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The effects of political advertising on Facebook and Instagram before the 2020 US election

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We study the effects of social media political advertising by randomizing subsets of 36,906 Facebook users and 25,925 Instagram users to have political ads removed from their news feeds for 6 weeks before the 2020 US presidential election. We show that most presidential ads were targeted towards parties' own supporters and that fundraising ads were the most common. On both Facebook and Instagram, we found no detectable effects of removing political ads on political knowledge, polarization, perceived legitimacy of the election, political participation (including campaign contributions), candidate favourability and turnout. This was true overall and for both Democrats and Republicans separately.

Digital advertising plays an increasingly important and controversial role in US politics. Digital ads rose from 2–3% of political ad spending in 2016 to 18% in 2020, totalling US$1.6 billion[1]. Many commentators have suggested that online ads may have profound impacts on elections[2–4]. For example, in his announcement that Twitter would not run political ads in 2020, then-CEO Jack Dorsey wrote that "internet advertising … brings significant risks to politics, where it can be used to influence votes to affect the lives of millions"[5].

Moreover, there is considerable worry about campaigns' ability to target political ads to specific individuals on social media[6,7]. In 2020, over three-fourths of Americans said that it is not acceptable for social media companies to use data to target political ads, and over half said that social media companies should not allow political ads at all[8]. Internal Meta documents described in ref. 9 outline potential concerns: "targeted political content can potentially harm people by narrowly delivering divisive appeals to vulnerable audiences; inciting violence; intimidating, discouraging, or misleading voters; creating echo chambers; and decreasing accountability for politicians."

We first present descriptive findings on the reach, targeting and purpose of ads related to the US 2020 presidential campaign using internal data from Meta. We then present results from a randomized experiment that removed all political ads from users' Facebook or Instagram feeds for the 6 weeks before Election Day and replaced them with non-political ads. To test concerns specific to targeted political ads, we also implemented a condition that removed and replaced only the subset of political ads on Facebook that were targeted with user data provided by the advertiser ('list-targeted ads'). We estimate effects on political knowledge, polarization, perceived legitimacy of the election, political participation (including campaign contributions), candidate favourability, turnout and vote choice. Prior literature has proposed

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mechanisms by which political ads could substantially impact all of these outcomes[10–12]. More details on our sample, experimental design, outcomes and data can be found in the Methods section.

We contribute to a distinguished literature on the effects of political advertising[10,11,13–16]. Many papers report 'minimal effects' of political advertising[17–22], but others find evidence of meaningful effects both online[22–25] and offline[12,26–28]. Our article differs conceptually from most prior work because we remove all or most political ads from users' feeds instead of randomly varying exposure to specific ads across users. Thus, our article complements previous literature by providing information on the effectiveness of an entire social media campaign in the weeks before the elections. Many hypothesized mechanisms by which ads could affect outcomes—including voter (de-)mobilization, polarization and perceptions of election legitimacy—concern the aggregate impact of all ads to which voters are exposed[11], and our large-sample study can capture these aggregate effects for online ads. In addition, like ref. 28, we are able to estimate the effects of a single party's ads by exploiting the fact that partisans saw almost exclusively ads from their own parties. We also go beyond prior work in surveying tens of thousands of participants multiple times, observing a large suite of outcomes from both surveys and administrative data, and incorporating internal data from Meta that provides insights into the reach and targeting of political ads.

We emphasize several important limitations. First, our estimates are directly informative only about the set of individuals who agreed to participate in the study. Second, although we include direct measures of turnout and contributions, many of our outcome variables are self-reported. Third, our estimates are specific to removing the last 6 weeks of social media ads in the US 2020 general election. Existing evidence suggests that political advertising could have larger effects earlier in the election cycle and in elections that are less politically polarized and heavily covered in the media, either in the USA[29] or in other countries[24].

This project is part of the US 2020 Facebook and Instagram Election Study, a collaboration between Meta and academics. The lead academic authors had final authority over the preregistered analysis plan and analysis decisions and controlled the text; Meta could not block the publication of any results. More details of this partnership are available in Supplementary Notes I and J.

Results

Descriptive evidence

This section presents descriptive findings on the reach and targeting of political advertising on Facebook and Instagram in the 6 weeks before the 2020 election. Unless noted otherwise, these findings are based on ad exposure data for our control group participants. We classify the ad affiliation and its type using two external sources: OpenSecrets (OS) and Wesleyan Media Project (WMP). OS provides a relatively narrow definition of presidential ads based on financial ties (unless stated otherwise, we use this definition). WMP provides a broader definition of presidential ads by predicting whether an ad can be associated with one of the campaigns according to its text. For more details on ad classification, see the Methods section. Additional figures and tables are presented in Supplementary Note F.

Ad loads were highly skewed. Figure 1 (top left) presents the distribution of political ad load during the study period (in impressions per week) on Facebook and Instagram. We define an 'impression' to be a single instance in which an ad appeared in a user's feed; a given user may have multiple impressions of the same ad. The Facebook control group mean, median and 99th percentile of political ad impressions were 23, 9.1 and 175 per week, respectively; 79% averaged at least one impression per week. The Instagram control group mean, median and 99th percentile of political ad impressions were 6.2, 1.8 and 71 per week, respectively; 57% averaged at least one impression per week.

The variation in political ad load is partially predicted by observable characteristics (Supplementary Fig. 2). For example, on both Facebook and Instagram, participants in swing states and participants who voted in 2016 have higher ad loads.

Individual ads were often shown multiple times to the same user: over the full study period, the average Facebook control group user was shown 132 political ad impressions corresponding to 72 unique ads.

Ads were seen primarily by their own-partisans. Figure 1 (top right) presents the average political ad load in the Facebook control group by party identification. The top line presents the overall political ad load, while the remaining four lines present presidential ads favouring each party. Using their broader and narrower definitions, respectively, WMP and OS coded 61% and 20% of control group political ad impressions as presidential. Using the WMP definition, 67% of the presidential partisan ad impressions were Democratic (the corresponding figure among the full US adult Facebook population is 65%).

While individuals saw on average more Democratic ads, there was substantial variation based on the audience: Democratic presidential ads were shown mainly to Democrats, while Republican presidential ads were shown mainly to Republicans. Both types of ad were most likely to be shown to the strongest partisans. Independents were shown the fewest political ads overall (Supplementary Fig. 3) as well as the fewest presidential ads. Relatedly, Supplementary Fig. 2 shows that participants who reported being undecided voters on the baseline survey see fewer political ads than those who reported that their minds were made up. Supplementary Figs. 4 and 5 show that almost all ads on Instagram that we can associate with a party were pro-Democratic (based on the subsample of Instagram users for whom we have ad load data). This may partly reflect Instagram's younger user base and the tendency of young voters to favour the Democratic party.

Most political ads were list-targeted. About 62% of all political ad impressions shown to the Facebook control group were list-targeted. For Instagram, the share is 71%. When focusing specifically on Facebook presidential campaign ads, this share is especially high: 73% and 90% of presidential ads were list-targeted according to the presidential ad coding from WMP and OS, respectively. This heavy use of lists suggests that campaigns' targeting of their own-partisans may be an intentional strategy.

Most list-targeted ads use multiple targeting strategies. About 54% of all political ad impressions shown to the Facebook control group used customer list custom audiences, 25% used website custom audiences and 20% used lookalike audiences (Supplementary Table 7).

Fundraising ads were most common. Figure 1 (bottom) presents the distribution of ad goals for presidential ads coded by WMP separately on the basis of the participant's party identification and whether the ad is pro-Democratic or pro-Republican. Overall, 46% of the presidential ad impressions seen by the Facebook control group are categorized as seeking donations, 26% as persuasive, 17% as collecting user information and 5.5% as merchandise sales. Looking across the facets, own-partisans primarily saw donation ads, while persuasion was the most common goal of ads shown to supporters of the other party.

The goals of the ads indicate which outcomes are more likely to be affected when ads are removed. Because persuasion and fundraising ads were the most common, we study the effects on attitudes and turnout, and the effects on engagement (including on-platform donations) and participation (including self-reported donations).

Beyond our sample, ads reached diverse ages and genders. Figure 2 shows characteristics of Facebook political ads shown to the full US adult Facebook population (rather than just our study sample) in the 40 days before the election. The first panel shows the distribution across ads of the share of impressions that were seen by male users.

