Access to social media and support for dominant incumbents: Natural and field experimental evidence from Uganda*

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To limit the potentially destabilizing effects of social media, incumbents in competitive authoritarian regimes often limit citizens' access. We examine how variation in access to social media affects political attitudes during Uganda's 2021 election period and during comparatively "normal" post-election times. Leveraging a difference-in-differences design during a social media ban at the climax of the election campaign, we find that respondents able to maintain access due to VPN usage—who are more likely to be opposition partisans—came to view the dominant NRM party relatively positively. In contrast, a field experiment implemented half a year later finds that NRM partisans whose social media use was subsidized for three months came to view the NRM more negatively. While the latter moderating effect aligns with greater content on social media produced by opposition groups, the election-time finding appears to reflect both a relative increase in pro-NRM social media content during the social media ban and VPN users' low prior expectations of NRM performance. While our findings suggest that social media can support opposition parties in competitive authoritarian contexts, they also highlight the ability of dominant incumbents to control content—especially in politically salient moments.

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1 Introduction

In authoritarian settings where traditional media outlets are controlled by the government, social media has the potential to play a central role in providing citizens with access to alternative sources of political information (Zhuravskaya, Petrova and Enikolopov, 2020). Initial optimism regarding the potential of such "liberation technologies" (Diamond, 2010), however, has been tempered by the efficacy with which repressive regimes have exerted control over such platforms (Morozov, 2012). Efforts to limit citizens' access to social media are widespread—whether through bans on access to particular platforms (Chen and Yang, 2019), wholesale internet blackouts (Roberts, 2020), or through elevating the financial costs of access (Boxell and Steinert-Threlkeld, 2021). Furthermore, others have suggested that social media—especially when combined with state-orchestrated censorship—is used to distract, misinform, and polarize citizens in ways that benefit autocrats (King, Pan and Roberts, 2017; Nyabola, 2018; Roberts, 2018). While a rich body of literature studies developed democratic settings (Allcott et al., 2020; Farrell, 2012), our understanding of how social media access affects citizens' political attitudes in competitive authoritarian contexts—where incumbent governments often restrict access to social media and threaten critics—remains limited.

We consider *whether and when* social media access affects support for the long-standing incumbent party in the context of Uganda around its 2021 elections. A canonical electoral autocracy, the dominant National Resistance Movement (NRM) led by Yoweri Museveni exerts substantial control over traditional media sources. By contrast, social media activity is widely used by opposition-leaning figures—most prominently, by the main challenger party, the National Unity Platform (NUP) led by Bobi Wine. In response to the potentially destabilizing effects of social media usage on the NRM's grip on power, the government has enacted a number of policies intended to limit citizens' access. These include the imposition of the "over-the-top" (OTT) tax on daily social media use, the existence of high indirect taxes on citizens' purchases of mobile data bundles, and—most overtly—the imposition of both a complete internet blackout immediately around the 2021 election and a longer-lasting nominal ban on social media use following the election. Citizens' restricted access, or their reservations about circumventing bans to post critical content, might limit social media's effects on fomenting anti-regime sentiment.

To evaluate the effects of variation in individual-level access to social media on support for Uganda's incumbent NRM party, we leverage a natural experiment around the month-long election-time social media ban and a three-month field experiment beginning five months after the elections. These complementary designs both draw from a three-wave panel survey of occasional social media users in electorally competitive districts conducted by telephone in 2021. To estimate effects of greater access to social media at election time, we use a difference-in-differences design to compare changes in support for the NRM across respondents that did and did not use virtual private networks (VPNs)—which enabled individuals to circumvent the social media ban—at baseline. To estimate effects of access to social media during comparatively "normal" times, when politics and censorship were far less salient, we randomly assigned half of our respondents to receive significant subsidies to facilitate their use of social media for three months. The control group instead received a more flexible mobile money transfer. Using detailed behavioral data, we establish that both research designs isolate significant variation in respondents' social media usage.

Our difference-in-differences results suggest that elevated use of social media during the election-time ban period significantly *improved* respondents' attitudes towards the incumbent NRM party. Relative to individuals that did not use VPNs, VPN users became more likely to believe the NRM cared most about Ugandans' welfare, felt more warmly about the NRM relative to opposition parties, and became more open to voting for NRM candidates in the future after the election period relative to before. Though VPN users are, cross-sectionally, younger and more likely to be non-NRM partisans, the results are robust to alternative operationalizations of VPN usage and to potential violations of the parallel trends assumption through the inclusion of interactive locality, age, and partisan controls. While it was undoubtedly unpopular, the results are not simply driven by those most adversely impacted by the social media ban punishing the incumbent party. These findings cut against the hope of many that social media can promote influential anti-government voices during politically salient moments.

In contrast, the experimental analysis provides evidence of conditional effects on political attitudes outside of the election campaign that cut *against* the ruling party. We detect only modest average effects on respondents' support for the NRM, but find heterogeneity according to pre-treatment measures of partisanship. NRM-aligned treated respondents came to view their own party more negatively and opposition parties more positively. In contrast, non-NRM respondents' views of the NRM and opposition parties were largely unaffected. The experimental results thus provide evidence of a moderating effect of social media access among NRM supporters outside of an election campaign period. This finding more closely aligns with the hopes of some that social media might buttress opposition movements in competitive authoritarian regimes.

What drove the different effects of access to social media during these different periods? Our analysis of mechanisms explores several possibilities. First, reweighting exercises suggest that the differences do not appear to be driven by distinct compliers for each intervention—that is to say, the types of individuals that were induced to increase their social media usage are relatively similar across the two analyses. Second, we provide evidence suggesting that the conflicting effects are driven by differences in the content on social media during different political moments. Our analysis of public Facebook activity suggests that social media content generally favors the opposition, but this difference became less stark during the election-time social media ban. Moreover, consistent with differences in content relating to NRM performance and election integrity shaping support for the government during the social media ban, we find that respondents with VPNs became more likely to view government performance favorably and became less critical of the quality of Ugandan democracy. Furthermore, increased support for the NRM was concentrated among respondents whose prior beliefs about NRM governance and Ugandan democracy were least favorable. These results suggest that social media content shapes political beliefs, but this need not always favor opposition parties. In particular, our findings suggest that—whether naturally or because of the social media ban—election-time social media content can also turn to favor incumbents.

These findings make several contributions. First, the results suggest that social media's low barriers to entry *can* support opposition movements in competitive authoritarian regimes. Prolonged exposure to disproportionately opposition-leaning content appears to modestly reduce incumbent party partisans' favorability toward the regime, as Miner (2015) and Guriev, Melnikov and Zhuravskaya (2021) similarly find for internet and 3G access more generally. However, whether social media exposure does so at defining political moments—like during the social media ban, which our study provides a rare opportunity to assess—may also be shaped by government policies in conjunction with citizen expectations of what content they would encounter on social media. In both regards, our findings align with an extensive literature documenting the politically persuasive effects of partisan media content (e.g. Adena et al., 2015; DellaVigna and Kaplan, 2007; Enikolopov, Petrova and Zhuravskaya, 2011; Martin and Yurukoglu, 2017), as well as a growing literature that emphasizes the scope for counter-attitudinal perspectives to influence voters in the Global South (Asimovic et al., 2021; Brierley, Kramon and Ofosu, 2020; Conroy-Krutz and Moehler, 2015) as well as the Global North (Broockman and Kalla, 2022; Levy, 2021). They also provide a less sanguine perspective on whether social media can level the political playing field (Diamond, 2010).

