

Stop Blaming Social Media for Everything: The Minimal Effects of Facebook in the 2024 U.S. Presidential Election*

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Abstract

Social media are ubiquitous, with billions across the world using these platforms to connect with others and learn about the world around them. Many accuse these platforms of spreading misinformation, polarizing politics, undermining democracy, and making their users miserable. We conducted a randomized field experiment during the 2024 United States presidential election (N=4971 assigned, N=4278 observed) that incentivized half of participants to deactivate their Facebook accounts for the two weeks before the election. The factorial experiment design also encouraged a randomly selected half of the participants to install an application that provided them with daily reminders to engage with the ideologically balanced news that it provided them. In line with previous Facebook deactivation studies, we find little evidence for concerns that it has dramatic and drastic damaging effects on society. Moreover, encouragements to read ideologically balanced news also had mostly null effects. We conclude with thoughts about why research on social media should move past an often implicit theoretical model that treats ordinary people as passive users and treat them as active agents.

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The world that we have to deal with politically is out of reach, out of sight, out of mind. It has to be explored, reported, and imagined.

—Lippmann (1922, 18)

In his attempt to explain how the American public came to support World War I, Walter Lippmann observed that the complexity of the world made it impossible for an individual to understand everything at once. Nonetheless, individuals extrapolate from what they do know about the world to construct a “trustworthy picture inside [their] head of the world beyond [their] reach.” In doing so, people rely on mediated accounts of others’ experiences (Mutz, 1998). When Walter Lippmann was writing, most Americans got news from reading newspapers. Today, most Americans get their news from their “digital devices,” with many learning about the news from social media platforms (Pew Research Center, 2024). Given that social media platforms allow people to learn about politics in a way that mixes traditional and nontraditional news sources with personal interactions, it is possible that they have political effects that are uniquely different from traditional news sources (Settle, 2018).

The ubiquity of social media today, with billions of people the world over consuming information via a hodgepodge of platforms, has led many casual observers, journalists, politicians, and academics to see it as the root of the ills that afflict us. These media platforms are responsible for the steady decline in political trust and steady growth in polarization observed across the globe, undermining democracy in the process (Lorenz-Spreen et al., 2023). They are also responsible for turning us into doom-scrolling zombies, besieged by misinformation and messages that make us become anxious and depressed (Twenge and Campbell, 2018). As intuitive as this depiction of social media may seem, the main problem with it is that it simply does not align with the prevailing evidence. Not only is correlation not tantamount to evidence of causation, the correlational evidence in this case is not even all that clear. There is correlational evidence, for instance, that social media enhances democracy (Acheampong and Taden, 2024) and informs people about politics (Boulianne, 2019). The evidence is decidedly inconsistent on whether these platforms polarize (e.g., Rathje, Van Bavel and Van Der Linden, 2021; Beam, Hutchens and Hmielowski, 2018), and

while social media may cause people to be less happy than they otherwise would be, these negative effects are modest (Orben and Przybylski, 2019).

Of course, the key limitation of correlational studies is that they make it difficult to establish causal effects, especially in cases like these where social media use and experiences are endogenous to the outcomes under investigation. We address this issue by conducting a field experiment in which a large group of randomly selected individuals (nearly 2,500) were incentivized to deactivate their Facebook accounts for the two weeks leading up to the 2024 US presidential election. This approach allows us to estimate the *overall* effect of Facebook (or the lack of it to be more precise) on a host of political and social indicators. Our study joins four other Facebook deactivation studies (Allcott et al., 2020, 2024; Asimovic et al., 2021; Arceneaux et al., 2024), allowing us to produce a meta-analysis of Facebook’s effects across different contexts and times. We study the effects of Facebook, because its potential impact looms particularly large. As of 2025, there were 2.11 billion active Facebook users, 193 million users in the US across age and social demographic categories (Kumar, 2025). Indeed, Facebook is the only social media platform that is widely consumed by different age groups, genders, and types of political partisanship, and over 70% of Facebook users say they use it at least once a day, and over half use it multiple times a day (Gramlich, 2021). In addition, we extend these previous studies by testing a positive intervention that encourages a large group of randomly selected people (nearly 2,500) to seek out ideologically balanced news, allowing us to study whether digital media can be harnessed for good.

In line with previous research, we find that not only did Facebook deactivation have mostly null and sometimes very small effects on political and social outcomes, but also that the vast majority of our participants chose not to heed daily encouragements to engage with ideologically balanced news that we literally placed at their fingertips. Our findings, along with mounting evidence of social media’s minimal effects, lead us to conclude that if we do live in a polarizing, distrustful, depressing dystopia, social media provide the canvass on which our nightmare society unfolds, as opposed to its source. As we discuss in the last section of the paper, research on social media should move past an often implicit theoretical model that treats ordinary people as passive users who cannot help but to be massively influenced by the content that they encounter on these platforms.

Decades of research on traditional media show that people are active agents of the media that they consume, rendering media effects variable, mostly small, and contingent. We conclude that this is no different for social media. Nonetheless, if one continues to search for the hapless victims of social media, we sketch out reasons for why political activists and politicians offer the best place to look.

The Political and Social Effects of Social Media

New media technologies are often feted with optimism in the initial stages, followed by more pessimistic analyses as the media technology becomes widely adopted. Just as the advent of radio, and later cable television, was first met with optimism about their ability to inform the masses, once these media technologies became mainstream, political observers and scholars fretted about their ability to manipulate and mislead the mass public ([Arceneaux and Johnson, 2013](#)). Similarly, social media were greeted with a great deal of optimism regarding their potential to inform and empower the mass public (e.g., [Howard and Hussain, 2011](#)), and yet as social media became a ubiquitous aspect of quotidian life, the popular discussion of social media has become more focused on their potential negative effects ([Lane, Overbye-Thompson and Gagrčin, 2023](#)).

As the utopian vision of social media gave way to a more dystopian one, the common discourse often asserts that social media is responsible for increased political polarization as well as deteriorating mental health. In considering these concerns, [Ferguson \(2021, 116\)](#) takes a historical perspective, asking the question: do “we truly live in a uniquely difficult time” in which social media has unleashed the worse aspects of human nature in a way that other forms of media did not or could not? He notes that polarization existed well before the invention of social media and that it has been arguably worse at other times in the United States and other contexts. Similarly, there is little evidence that mental health has deteriorated over the past three decades in a uniform or drastic way, as implied by dystopian accounts of social media’s effects. Ferguson concludes that while it is unlikely that we live in a “uniquely difficult time,” it is nonetheless possible that social media could have, on average, negative effects on politics and mental health, and calls for more

rigorous preregistered studies that go beyond anecdotal and correlational evidence.

Indeed, even if the effects of social media are not as dramatic as some observers imagine, there are good reasons to believe that social media platforms may influence people in ways that traditional mass media could not. In particular, the *affordances* (or actions enabled by the environment) provided by social media platforms that set them apart from traditional mass media allow users to engage in computer-mediated social interaction on a potentially broad scale. In her discussion of the theoretical implications of social media affordances, [Settle \(2018\)](#) explains that since individuals neither directly control what others post nor what platform algorithms prioritize to be shown in their feed, social media platforms are more likely to expose individuals to political content that they may not have sought out ([Bode, 2016](#)), which could increase political information among what [Prior \(2007\)](#) calls “entertainment seekers,” who tend to avoid political news when they can. Moreover, social media platforms are more likely to provoke and promote inflammatory content, making it more likely that when people do encounter political content it tends to be polarizing. With respect to mental health, social media platforms are designed to encourage continued use, which may lead to addiction-like symptoms and behavior, such as compulsive use ([Sun and Zhang, 2021](#)). Whether social media use becomes compulsive or not, some mental health scholars contend that increased time spent on social media platforms reduces people’s emotional stability, increases anxiety and depression, and lowers overall subjective well-being ([Marino et al., 2018](#); [Twenge and Campbell, 2018, 2019](#)).

Thus, social media platforms have the *potential* to inform, polarize, and depress their users. The current state of knowledge of the political and social effects offers conflicting evidence regarding each of these effects. Evidence that social media exposure can increase political knowledge is mixed (e.g., [Cacciatore et al., 2018](#); [Fletcher, Robertson and Nielsen, 2021](#); [Haim, Breuer and Stier, 2021](#)), with the balance of the observational evidence from an influential meta-analysis of 133 studies conducted across 28 countries suggesting that social media use is positively correlated with political knowledge ([Boulianne, 2019](#)). With respect to polarization, some scholars offer evidence that more polarizing rhetoric indeed appears on social media platforms ([Rathje, Van Bavel and Van Der Linden, 2021](#)), and survey experiments demonstrate that exposure to such content can

increase polarization (Bail, 2022; Settle, 2018). However, some observational research suggests that exposure to social media could be depolarizing on average (Beam, Hutchens and Hmielowski, 2018). The empirical evidence regarding the mental health effects of social media are also contested, with some scholars contending that while social media use may have small negative effects on mental well-being, the effects are so small as to not warrant major policy changes (Orben, Dienlin and Przybylski, 2019; Orben and Przybylski, 2019), and others contending that social media’s deleterious effects for mental health are on par with heroin addiction, particularly for adolescent girls (Twenge et al., 2020).

Up to this point, we have focused on studies that analyze either observational data or lab-based/survey-embedded experiments to study the effects of social media, as they make up the bulk of the research on the subject. While these studies are informative, they are not without limitations. The key limitation of observational studies lies in the personalized nature of people’s social media experiences. People shape and customize their social media experience by making choices about their social networks and provide behavioral feedback (e.g., liking, re-posting, and commenting) to algorithms regarding their interests, which in turn shapes the content to which they are exposed. Consequently, the outcomes we seek to study, such as polarization or mental health, are potentially endogenous to people’s behavior on social media. For instance, do social media polarize people or do the group of people with polarized attitudes use social media to learn about politics? Does heavy social media use depress individuals or do individuals with depression choose to consume high quantities of social media as a coping mechanism? In sum, correlations between social media use and political and social outcomes may reflect selection bias as opposed to causal effects. Selection bias is a particularly pernicious obstacle to making causal inferences from observational data, because one cannot address the problem by using statistical modeling to control for alternative explanations (Achen, 1986). Lab-based and survey-embedded experiments address the selection bias problem, allowing scholars to make credible inferences about the casual effects of particular aspects of the social media experiment. The key limitation to these approaches is that they typically feature experimental manipulations administered in a stylized setting. For instance, study participants may be asked to participate in a survey that simulates a social media

feed, allowing researchers a good deal of control over the content to which participants are exposed, removing the problem of selection bias. Yet, it does so by exposing participants to content that they might not have otherwise been exposed to, making it difficult to infer that their behavior in the study setting would be the same as their behavior in a real-world setting.

By combining the real-world settings of observational studies with the rigor of random assignment, field experiments can help fill the gap in knowledge created by these limitations. Over the past decade, a handful of *deactivation* experiments have been conducted to test the overall effects of Facebook use.¹ As the name implies, the randomized intervention in these experiments is not exposure to Facebook, but the privation of Facebook. [Allcott et al. \(2020\)](#) pioneered research in this area, and their protocol has been largely adopted by the deactivation experiments that followed. They recruited Facebook users living in the United States who were eligible to vote in the 2018 midterm federal elections. Those who agreed to deactivate their Facebook account for 4 weeks were randomly assigned to either the treatment group, which deactivated their Facebook account for the study period in return for a generous incentive (over 100 US dollars), or the control group, which did not have to deactivate their Facebook account.

After the conclusion of the study, participants were recontacted and completed a questionnaire with questions that measured news knowledge, partisan polarization, and subjective well-being. In sum, [Allcott et al. \(2020\)](#) found that Facebook deactivation caused people to engage less with news about politics and, thus, know less about recent political news. This finding supports the “news-finds-me” phenomenon in which entertainment-seekers learn about the news through incidental exposure to the news on social media. The researchers also found that relative to those in the control group, those in the Facebook deactivation group had a lower average on an index that the researchers created to measure partisan polarization, which they interpreted as evidence that Facebook use may have contributed to partisan polarization during the 2018 midterm elections. However, it is worth noting that this effect was ostensibly driven by the Facebook deactivation group reporting less exposure to congenial partisan news. There was essentially no difference

¹Deactivation experiments are a subset of the encouragement design where the randomized intervention entails encouraging participants to engage in a particular activity, such as signing up for a government program. In this instance, the “encouragement” is in effect a discouragement to use Facebook

between the two experimental groups with respect to affective polarization (i.e., the degree to which participants like members of their party versus those of the opposing party). Finally, the research team observed that those assigned to the Facebook deactivation group reported higher levels of subjective well-being relative to those in the control group, which supports the thesis that Facebook use reduces subjective well-being.

