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ABSTRACT

We estimate the effect of social media deactivation on users' emotional state in two large randomized experiments before the 2020 U.S. election. People who deactivated Facebook for the six weeks before the election reported a 0.060 standard deviation improvement in an index of happiness, depression, and anxiety, relative to controls who deactivated for just the first of those six weeks. People who deactivated Instagram for those six weeks reported a 0.041 standard deviation improvement relative to controls. Exploratory analysis suggests the Facebook effect is driven by people over 35, while the Instagram effect is driven by women under 25.

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A data appendix is available at: http://www.nber.org/data-appendix/w33697
A randomized controlled trials registry entry is available at: https://osf.io/t9q2f

There is an active debate over how social media affect users' psychological well-being. Do social media make people happier, for example by facilitating beneficial social connections?[1] Or do they make people depressed and anxious, for example by reducing face-to-face interactions or increasing unfavorable social comparisons?[2] Some analysts argue that social media have contributed to the alarming recent decline in young people's mental health,[3] and policymakers have responded with legislation and legal action.[4] These high-stakes debates have relied primarily on evidence from time-series and cross-sectional correlations, plus a few relatively small randomized experiments, and scholars disagree on the implications (Odgers 2024; Dubner 2024; Capraro et al. 2024).

In a separate trend, American elections have become increasingly stressful: an August 2020 study found that 68 percent of American adults cited the upcoming election as a significant source of stress, a material increase from 2016 (American Psychological Association 2020). Other studies find that exposure to political news reduces psychological well-being (Pierce, Rogers and Snyder 2016; Simchon et al. 2020; Gray, Pickard and Munford 2021; Ford et al. 2023; Kimball et al. 2024). Since many people get political news on social media (Allcott and Gentzkow 2017), these facts raise the question of how using social media before an election affects people's emotional state.

In this paper, we report the results of the largest-ever experimental study on the effect of social media deactivation on users' emotional state, which we carried out as part of a broader study of political outcomes before the 2020 U.S. presidential election. We recruited 19,857

[1]See Reis, Collins and Berscheid (2000), Chopik (2017), and Chetty et al. (2022) on the importance of human connection and social capital.

[2]See Verduyn et al. (2015), Tromholt (2016), Hunt et al. (2018), Turel, Cavagnaro and Meshi (2018), Cohen et al. (2019), Brailovskaia et al. (2020), Mosquera et al. (2020), Ozimek and Bierhoff (2020), Siegel (2020), Castaño-Pulgarín et al. (2021), Przybylski et al. (2021), van Wezel et al. (2021), Collis and Eggers (2022), Brailovskaia et al. (2023), González-Bailón et al. (2023), and Thai et al. (2023).

[3]See Twenge (2017), Engeln et al. (2020), Office of the Surgeon General (2021), Wells, Horwitz and Seetharaman (2021), and Haidt (2023).

[4]See Archie (2023), New York State Attorney General (2023), Utah Governor's Office (2023), European Commission (2024), and National Conference of State Legislatures (2024).

Facebook users and 15,585 Instagram users who spent at least 15 minutes per day on the respective platform. We randomly assigned 27 percent of participants to a treatment group that was offered payment for deactivating their accounts for the six weeks before the election. The remaining participants formed a control group that was paid to deactivate for just the first of those six weeks. Our baseline and endline surveys elicited three measures of self-reported emotional state—how much of the time during the past four weeks that people felt happy, depressed, or anxious—along with a large suite of political outcomes that we study separately in Allcott et al. (2024).

We estimate that users in the Facebook deactivation group reported a 0.060 standard deviation improvement in an index of happiness, anxiety, and depression, relative to control users. The effect is statistically distinguishable from zero at the $p < 0.01$ level, both when considered individually and after adjusting for multiple hypothesis testing along with the full set of political outcomes considered in Allcott et al. (2024). Non-preregistered subgroup analyses suggest larger effects of Facebook on people over 35, undecided voters, and people without a college degree.

We estimate that users in the Instagram deactivation group reported a 0.041 standard deviation improvement in the emotional state index relative to control. The effect is statistically distinguishable from zero at the $p = 0.016$ level when considered individually, and at the $p = 0.14$ level after adjusting for multiple hypothesis testing along with the outcomes in Allcott et al. (2024). The latter estimate does not meet our pre-registered $p = 0.05$ significance threshold. Substitution analyses imply this improvement is achieved without shifts to offline activities. Non-preregistered subgroup analyses suggest larger effects of Instagram on women aged 18-24.

We offer several points of comparison for the effect sizes. After controlling for other demographics, emotional state is 0.48 standard deviations higher for Republicans than for Democrats in our sample. The average psychological intervention reported in the van Agteren et al. (2021) meta-analysis improved emotional state by 0.27 standard deviations. Finally, a different index of young people's emotional state worsened by 0.37 standard deviations between 2008 and 2022.

Two additional results provide insight into mechanisms. First, app use data show that when people deactivate, most of time freed by Facebook deactivation and all of time freed by Instagram deactivation is substituted to other smartphone apps. Thus, we expect no effect on offline time for individuals in the Instagram group and some moderate effects on offline time for those in the Facebook group. Instead, this suggests that the effects are mostly driven by the different user experiences of Facebook or Instagram compared to other apps. Second, the effects are not significantly different for people with higher online or offline baseline political engagement. This provides no evidence that the effects are driven by factors specific to the election period.

Our work relates to a large literature on the relationship between social media use and emotional state. The vast majority of prior studies focus on non-experimental results such as cross-sectional or longitudinal correlations; the recent Hancock et al. (2022) meta-analysis included 226 correlation studies. We find that in our data, standard non-experimental approaches yield results that are biased in unpredictable directions relative to the experimental estimates.

