exposed to these campaigns and scale that by our treatment effect estimate to get a back of the envelope sense of how many people may have been influenced. To derive an estimate of cost per influenced person, we can then divide total spend for those campaigns by this number.

As a numerical example, to derive the overall estimate of cost per influenced person, we take \$39.4 million (total spend) and divide it by .0055 (ATE estimate) times 2.1 billion (unique reach), which gives \$3.41. We note at the individual campaign level, this procedure is a common industry practice and how advertisers are used to estimating total campaign effects.

One issue with this approach is that it ignores the fact that there were users who were in multiple campaigns. If a campaign changed a person’s mind about Willingness and a separate campaign changed her mind about Importance, some would argue that should count as two influenced events instead of one. This would be an argument for scaling up each individual campaign by its reach, and then summing those estimates across all campaigns; here, instead of influenced ‘people’ arguably influenced ‘opinions’ would be more appropriate. In Table S4 we report out results from this method as well; unsurprisingly, the cost per influenced outcome droops substantially.

Finally, we note because there are individuals who were exposed to multiple campaigns, when we sum the total number of influenced people across columns it will not add up to the overall estimate (the overall estimate will be smaller). This is because a person will appear in as many columns as they were exposed to campaigns that asked a given question; in the overall calculation we are only counting such people once, weighed by the average treatment effect estimate.

Table S4. Alternative Approaches to Calculating Cost per Outcome Influenced.

Category	Effectiveness	Importance	Knowledge	Safety	Social Norms	Trustworthy Source	Willingness	Overall
<b>Main results</b>								
Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0012 (0.0027)	0.0010 (0.0042)	0.0055*** (0.0008)
p-value	0.114	0.0004	5e-7	8e-5	0.0006	0.639	0.807	2e-13
# Total Reach	791,144,678	2,985,622,768	123,2041,831	2,595,117,781	1,312,200,394	561,139,998	1,091,904,828	4,842,392,644
# Unique People Reached	623,509,126	1,424,954,066	859,760,237	1,472,804,702	828,296,583	411,859,697	835,456,995	2,094,285,077
# Total Spend	\$7,072,815	\$14,676,490	\$8,294,165	\$28,750,458	\$6,673,498	\$6,146,421	\$15,707,139	\$39,443,567
<b>Preferred Approach</b>								
<i>Number of Influenced People</i>	2,815,542	6,128,713	10,596,845	9,061,267	6,723,167	511,803	854,596	11,583,543
<i>Cost per Influenced Outcome</i>	\$2.51	\$2.39	\$0.78	\$3.17	\$0.99	\$12.01	\$18.38	\$3.41
<b>Alternative Approach</b>								
<i>Number of Influenced People</i>	3,572,524	12,841,133	15,185,346	15,966,173	10,650,947	697,309	1,116,919	26,783,394
<i>Cost per Influenced Outcome</i>	\$1.98	\$1.14	\$0.55	\$1.80	\$0.63	\$8.81	\$14.06	\$1.47

## Characteristics of Survey Respondents Across Groups

Below we compare how the sample of survey respondents may differ (i) across treatment and control, and (ii) versus the overall population who was exposed to the campaign. The aim of the first analysis (Table S5) is to mitigate concerns that differences in survey outcomes between the groups may be due to a factor other than the treatment ad (e.g., differential response rates). If we observed imbalances in any observable that may be cause for concern given our exposure randomization. Due to data retention limitations, we unfortunately could not recover this data from the same set of experiments in our sample; however, the data below are pulled from 15 COVID-related Brand Lift Studies that were run more recently (these are from the same collection we analyze, just more recent). We can see that at least in this sample, the demographics are not significantly different other than for age, which we control for.

Table S5. Balance of demographics across treatment and control for survey respondents. The p-value is from a two-sided t-test comparing the difference in means across treatment and control; the adjusted p-value uses the same Benjamini-Hochberg procedure as in the main text.