To our knowledge, there are three published replications of this study. [Asimovic et al. \(2021\)](#) incentivized Facebook users in Bosnia-Herzegovina to deactivate their accounts during Srebrenica Memorial Week, which is dedicated to remembering the genocide committed in the town of Srebrenica in July 1995. The researchers found that those assigned to the deactivation group performed more poorly on a current events quiz relative to those in the control group, corroborating the news-finds-me phenomenon observed by [Allcott et al. \(2020\)](#). They also found that those assigned to the deactivation group reported higher levels of subjective well-being than those in the control group. In contrast to [Allcott et al. \(2020\)](#), however, they found that those assigned to the deactivation group reported *higher* levels of ethnic polarization. The researchers speculated that this may have happened because those in the Facebook deactivation group were more likely to have conversations about the Srebrenica genocide with co-ethnics, whereas those who continued to use Facebook in the control group may have been more likely to encounter positive expressions from outgroup members. [Allcott et al. \(2024\)](#) replicated their study during the 2020 presidential elections in the United States. In this study they partnered with Facebook to help recruit participants as well as to administer the deactivation with precision among those who agreed to participate and who were assigned to the deactivation group. In this study, participants assigned to the deactivation group were given a generous incentive (150 US dollars) to deactivate their accounts for six weeks (25 dollars a week). The research team found that in this election, Facebook deactivation reduced political participation, general news knowledge, may have reduced support for President Donald Trump, but had precise null effects with respect to affective polarization, and perceptions of the legitimacy of the election outcome.

[Arceneaux et al. \(2024\)](#) replicated and extended this line of research during the 2022 French presidential elections. They gave participants randomly assigned to the treatment arm an 80 euro

incentive to deactivate their Facebook accounts for three weeks. In addition, the researchers divided the treatment arm into two subgroups, with half assigned to a pure deactivation condition (in line with previous research) and the other half assigned to receive four informational messages over the course of the deactivation period. The informational messages were broadly inspired by digital literacy interventions that previous research had found to be effective (e.g., [Bail, 2022](#); [Pennycook et al., 2021](#)). The goal of the messages was to encourage the participants who had deactivated their Facebook accounts to continue seeking out news about politics and to discern accurate news from misinformation. The purpose of the deactivation + information experimental condition was to test whether minimalist informational treatments could induce participants who no longer had access to Facebook to continue learning about politics, thus counteracting the negative effects of Facebook deactivation on news engagement found in previous research.

The researchers found that Facebook deactivation decreased news knowledge, decreased news engagement, and increased subjective well-being, replicating previous research. Although the standardized intent-to-treat effect sizes were smaller than those observed in previous research, the authors also noted that compliance with the deactivation protocol was lower than researchers had achieved in previous studies ([Arceneaux et al., 2024](#)). In contrast, Facebook deactivation had precise null effects on affective polarization, with respect to partisan, ideological, and ethnic identification. Moreover, the informational treatments did not overcome the negative effects of Facebook deactivation on news engagement and news knowledge. Because previous research on digital literacy interventions measured outcomes right after exposure to the message, whereas the outcomes in this study were measured days after the last message, the researchers surmised that perhaps minimalist informational interventions are effective but have relatively short-term effects.

Taken together, these deactivation experiments imply that Facebook exposure provides users with accurate information, on balance, but that it may reduce somewhat users' subjective well-being. Three caveats are in order. First, the effect sizes reported in previous studies are small (Cohen's $d < 0.2$). Even though small effects may be meaningful ([Anvari and Lakens, 2021](#)), these studies show that Facebook is neither a boon for political knowledge, nor responsible for a dystopic world of depressed doomscrollers. If anything, it may cause people to be a bit less knowledgeable about

politics and a bit less happy, too. With respect to other politically important outcomes, affective polarization, voting behavior, and such, Facebook appears to have no overall impact. Second, it is important to keep in mind that Facebook deactivation is a bundled treatment that estimates the effect of what happens when people pause their regular use of Facebook. As such, the effect of treatment is not simply that of forgoing exposure to one’s Facebook feed, but also the effect of the behavior that people engage in when they would have normally scrolled their Facebook feed. Consequently, deactivation studies teach us about the overall implications of avoiding social media platforms in a world where those platforms continue to exist, and the design can teach us little about the *mechanisms* through which avoiding social media platforms shape downstream attitudes and behavior. Third, social media platforms like Facebook are not static. The owners of social media platforms constantly update and tweak the algorithm that affect the content of users’ feed. How users engage with the platforms change — from politeness norms to the type and modality of content that users post. In short, social media platforms, like Facebook, are a moving target, making it vitally important that researchers continue to study the effects of social media platforms by replicating and extending previous research. It is with this goal in mind that we conducted the deactivation experiment that we discuss in the next section.

2024 Facebook Deactivation Study

Study Rationale. We conducted a Facebook deactivation experiment during the 2024 US presidential elections with three aims: 1) replicate previous research with an adequately powered sample to detect meaningfully small effects ($d = 0.1$); 2) test whether participants could be motivated to consume balanced news about politics; and 3) fully cross the news encouragement treatment with the deactivation treatment. Following the rationale elaborated by [Chambers \(2019\)](#), building on and refining previous research requires researchers to make replicating the experimental manipulations of previous studies part of their experimental designs in order to establish whether treatment effects observed (or not) in previous research are robust. Beyond our close replication of previous deactivation interventions, we were interested in studying whether a more focused

information nudge than the one employed by [Arceneaux et al. \(2024\)](#) could effectively encourage individuals in the deactivation condition to seek out information about news and politics. In this vein, we designed a treatment that would provide participants with daily encouragements to consume balanced political news as opposed to the variegated and minimalist approach used by Arceneaux and colleagues. We focused on repeated encouragements to consume news, because previous deactivation studies converge on the finding that Facebook deactivation reduces news knowledge. One explanation for this finding is that since entertainment-seekers tend to avoid the news ([Arceneaux and Johnson, 2013](#); [Prior, 2007](#)), Facebook deactivation deprives entertainment-seekers of inadvertent news exposure on their feeds. If so, the most effective way to circumvent the negative effects of Facebook deactivation on political knowledge would be to nudge participants to consume it daily. Finally, we opted for a fully factorial design to account for the possibility that the news encouragement treatment combined with continued access to Facebook would enable participants to better contextualize the news that they encountered (perhaps through incidental exposure) on Facebook.

Study Protocol. We engaged a respected survey research firm, YouGov, to recruit adult participants from their American panel who regularly use Facebook. Following the protocol used by [Arceneaux et al. \(2024\)](#), participants were retained in the sample if they agreed to participate in a study where they might be asked to deactivate their Facebook account for a two-week period from 24-26 October 2024 to 7 November 2024. The deactivation period included Election Day (5 November). The choice to study only those willing to deactivate their Facebook account necessarily means that we cannot make inferences to the population of Facebook users who are unwilling to deactivate their Facebook accounts. With the sample in place, YouGov randomly assigned participants into one of four experimental conditions. First, those in the *Deactivation* condition were asked to deactivate their Facebook account in return for a \$50 gift certificate (i.e., the same weekly rate used by [Allcott et al. 2024](#)).² YouGov was able to monitor whether the participants in this condition did in fact deactivate their Facebook account and sent regular reminders to those who did not. Second, those assigned to the *News Encouragement* condition were asked to install and

²Participants in the deactivation condition could continue to access Facebook Messenger

enable banner notifications of the AllSides news application, but they were not asked to deactivate their Facebook account and could continue using it if they so desired. Allsides is a nonpartisan news aggregator that seeks to expose users to how news outlets with different ideological slants (left, center, right) cover the same topics with the goal to break ideological echo chambers in which people consume news primary from one ideological perspective (Chu, 2020). The app’s interface shows users the headlines attached to top-news stories as covered by news organizations on the left, center, and right, and we asked participants to turn on banner notification so that they would get daily (and sometimes multiple times a day) notifications on their phone encouraging them to engage with the app and consume balanced news in the process. Third, participants assigned to the *Deactivation+News Encouragement* condition were asked to deactivate their Facebook account and install the Allsides app. Fourth, those assigned to the *Control* condition were neither asked to deactivate their Facebook account nor install the Allsides app.

Sample. We preregistered³ that we would recruit 4,800 participants (1,200 per treatment group), and YouGov was able to exceed this goal (N = 4,971; Control = 1,242, Allsides = 1,243, Deactivation = 1,244, Deactivation + Allsides = 1,242). Yougov successfully recontacted 4,278 participants after the conclusion of the deactivation period (14% attrition; Control = 1,051 [15.4%], Allsides = 1,058 [14.9%], Deactivation = 1,073 [13.7%], Deactivation + Allsides = 1,096 [11.8%]). These attrition rates are comparable to previous Facebook deactivation studies (Allcott et al., 2024). We asked YouGov to collect a politically representative sample of US adults who are active Facebook users, and while the survey firm provided us with survey weights modeled on publicly available demographic data, we analyze the unweighted data, since our goal is to estimate the causal effects of Facebook deactivation and news encouragement on this sample as opposed to estimate population statistics. The unweighted sample was balanced with respect to gender (52.6% female) as well as racially diverse (69.5% white, 13.3% black, 7% Hispanic, 3.2% Asian, with the rest identifying as multiracial or something else). The sample was also similar to the US adult population with respect to socioeconomic characteristics (46.7% had a college degree) and the median income fell in the \$60,000-\$69,999 range. Finally, in terms of political preferences, the

³see <https://osf.io/a8t4f/> for our preregistered analysis plan.

sample leaned left, with 57.8% self-identifying as Democrats, 29.2% as Republicans (including those who say that they “lean” toward the Democratic or Republican parties), and 13% identified as Independents. With respect to 2024 presidential vote choice, among those who participated in the post-study survey and reported voting ($N = 3,606$), 62.6% voted for Democratic candidate Kamala Harris and 34% voted for Republican candidate Donald Trump (see Section A1 for descriptive statistics). These covariates along with others were balanced across the treatment conditions in both the full sample as well as the sample that participated on the end-line survey (see Section A2 of the Appendix).

Outcomes. We estimate the effects of Facebook deactivation and news encouragement on several outcomes, which we organized via 11 conceptual categories in our preregistration plan.⁴ To further clarify and simplify our presentation of findings, Table 1 organizes our preregistered hypotheses into three overarching groups: 1) political effects, 2) media effects, and 3) social effects.⁵ Table 1 describes the measures that we employ to measure each outcome, states the hypotheses for the effects of Facebook deactivation and news encouragement for each outcome along with the specific prediction that we preregistered regarding treatment effects. We will describe in greater detail the measures and our rationales for the hypotheses in the results section. In the pages that follow, we report intent to treat (ITT) effects using two-tailed t-tests to establish statistical significance ($\alpha = 0.05$).⁶ We estimate the effects of our interventions on 24 outcomes, which means that with four experimental conditions, we make six comparisons for each outcome, resulting in 144 (24×6) comparisons overall and a varying number of comparisons across conceptual categories. For instance, the voting behavior category has three outcomes that total 18 (3×6 comparisons).⁷

⁴The conceptual categories were: 1) voting behavior, 2) support for democracy, 3) political violence, 4) polarization, 5) media agenda setting, 6) news knowledge, 7) subjective well-being, 8) trust in people, 9) substitution effects in media and time use, 10) echo chambers and media trust, 11) self-conscious emotions.

⁵We analyze the effects of Facebook deactivation and news encouragement on substitution behavior separately, because our hypothesis with respect to these variables is relatively pedestrian (deactivating Facebook increases time spent on other activities), and our motivation for including these variables was mostly to understand how participants changed their media consumption in the treatment conditions rather than to estimate precise causal effects.

⁶We preregistered that we would estimate Compiler Average Causal Effects (CACE) if the ITT effect is statistically significant. These findings can be found in the appendix.

⁷We preregistered that if there was no difference in means within the two treatment arms, which we defined as having a Bayes Factor < 0.03 , we would collapse across the factorial conditions: Facebook deactivation vs. no deactivation and news encouragement vs. no encouragement. These findings can be found in the appendix.

With so many comparisons, it is possible to observe statistically significant effects by random chance. To guard against this possibility, we preregistered that we would calculate False Discovery Q rates for treatment effects within and across categories. To the extent that one believes that the false discovery rate should only apply to the hypotheses generated within theoretically relevant categories the Q_{within} values are most relevant, whereas if one believes that the false discovery rate should apply to the number of hypotheses tested, irrespective of whether they are theoretically related, the $Q_{overall}$ values are more relevant.⁸ Q values are reported in Section A3 of the Appendix.