We are aware of seven prior experiments estimating the effects of at least one week of social media abstention.[5] The largest prior experiment, Allcott et al. (2020), was written by two of the lead authors of this study, and we reused key elements of that earlier design. Our work goes significantly beyond Allcott et al. (2020) and the other prior experiments in several ways. First, our experiment is the first to specifically estimate the impact of Instagram access. Given the different user experiences and populations on Instagram versus Facebook, it would have been unclear whether effects of Facebook deactivation translate to Instagram. The second is

[5]The seven experiments are Tromholt (2016), Turel et al. (2018), Allcott et al. (2020), Mosquera et al. (2020), Hall et al. (2021), Lambert et al. (2022), and Arceneaux et al. (2023); see Appendix Table S30.

size and scope: our total sample is about 20 times larger than in the largest prior experiment, and we required longer abstention than any prior study. Given this much larger sample size, we are better powered for subgroup analysis; the heterogeneous effects we document by age and gender had gone undetected in previous experimental work. The third is data: we are the only experiment to leverage internal data from a social media platform and to meter substitution to other smartphone apps. Our result that most of time freed by deactivation is substituted to other apps is crucial to understanding mechanisms, but Allcott et al. (2020) and others had relied on self-reports that did not make this clear. Fourth, ours is the only experiment to be carried out in the context of a U.S. presidential election. Social media use before a presidential election could have different effects on emotional state than use before midterm elections in non-election periods, given heightened media coverage and perceived higher stakes.

Allcott et al. (2024) studies the same experiment and is covered by the same pre-analysis plan, but that paper considers only political outcomes, not emotional state. We ran this study during the election period because our primary goal was to estimate effects on political outcomes. Our work also builds on quasi-experimental evidence in Braghieri, Levy and Makarin (2022), which finds that Facebook worsened college students' mental health when it was rolled out in 2004 and 2005. Facebook's user experience was very different during that rollout period—for example, there was no news feed—so the effects could be quite different two decades later.

We also build on prior work on emotional responses to elections. Existing studies primarily describe the time path of emotional state for different groups, including the response to election outcomes (e.g., Pierce et al. 2016; Suzuki et al. 2023; Kimball et al. 2024). We focus on how social media use during an election season affects emotional state.

This paper has several important limitations. First, our findings are only directly informative about the people who agreed to participate and deactivate their accounts for the payments we offered. While we use weights to adjust for sample selection, our sample may also be selected on unobserved characteristics. Second, our experiment measures the effects of an incremental five weeks of individual deactivation before the 2020 election. Effects could differ for longer-term deactivation, simultaneous deactivation of many users, deactivation during a non-election period, or deactivation during a future election. For context, about six percent of Facebook content viewed in 2020 was politics-related (Schultz 2020), and Facebook and Instagram have both reduced political content in news feeds since 2020 (Meta 2024). Third, our analysis relies on three specific self-reported emotional state survey questions in the context of a larger survey focused on political outcomes. Effects could differ for other emotional state measures or on a different survey instrument (see, e.g., Zaller and Feldman (1992)). Fourth, although we designed the experiment to mitigate experimenter demand effects and previous work suggests that such effects may be limited in our context (De Quidt, Haushofer and Roth 2018; Mummolo and Peterson 2019; Allcott et al. 2020), we cannot definitively rule out the possibility that survey responses were influenced by participants' knowledge that they were in an experiment. Finally, the baseline emotional state outcomes were imbalanced by chance in the Facebook sample, and not all participants completed the endline surveys, although the data suggest that these issues are unlikely to substantially affect the results.

This project is part of the U.S. 2020 Facebook and Instagram Election Study (González-Bailón et al. 2023; Guess et al. 2023a,b; Nyhan et al. 2023; Allcott et al. 2024), a partnership between Meta researchers and unpaid independent academics. Under the terms of the collaboration, the independent academic authors had final authority over the pre-analysis plan, data analysis, and manuscript text, and Meta could not block any results from being published. More details of this partnership are in Appendices E and F. We have posted answers to frequently asked questions online at this website.

Sections 1-5, respectively, present the experimental design, descriptive statistics, empirical strategy, impact evaluation results, and conclusion.

1 Experimental Design

We ran two parallel experiments, with Facebook and Instagram as the respective "focal platform." For each focal platform, Meta drew a stratified random sample of users who were in the U.S., were age 18 or older, and had logged in at least once in the past month. From August 31 to September 12, Meta placed survey invitations at the top of these users' focal platform news feeds. People who clicked on the invitations were told about the study and asked what weekly payments they would be willing to accept to deactivate their focal platform accounts for either one or six weeks. Those who were willing to deactivate for $25 per week and consented to participate were immediately sent to the National Opinion Research Corporation (NORC) website to complete a short enrollment survey. NORC fielded the baseline and endline surveys on September 8-21 and November 4-18, respectively.

Participants who completed the baseline survey were randomized into two groups: Deactivation (27 percent) and Control (73) percent.[6] The Deactivation group was told that they would receive $150 if they did not log into the focal platform for the next six weeks, while the Control group was told that they would receive $25 if they did not log in for the next week.[7] By including a short deactivation period for the Control group, we guaranteed that the only differences between Control and Deactivation were the total payment amount and deactivation length, and that all participants would perceive themselves to be part of a study that involved deactivating their social media accounts. This reduced the risk of experimenter demand effects, differential attrition, and any spurious effects that might be artifacts of the deactivation process itself.

Meta deactivated participants' focal platform accounts starting September 23. Control and Deactivation group accounts were automatically reactivated on September 30 and November 4,

[6]Randomization was stratified into 36 strata defined by an indicator for residence in an election swing state, average daily time spent on Facebook or Instagram over the previous 30 days, self-reported political party identification, and race.