Demographic	$\bar{x}_{control} - \bar{x}_{test}$	Std. Error of Diff.	p-value	Adj. p-value
Age (years)	-0.71	0.23	0.002	0.0374
Gender	-0.01	0.01	0.389	1.000
Friend Count	0.01	15.81	0.9995	1.000
# Friendships Initiated	-2.96	7.58	0.6965	1.000
Subscription Count	-6.75	13.82	0.6251	1.000
Subscriber Count	-7.86	11.06	0.4774	1.000
Long term user	0.00	0.01	0.6196	1.000
Android user	-0.01	0.01	0.1864	1.000
Profile Photo Present	0.00	0.00	0.9974	1.000
Birthday Privacy setting	0.00	0.00	0.5651	1.000
Contact Email Confirmed	0.00	0.00	0.2604	1.000
# Days Active on Platform last 7d	0.00	0.00	0.9724	1.000
Month of birth	-0.01	0.05	0.8029	1.000
Single	-0.01	0.00	0.1341	1.000
US Based	0.00	0.01	0.4413	1.000

Note: Long term users are defined as having been on the site more than five years. Birthday privacy setting is a dummy indicating whether other users on platform can see it or not. These are common, well-populated descriptives of users, either from self-report on their profiles or log data.

The aim of the second analysis (Table S6) is to address representativeness of our sample, and how well we think it might generalize to the broader target population. For this, we take a sample of survey respondents that has been post-stratified by age bucket and gender category back to the population that the campaign reached (as we do in the main text) and compare the reweighted demographics versus those of the target population. To the extent that the reweighted demographics match those of the target population, we can feel more comfortable in how generalizable our

survey-based answers are to the broader population. We note here that in this comparison it is less paramount to see close matching along all demographics; indeed, conditional only on age and gender, we would still expect to see differences across populations. To the extent that the two may differ along a demographic that we think may be correlated with our treatment variable represents a concern, however.

Again due to data retention issues, we cannot use data from our broader study, but report results here for a recent COVID Brand Lift Study where we were able to obtain the relevant data. We can see below that along a relatively large number of demographics the reweighted sample and population means are not significantly different.

The variable which has the largest difference between the groups and which we may be concerned about as correlating with our treatment effect is the Android user indicator. We note that rerunning our main specification for Android users only, we do not detect a significant difference versus our current estimated treatment effect ( $p=0.79$ ).

Table S6. Balance of demographics across campaign population and age bracket and gender post-stratification weighted survey responses. The last column reports the p-value for a two-sided t-test comparing the difference in means across treatment and control.

Demographic	$\bar{x}_{pop} - \bar{x}_{sample}$	Std. Error of Diff.	p-value	Adj. p-value
Age (years)	0.03	0.10	0.779	1.000
Gender	-0.00	0.00	0.617	1.000
Friend Count	47.34	30.43	0.120	1.000
# Friendships Initiated	27.16	14.38	0.059	0.589
Subscription Count	-83.10	27.56	0.002	0.036
Subscriber Count	-28.20	26.30	0.284	1.000
Long term user	0.05	0.02	0.003	0.043
Android user	0.23	0.01	0.000	0.000
Profile Photo Present	-0.01	0.00	0.031	0.366
Birthday Privacy setting (=not visible)	-0.01	0.01	0.317	1.000
Contact Email Confirmed	-0.01	0.01	0.302	1.000
# Days Active on Platform last 7d	-0.02	0.01	0.038	0.416
Month of birth	-0.14	0.13	0.267	1.000
Single	-0.01	0.01	0.419	1.000
US Based	-0.00	0.00	0.288	1.000

## Analysis by Campaign Characteristics: Manually labeled ads

Our main results suggest that effects were larger on knowledge relative to other survey outcomes. In principle, this could be because these campaigns tended to be aimed at providing information to people. Alternatively, it could be that knowledge is easiest to influence or retain. While we are not able to fully disentangle these two possibilities, we were able to conduct an exploratory analysis by looking at the features of campaigns.

Specifically, we pulled the 4,057 English-language ads from across our sample and manually labeled each according to which of our outcome variables was likely to be influenced by the ad.<sup>27</sup> This encompassed 461 of the 819 experiments in our sample; hence, this is a selected sample but still attains relatively broad coverage.<sup>28</sup>

If an ad addressed multiple outcome questions simultaneously, it could be coded as such. For example, if an ad spoke to how safe and effective COVID vaccines are, it would be scored as affecting each outcome. For a specific example, see the Figure.