⁸We can see arguments for both within and overall Q values. Overall Q values guard against the possibility of finding fluke effects across many tests. Yet, because our preregistered hypotheses come from different theoretical models, it may be better to only consider the number of hypotheses generated by a specific theoretical model when calculating Q values. Indeed, one of the potential perverse incentives of only reported overall Q values is for the researcher to minimize the number of hypotheses. We report both so readers can be free to decide.

Table 1: Preregistered Hypotheses

Category	Measure	Hypothesis	Prediction
I. Political Effects			
<u>A. Voting Behavior</u>			
1. Turnout	1 = voted, 0 = did not vote	Deactivation reduces turnout; news encouragement counteracts or increases turnout	$NE \geq C = FD + NE > FD$
2. Vote Choice	1 = voted Trump, 0 = did not vote Trump	News encouragement increases support for candidate who outperformed poll averages (Donald Trump)	$NE \geq C = FD + NE > FD$
3. Election Outcome Belief	1 = Believes Trump won — voted Harris, 0 = Believes Harris won — voted Harris	News encouragement and Facebook deactivation increase belief that winning candidate (Trump) won among supporters of the losing Candidate (Harris)	$FD + NE \geq FD = NE > C$
<u>B. Affective Polarization</u>			
1. Partisan	Inparty Feeling Thermometer - Outparty Feeling Thermometer	Deactivation and news encouragement reduce polarization	$FD + NE < FD < NE < C$
2. Ideological	Ingroup Ideology Feeling Thermometer - Outgroup Feeling Thermometer	Deactivation and news encouragement reduce polarization	$FD + NE < FD < NE < C$
3. Racial (among whites)	White Feeling Thermometer - Black / Latino Feeling Thermometer	Deactivation and news encouragement reduce polarization	$FD + NE < FD < NE < C$
<u>C. Political Violence</u>			

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Table 1: Hypothesis Table (Continued)

Category	Measure	Hypothesis	Prediction
1. Violence okay to obtain political ends	Higher values indicate more support for using violence to obtain political ends	Deactivation and news encouragement reduce support for political violence	$FD + NE < FD < NE < C$
<u>D. Support for Democracy</u>			
1. Democracy Support Index	Higher values indicate more support in democracy	Deactivation and news encouragement increase support for democracy	$FD + NE > FD > NE > C$
II. Media Effects			
<u>A. News Knowledge</u>			
1. Belief in True News	Sum of correct responses to news quiz	Deactivation reduces news knowledge; news encouragement increases news knowledge	$NE \geq C = FD + NE > FD$
2. Belief in Misinformation	Sum of belief in misinformation items on news quiz	Deactivation and news encouragement reduce belief in misinformation	$NE = FD + NE \geq FD > C$
<u>B. Agenda Setting</u>			
1. Most Important Problem	1 = named issue as MIP that was covered in the top-5 New York Times headlines during deactivation period, 0 otherwise	Deactivation reduces agenda setting	$FD + NE = FD > NE = C$
<u>C. Echo Chambers and Media Trust</u>			
1. Exposure to uncongenial political information on social media	Higher values indicate more exposure to differing political opinions on social media	Deactivation decreases exposure to differing political opinions on social media	$NE \geq C = FD + NE > FD$

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Table 1: Hypothesis Table (Continued)

Category	Measure	Hypothesis	Prediction
2. Exposure to uncongenial political information on non-social media	Higher values indicate more exposure to differing political opinions on other media	Deactivation increases exposure to differing political opinions outside of social media	$FD + NE \geq FD > NE > C$
3. Media Trust Questions	Higher values indicate more trust in 1) social media; 2) television news; 3) local news; 4) newspapers; 5) friends and family	Deactivation has no effect on media trust	$FD + NE = FD = NE = C$
4. Trust in Newspapers	Higher values indicate feeling more trust in newspapers	News encouragement increases trust in newspapers	$NE = FD + NE > FD > C$
III. Social Effects			
<u>A. Subjective Well-being</u>			
1. Subjective Well-being Index	Higher values indicate feeling more positive emotions over past two weeks	Deactivation increases subjective well-being	$FD + NE = FD > NE = C$
2. Depression	Higher values indicate feeling depression symptoms over past two weeks	Deactivation reduces depression symptoms	$FD + NE = FD < NE = C$
<u>B. Social Trust</u>			
1. Trust in people	Higher values indicate more trust	Deactivation increases trust	$FD + NE = FD < NE = C$
2. Conversations with friends (number)	Higher values indicate more conversation partners	Deactivation increases number of conversation partners	$FD + NE = FD < NE = C$

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Table 1: Hypothesis Table (Continued)

Category	Measure	Hypothesis	Prediction
3. Conversation with friends (frequency)	Higher values indicate more frequent conversations	Deactivation increases frequency of conversations	$FD + NE = FD < NE = C$
<u>C. Self-conscious Emotions</u>			
1. Pride	Higher values indicate feeling more pride	Deactivation decreases feelings of pride expressed by Republicans after the election	Among Republicans: $FD + NE < FD < NE < C$
2. Relief	Higher values indicate feeling more relief	Deactivation decreases feelings of relief expressed by Republicans after the election	Among Republicans: $FD + NE < FD < NE < C$
3. Guilt	Higher values indicate feeling more guilt	Deactivation decreases feelings of guilt expressed by Democrats after the election	Among Democrats: $FD + NE < FD < NE < C$
4. Shame	Higher values indicate feeling more shame	Deactivation decreases feelings of shame expressed by Democrats after the election	Among Democrats: $FD + NE < FD < NE < C$

Note: C = Control Group Mean; NE = News Encouragement Group Mean; FD = Facebook Deactivation Group Mean; FD+NE = Facebook Deactivation + News Encouragement Group Mean

Results

Compliance and Substitution Behavior

To measure compliance, we asked participants to report how much they checked Facebook in the past two weeks as well as whether they had the Allsides app installed on their phone. We coded participants in the Facebook deactivation arm as complying with the experimental protocol (0,1) if they reported “never” checking their Facebook account during the deactivation period, and we coded participants in the News Encouragement arm as complying (0,1) if they reported having Allsides installed on their phone. Figure 1 shows the differences in average compliance between each experimental group and the control group. We observe a high degree of compliance in the Facebook Deactivation arm that is uniform between the Facebook Deactivation group (58.2 percentage-point decrease in use relative to the control group, $SE = 1.7, p < 0.01$ and a compliance rate of 67%) and the Facebook Deactivation + News Encouragement group (59.7 percentage-point decrease in use relative to the control group, $SE = 1.7, p < 0.01$ and a compliance rate of 68.8%). These compliance rates are lower than that observed in [Allcott et al. 2020](#), but much higher than that observed in [Arceneaux et al. \(2024\)](#). In contrast, the compliance rate for the News Encouragement treatment was relatively low and the uptake rate was higher in the Facebook Deactivation + News Encouragement group (14.9 percentage-point increase in use relative to the control group, $SE = 1.4, p < 0.01$ and a compliance rate of 22.1%) than it was in the News Encouragement group that was allowed to continue using Facebook (8.2 percentage-point increase in use relative to the control group, $SE = 1.4, p < 0.01$ and a compliance rate of 15.4%). The relatively low compliance rate in the News Exposure treatment arm echos (but is higher than) a similar field experiment ([Hopkins and Gorton, 2024](#)) in which researchers offered to purchase free subscriptions to a local news paper and only 44 participants in the treatment group (N=2,529) accepted the offer (1.7% compliance rate). Note that it is completely normal in field experiments for the compliance rate to be less than (sometimes a good deal less than) 100%, and more importantly, noncompliance does not bias ITT effects ([Gerber and Green, 2012](#)). We also take it as a positive sign of divergent validity that participants assigned to the News Encouragement group, who were not incentivized

to deactivate their Facebook account, reported essentially the same Facebook use than those in the control group, and those assigned to the Facebook Deactivation group, who were not encouraged by us to install the Allsides app, were no more likely than participants in the control group to report having it on their phones.

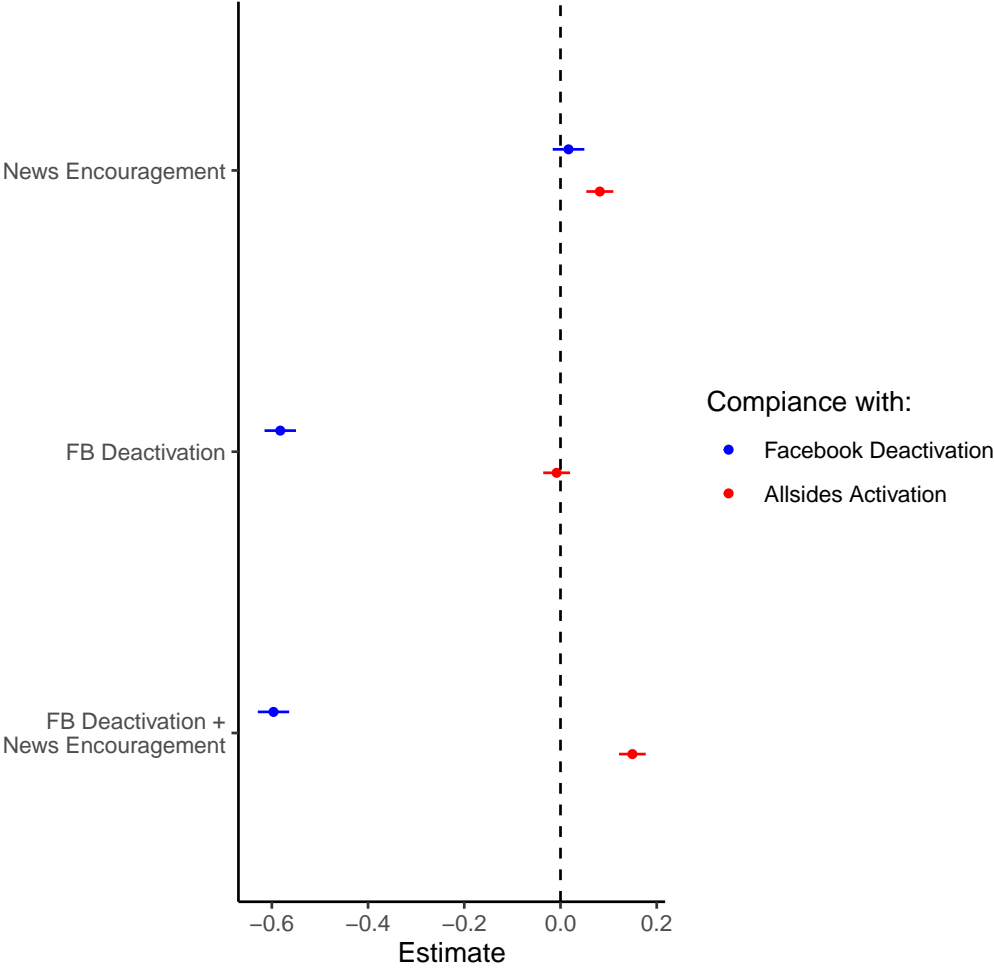


Figure 1: Compliance with Experimental Protocol

In addition to measuring compliance, we also asked participants to report how much time they spent doing a number of activities “in a typical day in the last two weeks.” The list included online activities (spending time on YouTube, Facebook, Instagram, TikTok, Twitter/X), watching television (national programming, local programming, and streaming), offline social activities (walking outside, exercising, talking to friends, going to a coffee shop, eating out at a restaurant), and offline reading activities (reading a book or newspaper). For each activity, participants could

choose among six options: 1) no time, 2) less than 30 minutes, 3) between 30 and 60 minutes, 4) between 1 and 2 hours, 5) between 2 and 5 hours, and 6) more than 5 hours. Figure 2 shows the average response for each experimental group for each of these activities (except Facebook use, which is discussed above). While there is variance in the average time devoted to these activities — participants spent more time talking with friends or watching television and less time going to coffee shops — there are no important differences across experimental conditions. It does not appear that participants in the Facebook deactivation arm systematically substituted one activity (e.g., television watching or spending time with friends) for their previous Facebook use.