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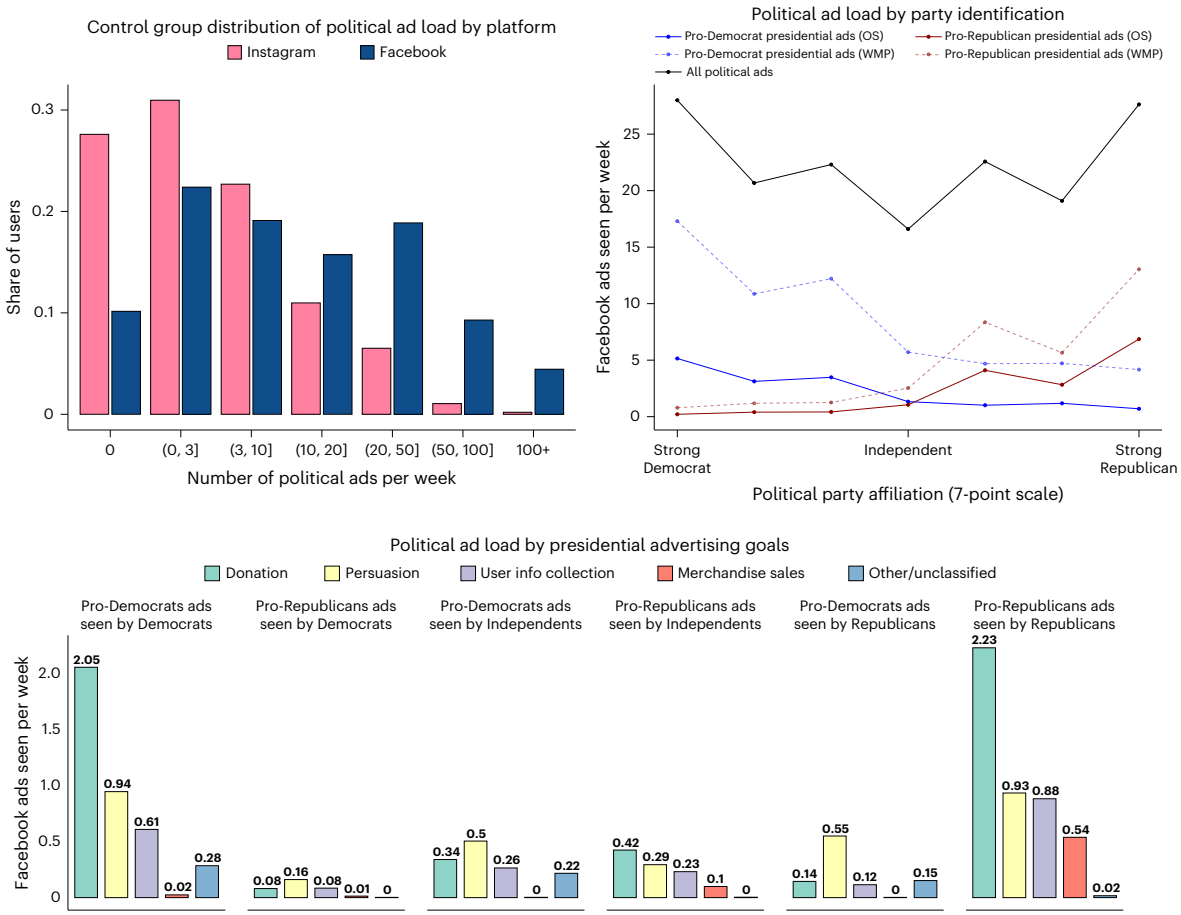

Fig. 1 | Control group political ad characteristics. Top left: the distribution of political ad load in the Facebook and Instagram control groups. Top right: the average political ad load by political party identification in the Facebook control group, as well as the average presidential ad load favouring each party, as coded by OS and the WMP. Bottom: the distribution of presidential ad goals (as coded by WMP) shown to the Facebook control group. The panel shows the distribution separately by participants' party identification (with independents who 'lean' towards either party included with that party) and by whether the ad is pro-Democratic or pro-Republican.

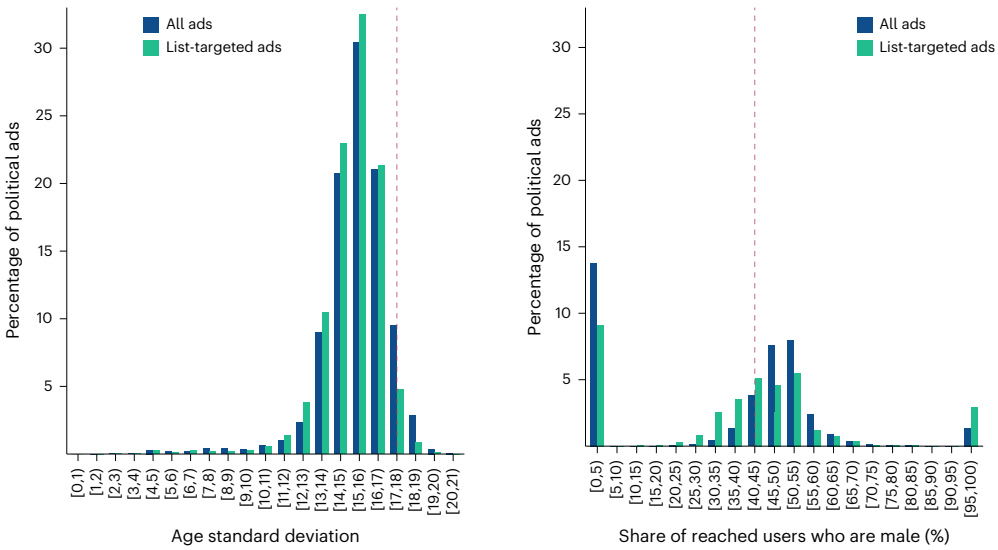

Fig. 2 | Distribution of user demographics across political ad impressions. This figure presents distributions of user demographics across political ads shown to US adult Facebook users in the 40 days before the 2020 election. The unit of analysis is an ad, and the observations are weighted by the number of impressions. The dashed red lines display user-level averages across all users who saw at least one political ad, constructed using internal data from Meta.

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Most ads reached fairly mixed audiences by gender, with 74% having between 40% and 60% impressions to males. For comparison, the vertical line on the figure shows that 44% of users are male. However, approximately 15% of political ads do appear to be narrowly targeted by gender, with male shares close to either 0% or 100%.

The second panel shows the distribution of ads by the standard deviation of age across their impressions. Most ads have a fairly large standard deviation, indicating that campaigns typically do not target a narrowly defined age group. Still, for most ads, the standard deviation across ages is slightly lower than the population standard deviation of 17, indicating that ads are not distributed at random and reach a somewhat narrower age range compared with the overall Facebook user base. This is consistent with Democrats and Republicans typically seeing different ads, as illustrated in Fig. 1, and age being correlated with party identification.

Experimental estimates

Estimating equation. We define Y_i as an outcome, T_i as a vector of treatment group indicators (with two elements for Facebook and one for Instagram), $\mathbf{X}_i$ as a vector of controls and $\mathbf{v}_s$ as a vector of randomization stratum indicators. As prespecified, the controls $\mathbf{X}_i$ are the variables selected in a lasso regression of Y_i on the baseline value of Y_i (if available) and a vector of demographics and baseline survey variables. Controlling for these predictors improves precision and reduces bias due to naturally occurring imbalances. The estimating equation is

$$Y_i = \tau T_i + \rho \mathbf{X}_i + \mathbf{v}_s + \varepsilon_i, \quad (1)$$

where the coefficient τ captures the average treatment effect of ad removal. Supplementary Note C provides additional detail on control variables and variable construction.

Effects on ad load. The All Ad Removal groups mechanically saw zero political ads over the study period. As described above, 62% of political ads and 90% of presidential ads in the control group would have been removed in the List-Targeted Ad Removal treatment. Some of these ads were replaced by other (non-list-targeted) political ads. The average Facebook user in the List-Targeted Ad Removal group saw 10 political ad impressions and 0.55 presidential ad impressions per week over the study period, compared with 23 political impressions and 4.7 presidential impressions in the control group.

Effects on primary outcomes. We study the effects of ads on nine prespecified primary outcomes: knowledge, affective polarization, issue polarization, perceived legitimacy, participation, engagement, Trump favourability, turnout and Trump vote. 'Knowledge' is the participant's score on factual questions related to the election, news or general facts; 'affective polarization' is the difference in attitudes towards the participant's own party versus the other party; 'issue polarization' is the extent to which the participant holds opinions closely aligned with their party; 'perceived legitimacy' is agreement with various statements implying that elections are free and fair; 'participation' is self-reported political participation, including attending protests or contributing to candidates; 'engagement' is engagement with political content on the focal platform, measured using platform data; 'Trump favourability' is the sum of Trump's approval rating and the difference between Trump's and Biden's thermometer ratings; 'turnout' is whether the participant reported voting; and 'Trump vote' is defined as 1 for participants who voted for Trump, −1 for those who voted for Biden, and 0 otherwise. See Methods and Supplementary Note C for the precise definitions of these variables.

Figure 3 shows the average treatment effects of ad removal on these outcome variables. The first seven variables are standardized into units of standard deviations within the control groups. The bottom two variables (turnout and Trump vote) are reported in their original units.