Second, our findings also point to authoritarian resilience. Not only do our results show that social media is far from a panacea for differential access to traditional communication tools, but the social media ban also suggests that government policies can be strategically deployed at critical times to reshape content production as well as limit access to information. In these regards, our findings align with prior studies documenting the ways through which authoritarian rulers maintain control over the information environment (e.g. Guriev and Treisman, 2019; Morozov, 2012; Roberts, 2020).

Third, our findings speak to a growing literature on the effects of social media more broadly. A number of studies have now found that social media produces deleterious welfare outcomes in advanced democracies and increases political polarization (Allcott et al., 2020; Mosquera et al., 2020). Our results suggest that there may be less negative consequences of social media in young democracies, where citizens' experience with online activity may be more limited and political attachments may be weaker (Lawson and McCann, 2005). This aligns with a growing literature suggesting that the negative impacts of social media in the Global North are less pronounced in the Global South (Lorenz-Spreen et al., 2021). For example, Enríquez et al. (2022) show that the mass reach of non-partisan Facebook ads can generate social interactions that substantially increase electoral accountability in Mexico.

2 Context

In this section we first describe the Ugandan political context. We then discuss the role of social media in the 2021 election before describing barriers to accessing social media. Figure 1 provides a timeline of the key events during the study period relating to politics, social media access, and data collection.

2.1 Political context

Uganda, a canonical electoral authoritarian regime, has been led by Yoweri Museveni and his National Resistance Movement (NRM) party continuously since 1986. The most recent presidential elections were held on January 14.[1] Museveni faced his most credible opposition from Robert Kyagulanyi Ssentamu—nicknamed Bobi Wine, a rapper-turned-MP with broad support among younger voters and wide reach through social media platforms. Kyagulanyi represented the National Unity Platform (NUP), which was founded in July 2020 and rapidly became the most popular opposition party. Given that the elections coincided with the COVID-19 pandemic and restrictions on public gatherings, the Electoral Commission (EC) dictated that the electoral campaigns would follow a "scientific" model of using traditional broadcast and online media to appeal to voters, rather than through the typical holding of mass rallies across the country.

Enforcement of these campaigning rules was heavily imbalanced, with widespread NRM rallies taking place while attempted rallies by opposition parties were violently disbanded, resulting in deaths, injuries, and arrests (Freedom House 2021). In November 2020, Kyagulanyi was arrested during a rally for violating Covid restrictions, sparking widespread protests. These were violently repressed by the regime, resulting in 54 reported deaths (Amnesty International 2021). In this context of repression, it is likely that many voters anticipated further crackdowns and electoral malpractice during the January elections.

President Museveni won the 2021 presidential election with 58% of the official vote, followed by Kyagulanyi with 35%, and was inaugurated on May 12, 2021. In the parliamentary elections, the largest shares of elected seats went to the NRM (64%), independents (14%), NUP (11%), and

[1] A host of other lower-level elections took place over the same period, including parliamentary elections (also on January 14) and district, municipal, and subcounty council elections (between January 20 and February 3).

the formerly main opposition party FDC (6%). The United States described the 2021 elections as "neither free nor fair" and imposed visa restrictions against those believed to have undermined the democratic process (US Department of State 2021).

2.2 Social vs. traditional media

Social media has become increasingly popular in Uganda over the past decade and constitutes the vast majority of internet usage in the country. As in most sub-Saharan African settings, access to the internet and social media is almost entirely through cell phones: in late 2020, 52% of Ugandans had mobile internet connections. Social media is the chief way of using the internet. In our baseline survey, respondents report spending over five times as much time on social media applications in a normal week as they do browsing websites. Facebook and WhatsApp are by far the most popular platforms. In our baseline survey, 79% (78%) of respondents report using Facebook (WhatsApp), while only 17% do so for Twitter.

Ugandan social media contains a high amount of political content. Among the Facebook users in our sample, 71% view getting news about politics as one of their main reasons for using the platform; 25% state discussing politics and current events as one its main uses. Among WhatsApp users in our sample, these figures are 53% and 26%, respectively (see Table A1). Importantly, as in many countries in the Global South, WhatsApp is not just used as a private messenger app, but also as a form of mass communication via groups of up to 256 users. Information can easily be forwarded from one group to another.

Social media platforms in Uganda offer a relatively level playing field for political parties and their campaigns. If anything, posts by opposition-affiliated accounts and views thereof dominate. This stands in stark contrast with traditional forms of media such as newspapers, TV, and radio, over which the regime has considerable control. Although many media outlets exist, journalists and outlets regularly face state repercussions for their work, including raids of radio stations, arrests, harassment, and intimidation, earning Uganda a Freedom House score of 1 out of 4 for a free and independent press (Freedom House 2021). In the lead-up to the 2021 elections, journalists were arrested for hosting opposition candidates on their shows, a radio station was raided, journalists were prevented from covering opposition rallies, and foreign journalists were denied accreditation (US Department of State 2020).

Social media was particularly important in the 2021 campaign season, in light of government control of traditional media outlets and limits on in-person campaign rallies. According to the Uganda Communications Commission, "for the first time in the Uganda electoral history, the use of electronic media channels outpaced traditional mass rallies and the use of print campaign material" (Uganda Communications Commission 2021).

Especially the newly-founded National Unity Party, with its many young and often urban voters, relied heavily and successfully on social media to broadcast its message. As we show in Section 6.2, posts by accounts associated with the NUP were more frequent and received substantially more interactions (comments and reactions) than those by the NRM or other opposition parties in the run-up to the elections. Similarly, viral political posts in the weeks before the election were almost all authored by NUP-associated accounts.[2]

2.3 Access to social media

Perhaps not surprisingly in light of NUP's dominance of social media content, social media became deeply politicized in the run-up to the 2021 elections. In December 2020, the government wrote to Google to request the shutdown of 14 popular YouTube channels sympathetic to Kyagulanyi. In early January, Facebook removed a network of hundreds of accounts linked to the Ugandan Ministry of Information and Communications Technology for engaging in "coordinated inauthentic behavior" promoting the ruling party and denigrating the NUP. Twitter followed suit. On January 12, the government announced a complete ban of all social media platforms, including WhatsApp, Facebook, and Twitter, which remained in place until February 10. Access to Facebook—the most popular social media platform in the country—remains blocked until this day. However, many individuals use VPNs to maintain access. This includes government officials, who kept posting from official government accounts during the ban. The government also introduced a new tax on Facebook ads in October 2022, even though Facebook access remains blocked. It may thus be more accurate to think of the effect of the "ban" as introducing friction and uncertainty about the consequences for using social media.

On the eve of the election, the internet was shut down completely. Internet access resumed five days later, shortly after President Museveni was declared the election's winner.

Efforts to curtail social media access already started in 2018, when the Government of Uganda introduced a tax on social media access, named the "over-the-top" (OTT) tax.[3] The official motivations for the OTT tax were to curtail citizens' exposure to "gossip" online and raise domestic tax revenues. The tax cost 200 shillings per day (approximately $0.055). Payment is required for accessing social media platforms, including Facebook, WhatsApp, Instagram, and Twitter within the country. The OTT tax quickly triggered pushback and protest, with civil society organizations decrying its introduction as "a clear attempt to silence dissent, in the guise of raising government revenues" (Amnesty International 2018). Since citizens using social media are younger, more urban,

[2]Based on our analysis of 1.6 million public Facebook posts downloaded from Crowdtangle.