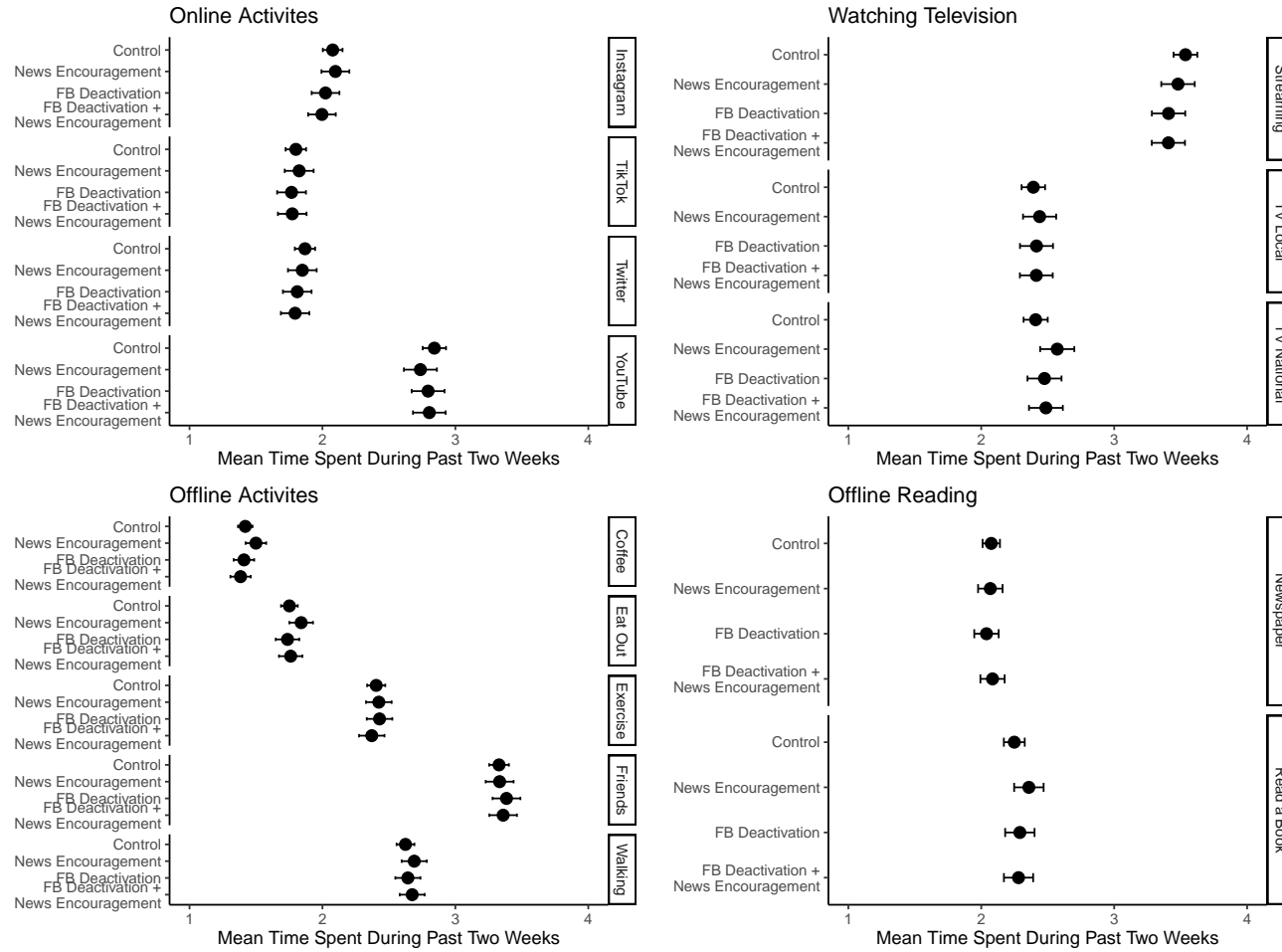


Figure 2: Substitution Behavior

Political Effects

As outlined in Table 1, we estimate the political effects of Facebook deactivation across four categories: voting behavior, affective polarization, violence, and democracy support. While these are certainly not exhaustive of the possible political effects of Facebook, they do represent concepts measured in previous Facebook deactivation studies as well as additional ones (e.g., political violence and democracy support), allowing us to replicate and extend previous research in this domain. All of the treatment effects for the political variables can be found in Figure 3. Unless otherwise noted, the points represent the standardized treatment effect relative to the control group and the horizontal lines represent the 95% confidence interval.

Voting Behavior. We measured three aspects of voting behavior: turnout, vote choice, and election outcome belief. With respect to turnout, based on previous research that shows that exposure to Facebook during election time can boost turnout (e.g., [Bond et al., 2012](#); [Allcott et al., 2024](#)), we hypothesized that Facebook deactivation would lead to lower turnout, on average, in the Facebook deactivation group and we anticipated that the news exposure condition would counteract these effects by encouraging participants to seek out news and, thus, remain engaged in politics. We were agnostic regarding the extent to which news exposure would counteract the negative effects of Facebook deactivation, hypothesizing that turnout in News Exposure condition could be greater than or simply equal to turnout in the Control group and the Facebook Deactivation + News Exposure group. The results shown at the top of Figure 3 are not consistent with these expectations. If anything Facebook deactivation increased turnout, while news encouragement lowered it. However, none of these effects are statistically significant.

With respect to vote choice, we focus on the implication of decisions made by late-deciding voters, whose choices are more likely to be influenced by short-term forces ([Box-Steffensmeier et al., 2015](#)). Before the election took place, we surmised that if one of the two major-party candidates had a surge at the end, some of that surge may be caused by a flow of information on both social media and traditional news outlets that benefits one candidate over the other, leading late deciders to more likely choose the advantaged candidate. In our preregistered expectation, we specified that

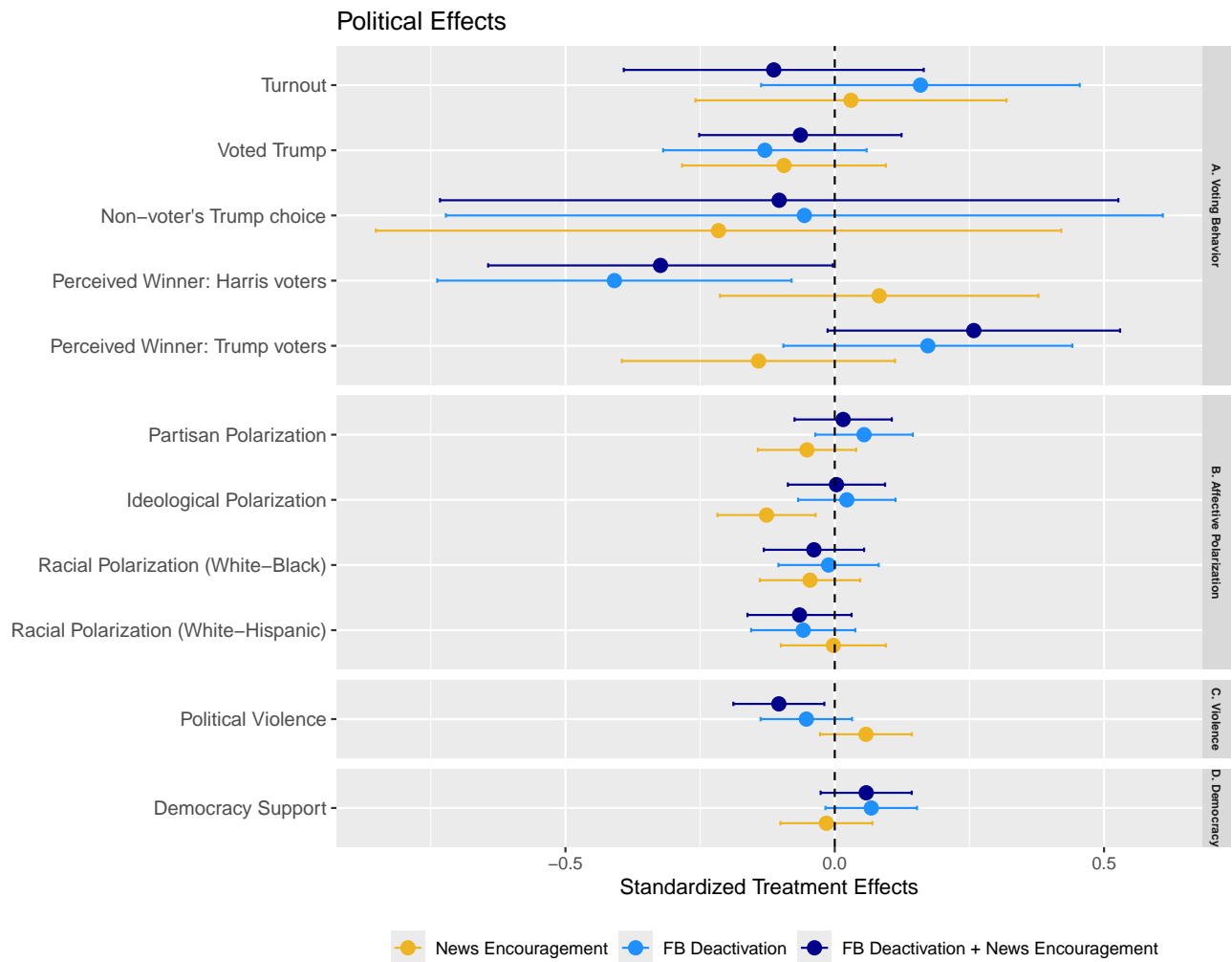


Figure 3: The Effects of Facebook Deactivation and News Encouragement on Political Attitudes and Behaviors

Note: Points represent the standardized treatment effects relative to the control group (with the exception of turnout and candidate support which are dichotomous variables) and horizontal lines represent the 95% confidence intervals.

if either Harris or Trump did 1.2 percentage points better than the average of their polls measured by noon on Election Day, we would consider this candidate as outperforming expectations. Donald Trump’s victory met this definition and, thus, we coded the vote choice variable as 1 for those who reported voting for Trump and 0 for those who voted for a different candidate (mostly Harris). We estimated effects among voters and those who did not vote but did express a preference for a candidate. Thus, our preregistered expectation is that individuals in the Facebook Deactivation condition should be less likely to vote for Trump than the other conditions. The results shown in Panel A of Figure 3 are inconsistent with these expectations and statistically insignificant.

Finally, in light of the turn of events after the 2020 US presidential election in which the losing candidate claimed to have really won ([Arceneaux and Truex, 2023](#)), with those claims spread on social media to a considerable extent ([Benaissa Pedriza, 2021](#)), we hypothesized that Facebook deactivation would reduce exposure to false claims from the losing candidate’s supporters (Kamala Harris’s supporters in this case) and that news exposure would increase the likelihood that people encountered accurate information about which candidate rightly won the election (Donald Trump in this case). We asked participants which candidate they believed really won the 2024 presidential election, and we coded responses as 1 if participants reported Trump and 0 if they reported Harris. We estimated treatment effects separately for Trump and Harris supporters. The results reported in Panel A of Figure 3 are intriguing and not fully consistent with our hypotheses. Facebook deactivation reduced accurate beliefs about the election outcome among Harris supporters, while it may have increased accurate perceptions among Trump supporters (although these effects are not statistically significant). We do not have a good explanation for this pattern of effects, and resist speculating about it.

Affective Polarization. Building on previous Facebook deactivation studies, we measured affective polarization in terms of three different identities: partisanship, ideology, and race. Much has been written on ingroup bias and affective polarization (e.g., [Iyengar et al., 2019](#)), which we will not summarize here. Our main concern is to test the hypothesis that exposure to social media leads people to view polarizing content, increasing affective polarization with respect to politically salient social groups ([Bail, 2022](#); [Rathje, Van Bavel and Van Der Linden, 2021](#); [Settle, 2018](#)) along with the hypothesis that exposure to an ideologically balanced diet of news (as Allsides attempts to provide) can reduce affective polarization ([Broockman and Kalla, 2025](#)). We measured affective polarization following the standard approach of asking participants to self-report how they feel about partisan (Democrats, Republicans), ideological (conservatives, liberals), and racial groups (blacks, Latinos, and whites) on an 11-point scale that ranges from 0 (“I do not like them at all”) to 10 (“I have very warming feelings toward this group”) and then subtracting the feeling-thermometer rating of each participant’s ingroup (e.g., Republicans for those who identify as Republican, conservatives for those who identify as conservative, and for racial affective

polarization we focus on whites) from the rating of their outgroup (e.g., Democrats for Republicans, liberals for conservatives, and blacks and Hispanics, separately for whites).⁹ The results are shown in Panel B of Figure 3. By and large, neither Facebook deactivation nor news encouragement affected levels of affective polarization. The sole exception is that news encouragement reduced ideological polarization, but only in the treatment group where participants continued having access to Facebook ($d = -0.13, p < 0.006, Q_{within} = 0.085, Q_{overall} = 0.298$) and the Q values (both within categories and across) suggest a false discovery rate above the preregistered 5% threshold.