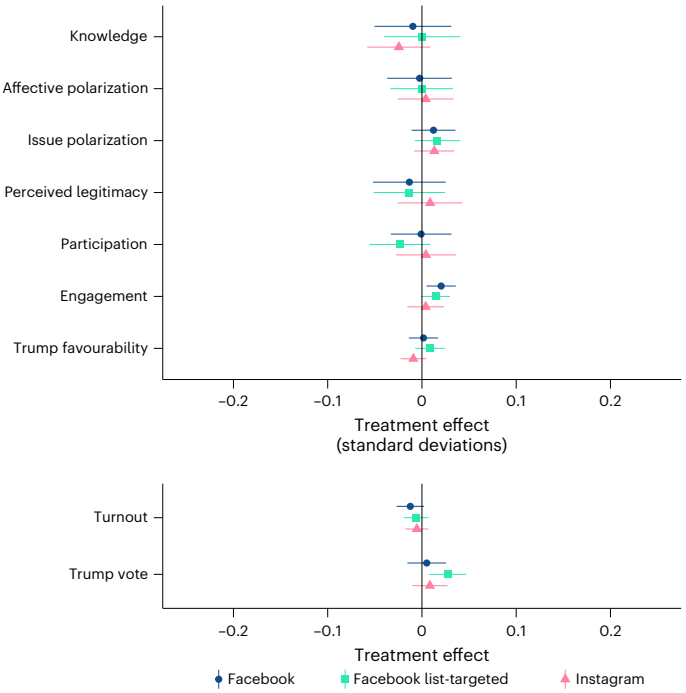

Fig. 3 | Effects of political ad removal on primary outcomes. This figure presents average treatment effects of political ad removal estimated using equation (1). Our Facebook sample consists of 36,906 participants, of whom 27,608 completed the endline survey. Our Instagram sample consists of 25,925 participants, of whom 19,483 completed the endline survey. The horizontal lines represent 95% confidence intervals. For detailed results, see Supplementary Tables 8–10.

We estimate statistically zero effects on all primary outcomes, with tight confidence intervals. None of the effects in Fig. 3 are statistically significant at the 5% level after adjusting for multiple hypothesis testing (Supplementary Tables 8–10). The 95% confidence intervals rule out effects greater than 0.043 standard deviations for knowledge, affective polarization, issue polarization, perceived legitimacy and participation. The confidence interval is even tighter for Trump favourability, ruling out effects greater than 0.024 standard deviations. The confidence intervals rule out turnout effects of greater than 0.0069 percentage points, and effects on Trump vote for the Facebook All Ad Removal and Instagram treatments of more than 0.027 percentage points.

Many of our null results are precise enough to rule out substantively meaningful effects. For comparison, college graduates in the control groups have 'knowledge' 0.67 standard deviations higher than non-college graduates, and ref. 30 estimates that affective polarization has grown by an average of 0.021 standard deviations per year since 1978. We show below that our effects on vote choice are substantially more precise than previous estimates in the literature.

The effects on two primary outcomes are significant before adjusting for multiple hypothesis testing. We still reject the null hypothesis for these two outcomes because they are not significant when adjusting for multiple hypothesis testing, but we discuss them in more detail below.

In our sample, List-Targeted Ad Removal on Facebook is associated with a Trump vote increase of 0.027 units ($P = 0.0069$, $q = 0.158$). However, several other results suggest that this is likely to be idiosyncratic rather than a reflection of a true causal effect. First, we do not find that All Ad Removal on Facebook affected Trump vote (Fig. 3 and Supplementary Table 11), and most presidential ads were list-targeted. Second, any effects of Facebook List-Targeted Ad Removal seem to be driven by users with below-median predicted ad load (Supplementary Fig. 8), whereas we might expect users with

lower ad load to have smaller treatment effects. Finally, we do not find any detectable effect of political ad removal on outcomes that one would expect would move in the same direction as vote choice: Trump favourability and the secondary outcomes pro-Republican affect, pro-Republican issue positions and Republican vote share in state-level races (Supplementary Table 12).

In our sample, All Ad Removal on Facebook is associated with a slight increase in engagement of 0.02 standard deviations ($P = 0.01$, $q = 0.158$). Supplementary Fig. 6 shows effects on individual components of the engagement index. The increase in online engagement could arise because political ad removal increases overall time spent on the platform (regardless of how people spent their time) or because political ad removal shifts online behaviour (regardless of total time spent on the platform). Supplementary Fig. 7 provides some support for both channels: removing political ads is associated with an increase in total engagement with content on the platform and is associated with a larger increase in civic content views than total content views. However, we emphasize that this analysis is exploratory and that we cannot reject the hypothesis that the removal of political ads on Facebook did not causally affect engagement.

Secondary outcomes, including administrative data. In addition to primary outcomes, we also prespecified a set of 40 secondary outcomes, including individual components of the primary outcome composite variables as well as administrative data on campaign contributions and voter turnout. While a few of the effects are individually statistically significant (as would be expected to occur by chance), none of the effects is significant after adjusting for multiple hypothesis testing (Supplementary Note G.2). For both outcomes for which we have administrative data (campaign contributions and voter turnout), the estimated effects are not statistically different from the estimates using self-reported data. In Supplementary Note G.3, we also analyse a set of prespecified auxiliary outcomes.

Given the large share of ads focused on donations, effects on campaign contributions are of particular interest. We find no statistically significant effect of ad removal on contributions, with 95% confidence intervals −7.7 to 2.3 and −2.5 to 0.78 in units of US dollars in the survey and administrative data, respectively. For example, we can rule out that seeing political ads over the study period increased contributions by more than US$2.5 per person in the administrative data. In a non-preregistered analysis in the Facebook sample, we also estimate effects on the probability of making campaign contributions. Compared with the continuous campaign contribution variable, this binary variable better captures possible treatment effects on small donations that might be induced by social media ads. We find no statistically significant effect of ad removal on the binary outcome, with 95% confidence intervals of −0.016 to 0.004 and −0.0049 to 0.0022 in the survey and administrative data, respectively. Thus, we can rule out that seeing political ads over the study period increased the probability of making a contribution by more than 0.49 percentage points in the administrative data. These insignificant effects may suggest that few participants made contributions via donation ads, that those who did would have contributed through other channels in the absence of the ads, and/or that the effects on contributions were non-zero but smaller than we can detect even in our large sample. As we show below, however, our confidence intervals include contribution effects that would be consistent with a positive return to ad spending.

Subgroup analysis. As prespecified, we estimate heterogeneous effects using four primary moderators: (1) political party, (2) an indicator for undecided voters, (3) an indicator for voters who identify as Black or Hispanic, and (4) above-median predicted political ad load. The last of these is designed to focus on users who would have seen particularly high volumes of political ads absent our treatment and who would therefore be most likely to exhibit large treatment effects. We define predicted political ad load based on data from our control group, where we run a lasso regression of ad load in our study period on a set of predictors including baseline political ad load.

Supplementary Note H presents the subgroup analyses. Even among those above the median or 75th percentile of predicted political ad load, we do not find that the treatments had a significant effect on any primary outcome other than engagement. There is no statistically significant heterogeneity on any primary outcome by the other three primary moderators.

We also analyse heterogeneous effects by the predicted number of pro-Democratic ads (this moderator was not prespecified). We test whether participants who would have seen more Democratic ads and were not exposed to these ads because of the treatment, were less likely to vote or support Trump, compared with participants who would have seen more Republican ads. We do not find evidence for such an effect.

Advertising effectiveness. To provide more context on the precision of the null effects on vote choice, turnout and contributions, we compute the implied returns to advertising spending by Democratic and Republican presidential advertisers. Our experiment does not directly identify the returns for a single campaign or party because our treatments removed all ads simultaneously. However, Fig. 1 shows that people mostly saw ads from their own party. This means that the effect of political ad removal on Democrats was similar to the effect of removing only Democratic ads, and the effect on Republicans was similar to the effect of removing only Republican ads.

To account for the fact that targeting to own-partisans was not perfect, we assume that ads from a person's own party and ads from the opposing party have equal and opposite effects. (If we instead assumed that the effect of own-party ads was greater than opposite-party ads, our estimates of returns to political ad spending would be closer to zero.) We also conservatively assume that non-presidential ads have no effect on our outcomes. We define τ to be the treatment effect of Facebook All Ad Removal among users from a particular party, Δ to be the incremental pro-party presidential ad impressions (that is, pro-party impressions minus opposite party impressions) seen by users from that party in the control group, and p to be the ad cost per impression. The treatment effect of ad removal per dollar of incremental pro-party ad spending is then $\tau/(p\Delta)$. We assume that p is equal to US$25.10 per thousand impressions, the average costs of the ads in our WMP sample, calculated using the cost data provided in the Meta Ad Library (to calculate costs, we impute values at the midpoints of ranges provided by the Ad Library).

Table 1 presents the results. The panel 'Control group ad impressions' calculates Δ, the control group incremental pro-party ad impressions, separately using the OS and WMP ad definitions. The first and second columns, respectively, show results for Democratic and Republican users (including independents who 'lean' towards the respective party). For example, using the WMP presidential ad sample, the average Democratic user saw 74 more pro-Democrat presidential ads than pro-Republican presidential ads over the study period. The third column pools Democrats and Republicans to calculate incremental pro-party ad impressions across both parties.

'Cost-effectiveness of ads' in Table 1 computes the cost-effectiveness point estimates and confidence intervals for net presidential votes, turnout and contributions. Turnout and contributions both use administrative data instead of self-reports. All outcomes are signed so positive effects benefit the party corresponding to the respective column. For example, the ads shown on Facebook increased Democratic users' net votes for Biden (that is, decreased Trump vote) by a point estimate of 0.023. Dividing that effect by the Δp = US$1.86 of additional pro-Democrat presidential ad spending (using the WMP coding) seen by the control group implies that each pro-Democratic incremental ad spending increased the Democratic vote by a point estimate of 0.012 votes per US dollar.