Figure S3. An ad from the Kaiser Family Foundation’s Greater Than COVID initiative that was in our study. This was scored as a ‘Knowledge’ ad given the clear statement of facts, the links to additional information, and the Learn More call to action.

Once each ad was scored, a campaign would be designated as influencing any outcome that its ads addressed. So for example, if a campaign had 10 ads, three were for Knowledge and 7 were for Effectiveness, this campaign would be coded as designed for each.

<sup>27</sup>To aid objectivity, the manual review was done with only random identifiers available for the campaigns and ads so no study-level treatment effects were known when the labelling was occurring.

<sup>28</sup>No ads explicitly invoked making users trust the advertiser more, let alone as a source of COVID-related information. The Trustworthy Source outcome variable captures more baseline sentiment toward an advertiser and so we do not analyze it here.

Given this scoring of campaigns, we could then reprise our main analysis where for each outcome variable we restrict to experiments that were labeled as addressing that question. Our results are in Table S7.

While some of the point estimates are different from the estimates from our main analysis, we again see that many of the outcomes (including the pooled, overall analysis) have highly significant average effects.

Comparing the coefficients across the relevant row in Table S7, we find that the Knowledge coefficient is significantly higher than those for Importance ( $p = 0.0215$ ), Safety ( $p = 0.0959$ ), and Willingness ( $p = 0.0032$ ), while it is not significantly different from Effectiveness ( $p = 0.1226$ ) and Social Norms ( $p = 0.1835$ ). We note that the point estimate for Effectiveness has increased substantially versus that in our main text. However, the impact on Knowledge is similar in the subset of campaigns coded as geared toward influencing Knowledge as it is for the full set of campaigns. While not dispositive, this exploratory analysis suggests that Knowledge might be able easier to be influenced, and that targeted campaigns are more important for impacting perceived effectiveness, but leave it to future research to disentangle further.

Table S7. Labeled Ads Analysis

Category	Effectiveness	Importance	Knowledge	Safety	Social Norms	Willingness	Overall
<b>Main results</b>							
Main Treatment Coefficient	0.0045 (0.0029)	0.0043*** (0.0012)	0.0123*** (0.0025)	0.0062*** (0.0016)	0.0081*** (0.0024)	0.0010 (0.0042)	0.0055*** (0.0008)
Coefficients from Labeled Campaigns	0.0242*** (0.0074)	0.0027 (0.0019)	0.0117*** (0.0034)	0.0048*** (0.0023)	0.0196*** (0.0048)	-0.00999 (0.0065)	0.00589*** (0.00079)
# labeled experiments	199	379	392	256	216	400	461

## Correlation with Vaccine Takeup

To understand how our survey-based outcomes relate to actual behavior, we compare survey responses at the county level to Center for Disease Control (CDC) data on vaccine series completion rate. For each county, we aggregate survey responses and calculate a rate of positivity (e.g., the percent of positive responses). Then, we combine this with county-level vaccine series completion estimates from the CDC from November 15, 2021, when the final campaigns in our sample concluded. We find a positive association between survey positivity rate and vaccine uptake, which can be seen in Figure S4.

To evaluate the magnitude of the correlation, we estimate ordinary least squares regressions where the outcome is survey positivity rate and the independent variable is vaccination rate, where each variable is aggregated at the county level:

$$y_i = \beta_0 + \beta_1 r_i + \beta_2 p_i + \beta_3 r_i p_i + \varepsilon_i \quad (3)$$