Political Violence. Building on recent scholarly disagreements regarding the degree to which the American political environment fuels support for violence against one’s political outgroup (Kalmoe and Mason, 2022b,a; Westwood et al., 2022a,b), we asked participants to tell us “how often is it okay for individuals to use violence to pursue political ends? Very often, often, sometimes, rarely, never,” creating a 5-point scale that we coded such that higher values indicate more support for political violence. Based on the finding the interactions surrounding politics on social media often feel more hostile (Bor and Petersen, 2022), we test the prediction that Facebook deactivation would reduce self-reported support for political violence. We also surmised that cross-cutting news exposure would reduce support for violence. The results are shown in Panel C of Figure 3. Consistent with our hypothesis, Facebook deactivation decreases self-reported support for political violence, but the effect is only statistically significant for the Facebook deactivation + news encouragement condition ($d = -0.10, p = 0.016, Q_{within} = 0.051, Q_{overall} = 0.319$). If we calculate the Q value in the context of the political violence category, the false discovery rate is just at the preregistered 5% threshold, where as if we calculate the Q value across all of the effects that we estimated the false discovery rate is well above the threshold.

Democracy Support. With the rise of illiberal democracy across the world (Diamond, 2021) and concerns about the strength of support for democracy in the polarized political context of the US (Graham and Svobik, 2020, but see, Holliday et al. 2024), we also tested the hypothesis,

⁹For partisan affective polarization, we coded those who lean toward a political party as identifying with that party and we excluded Independents who did not lean toward one of the two major parties from the analysis.

often offered in journalistic explanations of the rise of illiberalism, that social media exposure can fuel support for populist ideas, such as the notion that a strong leader should enact the will of the people without concern for limits on his or her power, and ultimately reduce support for liberal democracy (Boulianne, Koc-Michalska and Bimber, 2020). We measured support for liberal democracy by creating an index from participants' responses to 4 questions on the end-line survey: 1) satisfaction with democracy in the US, 2) support for the rule of law, 3) support for the notion that the law can constrain majorities, and 4) support for a free press. Higher values indicate more support for liberal democracy. Neither Facebook deactivation nor news encouragement increased support for liberal democracy in a robust way (see Panel D, Figure 3).

Media Effects

Again with reference to Table 1, we estimate the media effects of Facebook deactivation across three categories: news knowledge, agenda setting, and echo chambers and media trust. Previous Facebook deactivation studies have focused largely on the influence of Facebook deactivation on news knowledge, finding consistent evidence that deactivation Facebook reduces knowledge about the news (Allcott et al., 2020, 2024; Asimovic et al., 2021; Arceneaux et al., 2024). In addition, we consider the secondary effects of Facebook deactivation on agenda setting as well as the prevalence of echo chambers and their implications for media trust. All of the treatment effects for the media variables can be found in Figure 4.

News Knowledge. Following the practice used in previous deactivation studies, we presented participants with a list of 12 possible news events asked them to say whether each event happened in the last month. Because previous studies have shown that Facebook deactivation effects knowledge about politics (as opposed to knowledge about non-political events) (Allcott et al., 2024; Arceneaux et al., 2024), each of the statements was about political events. We selected six statements that were true and six that were false. The false statements were selected from actual misinformation circulating on social media, including Facebook, during the deactivation period. We selected three that would be tempting to Trump voters to believe (e.g., Michael Jordan en-

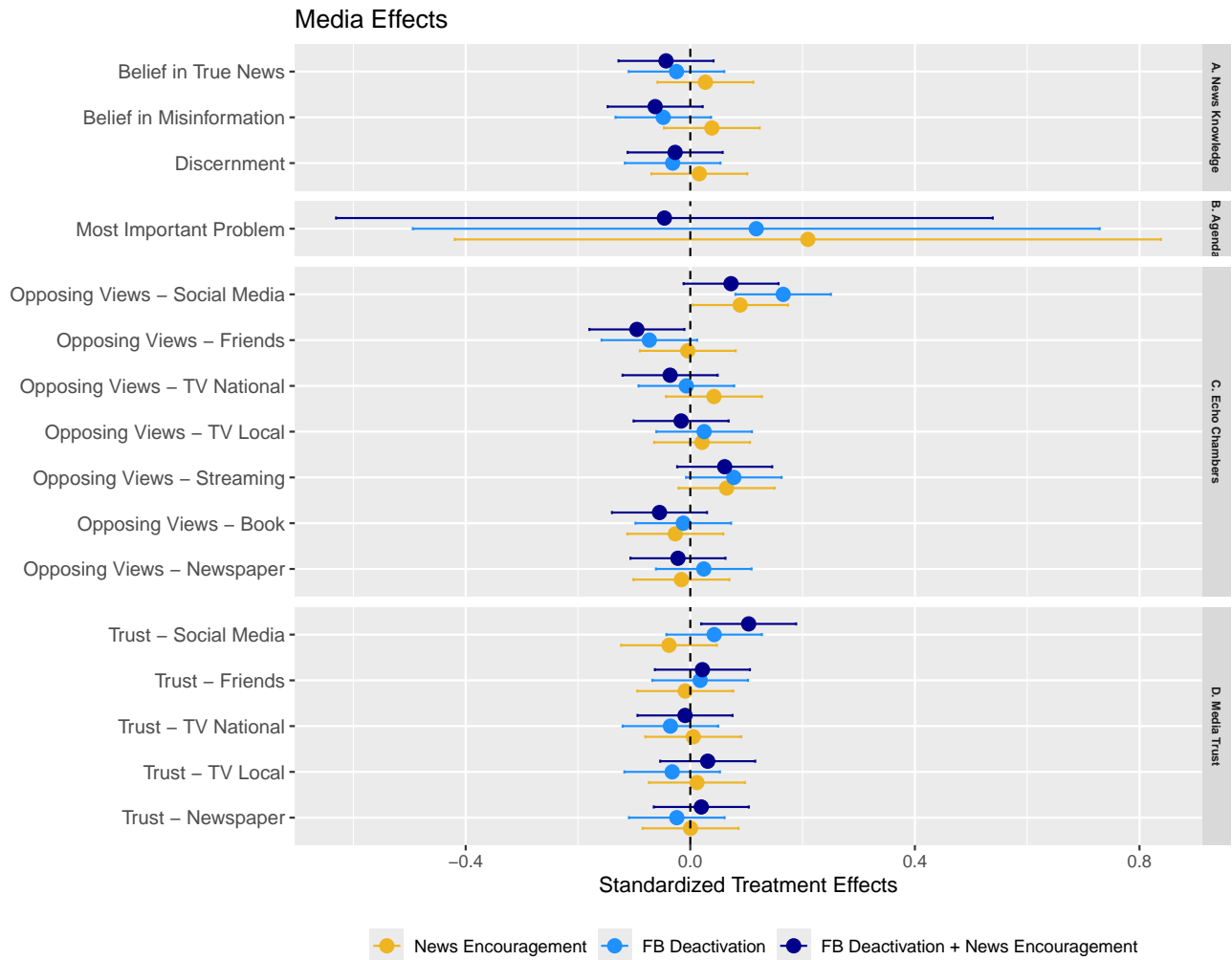


Figure 4: The Effects of Facebook Deactivation and News Encouragement on Media Influence and Behaviors

Note: Points represent the standardized treatment effects relative to the control group and horizontal lines represent the 95% confidence intervals.

dorsed Donald Trump) and three that would be tempting to Harris voters (e.g., Harris released a photo of her working at McDonald’s when she was younger, rebutting the Trump campaign’s claim that she lied about working at the fast food chain). By providing participants with actual examples of circulating misinformation, our study replicates Allcott et al. (2024) and goes beyond previous Facebook deactivation studies that present participants with placebo news statements that were fabricated by the research team (Allcott et al., 2020; Arceneaux et al., 2024; Asimovic et al., 2021). In doing so, we are able to estimate the degree to which Facebook deactivation reduces belief in actual misinformation about the presidential campaign. We preregistered hypotheses con-

cerning the effects of the treatments on having accurate knowledge regarding true news as well as inaccurately believing misinformation. It is also common for researchers in this area of scholarship to measure *discernment*, which is the difference between average number of statements that participants recognize accurately as being false and the average number of statements that they recognized accurately as being true (Pfänder and Altay, 2025). It was an oversight to not preregister hypotheses regarding discernment, given that it is so common in the literature. Consequently, we report the non-preregistered effects of the treatments on discernment here. As Panel A of Figure 4 shows, neither Facebook deactivation nor news encouragement had statistically significant effects on belief in true news, belief in misinformation, or discernment.

Agenda Setting. A subtle type of media effect, “agenda setting” occurs when media messages influence the salience of a political issue as opposed to the position that the public takes on that issue — to paraphrase Bernard Cohen, it happens when the media tell people what to think about and not what to think (e.g., Cohen, 1963; Iyengar and Kinder, 1987; McCombs and Valenzuela, 2021). Given that Facebook users often learn about politics by encountering reposted news stories from traditional media sources (Guess et al., 2023), Facebook offers a pathway for agenda setting to occur. Those who were randomly assigned to deactivate their Facebook accounts during the 2022 French presidential election were less likely to cite issues commonly reported in the mainstream news media as among “most important problem” facing the country, suggesting evidence that Facebook is a source of agenda setting (Zengin et al., 2024). In this study, we provided participants with a list of 18 issues facing the country and ask them to rank the top-5 most important problems. We measured agenda setting by determining the top five issues covered on the front page of the *New York Times* over the two-week deactivation period and then coding participants’ responses to the most important problem question as 1 if they mentioned one of the five issues in their top-5 ranking, 0 otherwise. We hypothesized that Facebook deactivation would make it less likely that participants would select the issues most covered by the paper of record as one of the most important problems, where as news encouragement would counteract the negative effect. The results reported in Panel B of Figure 4 are inconsistent with our hypotheses.

Echo Chambers and Media Trust. Social media affordances, especially those on Face-

book, enable users to construct a social networks that minimizes encounters with ideologically like-minded political content, fueling concerns that social media platforms created “echo chambers” (Pariser, 2011; Quattrociochi, Scala and Sunstein, 2016). While Facebook users are more likely to encounter ideologically like-minded content (Nyhan et al., 2023), the algorithm that filters and organizes posts on users’ news feeds does not full suppress exposure to counter-attitudinal content (Bakshy, Messing and Adamic, 2015). Moreover, when Facebook users do encounter counter-attitudinal news, they engage with it (Levy, 2021). Consequently, we hypothesized that Facebook deactivation would decrease self-reported exposure to counter-attitudinal information. The results reported in Panel C of Figure 4 are inconsistent with these expectations. Across many different sources, we observe mostly null effects of Facebook deactivation and news encouragement on encountering opposing views. The one exception is social media, which is a bit puzzling as it suggests that Facebook deactivation and news encouragement may lead people to search for opposing views on *other* social media platforms.

Research on traditional news media shows that exposure to counter-attitudinal news content creates the perception that the news media are hostile toward one’s point of view (Vallone, Ross and Lepper, 1985) and reducing trust in news media (Ladd, 2012). In contrast, we theorized that because Facebook is not a news media source — it is a social media platform — exposure to counter-attitudinal political content should have no effect on trust in the traditional news media (for an alternative thesis, see Karlsen and Aalberg, 2023). In contrast, we anticipated that by exposing participants to balanced news from traditional news media, the news encouragement treatment would increase trust in newspapers. As expected, with respect to the effects of Facebook deactivation, we observed mostly precise null effects in Panel D of Figure 4 ($BF_{National} = 0.0006$, $BF_{Local} = 0.001$, $BF_{Newspaper} = 0.0006$). The one exception is that Facebook deactivation + news encouragement increased trust in social media ($d = 0.10$, $p = 0.016$, $Q_{within} = 0.316$, $Q_{overall} = 0.319$), yet the false discovery rates associated with it are well above the 5% threshold, suggesting that this finding may be a fluke.

Social Effects

We now turn to the social effects of Facebook deactivation (see Table 1). Previous Facebook deactivation studies have focused largely on the influence of Facebook deactivation on subjective well-being, which measures the degree to which people reporting feeling positive emotions as well as not feeling negative ones (Allcott et al., 2020, 2024; Asimovic et al., 2021; Arceneaux et al., 2024). We extend this area of research by considering the effects of Facebook deactivation on depression symptoms, social trust, and self-conscious emotions. All of the treatment effects for the social variables can be found in Figure 5.