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Table 1 | Electoral effects by party (Facebook)

	Control group ad impressions		
	(1)	(2)	(3)
	Democrats	Republicans	Pooled
Share of users over study period	0.60	0.40	1.00
Political ad impressions per user over study period	139	133	137
Using OS definition of presidential ads			
Share coded presidential	0.182	0.243	0.206
Share of presidential coded pro-party	0.927	0.832	0.889
Incremental pro-party presidential impressions	22	21	22
Incremental pro-party presidential spending	US$0.542	US$0.538	US$0.540
Using WMP definition of presidential ads			
Share coded presidential	0.630	0.602	0.619
Share of presidential coded pro-party	0.917	0.657	0.814
Incremental pro-party presidential impressions	74	27	55
Incremental pro-party presidential spending	US$1.863	US$0.674	US$1.391
	Cost-effectiveness of ads		
	(1)	(2)	(3)
	Democrats	Republicans	Pooled
	Point estimate	Point estimate	Point estimate
Net President votes per person seeing all ads versus no ads	0.023	0.013	0.020
	−0.004 to 0.049	−0.021 to 0.046	−0.002 to 0.042
Per US$ incremental ad spending (OS)	0.041	0.024	0.037
	−0.006 to 0.089	−0.040 to 0.088	−0.003 to 0.077
Per US$ incremental ad spending (WMP)	0.012	0.019	0.014
	−0.002 to 0.026	−0.031 to 0.069	−0.001 to 0.030
Turnout (percentage points) per person seeing all ads versus no ads	−0.016	0.002	−0.009
	−0.044 to 0.012	−0.033 to 0.038	−0.032 to 0.013
Per US$ incremental ad spending (OS)	−0.029	0.004	−0.017
	−0.079 to 0.021	−0.063 to 0.072	−0.058 to 0.024
Per US$ incremental ad spending (WMP)	−0.009	0.003	−0.007
	[−0.023, 0.006]	[−0.049, 0.056]	[−0.023, 0.009]
Contributions (US$) per person seeing all ads versus no ads	3.000	−0.640	0.956
	−0.200 to 6.199	−3.298 to 2.018	−1.159 to 3.072
Per US$ incremental ad spending (OS)	5.432	−1.215	1.763
	−0.361 to 11.225	−6.257 to 3.828	−2.138 to 5.665
Per US$ incremental ad spending (WMP)	1.610	−0.950	0.687
	−0.107 to 3.327	−4.891 to 2.992	−0.833 to 2.208

This table computes estimates of returns to political ad spending on Facebook, for outcomes measured in administrative data. The first and second columns present separate results for Democratic and Republican users, including independents who 'lean' towards the respective party. The third column presents estimates for the pooled sample. 'Control group ad impressions' presents data on control group political ad impressions. 'Cost-effectiveness of ads' presents point estimates and confidence intervals for the effects of presidential ad spending on net presidential votes, validated turnout and validated campaign contributions. 'Net President votes' refers to the candidate of the respective party in each column (net Democratic votes in the first column and net Republican votes in the second column). The treatment effect per US dollar of incremental ad spending is $\tau/(p\Delta)$, where τ is the treatment effect of ad removal on an outcome, Δ is the treatment effect on net pro-party presidential ad impressions, and the ad cost p is equal to US$25.10 per thousand impressions.

Using the broader WMP presidential ad coding, and focusing on the pooled estimates, the 95% confidence intervals (based on the coefficients and confidence intervals reported in the table) reject that US$1,000 of incremental pro-party spending yielded more than 30 net votes, 8.7 additional voters and US$2,208 of additional contributions. As a point of comparison for the net vote effects, Spenkuch and Toniatti[28] estimate a cost per vote of US$170 for television ads—that is, a return of six votes per US$1,000. Thus, we cannot rule out reasonably positive returns to advertising. Supplementary Note G.4 presents additional tables analysing electoral effects, including focusing only on participants with above-median predicted ad load and analysing self-reported outcomes instead of validated outcomes. The conclusions are similar: none of the results is statistically significant, but we cannot rule out positive returns for advertising.

Discussion

This article provides experimental and descriptive evidence on the reach, targeting, goals and impacts of political advertising on Facebook and Instagram in the 6 weeks before the US 2020 presidential election.

Our descriptive results show that political advertising on Facebook and Instagram is not primarily used to target undecided voters with persuasive messaging. Instead, most social media ads seek campaign contributions and voter information, not mobilization or persuasion, and the overwhelming majority of ads are seen by a party's own

Table 2 | Comparison with the literature, votes per 1,000 impressions

Paper	(1)	(2)	(3)	(4)	(5)	(6)
	Setting	Treatment	Measurement level	*N*	Votes per 1,000 impressions	Standard error
This article	2020 US presidential election, pooled (WMP)	Remove all political ads on FB	Individual	24,546	0.351	0.201
This article	2020 US presidential election, pooled (OS)	Remove all political ads on FB	Individual	24,546	0.929	0.502
Broockman and Green (2014)	2012 non-battleground state legislative election.	Pro-candidate FB ads	Individual	2,984	0.889	0.778
Coppock et al. (2022)	2018 Florida US House election	Pro-Democrat ads on FB and IG	Precinct	853	−0.375	7.96
Enriquez et al. (2024)	2018 Mexican municipal election	Incumbent performance FB ads	Precinct	13,254	23.087	14.207
Hager (2019)	2016 Berlin state election	Pro-CDU FB + Google ads	Postal districts	189	0.918	0.656
Turitto et al. (2014)	2014 Texas Lieutenant Governor Republican primary	Pro-candidate digital ad campaign	Individual	5,842	0.701	1.283

This table compares point estimates and standard errors of the effect of impressions on net votes. The first two rows are based on this article. They show estimates for pooled Democrats' and Republicans' incremental pro-party impressions, where the party leaning is coded using either WMP or OS. These estimates can be derived directly from the table by dividing the effect of seeing all ads by the number of incremental pro-party impressions and rescaling by 1,000. For example, the number of additional votes per 1,000 impressions based on WMP data is (0.02/55)×1,000=0.4. The rest of the rows in the table provide benchmarks from other studies. Supplementary Note G.5 details how these estimates were computed. Measurement level is the level at which the study observes outcomes. *N* is the number of units for which the outcome is observed. FB, Facebook; IG, Instagram.

supporters. These patterns hold both for all presidential ads and for list-targeted presidential ads.

Our experimental results show that removing Facebook and Instagram political advertising had no detectable effects on outcomes including candidate favourability, turnout, participation (including contributions), political knowledge, polarization and perceived legitimacy of the election. This is true for both all political ads and the subset of list-targeted ads. This is true even though political ad exposure was substantial—23 ads per week for the average Facebook user over the study period and 175 ads per week at the 99th percentile. In comparison, ref. 28 reports that the average American was exposed to 8.4 political ads per week on television in the 60 days before the 2004, 2008 and 2012 elections. Our confidence intervals rule out even small-to-moderate effect sizes. Our null effects are consistent with the fact that relatively few ads were aimed at persuasion and mobilization. They stand in contrast to arguments in both the popular press[2–7,9] and academic literature[12,29,31–37] that digital ads are likely to be key drivers of political attitudes and election outcomes.

These results echo some prior experimental literature that has failed to find significant effects of online political ads[11,29,38]. However, the large scale of our experiment means that the precision of our estimates is greater than that in most prior work. We provide large-scale evidence of the combined impact of social media advertising on outcomes such as turnout, polarization and perceived legitimacy of the election. For the effect on vote choice—arguably the parameter that has received the most emphasis in the literature—Table 2 shows that our estimates of the number of votes per 1,000 impressions are more precise than those in prior studies. Supplementary Table 19 presents a similar comparison focusing on the cost-effectiveness (votes per US dollar) of ads. Supplementary Note G.5 provides more details on the calculations behind these tables.

Our experiment advances the literature in several ways beyond sample size and precision. First, we combine both surveys and linked administrative data from a social media platform in an experimental study of political advertising. This allows us to provide a much broader and more precise picture of the mix of advertising to which participants were exposed than would be possible with surveys alone. Second, we are able to link our treatments to directly measured turnout and contribution outcomes, limiting concerns about bias in survey self-reports. Third, previous experiments have not studied political advertising on Instagram, one of the most popular social media platforms. Finally, our interventions were relatively light-touch. Participants were aware that their feeds may have been modified in some way, but they did not know that this would concern ads in particular, and they were not made directly aware of their own treatment status. This may limit concerns about experimenter demand effects.

At least in the 2020 US presidential elections context, our results provide evidence against a number of specific hypotheses that have been prominent in the literature: for example, that political ads are demobilizing and would tend to reduce participation and turnout[39–41]; that political ads can be polarizing and that this is likely to be especially true for digital ads[31,32]; that 2020 political ads and political microtargeting were effective in undermining confidence in the electoral system[33,34]; and that 'microtargeted' (or, in our case, list-targeted) ads are particularly effective in mobilizing supporters[12,29,35–37].

While we estimate precise null effects on many outcomes, our confidence intervals are nevertheless consistent with a positive return to advertising spending. At the upper end of our confidence intervals, even the effect on donations alone would exceed what advertisers paid for their ads. Taken together, our results thus suggest that the impact of Facebook and Instagram advertising on aggregate political outcomes was minimal at best, but that even very small effects could have been enough to make the advertising worthwhile for campaigns, given its low cost.