[7]Since the Deactivation condition was more expensive than Control, we allocated a smaller share to the former to increase power per dollar of cost.

respectively.

The research goal was to evaluate the core Facebook and Instagram products. Thus, we allowed participants to still use their Facebook and Instagram credentials to access other apps, including WhatsApp and Facebook Messenger. Participants could log back into the Facebook or Instagram app and reactivate their accounts at any time. People who logged in were reminded that they would lose their deactivation payments but were asked to complete the remaining surveys.

Participants were paid at least $5 for completing the baseline survey and at least $20 for endline. Participants were also offered additional payment to allow passive tracking of their smartphone app and web browser use.

1.1 Emotional State Survey Questions

The baseline and endline surveys covered demographics and a wide range of political beliefs and behaviors. This paper focuses on the three emotional state questions that were also included: "Please tell us how much of the time during the past four weeks you felt [happy / depressed / anxious]." The response options were "All of the time," "Often," "Sometimes," "Rarely," and "Never." We code these responses as 4, 3, 2, 1, and 0, respectively, and then standardize each to have a mean of zero and a standard deviation of one in the sample-weighted combined Control groups, giving the variables *happy*, *depressed*, and *anxious*. We re-sign *depressed* and *anxious* as *depressed x (-1)* and *anxious x (-1)*, so that more positive values correspond to more positive emotional state. The *emotional state index* is the average of these three re-signed variables, re-standardized to have a standard deviation of one in the sample-weighted combined Control groups.

These three questions were drawn from the European Social Survey Well-being Module (Huppert et al. 2009) and are similar to other established emotional state measures. The *emo-*

tional state index has a Cronbach's alpha of 0.79 and 0.78 in the Facebook and Instagram samples, respectively, and is correlated in expected ways with demographics and other emotional state measures. See Appendix B for more information on item development, reliability, and validity.

1.2 Pre-Analysis Plan, Multiple Hypothesis Testing, and Subgroup Analysis

We submitted an initial pre-analysis plan (PAP) on September 22 and a slightly updated final PAP on November 3rd, the day before the endline survey began. We followed the PAP in all respects, with the exception of six minor deviations or clarifications reported in Appendix G, which were mainly driven by changes in data availability relative to what we anticipated. All of the pre-specified analyses except for effects on emotional state outcomes are presented in Allcott et al. (2024).

The PAP originally implied that results for all outcomes would be presented in a single paper. However, as we drafted the paper, it became clear that it was not possible to fully present the motivation, related literature, robustness, and interpretation for both the political and emotional state outcomes in a single paper, so we split the results into two.

The PAP stated that the primary analysis sample would include only participants with greater than 15 minutes per day baseline use, as we expected that deactivation would have limited effects for people who rarely use the platform.

The PAP stated that the *emotional state index* was a "secondary outcome," and the three individual components of emotional state were "auxiliary outcomes." To control for multiple hypothesis testing, the PAP stated that for *emotional state index* and all other secondary outcomes, we would present sharpened False Discovery Rate (FDR) adjusted q-values (Benjamini, Krieger and Yekutieli 2006) adjusted against all 61 primary and secondary outcomes, including political beliefs, political polarization, and voting behavior. We also present unadjusted p-values on *emotional state index*, which may be relevant to researchers with a specific a priori interest in effects on emotional state (Kling, Liebman and Katz 2007; Casey, Glennerster and Miguel 2012). The PAP stated that we would report unadjusted p-values on auxiliary outcomes such as the three emotional state components. Wasserstein and Lazar (2016) and Imbens (2021) argue against overemphasizing binary statements about whether a result is "statistically significant." The PAP stated that for any such statements we do make, we would use two-sided tests with a $p < 0.05$ threshold.

The pre-analysis plan described how we would carry out subgroup analyses for the experiment's primary outcomes, but it did not specify what subgroup analysis might be done for secondary outcomes such as emotional state. The preregistered subgroups were chosen with the primary political outcomes in mind, and some are thus less relevant for emotional state. We report effects in the preregistered subgroups in Appendix D.4. In the body of the paper, we present non-preregistered "exploratory" analysis of subgroups defined by above- vs. below-median values of several moderators that are potentially relevant for emotional state. We present unadjusted p-values for subgroup analyses.

Our first exploratory moderator is the interaction of gender and above- vs. below-median age. (The preregistered moderators for the primary outcomes included median splits of age and gender separately, but not interacted.) This is a key potential moderator because as described in the introduction, some people argue that social media have contributed to the recent decline in young people's mental health, with particular concern about Instagram's effect on young women (Twenge 2017; Engeln et al. 2020; Office of the Surgeon General 2021; Wells et al. 2021; Haidt 2023). The age survey question had coarse response categories; we allocate the median category to the above-median group. This yields 18-34 vs. 35+ and 18-24 vs. 25+ as the age groups in the Facebook and Instagram samples, respectively,.

The second exploratory moderator is baseline use. (This was also a preregistered moderator for the primary outcomes.) This is a key potential moderator because deactivation effectively imposes a larger dose of treatment on heavier users.

The third exploratory moderator is baseline emotional state. This is a key potential moderator because Allcott et al. (2020) find that Facebook deactivation had more beneficial effects for people with worse baseline emotional state, perhaps because they were more vulnerable to unfavorable social comparisons.

The final two exploratory moderators measure political engagement. They are key potential moderators because they speak to mechanisms. If more politically engaged people experience larger effects, this suggests that the effects of deactivation might be smaller outside of an election period or after Meta's decision to reduce political content in news feeds. The fourth potential moderator is baseline political participation, which is the sum of indicators for whether the participant reported doing the following six activities in the past month: (i) attended a protest or rally, (ii) contributed money to a political candidate or organization, (iii) signed an online petition, (iv) tried to convince someone how to vote, (v) posted political messages online, and (vi) talked about politics. (This was also recorded at endline and was preregistered as a primary outcome.) The fifth potential moderator is the count of civic and political pages that the user was following at the beginning of the experiment. This measure only exists for Facebook.