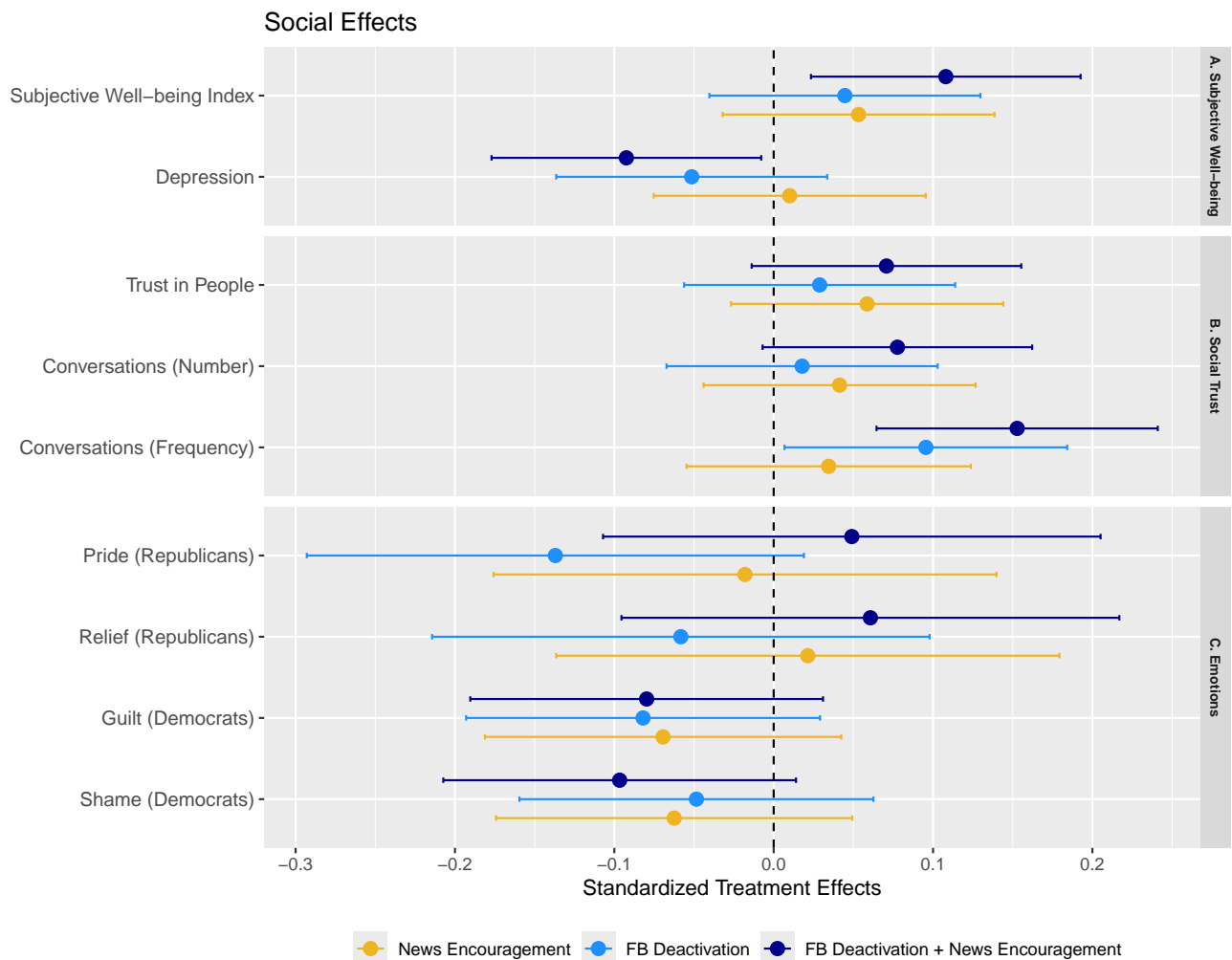


Figure 5: The Effects of Facebook Deactivation and News Encouragement on Social Measures
Note: Points represent the standardized treatment effects relative to the control group and horizontal lines represent the 95% confidence intervals.

Subjective Well-being. Previous research finds that Facebook deactivation increases self-

reported life satisfaction as well as experiences of positive emotions, while decreasing experiences of negative ones, which collectively measure “subjective well-being” (Allcott et al., 2020; Asimovic et al., 2021; Arceneaux et al., 2024). In addition to replicating this research, we also wished to address the controversy over whether social media increases depression (Orben, Dienlin and Przybylski, 2019; Orben and Przybylski, 2019; Twenge and Campbell, 2018; Twenge et al., 2020) by including two common measures of depression symptoms on the endline survey. Based on the findings in previous deactivation studies, we hypothesized that Facebook deactivation would increase subjective well-being and decrease depression. We had no expectations regarding news exposure. As Panel A of Figure 5 shows, Facebook deactivation + news exposure increased subjective well-being ($d = 0.11, p = 0.012, Q_{within} = 0.081, Q_{overall} = 0.319$). Facebook deactivation increased subjective well-being as well, but the effect is not statistically significant ($d = 0.04, p = 0.303$). Nonetheless, the Q values are above the preregistered 5% threshold. Likewise, Facebook deactivation may have decreased depression symptoms, but the effect is again only statistically significant for the Facebook deactivation + news exposure condition yet with Q values above the 5% threshold ($d = 0.09, p < 0.032, Q_{within} = 0.088, Q_{overall} = 0.545$).

Social Trust. Social media platforms, like Facebook, create social interactions in an online space that mimic but are more likely to be viewed as negative and hostile relative to offline social interactions (Bor and Petersen, 2022). To the extent that people substitute more negative online interactions on social media platforms for more positive offline ones, social media use could decrease social trust (Sabatini and Sarracino, 2019). We test this hypothesis by measuring both the number and frequency of friendly offline conversations along with the degree to which participants trust others. We anticipated that Facebook deactivation would increase friendly offline interactions and increase social trust. We had no expectations regarding the effects of news encouragement. While we do observe that Facebook deactivation ($d = 0.10, p = 0.034, Q_{within} = 0.163, Q_{overall} = 0.545$) and Facebook deactivation + news encouragement ($d = 0.15, p = 0.001, Q_{within} = 0.007, Q_{overall} = 0.039$) increased the frequency of offline conversations, it had essentially no statistically significant effects on social trust (Panel B, Figure 5).

Self-conscious Emotions. Humans are inherently social beings with a fundamental desire for

acceptance. Maintaining positive self-representation is critically important, both within our social groups and in broader contexts, such as social media platforms (Tracy, Robins and Tangney, 2007). Accordingly, self-conscious emotions (e.g., pride and shame) play a crucial role in helping individuals achieve and protect social status and avoid rejection (Keltner and Buswell, 1997). Elections represent a form of group conflict that can threaten the integrity of group identity, often intensifying negative feelings toward the opposing side (Zengin, 2024). We anticipate that Republicans will experience positive self-conscious emotions (pride and relief) as their ingroup emerges victorious, while Democrats will experience negative self-conscious emotions (shame and guilt) as their in-group faces defeat. Given that social media environments frequently amplify partisan and emotionally charged content (Kazlauskaitė and Salmela, 2022), we hypothesize that both Republicans and Democrats who deactivated their Facebook accounts prior to the election would be less likely to encounter such content and, as a result, would experience these emotions less intensely, compared to those in the control group. However, as shown in Panel C of Figure 5, while Facebook deactivation may have reduced the pride and relief felt by Republicans as well as the shame and guilt felt by Democrats, none of these effects are statistically significant.

Discussion

We conducted a sufficiently powered field experiment during the 2024 US presidential elections to study the effects of Facebook deactivation and encouragements to consume and ideologically balanced news diet on several political and social outcomes. Our study joins four other published Facebook deactivation studies, allowing us to make inferences about not only the effects Facebook in this particular context, but also about its effects across time and contexts. If stand back and consider the totality of the treatment effects that we estimated, we reach the conclusion that neither Facebook deactivation, nor encouragements to consume ideologically balanced news had much of an effect on political or attitudes, beliefs, or behavior. These findings stand in contrast to the sometimes hyperbolic descriptions of the political and social significance of social media.

However, our slew of null findings align with other experimental studies of Facebook that use

more fine-grained manipulations than deactivation studies. For instance, randomly varying the presence of ideological news in users feeds does affect the degree to which people engage with counter-attitudinal news, but it does not affect people’s political opinions and lowers polarization only slightly (Levy, 2021). Likewise, randomly removing reshares (i.e., the re-posts that others make) from users reduces people’s knowledge about the news, but has no effect on political opinions (Guess et al., 2023). The same goes for reducing the amount of likeminded information in users’ feeds: it increased exposure to counter-attitudinal news and reduced exposure to uncivil interactions, but had no effect on political attitudes, polarization, beliefs, and news knowledge (Nyhan et al., 2023). In sum, it would seem that exposure to political information has relatively small overall effects on Facebook users.

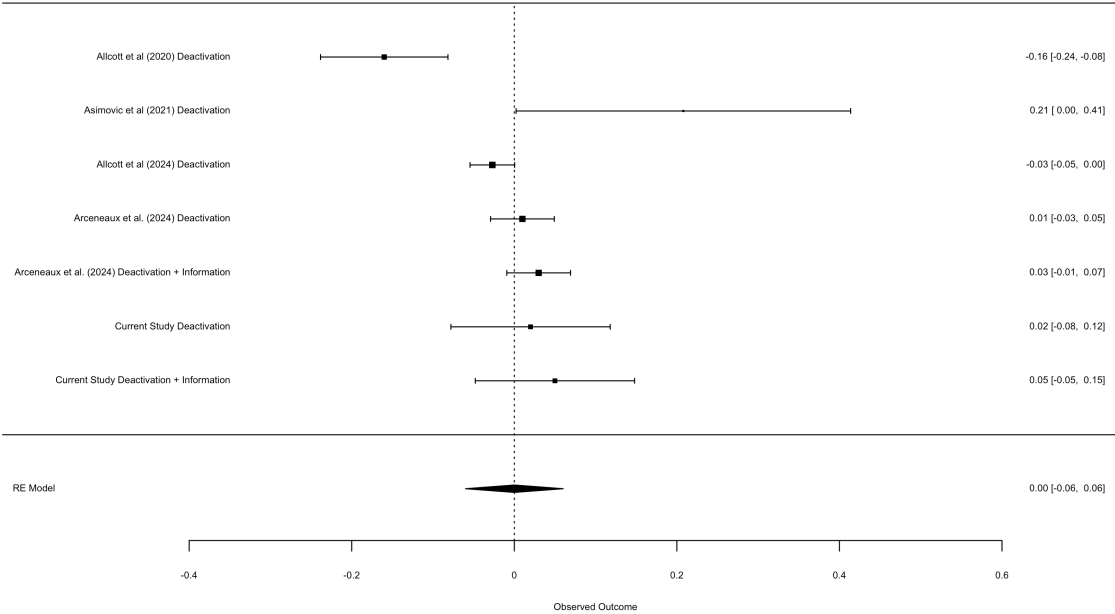


Figure 6: Forest Plot for the Effects of Facebook Deactivation on Affective Polarization

Turning our attention to the meaning of our findings in the context of previous Facebook deactivation studies, we performed meta-analyses for three outcomes that are common in almost all previous studies and this one: affective polarization, news knowledge, and subjective well-

being. Figure 6 shows the forest plot for the effects associated with affective polarization. These effects represented affective partisan polarization, except for [Asimovic et al. \(2021\)](#), who measured affective polarization with respect to ethnicity. We also chose to use the effect associated with the affective polarization index reported in [Allcott et al. \(2020\)](#), which was negative and statistically significant, given that many focus on this particular effect. As already noted, however, [Allcott et al. \(2020\)](#) do measure affective partisan polarization and find small statistically insignificant effects. If we were to use this effect, it would not change our conclusions about the effect of Facebook deactivation on affective polarization, which is that it basically has no effect.