[3]Similar taxes on social media access have been implemented in a number of African countries in recent years, including Benin, Mozambique, Tanzania, and Zambia.

and more likely to support opposition parties than the rural base of the NRM, the tax was widely seen as a tool intended to undermine opposition support ahead of the 2021 election (Namasinga and Orgeret 2020) by limiting social media usage in the country (Boxell and Steinert-Threlkeld 2021; Pollicy 2020).

Citizens were able to evade the OTT tax and social media block through the use of VPNs. Accessing social media platforms using a VPN is slower and more bandwidth-intensive than doing so through OTT payment (Pollicy 2020). Because data is very costly, with 1GB of mobile data costing 8% of average monthly income (A4AI 2019), VPN usage does not strictly dominate the payment of the social media tax. Among our baseline survey respondents, we find OTT payment to be more common: respondents report having paid the OTT tax on average 2.6 days in the last week, compared to an average of 2.1 days of VPN use. The cost of the OTT tax is listed by 55% of our sample as preventing them from using social media more, while 67% listed the cost of data. In light of its limited revenue-raising capacity, as well as perhaps the completion of the electoral cycle, the government abandoned the OTT tax in the 2021/22 fiscal year starting in July 2021. In its place, the government imposed a 12% tax on mobile data. Together with the 18% VAT, this raised taxation of mobile data to 30%.

3 Sampling and data collection

Both our observational and experimental analyses of the effects of access to social media in Uganda draw from an original three-wave panel survey. We first explain our sampling strategy before introducing our survey and behavioral data sources.

3.1 Sampling

Study recruitment reflected political and economic considerations. Politically, we sought to focus on individuals living in electorally competitive districts where exposure to diverse sources of information through social media could be politically salient. Economically, we sought to target individuals who owned a phone capable of accessing social media platforms but use social media relatively infrequently (and could thus be induced to use it more regularly).

To reach this population, we selected 11 districts—from each region of the country—where the incumbent NRM party had received 40-60% of the vote in the 2016 election and internet accessibility is good. Figure 2 plots the locations of these districts across the country. Within each district, we sought to recruit participants from peri-urban trading centers (TCs) on the fringes of large urban

Figure 1: Timeline of events during research study

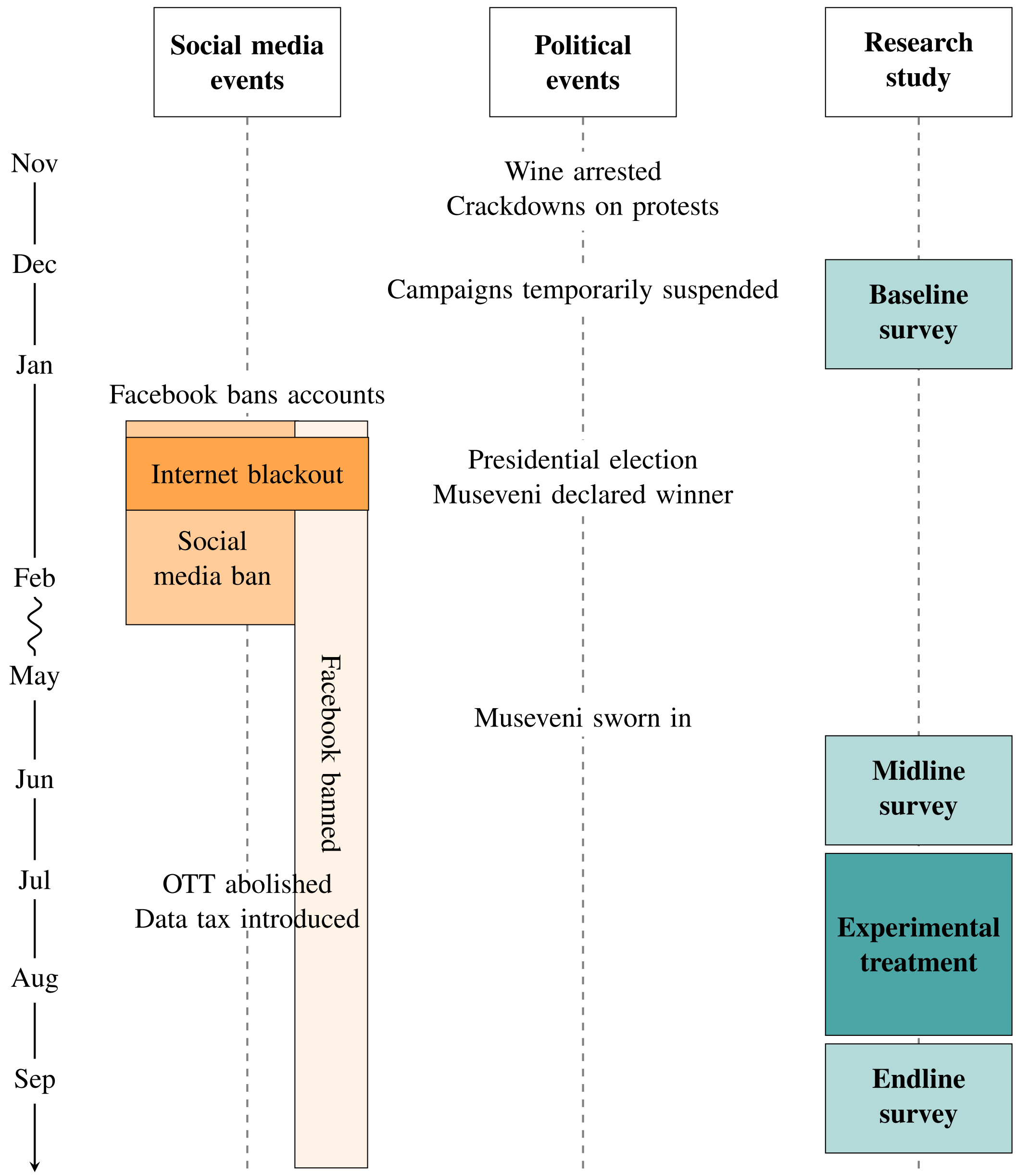
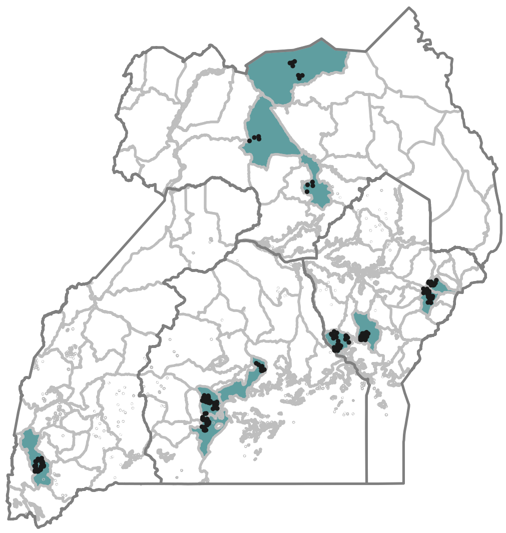


Figure 2: Spatial distribution of sampled districts and trading centers



Notes: Sampled districts comprise: Mpigi, Kalungu, Masaka in Central region; Iganga, Jinja, Mbale, Sironko in Eastern region; Gulu, Lamwo, Lira in Northern region; and Rukungiri in Western region.

localities with good 3G internet reception.[4] We anticipated that such areas would be most likely to yield semi-frequent social media users.