2 Descriptive Statistics

On Facebook, a total of 10.6 million users were invited to the study, 673,388 clicked the invitation, and 43,249 were willing to deactivate, consented to participate, and completed the enrollment survey. Of these, 19,857 completed the baseline survey, could be linked to platform data, and had at least 15 minutes of baseline use per day. This final group is our "primary analysis sample." On Instagram, the analogous numbers are 2.6 million invites, 319,271 clicks, 42,658 enrollment survey completes, and 15,585 participants in the primary analysis sample. See Appendix Table S10 for details.

The fact that less than one percent of the people who were invited to the study completed the experiment underscores that one should be cautious in generalizing results outside our sample. Most of this sample selection is driven by the fact that only a few percent of people click on research study invitations or social media ads. The degree of sample selection in our study is slightly less severe than previous social media deactivation studies, primarily because Meta's research study invitations (which were fixed at the top of users' news feeds) had a higher click-through rate than the standard social media ads used in prior work.[8] In comparison to the full populations of Facebook and Instagram users, our study sample has a higher proportion of users with liberal views and high civic engagement; see Appendix Tables S11 and S12. Our sample weights address sample selection on these and other observables.

Of participants in the Facebook and Instagram primary analysis samples, respectively, 17,802 and 13,480 completed the endline survey. These attrition rates (10 and 13 percent) are relatively low: they are less than the mean of 96 field experiments published in top economics journals that were surveyed in Ghanem, Hirshleifer and Ortiz-Becerra (2022). However, Appendix Table S13 shows that in both experiments, the Deactivation group was about two percentage points more likely to finish endline than the Control group, and this difference is statistically significant in our large sample. Relatedly, Appendix Figure S6 shows that the Deactivation group generally completed endline earlier than the Control group. We assess the possible importance of this differential attrition and response timing below.

The Deactivation and Control groups are balanced on a pre-specified set of demographics at both baseline and endline; see Appendix Tables S14 and S15. In non-preregistered exploratory analysis, we also test for differences between the Deactivation and Control groups in baseline

[8] In Allcott et al. (2020), 1.9 million users were shown ads, of whom 2,897 were randomized. In Asimovic et al. (2021), 365,599 users saw ads, of whom 556 were randomized.

values of the emotional state variables; see Appendix Tables S16 and S17. For Instagram, there are no statistically significant differences. For Facebook, the Deactivation group has statistically significantly worse baseline emotional state. Since there is no evidence of imbalance elsewhere in the experiments, this appears to have occurred by chance. If we did not control for baseline emotional state, this imbalance would bias against our finding that Deactivation improved emotional state. As pre-specified, our regressions control for baseline emotional state, which mitigates that bias. Bruhn and McKenzie (2009) find that after controlling for observables that predict the outcome (which we do), randomizations with chance imbalance are no more likely to generate false hypothesis rejections than those without chance imbalance.

Participants set up passive tracking gradually after the enrollment survey. We limit our passive tracking analyses to participants who have (i) at least two days of baseline data before deactivation began on September 23 and (ii) non-missing data on at least 85 percent of days both during their baseline period and in the post-intervention period between September 23 and November 4. About 29 and 25 percent of the Facebook and Instagram primary analysis samples have valid passive tracking data, and these shares are not statistically different between Deactivation and Control groups.

Appendix B provides descriptive evidence on the time path of emotional state in our sample. Prior work has documented (i) overall negative effects of exposure to politics (Suzuki et al. 2023), (ii) lower average well-being of Democrats relative to Republicans (Simchon et al. 2020), and (iii) short-lived drops in subjective well-being when someone's preferred candidate loses (Pierce et al. 2016). Our data are consistent with all three of these facts. Control group users reported worse emotional state on the endline survey (covering the period leading up to and including election day) than they do at baseline or on a post-endline survey in December. Democrats report worse emotional state than Republicans at baseline, but the gap may have narrowed among people who completed endline after media outlets called the election for

Biden.

3 Empirical Strategy

As we discuss below, not all Deactivation group participants chose to stay deactivated. To estimate the causal effect of deactivation in the presence of this imperfect compliance, we pre-specified that we would use the following instrumental variables regression. We define D_i as a measure of participant i's deactivation compliance, $\boldsymbol{X}_i$ as a vector of controls, ν_s as a vector of randomization stratum indicators, and Y_i as an outcome. The estimating equation is

$$Y_i = \tau D_i + \rho \boldsymbol{X}_i + \nu_s + \varepsilon_i, \tag{1}$$

where we instrument for D_i with a Deactivation group indicator variable T_i. Unless otherwise stated, all analyses below weight observations to make the sample representative of focal platform users with baseline use greater than 15 minutes per day on race, political party, education level, and several measures of platform use. Appendix A.2 further describes the sample weights.

As pre-specified, the control variables $\boldsymbol{X}_i$ are those selected by a lasso regression of the endline outcome Y_i on its baseline value plus a set of demographic variables and baseline political beliefs and behaviors. Deactivation compliance is defined as $D_i = 1 - U_i/\bar{U}_C$, where U_i is the share of days that participant i used the platform (measured by seeing five or more pieces of content) during the September 30 - November 3 treatment period, and $\bar{U}_C$ is the Control group average. $D_i = 1$ for participants who never used the platform, and $D_i = 0$ for those with usage equal to the Control group average. Thus, τ measures the local average treatment effect of never using the platform instead of using the Control group average, for people induced to deactivate by the $150 payment.