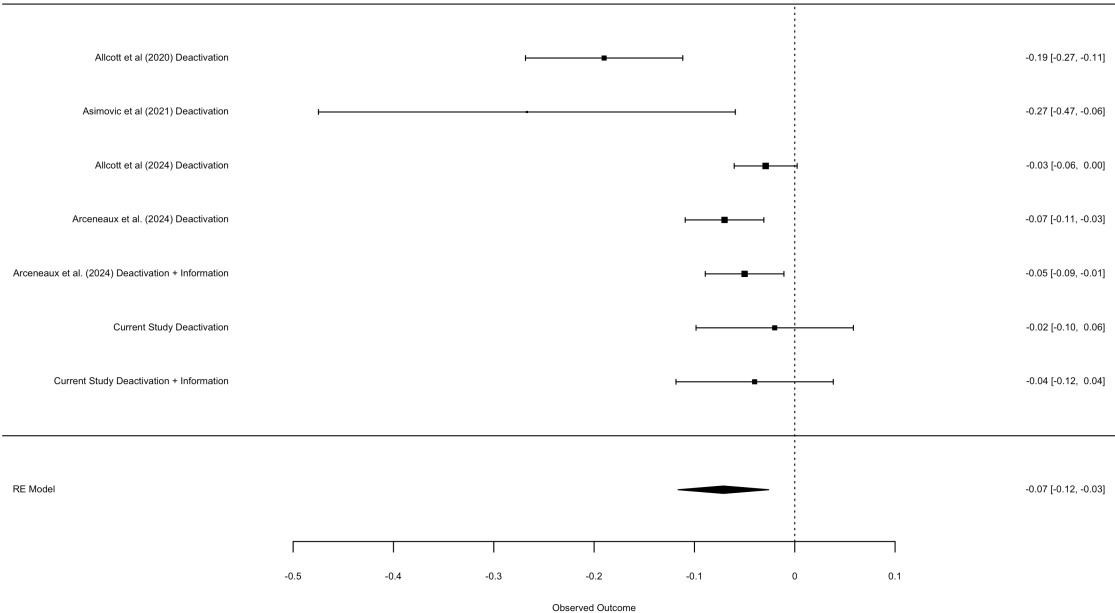


Figure 7: Forest Plot for the Effects of Facebook Deactivation on News Knowledge

In contrast, Figure 7 shows that the pooled effect of Facebook deactivation on news knowledge is a relatively small negative effect. We find three things noteworthy about this finding. First since 2019, the negative effects of Facebook deactivation have essentially decreased by half. One possibility is that the initial Facebook deactivation studies overestimated the negative effect of deactivation on news knowledge. Another possibility is that Facebook users have changed their

behavior since 2019 as other social media platforms provide information about news and politics. A final possibility is that Facebook itself has changed the algorithm in ways that either purposely or inadvertently de-emphasize political news content. Because Facebook’s algorithm is not publicly available, we cannot know if this is the case. Second, encouragements to seek out news do not seem to overcome the negative effects of Facebook deactivation. These findings align with research by [Hopkins and Gorton \(2024\)](#), and suggest that because interest in politics is low and stable over time ([Prior, 2019](#)), nudges alone are not enough to overcome the desire of some people to avoid news about politics. Third, a post-hoc analysis of our data shows that Facebook Deactivation, even when coupled with encouragements to seek out news, *reduced* the amount of self-reported exposure to news about the election — overall and driven by participants saying that they received less exposure from other social media and digital platforms (see Table A5 in Section A4 of the Appendix). Given that Facebook deactivation did not affect the amount of time participants spent on other social media and digital platforms (see Figure 2), we surmise that Facebook deactivation may have affected how participants engaged with news about politics.

The forest plot in Figure 8 shows that Facebook deactivation has a robust small positive effect on subjective well-being ($d = 0.06$). According to [Anvari and Lakens \(2021\)](#), in the context of subjective well-being, a standardized effect size of 0.06 is a “small effect of interest” in that it represents the point at which people can recognize that they feel a little bit better than yesterday. Taken together then, Facebook deactivation studies show that incentivized voluntarily deactivation for a finite period of time modestly boosts people’s mood and life satisfaction. These findings are inconsistent with some of the dramatic claims made about the deleterious effects of social media on mental health. Even if Facebook makes people a little bit less happy on average, it does not seem to be driving droves of its users to despair. The one caveat that we offer for this conclusion is that our study focused on adults, and not adolescents, which some researchers believe are especially vulnerable to the negative mental effects of social media ([Twenge et al., 2020](#)).

All considered, our results combined with the minimal, often null or contingent effects of social media observed in other field experimental studies converge on a portrait of social media effects that would be foreign to most casual observers of politics and many scholars in the domain. Far from

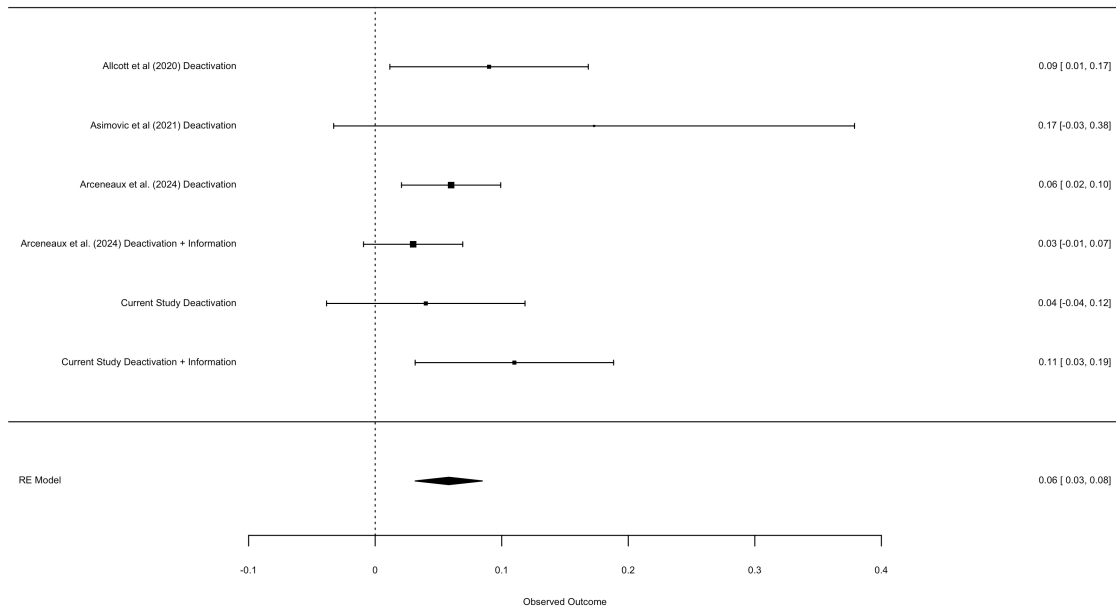


Figure 8: Forest Plot for the Effects of Facebook Deactivation on Subjective Well-being

being a dystopic ever-present influence on our lives, the largest and still most used social media platform, Facebook, has modest overall effects, which is in line with an active audience theory of media influence. Most people are not dupes. They use social media to obtain particular hedonic or informational ends, and in doing so, they are often capable of making sense of the information that they encounter. Social media does appear to help some people encounter information about politics that they otherwise would have missed, but given the low level of political content on social media in the first place (Nyhan et al., 2023), these sorts of news-finds-me effects are relatively modest.

This is not to say that Facebook does not influence some individuals a great deal. It simply means that these sorts of large arresting effects that make up the kind of anecdotes that make for good feature stories and dinner-table conversations are not representative of the effects that social media have on users. Almost everyone can likely point to a relative who has gone mad from exposure to conspiracy theories on social media, but this is usually the exception to the rule. We do not tell these sorts of stories about most people in our lives, much less ourselves. We offer the

provocative proposal that this goes as well for academic research on social media effects. It seems intuitive that social media must be responsible for the consequential secular shifts that we have witnessed in politics and society over the past 10 to 15 years, such as polarization, misinformation, and conspiracy theories. As evidence, one can point to large arresting effects in lab and survey experimental experiments that create the optimal conditions for people to be swayed by simulated social media content, but these examples offer anecdotal evidence at best. Field experimental research that studies the influence of social media in real-world context suggests that once we consider the effects of social media in the cacophonous media environment in which they exist, large arresting effects are not the norm. A more plausible explanation is that political observers and researchers alike are subject to the third-person effect, a common socio-cognitive bias in which people overestimate the degree to which others are influenced by media (Davison, 1983). Even if we consider that some people are more influenced by media than others, it is still possible for savvy media users, such as academic researchers, to have inaccurate intuitions about how much others are swayed by media content (Lyons, 2022). Consequently, we need to rethink our theoretical approach to studying the effects of social media, focusing more on users' agency in the process and less on their role as a passive receiver of content.

We also recognize that our study can only estimate the effects of deactivating Facebook. While this is a necessarily limitation baked into our experimental design, we do believe that the broad swath of users that Facebook has across the world and the United States makes it the sort of platform from which one can make generalizations. If misinformation is not swaying the average consumer of Facebook, why would we expect that other social media platforms would be all that different? We appreciate that an easy retort is that TikTok videos are mesmerizing or that the algorithm on X or YouTube is better at targeting and polarizing users that Facebook's is. It is certainly possible, and the only way to be sure would be to conduct experimental research with those platforms. Yet, the state of the evidence right now leads us to preregister the hypothesis that those platforms likely have minimal, contingent effects as well. Rather than hunting for the magical media that bamboozles people, we suggest that it might be better to take a better view of human beings. While we can all be bamboozled from time to time and there are certainly people

that get bamboozled more often than others, the average person can avoid being an easy mark most of the time (Mercier and Sperber, 2017). Human intelligence is more robust than many give it credit.

We conclude with thoughts about the next steps in this research agenda. Accumulating evidence suggests that social media have small, contingent effects on individual users. In many ways, this conclusion is not all that different from the one scholars reached after being concerned about massive media effects with respect to traditional news media (Zaller, 1992) and partisan news media (Arceneaux and Johnson, 2013). While we must continue studying the ways in which media influence the public, we should also broaden the study of media effects to include political activists and elites. While these savvy media users are unlikely to be manipulated by media messages, including those in social media, they may view social media content as a window into public opinion and calibrate their behavior accordingly (Arceneaux et al., 2025; Herbst, 1998). Indeed, political elites may be especially susceptible to the third-person effect by overestimating the degree to which media content influences others, leading them to presume that if a particular piece of misinformation is circulating on social media, for instance, and there are anecdotes of some people believing it, then *presumably* it must be persuading most people. The last piece of this reasoning chain is a non sequitur, as our data suggests. Just because some people are influenced by social media does not mean that we can presume that most are. Yet if this is the sort of presumption that many political elites make, social media content may shape their behavior — not because it influences them (although it may) but because they believe that it influences others. If so, social media may be responsible for increasing polarization, hostility, and the willingness to recycle misinformation among elites, even while it has a relatively modest impact on the rest of us.

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Appendix

A1 Descriptive Statistics

Table A1: Descriptive Statistics

Variable	Full Sample Mean (SD)	End-line Sample Mean (SD)
n	4971	4278
Age	48.76 (15.90)	49.01 (15.86)
Female	0.53 (0.50)	0.52 (0.50)
White	0.70 (0.46)	0.70 (0.46)
Education	4.05 (1.44)	4.08 (1.43)
Married	0.45 (0.50)	0.46 (0.50)
Children at Home	0.27 (0.45)	0.27 (0.44)
Employed	0.55 (0.50)	0.56 (0.50)
Income	6.86 (3.66)	6.96 (3.64)
Democrat	0.58 (0.48)	0.58 (0.47)
Republican	0.31 (0.48)	0.30 (0.47)
Registered	0.93 (0.25)	0.94 (0.24)
2020 Biden Vote	0.52 (0.50)	0.53 (0.50)
Conservative	2.76 (1.14)	2.75 (1.14)
Protestant	0.29 (0.45)	0.28 (0.45)
Catholic	0.21 (0.41)	0.21 (0.41)
Political Interest	3.29 (0.88)	3.31 (0.87)

A2 Balance Tables

Table A2: Balance Table for the Full Sample

	Control	News Encouragement	FB Deactivation	FB+Encouragement	p
n	1242	1243	1244	1242	
Age (mean (SD))	47.87 (16.01)	48.49 (16.12)	49.31 (15.44)	49.36 (16.00)	0.059
Female (mean (SD))	0.52 (0.50)	0.53 (0.50)	0.51 (0.50)	0.54 (0.50)	0.691
White (mean (SD))	0.69 (0.46)	0.70 (0.46)	0.70 (0.46)	0.68 (0.47)	0.693
Education (mean (SD))	4.05 (1.45)	4.05 (1.44)	4.03 (1.46)	4.05 (1.41)	0.977
Married (mean (SD))	0.45 (0.50)	0.46 (0.50)	0.46 (0.50)	0.44 (0.50)	0.581
Child at Home (mean (SD))	0.29 (0.45)	0.27 (0.44)	0.28 (0.45)	0.26 (0.44)	0.405
Employed (mean (SD))	0.56 (0.50)	0.55 (0.50)	0.56 (0.50)	0.54 (0.50)	0.597
Income (mean (SD))	6.81 (3.69)	6.85 (3.63)	6.99 (3.68)	6.80 (3.63)	0.549
Democrat (mean (SD))	0.59 (0.48)	0.57 (0.48)	0.57 (0.48)	0.58 (0.47)	0.865
Republican (mean (SD))	0.31 (0.48)	0.31 (0.48)	0.31 (0.48)	0.30 (0.47)	0.892
Registered (mean (SD))	0.93 (0.26)	0.93 (0.26)	0.94 (0.24)	0.93 (0.26)	0.796
2020 Biden Vote (mean (SD))	0.53 (0.50)	0.51 (0.50)	0.52 (0.50)	0.52 (0.50)	0.629
Conservative (mean (SD))	2.74 (1.16)	2.76 (1.15)	2.76 (1.11)	2.79 (1.13)	0.804
Protestant (mean (SD))	0.29 (0.46)	0.29 (0.45)	0.27 (0.44)	0.30 (0.46)	0.358
Catholic (mean (SD))	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)	0.990
Pol Int (mean (SD))	3.31 (0.87)	3.29 (0.87)	3.26 (0.89)	3.30 (0.87)	0.534