We end by reiterating that our results are subject to important limitations. The estimates are directly informative only about the sample that selected into our study. They are specific to the last 6 weeks of a highly contested US presidential election, a situation where advertising might have more limited effects compared with ads placed earlier or in elections where candidate evaluations are less solidified[29]. Many of our outcomes can be measured only in surveys, and we cannot rule out that they are affected by measurement error and/or experimenter demand effects. Further improving on these dimensions remains an important goal for future research.

Methods

Ethics

The US 2020 Facebook and Instagram Election Study, which includes this research, was evaluated and approved by the National Opinion Research Center (NORC) Institutional Review Board (protocol number 20.08.10, project number 8870). Academic researchers coordinated

with their specific university institutional review boards to ensure they followed regulations concerning human participant research when analysing data collected by NORC and Meta, as well as when authoring papers based on the results. In addition, the research group was provided ethical counsel by the independent company Ethical Resolve to inform the study designs.

All participants in the study provided informed consent and allowed access to data on their activity on the focal platform. Participants were informed that the experiment could involve changing aspects of their on-platform experiences, but they were not informed of the specific treatments for which they would be eligible or that the study was focused on political ads (several companion papers[42–45] study treatments unrelated to political advertising).

As a mitigation strategy to minimize unanticipated negative effects, we defined a preregistered stopping rule, inspired by clinical trials, which would have ended a treatment if we detected that it was generating changes in specific variables relevant to individual welfare that were much larger than expected. The stopping rule conditions were not met, and we did not have to stop the treatment. For more information on ethical considerations, see Supplementary Note J.

Experimental design

This section provides a high-level overview of the experimental design. Extended Data Figs. 1 and 2 present Consolidated Standards of Reporting Trials (CONSORT) diagrams summarizing the design. Additional details are provided in Supplementary Note A.

We ran two parallel experiments, with Facebook and Instagram as the respective 'focal platform'. For each focal platform, Meta drew a stratified random sample of users who lived in the USA, were at least 18 years old and had logged into their account at least once in the past month. Meta placed survey invitations in these users' focal platform feeds from 31 August to 12 September 2020. Participants completed a series of surveys implemented by NORC at the University of Chicago, including baseline (responses collected 8–21 September) and endline (4–18 November) surveys. The questionnaires are included in Supplementary Note M. Participants received base payments of US$5 for completing the baseline survey and US$20 for completing the endline survey; other payments are described in Supplementary Note A.

Just after the baseline survey, participants were randomized to a control group or one of several treatment groups that varied the content they saw on their focal platform. Our article studies the two treatment conditions that removed political ads: All Ad Removal and List-Targeted Ad Removal. We define 'political ads' to be all ads that Meta's classifiers determine to be about social issues, elections or politics (details at https://www.facebook.com/business/help/167836590566506?id=288762101909005). In the All Ad Removal condition, Meta removed all political ads and replaced them with non-political ads. In the List-Targeted Ad Removal condition, Meta removed the subset of political ads that were targeted on the basis of user data provided by the advertiser and replaced them with other political or non-political ads. More specifically, the List-Targeted Ad Removal condition removed and replaced ads targeted using any of the following three strategies: customer list custom audiences (described at https://www.facebook.com/business/help/170456843145568?id=2469097953376494), which are lists of people uploaded by the advertiser, website custom audiences (described at https://www.facebook.com/business/help/170456843145568?id=2469097953376494), which are sets of people who have visited the advertiser's website, and lookalike audiences (described at https://www.facebook.com/business/help/610516375684216?id=2469097953376494), which are users that Facebook's internal algorithm estimates to be similar to users in a set of users defined by the advertiser.

In both the All Ad Removal and List-Targeted Ad Removal conditions, users' total ad load was unchanged and the ads were replaced with the next non-removed ad assigned to the user based on the platform's algorithm, regardless of the ad's content. These two separate treatment conditions allow us to speak directly to concerns described in the introduction about political ads in general and targeted ads in particular.

In the Facebook experiment, we implemented both treatment conditions. In the Instagram experiment, to conserve on the total number of treatment arms, we implemented only All Ad Removal. In both experiments, the ads were removed starting on 24 September. We had originally planned to continue the experiment into December, but Meta made an unexpected decision[46] to stop serving political ads starting on Election Day (4 November). We refer to 24 September through 3 November as the study period. Data collection and analysis were not performed blind to the conditions of the experiments.

Balance and attrition

As described in Supplementary Note A.3, we stratified randomization on self-reported party identification, race/ethnicity, friend/following count and an indicator for residence in a swing state. Chance imbalance on some other baseline covariates occurred in our Facebook treatment groups (Supplementary Table 4). Other treatment conditions from the same randomization that we study in parallel papers were balanced on observables, making it unlikely that this reflects an error in randomization. Reference 47 finds that, after controlling for observables that predict the outcome (which we do), correctly implemented randomizations with chance imbalance are no more likely to generate false hypothesis rejections than those without chance imbalance. The Instagram treatment groups were balanced on all covariates at randomization. More than 70% of the primary analysis sample completed the endline survey. Supplementary Tables 5 and 6 show that our samples remain similarly balanced on covariates after attrition and that attrition rates did not differ significantly across control and treatment groups.

Participants and sample size

On Facebook, 14,643,120 users were invited to the study. Among these, 988,247 clicked the invitation, 193,880 consented to participate and 75,276 completed the baseline survey. A total of 36,906 were randomized to one of the two ad removal conditions or to the control group and did not delete their account or request data deletion. This final group is our 'primary analysis sample'. On Instagram, the analogous numbers are 4,618,628 invites, 531,164 clicks, 135,688 consents, 47,659 baseline completes and 25,925 participants in the primary analysis sample. We excluded users who deleted their accounts or requested data deletion from the analysis, both for ethical reasons and because their data were no longer accessible.

We determined the sample size to provide statistical power to detect small effects (with $\beta = 0.8$ and $\alpha = 0.05$). For example, our power calculations were designed to detect an effect of 1.5 percentage points on self-reported turnout in the Facebook sample and 1.4 percentage points in the Instagram sample.

Of the participants in our Facebook sample, 74% are white, 41% are male, 52% hold a college degree, 55% identify as Democrats or lean Democrat and 33% identify as Republicans or lean Republican. In our Instagram sample, 61% are white, 38% are male, 58% hold a college degree, 72% identify as Democrats or lean Democrat and 20% identify as Republicans or lean Republican.

For all analyses, including both descriptive findings and treatment effect estimation, we weight our samples to be representative of US focal platform (Facebook or Instagram) users on race/ethnicity, political party, education, baseline account activity and (on Instagram only) whether the respondent was in a swing state. See Supplementary Note A.5 for additional details on weighting.

Primary outcomes

We have nine prespecified primary outcome variables. All primary outcomes other than engagement are based on survey responses. Supplementary Note C provides precise definitions of these primary

outcomes as well as of our 40 secondary outcomes. Some of these outcomes are also defined in other papers in the US 2020 Facebook and Instagram Election Study[45].

The variable 'knowledge' is the average of standardized scores on three sets of factual questions: (1) election knowledge (knowledge of candidates' policy positions), (2) news knowledge (distinguishing recent news events from plausible placebo events that had not happened) and (3) fact knowledge (distinguishing true statements from misinformation that was circulating about topics such as coronavirus disease 2019 and fraudulent ballots).

'Affective polarization' is the average of three underlying standardized variables: (1) political supporters polarization (the difference in participants' favourability towards people who support their own party versus the other party), (2) political candidates polarization (the difference in participants' favourability towards their own party candidates running for office versus candidates in the other party) and (3) party smartness polarization (the difference in participants' perceived smartness of people in their own party versus people in the other party).

'Issue polarization' is an index of participants' opinions on eight political issues (immigration, repeal of Obamacare, unemployment benefits, mask requirements, foreign policy, policing, racial justice and gender relations). The signs of the variables are adjusted so that, for each issue, a higher value is closer to the participant's own-party mean. A higher issue polarization value should be interpreted as having opinions more closely aligned with one's party.

'Perceived legitimacy' is an index of participants' agreement with the following six statements: (1) elections are free from foreign influence, (2) all adult citizens have equal opportunity to vote, (3) elections are conducted without fraud, (4) government does not interfere with journalists, (5) government protects individuals' right to engage in unpopular speech and (6) voters are knowledgeable about candidates and issues.

'Participation' is the sum of indicators for whether a participant reported doing the following six activities: (1) attended a protest or rally, (2) contributed money to a political candidate or organization, (3) signed an online petition, (4) tried to convince someone how to vote (online or in-person), (5) wrote and posted political messages online and (6) talked about politics with someone they know.

'Engagement' is the average of 15 standardized components measuring engagement with political content on the focal platform, in the following seven groups: (1) likes, comments, clicks, reshares and reactions on content classified as 'civic' (that is, related to politics and social issues) by Meta's Civic classifier, described in Supplementary Note B, (2) views and clicks on Meta's Voter Hub (https://about.fb.com/news/2020/06/voting-information-center/; a tool that provided election-related information to voters), (3) likes, reactions, comments and reshares on content from politicians running for office, and on Facebook only, (4) indication of interest in going to civic events, (5) clicking on a petition, (6) donations to civic causes and (7) enabling a constituent badge. None of the 'engagement' components include engagement with political ads, so political ad removal has no mechanical effect on these outcomes. We note that we had to modify the construction of this variable compared with the pre-analysis plan because some data were not available and because some engagement indicators were always zero in our sample (for details, see Supplementary Note K).