4 Impact Evaluation Results

4.1 First Stage

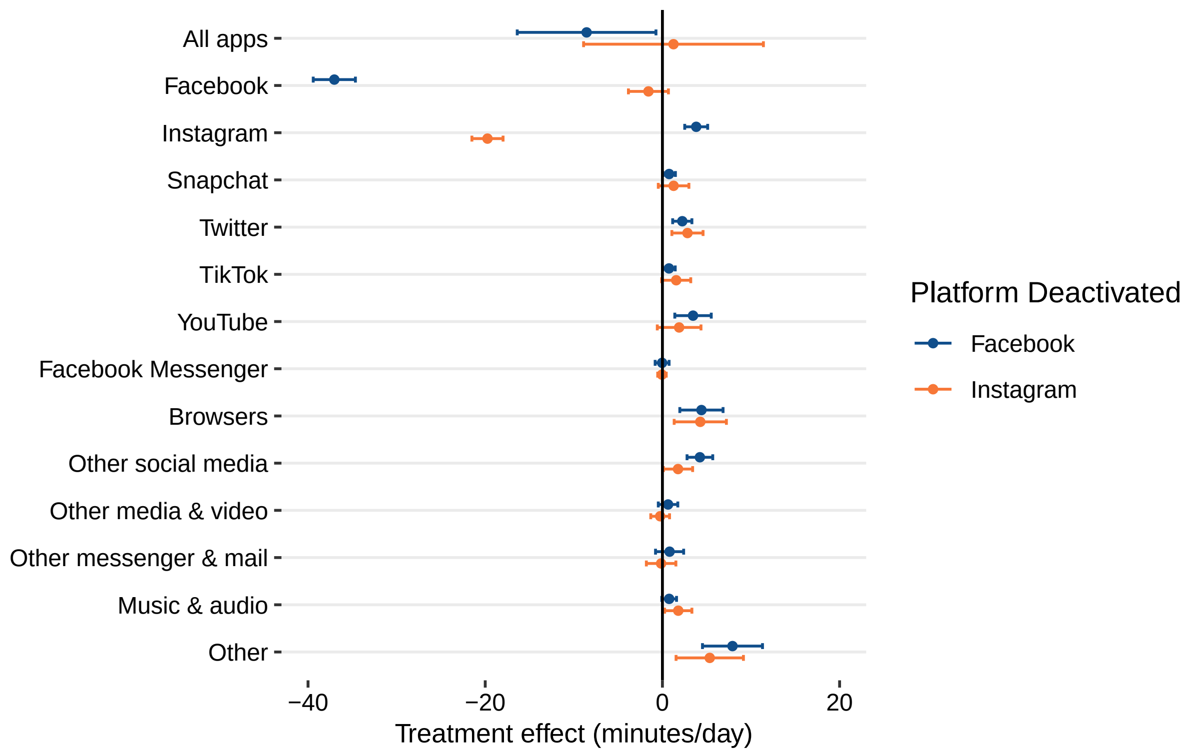
Before the experiment began, about 90 percent of participants in each experiment used the focal platform on any given day. During the five weeks from September 30-November 3, when only the Deactivation group was being paid to stay deactivated, the Control groups' daily usage rate was again about 90 percent, compared to 15-20 percent in the Deactivation groups. Correspondingly, the first stage coefficients in the Facebook and Instagram samples are 0.871 and 0.893, respectively. See Appendix D.1 for details.

4.2 Substitution to Other Apps

How people reallocate the time gained from deactivation could be a crucial determinant of the effects on emotional state (DellaVigna and La Ferrara 2015; Allcott et al. 2020). For example, if any effects of Facebook or Instagram are from exposure to stressful political content or content that induces unfavorable social comparisons, it would matter if users substitute to other apps with similar content. If any effects are from reduced in-person interactions, it might matter if deactivation reduces overall smartphone screen time.

Figure 1 presents the effects of Facebook and Instagram deactivation on time spent on mobile applications measured in our passive tracking sample. We present the combined effect on all mobile apps (including Facebook and Instagram), then individual effects on seven heavily used social media apps (again including Facebook and Instagram), and finally for all remaining apps grouped into six categories. Since the bottom 13 rows are mutually exclusive and exhaustive, those effects sum to the effect on all apps presented in the first row. For reference, the Facebook and Instagram Control group participants in the passive tracking sample averaged about 51 and 25 minutes per day, respectively, on the Facebook and Instagram mobile apps during the deactivation period; see Appendix Figure S8. These groups averaged 108 and 126 minutes per day, respectively, on all social media apps combined.[9]

Figure 1: Effects of Deactivation on Use of Selected Applications



Note: This figure presents local average treatment effects of Facebook and Instagram deactivation estimated using equation (1). "All apps" is the sum of time spent across all mobile applications. The apps and groups in the next 13 rows are mutually exclusive and exhaustive. The horizontal lines represent 95 percent confidence intervals.

The first row of Figure 1 shows that Instagram deactivation had a small and insignificant effect on total app usage, implying that all of the time participants would have spent on Instagram was substituted to other apps. Facebook deactivation decreased total app usage by an estimated 9 minutes per day. This suggests that while Facebook deactivation increased time spent offline, Instagram deactivation did not. Allcott et al. (2020) report larger substitution to time spent offline based on self-reported data.

[9]Several estimates in this section are slightly different than in Allcott et al. (2024) due to our improved approach to addressing missing passive tracking data.

The second and third rows show that Facebook and Instagram deactivation reduced use of the focal platforms by 37 and 20 minutes per day, respectively. Comparing the coefficients in the first and second rows indicates that around three-quarters of the reduction in Facebook use from deactivation was substituted to other apps.