Table A3: Balance Table for Sample in Endline Survey

	Control	News Encouragement	FB Deactivation	FB+Encouragement	p
n	1051	1058	1073	1096	
Age (mean (SD))	48.16 (15.87)	48.86 (16.15)	49.27 (15.52)	49.70 (15.89)	0.141
Female (mean (SD))	0.53 (0.50)	0.52 (0.50)	0.51 (0.50)	0.54 (0.50)	0.538
White (mean (SD))	0.70 (0.46)	0.70 (0.46)	0.71 (0.46)	0.69 (0.46)	0.740
Education (mean (SD))	4.10 (1.43)	4.08 (1.44)	4.08 (1.45)	4.08 (1.40)	0.988
Married (mean (SD))	0.46 (0.50)	0.47 (0.50)	0.48 (0.50)	0.44 (0.50)	0.465
Child at Home (mean (SD))	0.28 (0.45)	0.27 (0.44)	0.28 (0.45)	0.26 (0.44)	0.609
Employed (mean (SD))	0.57 (0.50)	0.55 (0.50)	0.57 (0.49)	0.54 (0.50)	0.339
Income (mean (SD))	6.91 (3.67)	6.94 (3.59)	7.12 (3.65)	6.87 (3.63)	0.407
Democrat (mean (SD))	0.59 (0.48)	0.58 (0.47)	0.59 (0.47)	0.58 (0.47)	0.988
Republican (mean (SD))	0.31 (0.48)	0.30 (0.47)	0.30 (0.47)	0.30 (0.47)	0.911
Registered (mean (SD))	0.94 (0.24)	0.93 (0.25)	0.94 (0.23)	0.94 (0.24)	0.886
2020 Biden Vote (mean (SD))	0.53 (0.50)	0.53 (0.50)	0.53 (0.50)	0.52 (0.50)	0.956
Conservative (mean (SD))	2.73 (1.17)	2.74 (1.16)	2.75 (1.11)	2.79 (1.14)	0.610
Protestant (mean (SD))	0.28 (0.45)	0.28 (0.45)	0.27 (0.44)	0.30 (0.46)	0.463
Catholic (mean (SD))	0.21 (0.41)	0.21 (0.41)	0.21 (0.40)	0.22 (0.41)	0.862
Pol Int (mean (SD))	3.34 (0.84)	3.32 (0.85)	3.28 (0.89)	3.30 (0.88)	0.393

A3 False Discovery Rates

Table A4: False Discovery Rates

Dependent Variable	Treatment	p-value	$Q_{overall}$	Q_{within}
I. Political Effects				
Turnout	News Encouragement	0.84	1.00	1.00
Turnout	FB Deactivation	0.29	1.00	0.97
Turnout	FB Deactivation + News Encouragement	0.43	1.00	1.00
Voted Trump	News Encouragement	0.33	1.00	0.97
Voted Trump	FB Deactivation	0.18	1.00	0.97
Voted Trump	FB Deactivation + News Encouragement	0.50	1.00	1.00
Non-voter's Trump choice	News Encouragement	0.51	1.00	1.00
Non-voter's Trump choice	FB Deactivation	0.87	1.00	1.00
Non-voter's Trump choice	FB Deactivation + News Encouragement	0.75	1.00	1.00
Perceived Winner: Harris voters	News Encouragement	0.58	1.00	1.00
Perceived Winner: Harris voters	FB Deactivation	0.01	0.32	0.29
Perceived Winner: Harris voters	FB Deactivation + News Encouragement	0.05	0.63	0.41
Perceived Winner: Trump voters	News Encouragement	0.27	1.00	0.97
Perceived Winner: Trump voters	FB Deactivation	0.21	1.00	0.97
Perceived Winner: Trump voters	FB Deactivation + News Encouragement	0.06	0.85	0.41
Partisan Polarization	News Encouragement	0.27	1.00	1.00
Partisan Polarization	FB Deactivation	0.24	1.00	1.00
Partisan Polarization	FB Deactivation + News Encouragement	0.73	1.00	1.00
Ideological Polarization	News Encouragement	0.01	0.30	0.08
Ideological Polarization	FB Deactivation	0.63	1.00	1.00
Ideological Polarization	FB Deactivation + News Encouragement	0.94	1.00	1.00
Racial Polarization (White-Black)	News Encouragement	0.33	1.00	1.00
Racial Polarization (White-Black)	FB Deactivation	0.81	1.00	1.00
Racial Polarization (White-Black)	FB Deactivation + News Encouragement	0.42	1.00	1.00
Racial Polarization (White-Hispanic)	News Encouragement	0.96	1.00	1.00
Racial Polarization (White-Hispanic)	FB Deactivation	0.24	1.00	1.00
Racial Polarization (White-Hispanic)	FB Deactivation + News Encouragement	0.18	1.00	1.00
Political Violence	News Encouragement	0.18	1.00	0.18
Political Violence	FB Deactivation	0.22	1.00	0.18
Political Violence	FB Deactivation + News Encouragement	0.02	0.32	0.05
Democracy Support	News Encouragement	0.72	1.00	0.36
Democracy Support	FB Deactivation	0.12	1.00	0.36
Democracy Support	FB Deactivation + News Encouragement	0.18	1.00	0.36
II. Media Effects				
Belief in True News	News Encouragement	0.54	1.00	1.00
Belief in True News	FB Deactivation	0.57	1.00	1.00
Belief in True News	FB Deactivation + News Encouragement	0.32	1.00	1.00
Belief in Misinformation	News Encouragement	0.38	1.00	1.00
Belief in Misinformation	FB Deactivation	0.27	1.00	1.00
Belief in Misinformation	FB Deactivation + News Encouragement	0.15	1.00	1.00
Discernment	News Encouragement	0.71	1.00	1.00
Discernment	FB Deactivation	0.47	1.00	1.00
Discernment	FB Deactivation + News Encouragement	0.53	1.00	1.00
Most Important Problem	News Encouragement	0.51	1.00	1.00
Most Important Problem	FB Deactivation	0.71	1.00	1.00
Most Important Problem	FB Deactivation + News Encouragement	0.88	1.00	1.00
Opposing Views - Social Media	News Encouragement	0.04	0.60	0.38
Opposing Views - Social Media	FB Deactivation	0.00	0.02	0.00
Opposing Views - Social Media	FB Deactivation + News Encouragement	0.09	0.89	0.41
Opposing Views - Friends	News Encouragement	0.92	1.00	1.00
Opposing Views - Friends	FB Deactivation	0.09	0.89	0.41
Opposing Views - Friends	FB Deactivation + News Encouragement	0.03	0.53	0.38
Opposing Views - TV National	News Encouragement	0.33	1.00	0.77
Opposing Views - TV National	FB Deactivation	0.87	1.00	1.00
Opposing Views - TV National	FB Deactivation + News Encouragement	0.41	1.00	0.91
Opposing Views - TV Local	News Encouragement	0.63	1.00	0.91
Opposing Views - TV Local	FB Deactivation	0.57	1.00	0.91

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Dependent Variable	Treatment	p-value	$Q_{overall}$	Q_{within}
Opposing Views - TV Local	FB Deactivation + News Encouragement	0.71	1.00	0.91
Opposing Views - Streaming	News Encouragement	0.14	1.00	0.48
Opposing Views - Streaming	FB Deactivation	0.07	0.89	0.41
Opposing Views - Streaming	FB Deactivation + News Encouragement	0.16	1.00	0.48
Opposing Views - Book	News Encouragement	0.54	1.00	0.91
Opposing Views - Book	FB Deactivation	0.77	1.00	0.96
Opposing Views - Book	FB Deactivation + News Encouragement	0.20	1.00	0.52
Opposing Views - Newspaper	News Encouragement	0.72	1.00	0.91
Opposing Views - Newspaper	FB Deactivation	0.58	1.00	0.91
Opposing Views - Newspaper	FB Deactivation + News Encouragement	0.61	1.00	0.91
Trust - Social Media	News Encouragement	0.38	1.00	1.00
Trust - Social Media	FB Deactivation	0.33	1.00	1.00
Trust - Social Media	FB Deactivation + News Encouragement	0.02	0.32	0.32
Trust - Friends	News Encouragement	0.84	1.00	1.00
Trust - Friends	FB Deactivation	0.68	1.00	1.00
Trust - Friends	FB Deactivation + News Encouragement	0.62	1.00	1.00
Trust - TV National	News Encouragement	0.90	1.00	1.00
Trust - TV National	FB Deactivation	0.41	1.00	1.00
Trust - TV National	FB Deactivation + News Encouragement	0.83	1.00	1.00
Trust - TV Local	News Encouragement	0.79	1.00	1.00
Trust - TV Local	FB Deactivation	0.46	1.00	1.00
Trust - TV Local	FB Deactivation + News Encouragement	0.47	1.00	1.00
Trust - Newspaper	News Encouragement	0.99	1.00	1.00
Trust - Newspaper	FB Deactivation	0.58	1.00	1.00
Trust - Newspaper	FB Deactivation + News Encouragement	0.65	1.00	1.00
III. Social Effects				
Subjective Well-being Index	News Encouragement	0.22	1.00	0.31
Subjective Well-being Index	FB Deactivation	0.30	1.00	0.32
Subjective Well-being Index	FB Deactivation + News Encouragement	0.01	0.32	0.08
Depression	News Encouragement	0.82	1.00	0.55
Depression	FB Deactivation	0.24	1.00	0.31
Depression	FB Deactivation + News Encouragement	0.03	0.54	0.09
Trust in People	News Encouragement	0.18	1.00	0.28
Trust in People	FB Deactivation	0.51	1.00	0.46
Trust in People	FB Deactivation + News Encouragement	0.10	0.94	0.22
Conversations (Number)	News Encouragement	0.34	1.00	0.40
Conversations (Number)	FB Deactivation	0.68	1.00	0.47
Conversations (Number)	FB Deactivation + News Encouragement	0.07	0.89	0.20
Conversations (Frequency)	News Encouragement	0.45	1.00	0.46
Conversations (Frequency)	FB Deactivation	0.03	0.54	0.16
Conversations (Frequency)	FB Deactivation + News Encouragement	0.00	0.04	0.01
Pride (Republicans)	News Encouragement	0.82	1.00	1.00
Pride (Republicans)	FB Deactivation	0.09	0.89	0.90
Pride (Republicans)	FB Deactivation + News Encouragement	0.54	1.00	0.90
Relief (Republicans)	News Encouragement	0.79	1.00	1.00
Relief (Republicans)	FB Deactivation	0.46	1.00	0.90
Relief (Republicans)	FB Deactivation + News Encouragement	0.45	1.00	0.90
Guilt (Democrats)	News Encouragement	0.22	1.00	0.90
Guilt (Democrats)	FB Deactivation	0.15	1.00	0.90
Guilt (Democrats)	FB Deactivation + News Encouragement	0.16	1.00	0.90
Shame (Democrats)	News Encouragement	0.27	1.00	0.90
Shame (Democrats)	FB Deactivation	0.39	1.00	0.90
Shame (Democrats)	FB Deactivation + News Encouragement	0.09	0.89	0.90

A4 News Engagement

Table A5: The Effects of Facebook Deactivation and News Encouragement on Exposure to News about the Election

	<i>Dependent variable:</i>				
	Social Media (1)	Digital Media (2)	Traditional Media (3)	Friends/Family (4)	Total Exposure (5)
Allsides	0.033 (0.043)	-0.013 (0.044)	0.057 (0.044)	0.059 (0.044)	0.048 (0.043)
Deactivate FB	-0.305*** (0.043)	-0.107** (0.043)	-0.014 (0.043)	-0.014 (0.043)	-0.190*** (0.043)
Allsides+Deactivate FB	-0.332*** (0.043)	-0.108** (0.043)	-0.013 (0.043)	-0.039 (0.043)	-0.207*** (0.043)
Control	0.153*** (0.030)	0.058* (0.031)	-0.007 (0.031)	-0.001 (0.031)	0.089*** (0.031)
Observations	4,267	4,271	4,270	4,272	4,258
R ²	0.028	0.003	0.001	0.001	0.013
Adjusted R ²	0.028	0.002	0.0001	0.001	0.012
Residual Std. Error	0.986 (df = 4263)	0.999 (df = 4267)	1.000 (df = 4266)	1.000 (df = 4268)	0.994 (df = 4254)
F Statistic	41.405*** (df = 3; 4263)	3.650** (df = 3; 4267)	1.181 (df = 3; 4266)	1.852 (df = 3; 4268)	18.190*** (df = 3; 4254)

Note:

*p<0.1; **p<0.05; ***p<0.01