The Trump favourability variable is the sum of two standardized components: (1) Trump approval ratings and (2) the difference between Trump and Biden feeling thermometer ratings.

Turnout is an indicator of whether the participant reported voting.

Trump vote is defined as 1 for people who reported voting for Trump, −1 for people who reported voting for Biden and 0 for those who did not vote or voted for some other candidate.

Voting records and campaign contribution data

We matched participants to state voting records and public records of campaign donations. Supplementary Notes A.6 and A.7 provide details on this matching. These directly measured outcomes are an important supplement to our survey-based outcome variables, as they have different measurement errors (from matching errors instead of self-reporting errors) and are available for matched users regardless of whether they completed the endline survey.

Advertising data

We record all political ads seen by our participants during the study period and match them to metadata from the Meta Ad Library (https://www.facebook.com/ads/library/?active_status=active&ad_type=all&country=ES&is_targeted_country=false&media_type=all) and to internal data on the type of targeting associated with each ad. We match a subset of these ads to additional metadata from two sources: OS and the WMP (https://mediaproject.wesleyan.edu/)[48].

OS provides data on all ads in the Meta Ad Library whose sponsoring page and/or disclaimer are known organizations that provide financial support to presidential campaigns, including Political Action Committees and political parties. This provides a relatively narrow definition of presidential ads based on financial ties. Unless stated otherwise, we use the OS definition of 'presidential ads'.

WMP provides data on all ads in the Meta Ad Library whose sponsoring pages ran at least one ad containing keywords related to the 2020 presidential campaigns. They predict whether each ad favoured Biden or Trump using the ad's text and a machine learning model trained on a sample of ads whose sponsor can be associated with one campaign or the other (as in the OS data). This provides a broader definition of presidential ads based on the ads' content. WMP also trains a separate model to predict the primary 'goal' of the ad from its text, based on a hand-labelled training sample with five categories: (1) donation, (2) purchase/merchandise, (3) prompting the user to sign up or provide information, (4) persuasion and (5) other.

We also use internal Meta data for the full set of political ads in the Meta Ad Library, including the distribution of exposure to ads by user characteristics and the targeting strategies associated with the ads. Supplementary Note D provides more details on the advertising data sources and how they were matched with Meta data.

Pre-analysis plan

The pre-analysis plan (https://osf.io/8m36y) was registered on 23 September 2020 and updated on 8 May 2023, the day before endline data collection began. It specified our sample, how weights would be created, the handling of missing data, the outcomes studied, our main specification and subgroup analyses. The plan also specified that we would use Benjamini–Hochberg-sharpened false discovery rate-adjusted q values[49] to control for multiple hypothesis testing, with $q < 0.05$. We did not substantively deviate from the pre-analysis plan. Supplementary Note K describes clarifications and minor modifications that are mainly driven by changes in data availability. These changes include focusing on ad exposure in Facebook due to missing Instagram data, a change in the construction of several variables (time use, income, urban–rural, engagement and knowledge) due to data availability and minor changes in the endline survey, not analysing secondary and auxiliary variables for which data do not exist (additional polarization variables, civic events in the feed, and distribution of ads by race/ethnicity) and fixing mistakes in the figure shells.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

De-identified data from this study will be stored in the Social Media Archive (SOMAR) housed by ICPSR. This will be accessible for university research approved by the institutional review board related to elections, or for the purpose of validating the results of this study.

ICPSR will handle and vet all applications requesting access to this data. Data access is controlled to protect the privacy of the study participants and to be consistent with the consent form signed by study participants. Requests for data can be made via SOMAR (https://socialmediaarchive.org/). The data for this article are based on the following datasets stored in SOMAR: Facebook Political Advertising Experiment Participants, https://doi.org/10.3886/62dk-b934; Instagram Political Advertising Experiment Participants, https://doi.org/10.3886/qq0b-pk42; Facebook Political Ads Reach and Impressions by Age Standard Deviation, https://doi.org/10.3886/8ffj-k214; Facebook Political Ads Reach and Impressions by Proportion of Males, https://doi.org/10.3886/23kb-eq21; Comparison Statistics About Monthly Active Users, https://doi.org/10.3886/r0sb-pb90.

Code availability

Code from this study will be stored in the ICPSR Virtual Data Enclave (VDE) and will be available after users apply for access to the US 2020 Facebook and Instagram Election Study Virtual Data Enclave at https://icpsr.atlassian.net/servicedesk/customer/portal/53. The data in this study were analysed using R (version 4.2.3), which was executed via R notebooks (2.25) on JupyterLab (3.6.3). The analysis code imports several R packages available on CRAN, including dplyr (1.1.4), ggplot2 (3.4.4), xtable (1.8-4), glmnet (4.1-8), estimatr (1.0.0) and BayesFactor (0.9.12-4.7).

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This study was conducted as part of the US 2020 Facebook and Instagram Election Study, a collaboration between a team of researchers at Meta and an independent set of external academic researchers. The team designed the project together (with lead academic authors having final say on the preregistered analysis plan). Data collection was carried out by Meta and NORC. The pipeline code used to preprocess raw platform data was conducted by Meta and reviewed and approved by the external researchers; both the Meta team and external researchers analysed the data. The article was written by the external researchers with feedback from the Meta team, but the lead academic authors had final control rights over text and publication. More details on the collaboration appear in the 'Competing interests' section and in Supplementary Note I. A full description of the roles and responsibilities of the independent academic research team, the Meta researchers and NORC can be found via the Open Science Foundation at https://osf.io/7wpgd. The Facebook Open Research and Transparency (FORT) team provided substantial support in executing the overall project. We are grateful for support on various aspects of project management from C. Nayak, S. Zahedi, I. Rosenn, L. Ahmad, A. Bhalla, C. Chan, A. Gruen, B. Hillenbrand, D. Li, P. McLeod and D. Rice; engineering from Y. Chen, S. Chen, T. Lohman, R. Pyke, and Y. Wan; data engineering from S. Chintha, J. Cronin, D. Desai, Y. Kiraly, T. Li, X. Liu, S. Pellakuru, C. Xie and B. Xiong; data science and research from H. Connolly-Sporing; academic partnerships from R. Mersey, M. Zoorob, L. Harrison, S. Aisiks, Y. Rubinstein and C. Qiao; privacy and legal assessment from K. Benzina, F. Fatigato, J. Hassett, S. Iyengar, P. Mohassel, A. Muzaffar, A. Raghunathan and A. Sun; and content design from C. Bernard, J. Breneman, D. Leto and S. Raj. NORC at the University of Chicago partnered with Meta on this project to conduct the fieldwork with the survey participants. We are particularly grateful for the partnership of NORC principal investigator J. M. Dennis and NORC project director M. Montgomery. The costs associated with the research (such as participant fees, recruitment and data collection) were paid by Meta, who collaborated with academics in this project as part of the US 2020 Facebook and Instagram Election Study (for more details, see Supplementary Note I). Ancillary support (for example, research assistants and course buyouts), as applicable, was sourced by academics from (authors' initials in parenthesis): the Democracy Fund (N.J.S.), the European Research Council Starting Grant (EXPO-756301) (M.W.), the Guggenheim Foundation (B.N.), the Hewlett Foundation (N.J.S. and J.A.T.), the Hopewell Fund (J.A.T.), the John S. and James L. Knight Foundation (D.F., D.L., N.J.S., J.A.T. and R.T.), the Charles Koch Foundation (J.A.T.), New York University (J.A.T.), the Alfred P. Sloan Foundation (M.G.), Stanford University (H.A., M.G., N.M. and J.P.), the Stanford Institute for Economic Policy Research (M.G.), the University of Texas at Austin (N.J.S.) and the University of Wisconsin-Madison (Y.M.K.). These funders had no role in the study design, data collection and analysis, decision to publish or preparation of the manuscript.

Author contributions

H.A., A.C.-T., N.D., M.G., R.L. and W.M. designed the study, supervised all analyses, analysed data and wrote the paper. As the academic lead authors, H.A., M.G. and R.L. had final control rights. A.C.-T., N.D., W.M. and D.M. were the lead authors at Meta. P.B., D.D., D.F., Y.M.K., N.M., D.M., B.N., E.T., R.T., C.V.R. and M.W. contributed study materials (for example, survey questionnaires, classifiers and software). P.B., T.B., A.F., D.F., S.G.-B., A.M.G., C.K.d.J., Y.M.K., D.L., N.M., D.M., B.N., J.P., C.V.R., J.S., N.J.S., E.T., R.T., J.A.T. and M.W. also contributed to the design of the project. P.B., A.C.-T., N.D., A.F., W.M. and C.V.R. coordinated the implementation of the experimental intervention and collected and curated all platform data. J.C.C., S.N.-D., A.C.P.d.Q., B.W., S.Y. and S.Z. contributed to the figures, tables and analysis. P.B., A.M.G., C.K.d.J., D.L., N.M., B.N., N.J.S., E.T. and J.A.T. provided feedback on the paper. N.J.S. and J.A.T. were joint principal investigators for the academic involvement on the paper. A.F., C.K.d.J. and W.M. led Meta's involvement on this project and were responsible for management and coordination.