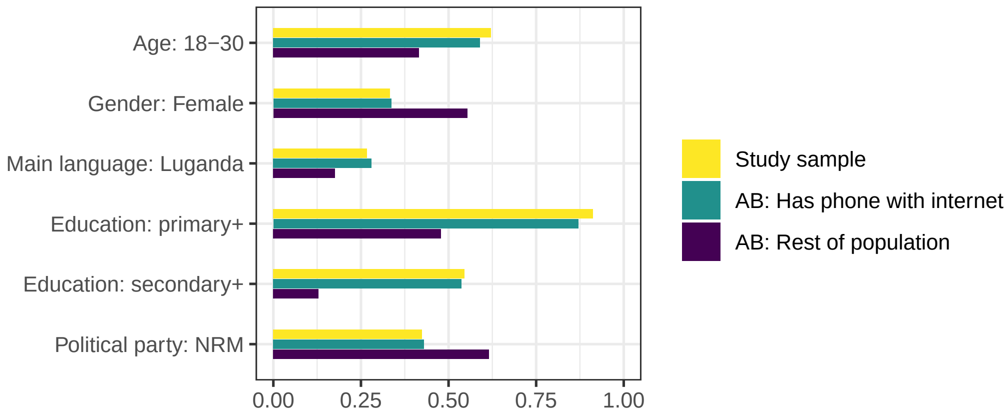
We designed a tiered phone-based recruitment process to construct the sampling frame. First, we obtained contact details of community leaders—including LC1 councilors, parish chiefs, and village health team members—within a given TC from district-level officials. Second, in phone calls with local leaders, we obtained a list of up to 8 "seeds" per TC stratified by their role (*boda boda* drivers, teachers, business-persons, or youth representatives) within the community. Third, we called every "seed" to solicit the contact details of a set of their personal contacts who might be interested in, and eligible for, the study. This process generated a sampling frame of 4,399 contact phone numbers for potential respondents for the study across 135 trading centers.

Drawing from this, we called potential respondents to assess their eligibility for the study and their interest in participation. We were able to contact 3,710 potential respondents, of which half met our three eligibility criteria: (i) aged between 18-50; (ii) possessed a cell phone able to access social media platforms; and (iii) reported using social media apps three or fewer days in the last week. Ultimately, 1,542 eligible individuals agreed to participate in the study and completed our baseline survey.

The baseline sample broadly approximates our target population within Uganda. Using data

[4]To assess internet coverage we used the Collins World Explorer GIS database of 3G coverage areas by country.

Figure 3: Characteristics of study sample



Notes: Figure compares average characteristics among our baseline survey sample with respondents from Afrobarometer Round 8 (2019) in Uganda.

from the nationally-representative Afrobarometer Round 8 (2019), we compare our sample's characteristics against Afrobarometer respondents possessing a phone capable of accessing the internet and those without. Figure 3 shows that our sample matches the former group well: mostly aged 18-30 and more likely to be male, well-educated, and less likely to be NRM partisans compared to the broader population.

3.2 Survey data

Our "baseline" survey was administered in December 2020. The "midline" survey, which was enumerated between late May and early June 2021, attempted to recontact every individual that completed the baseline survey and some further individuals that were eligible for the study but did not complete the baseline survey. We successfully re-interviewed 1,310 (85%) of all baseline respondents, and interviewed an additional 145 participants for whom we had collected contact details prior to the baseline survey but had previously been unable to reach, for a total of 1,455 respondents. The "endline" survey, administered in September 2021, successfully resurveyed 1,389 (95%) of respondents who had completed the prior midline survey.

All surveys were conducted remotely via telephone, given the COVID-19-related health risks associated with in-person enumeration during the study period.[5] Across the surveys, eligible respondents were asked batteries of questions relating to their demography; social media consumption behaviors (and whether accessed by VPN); attitudes towards incumbent and opposition parties;

[5] Telephone surveys are nevertheless relatively common in the Ugandan context, and previous studies accord with the low attrition rates we find (15% from baseline to midline; 5% from midline to endline).

willingness to vote for different types of candidates (given political constraints on asking about vote intentions and prior vote choices in the presidential elections) and perceptions of government.

For our observational analysis of social media access during the ban period, our analysis sample constitutes the balanced panel of 1,310 respondents who completed both baseline and midline surveys. For our experimental analysis of social media access following the election, our analysis sample constitutes the endline sample of 1,389 respondents.

3.3 Social media usage data

In addition to our survey data, we collected publicly-accessible data on respondents' WhatsApp usage throughout the study which, along with Facebook, is the most popular app among our sample and was affected by the social media ban.[6] We systematically audited the public WhatsApp status of the phone numbers of respondents in our sample by auditing each phone number between four and five times per day throughout the study, and thus construct respondent $\times$ date level measures for: (i) whether the respondent had been "last seen" using WhatsApp on that date; and (ii) the number of distinct timestamps the respondent had been "last seen" using WhatsApp on that date.[7] Around two-thirds of our baseline respondents had phone numbers linked to active WhatsApp accounts, of which 90% had publicly-viewable WhatsApp statuses.[8] Among the subset of phone numbers we are therefore able to audit, respondents use WhatsApp with probability 0.22 on a given day throughout the full study.

4 Access to social media around the election-time ban

We first investigate the political effect of access to social media during the ban imposed over Uganda's month-long election season in 2021. This period included the election day and counting period for the Presidential election and election of Members of Parliament (MPs), and later election days for various local officials. During the internet blackout in the days immediately surrounding the national-level elections, no citizens could access social media. For the remainder of the social

[6] We have limited data on Facebook activity since we did not record account URLs in our surveys for both logistical and surveillance concern reasons. Nonetheless, we are exploring different options to match our sample to active Facebook accounts which minimize the risk of false positive matches. Since this data processing remains ongoing, for now we only include this data descriptively in Figure A1.

[7] Such information about users' WhatsApp "last seen" status is public-facing by default. When users opt out of this, we observe whether the phone number is registered to a WhatsApp account and whether the user is "online" at the exact time of the audit, which is very rare due to the fact that each phone number is only audited a few times per day, but not when they were "last seen" online. Because we audit every phone number multiple times a day, measure (i) is an accurate measure of daily WhatsApp usage. The upper bound for measure (ii) is the number of times we audit that number on a given day, and can thus only capture so much variation along the intensive margin.

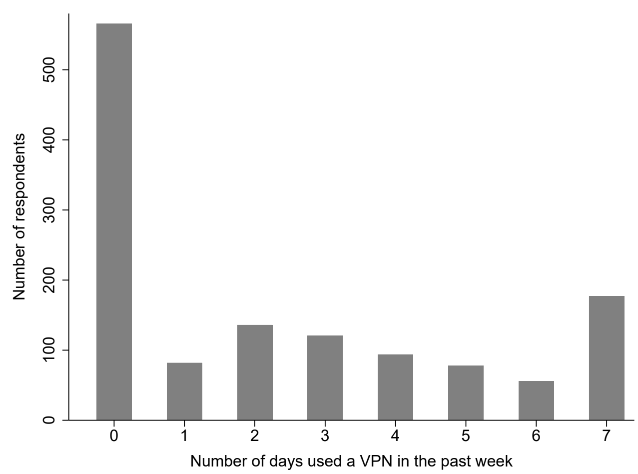
[8] This is likely an underestimate of actual WhatsApp usage, since some respondents have multiple SIM cards.

media ban, citizens could not access social media by making OTT payments or the internet using mobile data. Even as other social media apps were reinstated, the ban on Facebook has never formally been lifted. Exploiting variation in whether individuals already used the VPNs required to circumvent the social media ban, we find that VPN users were more likely to use social media during the ban. Furthermore, and perhaps somewhat surprisingly, we find that such individuals became *more* favorable toward the incumbent NRM government across various measures of preference.