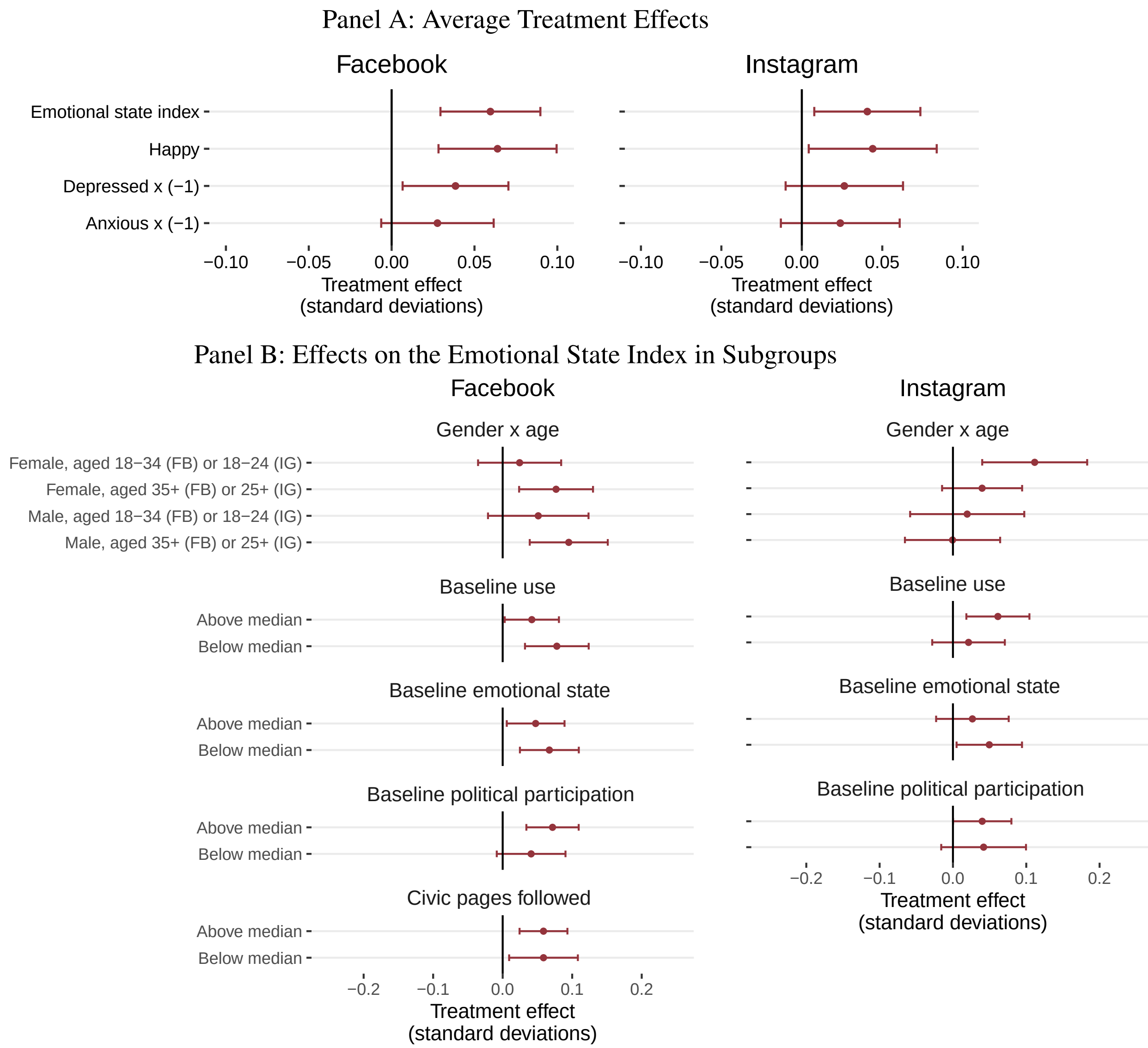
Facebook deactivation increased Instagram use by 4 minutes per day, while Instagram deactivation did not significantly affect Facebook use. The next eleven rows show that Facebook and Instagram deactivation both increased use of Twitter, Snapchat, TikTok, YouTube, web browsers, other social media apps, and other non-categorized apps by a few minutes per day.

Thus, the effects on emotional state that we document below reflect the combined effect of reduced use of the focal platform and increased use of other substitute apps.

4.3 Effects of Deactivation on Emotional State

Average effects. Panel A of Figure 2 reports the local average treatment effects of Facebook and Instagram deactivation on the *emotional state index* and its three components. Appendix D.3 presents all point estimates and p-values. Facebook and Instagram deactivation improved *emotional state index* by 0.060 standard deviations ($p < 0.001$) and 0.041 standard deviations ($p = 0.016$), respectively. The q-values adjusting for multiple hypothesis testing along with the full set of political outcomes considered in Allcott et al. (2024) are 0.002 and 0.139 for Facebook and Instagram; the latter is not statistically significant based on our pre-registered significance threshold of 0.05. Facebook deactivation improved the underlying *happy*, *depressed x (-1)*, and *anxious x (-1)* outcomes by 0.064, 0.039, and 0.028 standard deviations, respectively, with p-values of ¡0.001, 0.018, and 0.110. Instagram deactivation improved those outcomes by 0.044, 0.026, and 0.024 standard deviations, respectively, with p-values of 0.030, 0.156, and 0.205. All four point estimates are smaller for Instagram than for Facebook. In both experiments, the point estimate is largest for *happy* and smallest for *anxious x (-1)*.

Figure 2: Effects of Facebook and Instagram Deactivation on Emotional State



Note: This figure presents local average treatment effects of Facebook and Instagram deactivation estimated using equation 1. The horizontal lines represent 95 percent confidence intervals.