Competing interests

The costs associated with the research (such as participant fees, recruitment and data collection) were paid by Meta and some authors are employed by Meta. To ensure transparency and integrity in the research process, we adopted the following conventions. First, none of the academic researchers nor their institutions received financial or any other compensation from Meta for their participation in the project. Second, all of the papers resulting from the US 2020 Facebook and Instagram Election Study, including this one, were preregistered at the Open Science Foundation. Third, for every paper, a set of core authors with control rights over the final content of the paper were

specified. The core authors with control rights for this paper are H.A., M.G. and R.L. Fourth, Meta publicly agreed that there would be no prepublication approval of papers for publication on the basis of their findings. Finally, we appointed a rapporteur for the project—Michael Wagner of the University of Wisconsin, Madison—who was neither a paid employee of Meta nor a member of the independent academic research team. For more information, see Supplementary Note I. The following authors are employed by Meta: P.B., T.B., A.C.-T., D.D., N.D., A.F., C.K.d.J., W.M., D.M. and C.V.R. Below we list additional declarations from the academic author team: owns Meta Stock (J.S.), conducted paid consulting work for Meta (N.J.S.), received direct research funding from Meta (A.M.G., B.N., J.P., J.S., N.J.S., R.T., J.A.T. and M.W.), received an honorarium/fee from Meta for attending and/or hosting an event/serving as outside expert (M.G., J.P. and J.A.T.), attended a Meta event where food, travel or lodging was paid for by the company (D.F., M.G., S.G.-B., A.M.G., Y.M.K., D.L., N.M., B.N., J.P., J.S., N.J.S., E.T., R.T., J.A.T. and M.W.), owns individual stocks at a related company (J.S.), received direct research funding from a related company (N.J.S. and R.T.), received an honorarium/fee from a related company for attending and/or hosting an event/serving as outside expert (M.G.), attended an event at a related company where food, travel or lodging was paid for by the company (M.G., D.L., N.M., B.N., N.J.S., J.A.T. and M.W.). The other authors declare no competing interests.

Additional information

Extended data is available for this paper at https://doi.org/10.1038/s41562-025-02328-w.

Supplementary information The online version contains supplementary material available at https://doi.org/10.1038/s41562-025-02328-w.

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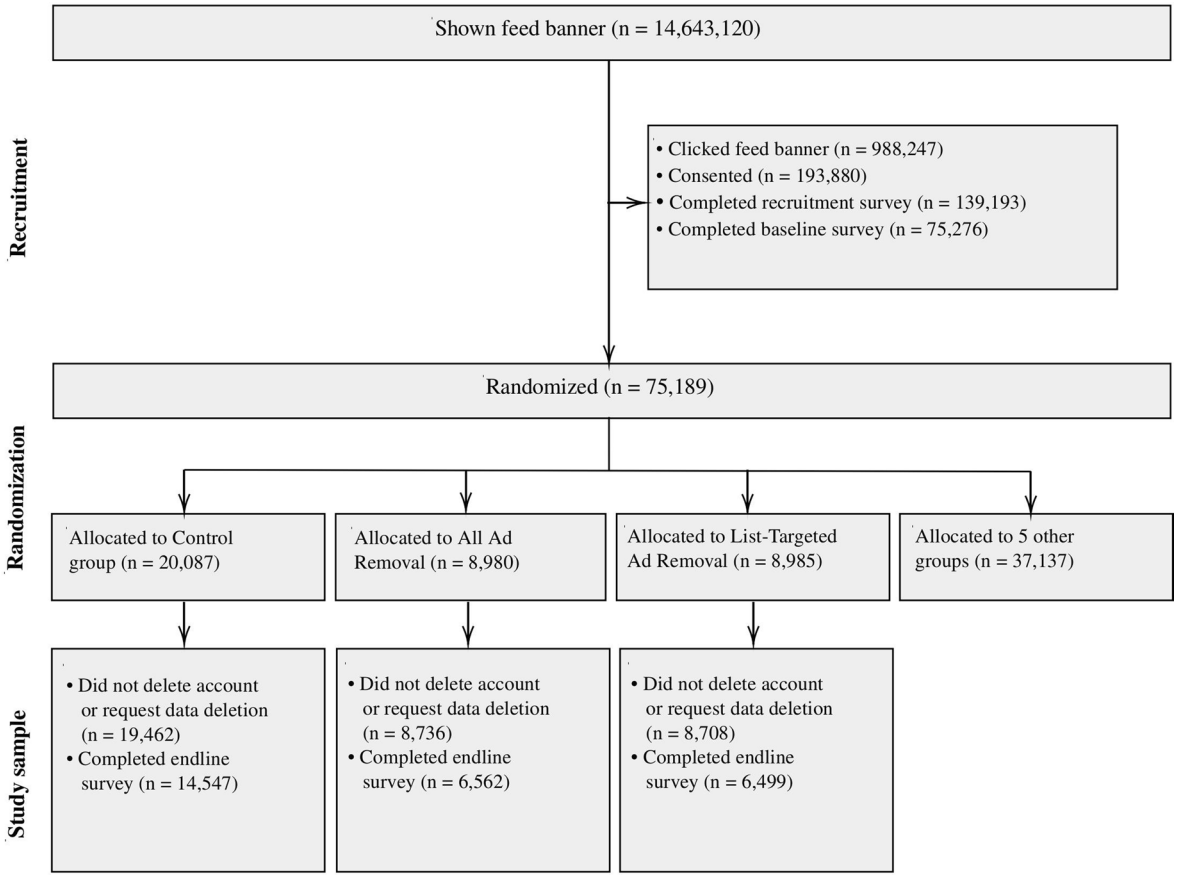

Extended Data Fig. 1 | CONSORT Flow Diagram Facebook. This figure presents a flow diagram of recruitment, randomization, and sample statistics for Facebook study participants.

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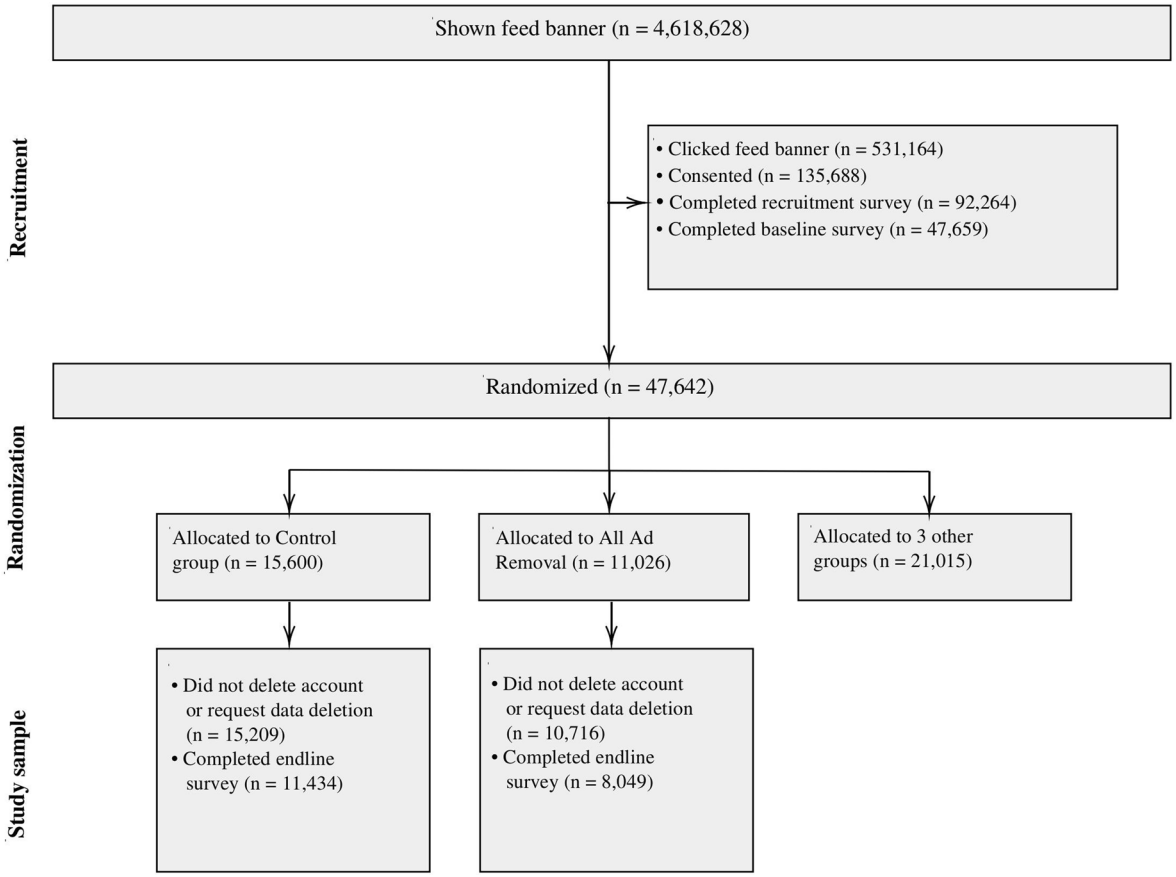

Extended Data Fig. 2 | CONSORT Flow Diagram Instagram. This figure presents a flow diagram of recruitment, randomization, and sample statistics for Instagram study participants.

nature portfolio

Corresponding author(s): Matthew Gentzkow

Last updated by author(s): Feb 25, 2025

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

Software and code

Policy information about availability of computer code

Data collection	*Provide a description of all commercial, open source and custom code used to collect the data in this study, specifying the version used OR state that no software was used.*
Data analysis	The data in this study was analyzed using R (version 4.2.3), which was executed via R notebooks (2.25) on JupyterLab (3.6.3). The analysis code imports several R packages available on CRAN, including dplyr (1.1.4), ggplot2 (3.4.4), xtable (1.8-4), glmnet (4.1-8), estimatr (1.0.0), and BayesFactor (0.9.12-4.7).