4.1 Difference-in-differences design

To estimate the effects of access to social media during the election-time social media ban, we leverage a difference-in-differences design comparing individuals that did and did not use VPNs before the ban. Specifically, we compare individuals that reported using a VPN one or more days in the week preceding the baseline survey with individuals that reported not using a VPN on any days in the preceding week. As Figure 4 shows, 57% of respondents used a VPN on at least one day, while 43% did not use a VPN at all. In addition to this extensive margin classification, our robustness checks in Appendix Tables A4-A6 report similar results using various alternative classifications of VPN usage, including a machine learned measure of intensity.

Figure 4: Distribution of VPN use across baseline survey respondents



VPN users unsurprisingly differ from non-VPN users in potentially salient ways. Panel A of Table 1 shows that VPN users are generally younger, less likely to use the more expensive MTN network, more likely to use social media, and more likely to support opposition parties and disapprove of the NRM's government performance. However, VPN users are relatively similar in various other ways, including gender, education, self-assessed living conditions, and religion. Conditioning on location and age, by introducing trade center and age fixed effects respectively in panels B and C, reduces most of these differences to statistical insignificance, although VPN users remain more knowledgeable about politics and politically distinct in their initial opposition to the government. To ensure that baseline differences across VPN and non-VPN users are not driving our findings, we exploit within-individual variation over time and adjust for period-specific influences of baseline characteristic differences.

We estimate regressions of the following form:

$$Y_{ict} = \tau(VPN_i \times Post\ election_t) + \mu_i + \eta_t + \varepsilon_{ict}, \tag{1}$$

where Y_{ict} denotes an outcome for individual i located in trading center c at time t (whether a survey wave or a measure of social media activity), $Post\ election_t$ indicates the period of or after social media was blocked by the government, and VPN_i indicates regular VPN users. In addition, we include individual fixed effects, μ_i, and time fixed effects, η_t, to absorb time-invariant differences across individuals and common period shocks. The former abstracts from baseline differences across respondents that differ in their use of VPNs, while the latter absorbs factors that influenced all respondents similarly. Standard errors are clustered at the trading center level to reflect community-level differences in the intensity of VPN use.

The coefficient τ captures the average effect of already using a VPN during the election-time social media ban—and its aftermath—under a parallel trends assumption. This assumption requires that VPN and non-VPN users would have followed similar trends in our social media and government support outcomes in the absence of the social media ban. Consistent with the plausibility of this assumption for social media usage, Figure 5 below shows that the two groups exhibit similar trends in observed WhatsApp use prior to the social media ban.

And, consistent with its plausibility for political attitudes, we leverage variation in the exact date of enumeration of the baseline survey (given that we have only a single pre-ban survey). Estimating differential trends in our key outcome measures of NRM support by date across VPN and non-VPN users, Figure A2 shows that these each appear parallel prior to the end of the baseline survey.[9] In our robustness checks below, we further include interactive fixed effects to exploit only variation within various voter groups—by trading center, age, political engagement and knowledge, and prior political disposition—that might have experienced non-parallel trends. The parallel trends assumption could also be violated by selective attrition from the panel. Fortunately, we detect no evidence of this. Appendix Table A2 shows that VPN and non-VPN users dropped out of the midline survey at statistically indistinguishable rates (about 15%).

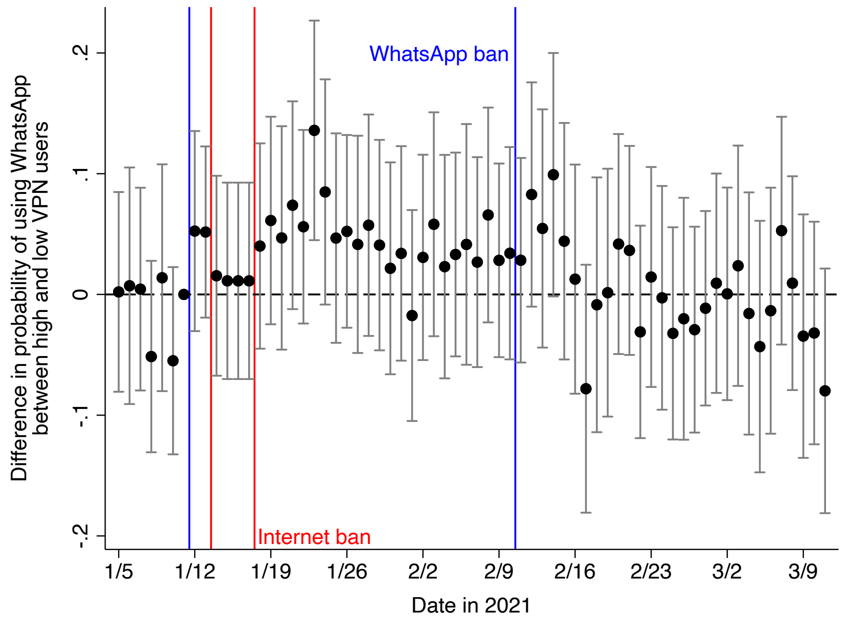
[9]We exclude dates on which fewer than 25 baseline surveys were conducted, which corresponds to the days surrounding Christmas 2020. Figure A3 plots the raw trends in these outcome measures during the baseline enumeration period.