Subgroup analyses. Panel B of Figure 2 presents subgroup analysis for the five emotional state moderators introduced above. Appendix D.4 presents all point estimates and p-values. The first set of results shows differing patterns of age and gender estimates in the two experiments. For Facebook, the point estimates are larger for people over 35 than for younger users. The effects for 35+ vs. 18-34 are statistically different with $p = 0.046$. For Instagram, the estimates are largest for women aged 18-24: an improvement of 0.111 standard deviations ($p =$ 0.002). The point estimates for all other groups are less than half as large and are statistically indistinguishable from zero. The effects on the four age-by-gender subgroups are statistically different with $p = 0.062$. These results are consistent with public concerns described above about the effects of Instagram on young women. Appendix Table S28 shows that the estimates change little when also controlling for baseline use or emotional state, suggesting that the effects on young women are not driven by those correlated factors.

The second and third sets of results show that the effects are not statistically different for above- vs. below-median baseline use or emotional state.

The fourth set of results shows that for both Facebook and Instagram, the point estimates are larger for more politically engaged users, although the two estimates are not statistically different. For Facebook, the effects also do not differ by the count of civic pages followed at baseline. This provides no evidence that the effects of deactivation are related to the political content that circulated during the election period.

Appendix D.4 suggests possible heterogeneity along two other moderators: the point estimates are larger for undecided voters ($p = 0.053$ in a test of equality with non-undecided) and for people without college degrees ($p = 0.058$ in a test of equality with non-college).

4.4 Contextualizing Magnitudes

We benchmark these effect sizes in several ways. First, we compute how far these effects would move people in the distribution of the *emotional state index*. Under the approximation that *emotional state index* is normally distributed, the estimated effects of Facebook or Instagram deactivation would move the median user from the 50th percentile to the 52.4th or 51.6th percentile, respectively.

A second benchmark is the effects in their original units, before standardization. Recall that the survey question response options were "All of the time," "Often," "Sometimes," "Rarely," and "Never," which were coded as 4, 3, 2, 1, and 0, respectively. In those original units, Facebook deactivation improved happiness, depression, and anxiety by 0.053, 0.045, and 0.031, respectively. Similarly, Instagram deactivation improved happiness, depression, and anxiety in those original units by 0.037, 0.031, and 0.027, respectively. The average of these six effects is 0.038. This is equivalent to 3.8 percent of people saying they feel happy "often" instead of "sometimes."

A third benchmark is the conditional associations from a regression of baseline *emotional state index* on demographic characteristics, which is reported in Appendix Table S23. The *emotional state index* is 0.09 standard deviations higher for college graduates, 0.16 standard deviations higher for people earning \$50,000 to \$100,000 than for people earning less than \$50,000, 0.23 standard deviations higher for Black and Hispanic people than for other groups, 0.22 standard deviations lower for women than for men, and 0.48 standard deviations higher for Republicans than for Democrats.

As a fourth benchmark, the Control groups report roughly 0.07-0.09 standard deviations worse emotional state at endline than they do at baseline or on a post-endline survey fielded in December. Thus, more than half of the drop in emotional state around the election was eliminated by deactivating Facebook or Instagram.

As a fifth benchmark, the van Agteren et al. (2021) meta-analysis of 419 randomized trials finds that nine types of psychological interventions, such as cognitive behavioral therapy and mindfulness, improve subjective well-being by 0.16 to 0.42 standard deviations. The average across the nine intervention types is 0.27 standard deviations. Thus, the point estimates imply that pre-election Facebook and Instagram deactivation, respectively, improved emotional state by 15 and 22 percent as much as the average psychological intervention.

As a sixth benchmark, we can compare the effects to the national decline in young people's subjective well-being, which some observers attribute partially or entirely to social media use. To our knowledge, the questions that are closest to our survey questions and have been asked for an extended historical period in the U.S. are the Kessler-6 psychological distress scale asked on the National Survey on Drug Use and Health from 2008-2022.[10] From 2008 to 2022, Kessler-6 scores worsened by 0.37 standard deviations for people aged 18-24. The estimated effect of Instagram deactivation on people aged 18-24 is about 17 percent as large as this time-series change. Of course, this comparison is not informative about the share of the national trend caused by Instagram: the effect of Instagram being adopted in the full U.S. population over a 15-year period could be quite different from the effect of our incremental five-week, pre-election, individual-level deactivation.

Comparison to other experimental estimates. We also compare our estimates to prior estimates of the effects of social media abstention. We limit this comparison to randomized evaluations of social media abstention with abstention periods of at least one week. To find the set of included studies, we carried out a literature search on Google Scholar and PubMed. Appendix Table S30 presents information on the seven other studies that satisfy these inclusion criteria. The table shows that our experiment goes beyond these prior studies in several ways. We have the longest abstention period (5 weeks, compared to a prior maximum of 4 weeks), largest sample size (31,282 compared to a prior maximum of 1,955), and most rigorous enforcement (directly measured by Meta, compared to self-reports in some papers). Only two other studies had a pre-analysis plan. Appendix Table S29 lists an additional 26 randomized experiments that