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

- Accession codes, unique identifiers, or web links for publicly available datasets
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De-identified data from this study will be stored in the Social Media Archive (SOMAR) housed by ICPSR. This will be accessible for university research approved by

the Institutional Review Board (IRB) related to elections, or for the purpose of validating the results of this study. ICPSR will handle and vet all applications requesting access to this data. Data access is controlled to protect the privacy of the study participants and to be consistent with the consent form signed by study participants. Requests for data can be made via SOMAR (https://socialmediaarchive.org/).

Research involving human participants, their data, or biological material

Policy information about studies with human participants or human data. See also policy information about sex, gender (identity/presentation), and sexual orientation and race, ethnicity and racism.

Reporting on sex and gender	For our main findings, gender is included as a candidate covariate (included covariates were selected via lasso). Several subgroup analyses in the SI employ gender (see Appendix H, Figure S17), which is measured via survey self-report (male, female, and other). Informed consent was provided prior to collecting survey data. Sex was not collected nor considered in any of the analyses.
Reporting on race, ethnicity, or other socially relevant groupings	For our main findings, race is included as a candidate covariate in the lasso. In addition, the sampling frame was stratified along race and randomization was blocked on race. Race was measured via survey self-report (non-Hispanic White; Hispanic; non-Hispanic Black; Asian-American or Pacific Islander; and Other). Several subgroup analyses in the SI employ race (see Figure S13, Table S22), Informed consent was provided prior to collecting survey data.
Population characteristics	We recruit active US adult Facebook and Instagram users. The sampling frames included all Facebook and Instagram monthly active U.S.-based users 18 years of age or older eligible to receive general surveys on a given platform (these represent a random set of users from the overall Facebook and Instagram populations) as of August 17, 2020. Of the participants in our Facebook sample, 74% are white, 41% are male, 52% hold a college degree, 55% identify as Democrats or lean Democrat, and 33% identify as Republicans or lean Republican. In our Instagram sample, 61% are white, 38% are male, 58% hold a college degree, 72% identify as Democrats or lean Democrat, and 20% identify as Republicans or lean Republican.
Recruitment	We summarize the recruitment strategy below. Further details are provided in Appendix A.2 and L in the SI. At the top of their Instagram or Facebook feed, randomly selected participants saw a recruitment message asking them if they would like to share their opinion. Those clicking "Start Survey" were directed to a consent form. Participants gave their consent to participate using an IRB-approved consent form that outlined the study procedure, benefits and risks, and compensation. For more information about sample balance and demographics, see Appendix E ("Sample, Balance, Attrition, and Compliance"). We design weights to reduce bias while maintaining a low design effect. See further details in Appendix A.5 ("Weighting"). These weights were used in the construction of our outcome indices as well as in our treatment effect estimation.
Ethics oversight	The entire study was reviewed and approved by the NORC IRB. Academic researchers worked with their respective university IRBs to ensure compliance with human subjects research regulations in analyzing data collected by NORC and Meta and authoring papers based on those findings. The research team also received ethical guidance from the independent firm Ethical Resolve to inform study designs. Appendix J in the SI ("Ethical Considerations") provides a detailed overview of the ethical concerns that were considered and the decisions to minimize the potential harms. In particular, this section details the informed consent process as well as the pre-registered "stopping rule", which would have ended a treatment if we detected that it was generating changes in specific variables relevant to individual welfare that were much larger than expected.

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

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Behavioural & social sciences study design

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Study description	We rely on an over-time experimental design. Experiments were run on both Facebook and Instagram. Meta placed survey invitations in these users' focal platform feeds from August 31 to September 12, 2020. Users who consented to participate were asked to complete a series of surveys implemented by NORC at the University of Chicago, including baseline (responses collected September 8 - 21) and endline (November 4 - 18) surveys. The questionnaires are included in Appendix M. Participants also agreed to allow us to access data on their activity on the focal platform. They were told that the experiment could

involve changing aspects of their on-platform experiences, but they were not informed of the specific treatments for which they would be eligible or that the study was focused on political ads.

Just after the baseline survey, participants were randomized to a control group or one of several treatment groups that varied the content they saw on their focal platform. Our paper studies the two treatment conditions that removed political ads: "All Ad Removal" and "List-Targeted Ad Removal." We define "political ads" to be all ads that Meta's classifiers determine to be about social issues, elections, or politics. In the All Ad Removal condition, Meta removed all political ads and replaced them with non-political ads. In the List-Targeted Ad Removal condition, Meta removed the subset of political ads that were targeted based on user data provided by the advertiser and replaced them with other political or non-political ads. In both conditions, users' total ad load was unchanged. Participants were not informed of the treatment to which they were assigned.

In the Facebook experiment, we implemented both All Ad Removal and List-Targeted Ad Removal. In the Instagram experiment, to conserve on the total number of treatment arms, we implemented only All Ad Removal. In both experiments, the ads were removed starting on September 24th through November 3rd, which is referred as "study period." Participants received base payments of $5 for completing the baseline survey and $20 for completing the endline survey, plus other payments described in Appendix A.

This study employs a quantitative research design, utilizing observational data and survey responses. The analysis incorporates statistical methods such as lasso regression for control variable selection, multiple hypothesis testing adjustments using FDR, and Bayes Factor calculations, ensuring rigorous evaluation of treatment effects and robustness of findings.

Research sample

For each platform, Meta drew a stratified random sample of users in the U.S. who were at least 18 years old and had logged into their account at least once in the past month. These individuals were invited to participate in the survey and remunerated for their participation. Our primary research sample consists of all such users who agreed to participate in the study and completed both survey waves (baseline and endline). For more information about the selection, see the Experimental Design section of the main paper and Appendix A in the SI. See Appendix D for sample balance and demographics.

We determined the sample size to provide statistical power to detect small effects (with $\beta = 0.8$ and $\alpha = 0.05$). For example, our power calculations were designed to detect an effect of 1.5 percentage points on self-reported turnout in the Facebook sample and 1.4 percentage points in the Instagram sample.

Sampling strategy

Below we summarize our sampling strategy. For more information, see Appendix A.1 ("Sampling").

We sampled separately from the populations of Facebook and Instagram users. The sampling approach was designed to achieve desired minimum detectable effect sizes (MDEs) based on power analyses conducted prior to recruitment. The sampling frames included all Facebook and Instagram monthly active U.S.-based users 18 years of age or older eligible to receive general surveys on a given platform (these represent a random set of users from the overall Facebook and Instagram populations) as of August 17, 2020. Sampling frames were stratified along the following dimensions: number of days a user was active on a given platform, a user's predicted census region, whether the user is predicted to live in a battleground state, a user's predicted ideology, and the census ethnic/racial composition in the zip code in which a user is predicted to live. For Instagram, predicted ideology was not used as this classifier had not been developed for Instagram.

Sampling probabilities were computed to achieve specific sample distributions for the set of demographics encoded in the stratification step across each of the samples of interest. The sampling probabilities took into account desired sample size as well as predicted differential non-response across different demographics based on prior Facebook surveys. The initial target distributions are reported in Table S1.

Data collection

Data was collected from participants on their own devices (e.g., mobile phones and computers). The survey vendor we used (NORC) was blind to the experimental condition of each subject as well as our hypotheses.

Timing

Facebook/Instagram-recruited respondents were invited to the survey between August 31-September 12. The baseline survey field period started on September 8 and continued through September 21. The endline survey field period started on November 14 and continued through November 18. For more information on timing, see Appendix A.2.

Data exclusions

Exclusion criteria were pre-established in the pre-analysis plan (see https://osf.io/8m36y/). In particular, For each platform, we sampled from the population of users who (i) live in the United States; (ii) are 18 years of age or older; (iii) have logged into their respective account at least once in the past month. To form our primary analysis sample, we then restrict to all users who completed the baseline survey and did not withdraw.

Non-participation

More than 70 percent of the primary analysis sample completed the endline survey. Appendix Tables S5 and S6 show that our samples remain similarly balanced on covariates after attrition and that attrition rates did not differ significantly across control and treatment groups.

Randomization

Randomization occurred just after the end of the baseline survey. Only participants who completed the baseline survey were eligible for randomization. Participants were randomized into the Control group, the All Ad Removal treatment, the List-Targeted Ad Removal treatment, or one of five other treatment conditions that are analyzed in separate studies. We adopted block randomization to minimize variance of treatment effect estimates and to ensure in-sample balance in a set of covariates that may be important determinants of the outcomes of interest. Randomization was blocked on swing state, friend count (Facebook) or following count (Instagram), self-reported party ID, and race (36 blocks).

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