Table 1: Correlation between baseline characteristics and VPN use

	Age (1)	Male (2)	MTN (3)	Education (4)	Better living conditions (5)	Traditional Christian (6)	Evangelical Christian (7)	Muslim (8)	Social media use scale (9)	Political knowledge scale (10)	Support government scale (11)	Approve government scale (12)	Support democracy scale (13)	Political polarization scale (14)	COVID-19 knowledge scale (15)	COVID-19 behavior scale (16)	General welfare scale (17)
Panel A: Bivariate correlations																	
VPN	-1.635***	-0.041	-0.117***	-0.098	-0.026	-0.025	-0.008	0.040	0.187***	0.084	-0.242***	-0.128**	-0.000	-0.079	-0.075	0.041	0.003
	(0.480)	(0.028)	(0.028)	(0.095)	(0.041)	(0.033)	(0.024)	(0.026)	(0.062)	(0.054)	(0.060)	(0.051)	(0.060)	(0.050)	(0.057)	(0.049)	(0.067)
Observations	1,308	1,308	1,310	1,310	1,310	1,302	1,302	1,302	1,310	1,310	1,310	1,310	1,310	1,310	1,310	1,310	1,310
R^2	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00
Control outcome mean	31.41	0.70	0.54	4.99	3.33	0.63	0.19	0.17	-0.10	-0.04	0.14	0.06	0.03	0.04	0.05	-0.07	-0.00
Control outcome std. dev.	8.18	0.46	0.50	1.72	0.77	0.48	0.40	0.37	1.01	1.04	0.97	0.95	0.95	1.00	1.02	1.03	0.99
Panel B: Bivariate correlations, conditional on trading center fixed effects																	
VPN	-1.506***	-0.033	-0.007	-0.001	-0.050	0.003	0.012	-0.008	0.120*	0.088*	-0.191***	-0.155**	0.007	-0.033	-0.090	-0.040	-0.024
	(0.484)	(0.029)	(0.020)	(0.101)	(0.043)	(0.034)	(0.027)	(0.024)	(0.062)	(0.049)	(0.065)	(0.060)	(0.066)	(0.055)	(0.063)	(0.052)	(0.072)
Observations	1,304	1,304	1,306	1,306	1,306	1,298	1,298	1,298	1,306	1,306	1,306	1,306	1,306	1,306	1,306	1,306	1,306
R^2	0.16	0.13	0.56	0.16	0.08	0.25	0.11	0.29	0.13	0.21	0.17	0.11	0.12	0.14	0.10	0.11	0.13
Control outcome mean	30.47	0.68	0.48	4.94	3.32	0.61	0.19	0.19	0.00	0.01	-0.00	-0.01	0.02	-0.00	0.01	-0.04	-0.00
Control outcome std. dev.	8.04	0.47	0.50	1.65	0.81	0.49	0.39	0.39	0.99	1.01	0.99	1.01	0.99	1.00	0.98	1.04	1.01
Panel C: Bivariate correlations, conditional on trading center and age fixed effects																	
VPN	-0.000	-0.022	0.008	0.006	-0.042	0.006	0.007	-0.007	0.104	0.124**	-0.172**	-0.127**	0.026	-0.014	-0.090	-0.032	-0.017
	(0.000)	(0.030)	(0.019)	(0.105)	(0.045)	(0.035)	(0.028)	(0.026)	(0.063)	(0.050)	(0.066)	(0.062)	(0.067)	(0.057)	(0.065)	(0.055)	(0.071)
Observations	1,304	1,304	1,304	1,304	1,304	1,297	1,297	1,297	1,304	1,304	1,304	1,304	1,304	1,304	1,304	1,304	1,304
R^2	1.00	0.17	0.58	0.19	0.12	0.27	0.13	0.30	0.16	0.25	0.20	0.14	0.14	0.18	0.12	0.14	0.16
Control outcome mean	30.47	0.68	0.48	4.94	3.32	0.61	0.19	0.19	0.00	0.01	-0.00	-0.01	0.02	-0.00	0.01	-0.04	-0.00
Control outcome std. dev.	8.04	0.47	0.50	1.65	0.81	0.49	0.39	0.39	0.99	1.00	0.99	1.01	0.99	1.00	0.99	1.04	1.01

Notes: Each specification is estimated using OLS, where outcome covariates differ by panel. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Figure 5: Differences in daily use of WhatsApp, by prior VPN use



Notes: Estimates are from equation (1), where the outcome is daily use of WhatsApp. The baseline category is the day before the WhatsApp ban was imposed. All bars indicate 95% confidence intervals.

4.2 Increases in social media use during the social media ban

We first confirm that VPN users were relatively more likely to use social media during the ban. To do so, we use the WhatsApp data collected for the 60% of respondents who could be linked to an active WhatsApp account with a publicly-available "last seen" status.[10] Although WhatsApp use strongly correlates with the self-reported use of Facebook and other apps in our sample, we are in the process of also identifying and scraping the public Facebook accounts of respondents.

Figure 5 plots our difference-in-differences estimates by day, relative to the day before the social media ban was imposed. Although the daily estimates are noisy, the average differences over time are clearer: VPN users became more likely to use WhatsApp on a given day during the social media ban starting in mid January, but quickly returned to pre-ban differentials once WhatsApp was reinstated in mid February. During the internet blackout, the difference is robustly zero because nobody was able to use online services during this period, regardless of VPN access.

We further test this relationship by pooling ban days and pooling non-ban days. Panel A of Table 2 shows that regular VPN users became 5 percentage points more likely to use WhatsApp on a given day during the social media ban, relative to a baseline level of 31% among non-VPN users.

[10] We also asked about social media use in our surveys. However, such data is far noisier, likely due to the difficulty of accurately reporting the number of hours spent on social media in a given week four months earlier. The behavioral data, on the other hand, is not subjective to such recall problems.

Table 2: Effects on daily WhatsApp usage during the social media ban across VPN/non-VPN users

	Used WhatsApp			Number of times used WhatsApp		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: VPN indicator						
VPN × WhatsApp ban	0.054***	0.047***	0.047***	0.084***	0.077**	0.081**
	(0.014)	(0.017)	(0.017)	(0.028)	(0.034)	(0.033)
Observations	112,943	111,655	111,512	112,943	111,655	111,512
R^2	0.35	0.44	0.47	0.34	0.44	0.47
Control outcome mean	0.31	0.30	0.30	0.54	0.53	0.53
Control outcome std. dev.	0.46	0.46	0.46	0.95	0.95	0.95
Interactive FEs		TC	TC & Age		TC	TC & Age
Panel B: Adaptive LASSO predictor of increased WhatsApp use						
Predicted WhatsApp usage during ban × WhatsApp ban	0.052***	0.048***	0.046***	0.097***	0.090***	0.086***
	(0.007)	(0.007)	(0.008)	(0.013)	(0.014)	(0.016)
Observations	112,371	110,940	110,940	112,371	110,940	110,940
R^2	0.35	0.44	0.47	0.34	0.44	0.47
Control outcome mean	0.30	0.30	0.30	0.53	0.53	0.53
Control outcome std. dev.	0.46	0.46	0.46	0.95	0.94	0.94
Interactive FEs		TC	TC & Age		TC	TC & Age

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. We exclude internet blackout days, where almost nobody was found to be online. Standard errors clustered by trading center are in parentheses. Predicted WhatsApp use is based on the (standardized) predictions of an adaptive LASSO model that predicts the individual change in WhatsApp use during the social media ban using baseline survey covariates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

The non-zero rate among non-VPNs users indicates that a significant share of these individuals started to use VPNs during the social media ban. In addition to limiting our "first stage" estimates, this finding conforms with evidence that citizens are also resilient in the face of government censorship (Chang et al., 2022; Roberts, 2020). Panel B further reports similar results when we use adaptive LASSO to retain predetermined covariates that predict the change in WhatsApp use during the ban period; this data-driven approach shows that prior VPN use is the most predictor. The results using this machine learning approach imply that a standard deviation increase in predicted WhatsApp use during the ban also translate into around a 5 percentage point increase in actual use.

4.3 Increases in support for the NRM governing party

Having established that VPN users were relatively more likely to use social media during the ban, we next examine changes in support for President Museveni's NRM party. Due to political constraints on our capacity to ask directly about presidential vote choice, we measure support for political party in several ways in our baseline and midline surveys. First, we asked respondents which party they believe cares most about the welfare of Ugandans; for our outcomes, we consider the incumbent NRM party, the new National Unity Platform (NUP) party of Bobi Wine, and the Forum for Democratic Change (FDC) which had been the main challenger to the NRM in prior elections. Second, we used a feeling thermometer to gauge how warmly respondents felt about the NRM relative to opposition parties; we carefully explained to respondents how to answer on a 11-point scale ranging from 0 (very cold) to 10 (very warm). Third, we asked respondents how open they would be to voting for NRM and opposition candidates for a generic political office in the future. Finally, we elicited party vote choice in their MP and LC5 elections. In the baseline survey, we use intended vote choice, while we use the party of the candidate they reported actually voting for in the post-election midline survey. Slightly more than half our respondents did not answer for MP, but did so much more often for LC5.