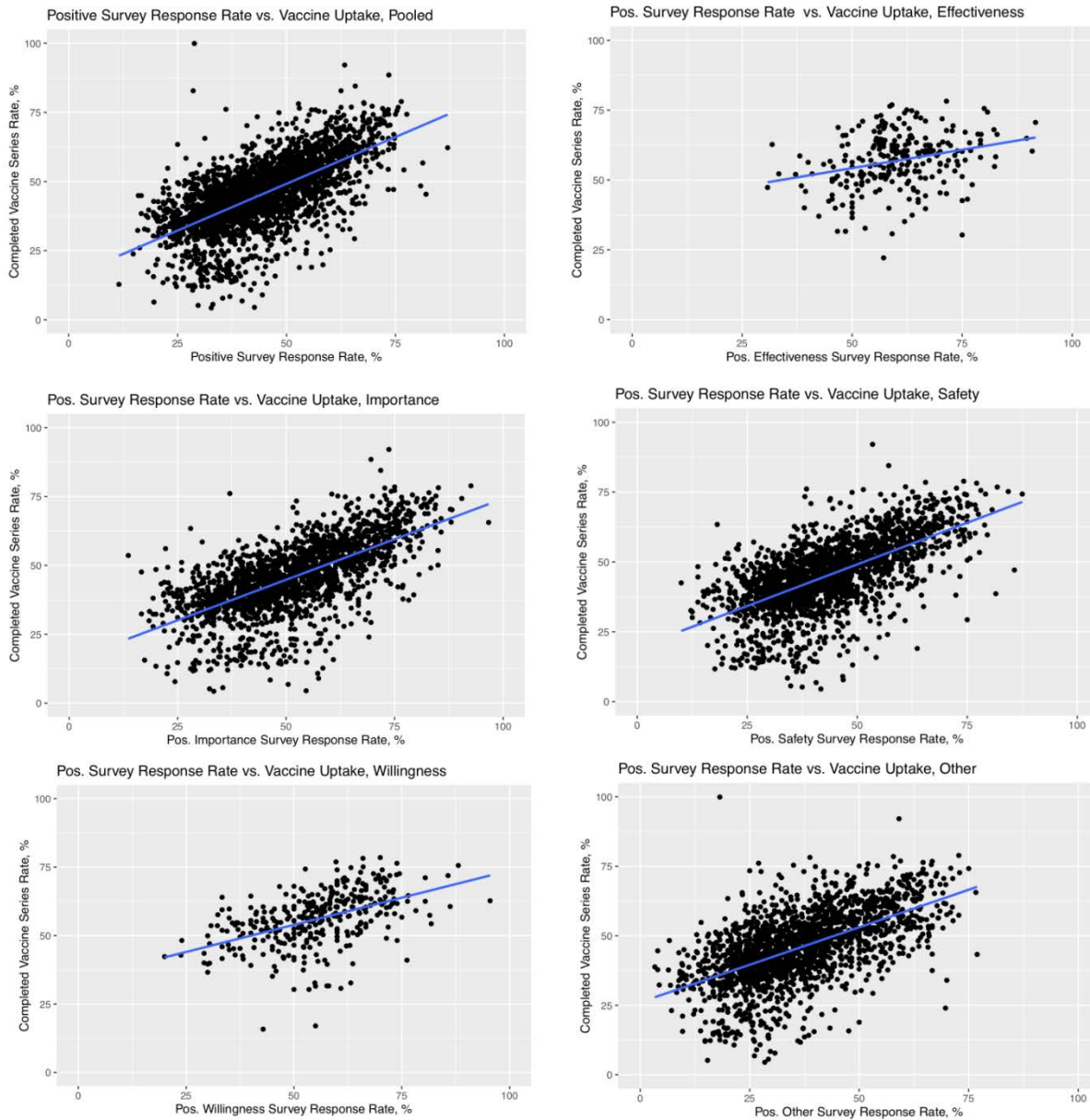


Figure S4. Scatterplots of county-level positive survey response rate versus vaccine series completion rate from the CDC. Note that the sample is limited to counties with at least 20 survey responses for each outcome.

For each county  $i$ ,  $y_i$  represents the vaccine series completion rate,  $r_i$  is the positive survey response rate, and  $p_i$  is the demeaned log population for that county. We further include each county's demeaned logged population and its interaction with survey positivity rate as controls. These adjustments are designed to address the concern that smaller counties have fewer survey responses, while county size is also correlated with  $y_i$  and  $r_i$ . We further consider specifications excluding counties with fewer than 10 and 20 survey responses, and as well as specifications with log-odds transformations of the positivity rate and vaccine completion rate.