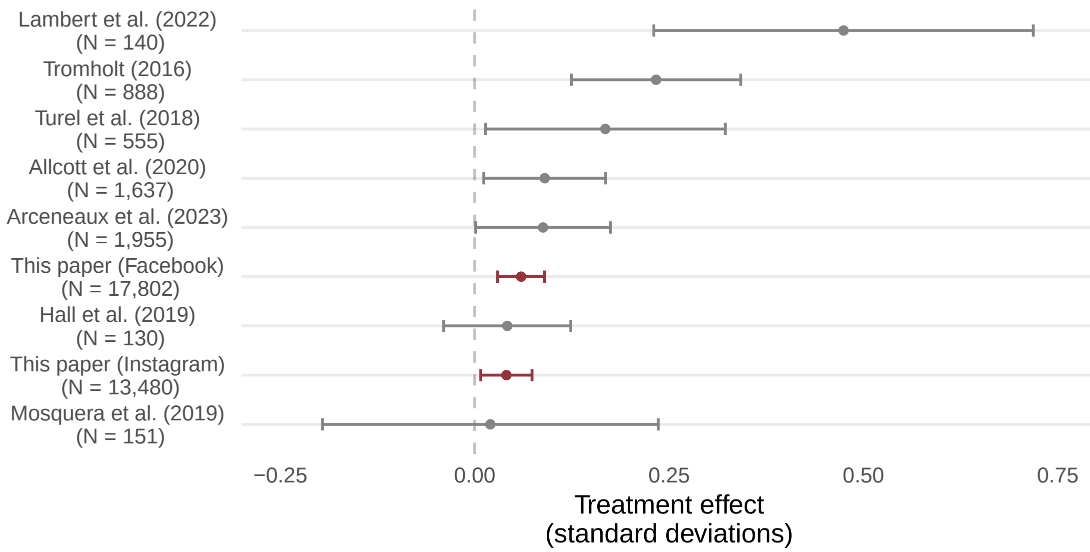
[10]Respondents are asked the share of the time that they felt six negative feelings ("nervous," "hopeless," "restless or fidgety," "so sad or depressed that nothing could cheer you up," "that everything was an effort," and "down on yourself, no good, or worthless"), with answers on the same scale as our questions (a five-point scale from "none of the time" to "all of the time"). We standardize and average these into a single index.

are related but fail our inclusion criteria.

Figure 3 presents the treatment effect estimates from the seven included studies, with effects in units of standard deviations of the outcome variable in the respective study's sample. The smallest effect size is in Mosquera et al. (2020). They find that one week of Facebook abstinence improves self-reported emotional state by a point estimate of 0.02 standard deviations, which is not statistically distinguishable from zero in their sample of 151 people. The largest effect size is in Lambert et al. (2022). They find that one week of social media abstinence improves self-reported emotional state by a statistically significant 0.47 standard deviations, albeit with a very wide confidence interval in their sample of 140 people. Across the seven prior studies, the average confidence interval is 0.28 standard deviations wide, which is 4.60 times larger than the confidence interval around our Facebook estimate. The inverse-variance weighted average effect size for these prior studies is 0.11 standard deviations, which is larger than our estimates.

Braghieri, Levy and Makarin (2022) provide quasi-experimental evidence that Facebook access worsened mental health among college students, leveraging the staggered rollout of the platform to colleges in 2004 and 2005. They estimate a 0.085 standard deviation decline in their mental health index, which is roughly forty percent larger than our point estimate. Their estimate is for a specific subset of our sample population (college students instead of adults 18 and older). Furthermore, the Facebook user experience has changed significantly in the past two decades: for example, there was no news feed, and the user base was over 100 times smaller.

Figure 3: Comparison to Other Experimental Estimates



Note: This figure compares our Facebook and Instagram estimates with other experimental results of social media deactivation by Tromholt (2016), Turel, Cavagnaro and Meshi (2018), Allcott et al. (2020), Mosquera et al. (2020), Hall et al. (2021), Lambert et al. (2022), and Arceneaux et al. (2023). For each paper, we compute treatment effects on the paper's subjective well-being outcomes, in units of standard deviations of the outcome variable in the respective study's sample.

Comparison to non-experimental estimates. We also compare our results to the estimates we would have obtained from non-experimental approaches, which have been used in hundreds of papers (Hancock et al. 2022). In Appendix D.6, we show that both cross-sectional comparisons (controlling for observables) and within-person panel/longitudinal designs give estimates that are biased in unpredictable directions and sometimes have the wrong sign. This highlights the importance of using randomized experiments or credible quasi-experiments for causal inference in this setting.

4.5 Robustness Checks

In Section 2, we documented that in both experiments, the Deactivation group responded earlier and at slightly higher rates than the Control group. Appendix D.7 presents a series of analyses to diagnose whether this affects the results. We find that excluding control variables, adding controls for endline survey response date, or constructing a sample with balanced endline response rates following Behaghel et al. (2015) all do not substantively affect the results. Lee (2009) bounds exclude zero for *happy* and *emotional state index* in the Facebook experiment and for *happy* in the Instagram experiment, and they rule out negative effects of larger than 0.010 to 0.025 standard deviations for the other five outcomes. Alternative sample weights have limited effects on the results.

5 Conclusion

The relationship between social media use and emotional state is widely debated and of first-order importance for policy. This link is particularly important in the context of an election, where social media may expose users to a range of divisive political content. Existing evidence relies primarily on evidence from time-series and cross-sectional correlations plus a few relatively small randomized experiments. Our experiments are 20 times larger than any previous experiment, the first to consider the effects of Instagram in isolation, and the first to estimate effects in the context of a U.S. presidential election. However, our experiments also have limitations described above, including generalizability, a time-limited intervention, individual-level deactivation, self-reported outcomes, and attrition.

Our estimates suggest that deactivating Facebook or Instagram before the 2020 election improved people's emotional state, although the Instagram effect is not significant at our preregistered threshold after adjusting for multiple hypothesis testing along with the suite of political outcomes in Allcott et al. (2024). The sign of these effects are consistent with public concerns about the effects of social media. However, the estimated effect sizes are smaller than benchmarks such as the effects of psychological interventions, nationwide mental health trends, and previous experimental estimates in smaller samples.

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