Together, these variables provide general measures of support for the NRM and Uganda's main opposition parties. For our primary outcomes, we aggregate these measures using inverse-covariance weighted (ICW) indexes (Anderson, 2008). Our index capturing support for the NRM combines three key outcomes that are available for all respondents: believing the NRM cares most about Ugandans; feeling toward the NRM; and openness to voting for the NRM. Our index capturing support for opposition parties combines: believing the NUP cares most about Ugandans; believing the FDC cares most about Ugandans; feeling toward opposition parties; and openness to voting for opposition parties. A final index captures differential support for the NRM; this ICW index is constructed using the three items indicating favorability toward the NRM and the reverse of the four indicators of favorability toward opposition parties.

Figure 6 plots changes in each outcome between the pre-election baseline and post-election midline surveys across VPN and non-VPN users. Confirming the cross-sectional differences suggested by Table 1, VPN users at baseline were less likely to believe that the NRM party cares most about the welfare of Ugandans (Figure 6c), less warm about the NRM relative to opposition parties (Figure 6d), less open to voting for an NRM versus opposition candidate in the future (Figure 6e), and less likely to report voting for an NRM candidate for MP (Figure 6f). However, for our aggregate indexes and each individual outcome, we observe a narrowing of the baseline differences between VPN and non-VPN users by the midline survey, suggesting that VPN users became more favorable toward the incumbent NRM party after the social media ban's imposition.[11]

We report our difference-in-differences estimates in Table 3 using equation (1), including trading center $\times$ period fixed effects in even columns. The results show that VPN users, who became relatively more likely to use social media during the ban period, came to view the incumbent NRM more favorably after the election relative to before. Specifically, VPN users became 8 percentage points more likely to believe that the NRM cares most about Ugandans' welfare, came to view opposition parties 0.2 units more negatively on a 10-point scale, and became 0.3 units more open

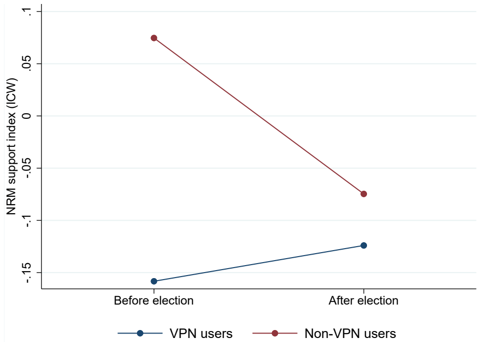
[11]Given the possibility that common shocks reduced NRM support by midline, our difference-in-differences analysis cannot entirely distinguish whether VPN users became more supportive of the NRM or non-VPN users became less supportive.

Table 3: Differential effects of VPN use on support for the NRM after the social media ban

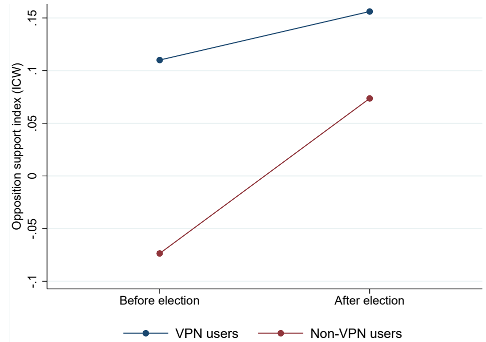
	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
VPN × Post election	0.184**	0.198***	-0.101	-0.155**	0.143*	0.183**
	(0.073)	(0.075)	(0.073)	(0.076)	(0.073)	(0.075)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.62	0.66	0.60	0.64	0.61	0.65
Control outcome mean	0.00	-0.00	0.00	0.00	0.00	-0.00
Control outcome std. dev.	1.00	1.00	1.00	1.00	1.00	1.00
Trading center × Post election FEs		✓		✓		✓
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
VPN × Post election	0.075**	0.078**	-0.016	-0.022	-0.020	-0.026
	(0.034)	(0.035)	(0.015)	(0.016)	(0.024)	(0.026)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.59	0.64	0.60	0.65	0.60	0.63
Control outcome mean	0.67	0.67	0.04	0.04	0.13	0.13
Control outcome std. dev.	0.47	0.47	0.21	0.21	0.34	0.34
Trading center × Post election FEs		✓		✓		✓
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM parties		**Opposition parties**		**Difference in thermometer**	
VPN × Post election	0.095	0.285	-0.334*	-0.438**	0.429*	0.723***
	(0.196)	(0.208)	(0.172)	(0.181)	(0.255)	(0.251)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.58	0.63	0.56	0.60	0.61	0.65
Control outcome mean	5.95	5.95	4.84	4.85	1.11	1.10
Control outcome std. dev.	2.67	2.67	2.50	2.50	4.03	4.03
Trading center × Post election FEs		✓		✓		✓
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
VPN × Post election	0.313***	0.258**	0.017	-0.044	0.296*	0.302*
	(0.112)	(0.121)	(0.116)	(0.122)	(0.152)	(0.154)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.55	0.61	0.55	0.59	0.58	0.62
Control outcome mean	3.46	3.46	3.02	3.03	0.44	0.43
Control outcome std. dev.	1.40	1.40	1.45	1.45	2.03	2.03
Trading center × Post election FEs		✓		✓		✓
Panel E: Indicators for self-reported voting for NRM						
	Voted NRM for MP		**Voted NRM for LC5**			
VPN × Post election	0.053	0.037	0.035	0.035		
	(0.055)	(0.058)	(0.038)	(0.041)		
Observations	910	864	1,904	1,886		
R^2	0.64	0.77	0.61	0.65		
Control outcome mean	0.50	0.51	0.52	0.52		
Control outcome std. dev.	0.50	0.50	0.50	0.50		
Trading center × Post election FEs		✓		✓		

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

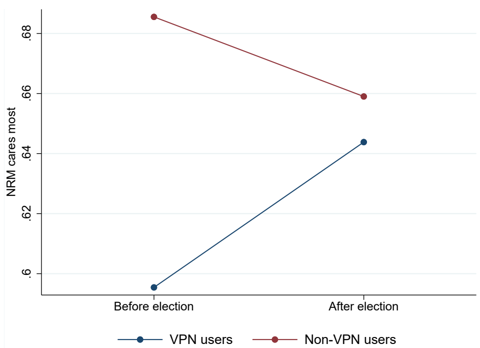
Figure 6: Changes in NRM support, by baseline VPN use



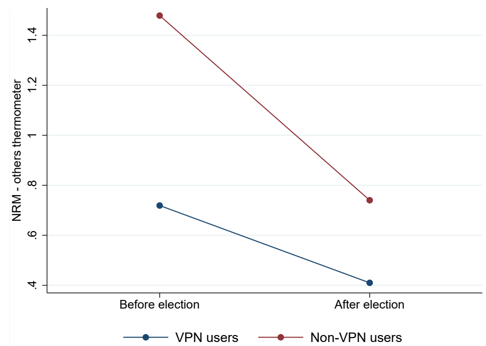
(a) Support NRM ICW index



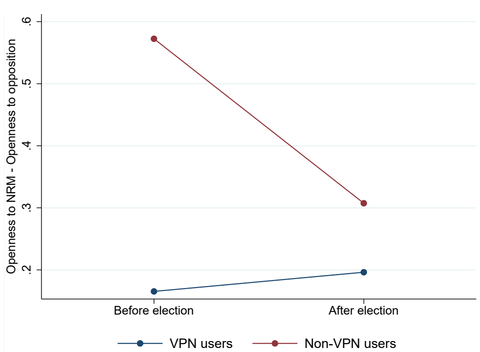
(b) Support opposition ICW index



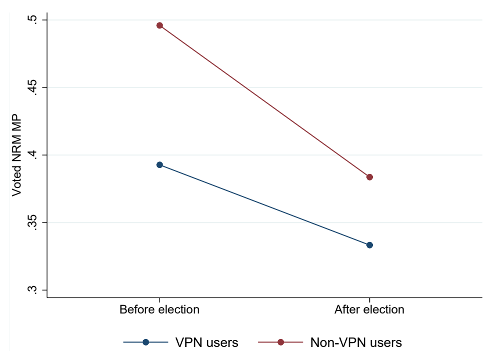
(c) NRM cares most about people like you



(d) Differential feeling toward NRM relative to opposition



(e) Differential openness to vote NRM relative to opposition



(f) Voted NRM for MP

towards voting for the NRM in the future on a 5-point scale. These point estimates are quite substantial in magnitude, with our support for the NRM index outcome in columns (1) and (2) of panel A increasing by almost 0.2 standard deviations among VPN users relative to non-VPN users. We also observe non-trivial, but not statistically significant, increases of about 4 percentage points in the probability of reporting having voted for NRM candidates for MP and LC5 in the 2021 elections.