Table S8. Analysis of Pooled Survey Responses vs. Vaccine Takeup Rate

Linear vs. Log-Odds	Linear	Linear	Linear	Log-Odds	Log-Odds	Log-Odds
Min. Response Count	None	10	20	None	10	20
Coefficient on Positivity Rate	0.5236*** (0.0150)	0.5632*** (0.0163)	0.5967*** (0.0174)	0.4495*** (0.0185)	0.5516*** (0.0229)	0.6011*** (0.0196)
Counties in Sample	3119	2931	2710	3119	2931	2710

Note: Standard errors in parentheses. For each column, we consider the designated model specification for the set of pooled county-level data, across all survey outcomes.

These results suggest that for every additional percent of positive survey responses, we would expect to see an incremental increase in vaccine takeup rate of about 0.6 percent, if we rely on the sample restricting to counties with at least 20 responses. Applying this to the average cost per influenced person of \$3.41, we estimate that the effective cost per incremental vaccinated person is about \$5.68.

To understand variation between survey outcomes, we also restrict the sample of counties to those for which we have at least 10 survey responses for the four outcome categories (including a grouped 'Other' category) with the most counties with at least 10 responses, and run the same linear specification for each outcome. Since not every county in our sample has survey responses for each category, restricting the sample to those counties which do have sufficient responses allows us to compare across outcomes.

These results suggest that for every additional percent of positive survey responses, we would expect to see an incremental increase in vaccine takeup rate of about 0.51 percent for Importance, 0.57 percent for Safety, 0.22 percent for Willingness, and 0.42 for Other outcomes. Applying this to the average cost per influenced person for each outcome, we estimate that the outcome-specific effective cost per incremental vaccinated person is about \$4.73 for Importance, \$5.61 for Safety, and \$77.91 for Willingness.

Table S9. Analysis of Survey Responses vs. Vaccine Takeup Rate, by Outcome

Outcome Category	Pooled	Importance	Safety	Willingness	Other
Coefficient on Positivity Rate	0.6988*** (0.0284)	0.5088*** (0.0253)	0.5714*** (0.0242)	0.2227*** (0.0225)	0.4151*** (0.0265)
Counties in Sample	815	815	815	815	815

Note: Standard errors in parentheses. For each column, we assess the positivity rate for the designated survey outcome.

## Survey Details

We note that exact wording within a language was largely the same across experiments, but advertisers did have autonomy to tweak if they wanted. Questions for Knowledge, Social Norms, and Trustworthy Source had a bit more variation than the others, but the questions still targeted the same concept. Questions were translated into the languages specified by a user's settings. Core questions are noted below, with responses and coding as desired/not in parentheses after each response:

### Importance

- How important do you feel a vaccine is to prevent the spread of COVID-19?
  - Very important (1)
  - Somewhat important (1)
  - Barely important (0)
  - Not important (0)
  - I don't know (0)

### Safety

- How safe do you think a COVID-19 vaccine is for people like you?
  - Very safe (1)
  - Somewhat safe (1)
  - Barely safe (0)
  - Not safe (0)
  - I don't know (0)

### **Willingness**

- How likely are you to get vaccinated for COVID-19 when the vaccine is available to you?
  - Very likely (1)
  - Somewhat likely (1)
  - Somewhat unlikely (0)
  - Very unlikely safe (0)
  - I don't know/I already got vaccinated (0)

### **Effectiveness**

- How effective do you think the COVID-19 vaccination is in preventing COVID-19?
  - Very effective (1)
  - Somewhat effective (1)
  - Barely effective (0)
  - Not effective (0)
  - I don't know (0)

### **Knowledge**

- Do you know where people in your local community can go to get a COVID-19 vaccine?
  - Yes (1)
  - No (0)
  - I don't know (0)

### **Social Norms**

- When you think of most people whose opinion you value, how much would they approve of people getting a COVID-19 vaccine?
  - Definitely approve (1)
  - Mostly approve (1)
  - Somewhat approve (0)
  - Not at all approve (0)
  - I don't know (0)

### **Trustworthy Source**

- Do you agree or disagree that [advertiser name] is a trustworthy source of COVID-19 vaccine facts and information?
  - Strongly agree (1)
  - Somewhat agree (1)
  - Somewhat disagree (0)
  - Strongly disagree (0)
  - I don't know (0)