In contrast, VPNs users became less favorable toward opposition parties by the midline survey. Columns (3)-(6) of panel B indicates that increased faith in the NRM appears to be drawn roughly equally from individuals that previously thought the FDC and NUP cared most about the welfare of Ugandans. The substantial drop in warmth toward opposition parties in columns (3) and (4) of panel C suggests that more favorable views toward the NRM may largely be driven by more negative views toward opposition parties. Together, panel A shows that the index capturing support for opposition parties decreases by around 0.1 standard deviation. The final columns of panels A, C, and D confirm these net shifts by examining differences in appraisal of the NRM relative to opposition parties.

In sum, these results show that individuals who were more likely to use social media during the election-time ban became relatively *more* supportive of the incumbent NRM party and opposed to the opposition FDC and NUP parties. Moreover, because post-election surveys were enumerated several months after the election and social media use largely reverted back to normal after the social media ban ended, our estimates may underestimate the effect at election time. At least during this politically-consequential period of elevated censorship, our findings suggest that social media does not prove to be an anti-authoritarian "liberation technology" supporting opposition candidates that struggle to reach voters through incumbent-dominated traditional means. Rather, under the threat of potential sanctions for violating the social media ban, access to social media strengthened NRM support among initially opposition-leaning VPN users.

4.4 Robustness checks and alternative interpretations

These increases in support for the NRM at the expense of opposition parties are robust across various alternative specifications and interpretations. First, we address potential parallel trend violations and compound treatment concerns in several ways. As the even columns in Table 3 illustrate, our results are robust to focusing on within-trading center variation, and thus comparing individuals that vary in VPN within the same area. Furthermore, Table 4 shows that our results are similarly unchanged after adjusting for interactions between period and baseline levels of respondent age, prior political news consumption, and political knowledge (panel A) as well as prior support for the NRM (panel B).[12] While VPN use may correlate with other respondent characteristics, these tests indicate that increased support for the NRM is driven by access to social media rather than trends support for the NRM driven by young people, politically-engaged citizens, anti-NRM respondents, or people in particular areas between our baseline and midline surveys.

[12] The interactions with various measures of prior support for the NRM should be treated with some caution, given that these moderators are themselves functions of the baseline outcomes. Partialing out part of the baseline outcome could introduce bias by undoing the difference-in-differences design.

Table 4: Differential effects of VPN use on support for the NRM after the social media ban, conditional on covariate × period fixed effects

	Outcome: NRM support ICW index									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Non-political support interactive covariates										
VPN × Post election	0.140**	0.170**	0.109*	0.161**	0.177**	0.194**	0.134*	0.164**	0.218*	0.272*
	(0.070)	(0.074)	(0.063)	(0.066)	(0.072)	(0.075)	(0.069)	(0.070)	(0.114)	(0.140)
Observations	2,620	2,612	2,620	2,612	2,620	2,612	2,620	2,612	910	864
R^2	0.63	0.67	0.69	0.73	0.64	0.68	0.64	0.69	0.63	0.71
Control outcome mean	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Control outcome std. dev.	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Trading center × Post election FEs		✓		✓		✓		✓		✓
Baseline covariate × Post interaction	Age FEs		Political news consumption		Political knowledge		Govt/NRM survey		Enumerator	
Panel B: Political support interactive covariates										
VPN × Post election	0.140**	0.170**	0.109*	0.161**	0.177**	0.194**	0.134*	0.164**	0.218*	0.272*
	(0.070)	(0.074)	(0.063)	(0.066)	(0.072)	(0.075)	(0.069)	(0.070)	(0.114)	(0.140)
Observations	2,620	2,612	2,620	2,612	2,620	2,612	2,620	2,612	910	864
R^2	0.63	0.67	0.69	0.73	0.64	0.68	0.64	0.69	0.63	0.71
Control outcome mean	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Control outcome std. dev.	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Trading center × Post election FEs		✓		✓		✓		✓		✓
Baseline covariate × Post interaction	NRM		Openness to NRM		NRM thermometer		NRM LC5 vote intent		NRM MP vote intent	

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects as well as interactions between period and the covariate(s) listed at the foot of each regression. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Second, we obtain similar results using alternative operationalization of VPN use. Appendix Table A4 reports similar—if not stronger—results using our adaptive lasso predictor of social media use during the ban, while Tables A5 and A6 show that our estimates are robust to defining regular VPN users as those who report using a VPN either at least two or at least three days in the week prior to baseline enumeration.

Third, an alternative interpretation of the results is that self-reported beliefs reflect socially-desirable, rather than meaningful, responses. This would upwardly bias our estimates if experimenter demand differs across VPN and non-VPN over time. In particular, violators of the social media ban may fear punishment and inaccurately profess greater support for the NRM in our midline survey to compensate for this risk. However, there are several reasons to doubt this concern. First, respondents were not aware that we tracked public WhatsApp activity and were clearly informed that their data would not be shared beyond the research team; consequently, they are unlikely to perceive a need to misreport their support for the NRM. Second, Appendix Table A3 shows that VPN users became no more likely to believe the survey firm had been sent by the government or the NRM. Third, Table 4 further shows that our results are robust to adjusting for the interaction between survey period and an indicator for the 12% of respondents who thought at baseline that the government or NRM sent enumerators to conduct the survey.

Finally, we address another alternative potential interpretation of the results—that non-VPN users were more likely to sanction the government for engaging in censorship (Kronick and Marshall, 2022). While the social media ban affected everyone in terms of restricting access to social media and intimidation, its impact on the livelihoods of people without VPNs could have been greater. Individuals without VPNs either incurred the time and/or financial costs of acquiring a VPN or faced a greater likelihood of loss of livelihood, consumption opportunities, social interaction, information, or entertainment content during the social media ban. Under this interpretation, non-VPN users may then have become relatively less supportive of the NRM, rather than VPN users becoming more supportive of the NRM.

To evaluate this possibility, we leverage a sequence of midline survey questions that asked respondents about how they were affected by the social media ban, which was notably longer than the 2016 social media ban. We asked respondents if the internet restrictions affected their business/job (40% of respondents), ability to purchase goods and services (10%), ability to talk to friends and family (78%), ability to find reliable news (58%), and ability to consume online entertainment content (18%). These questions proxy for the costs of social media censorship. These costs are weakly correlated with prior VPN usage, with lost access to news and entertainment registering the greatest associations. To investigate the effects of suffering from the social media ban in these various ways, we estimate the following difference-in-differences regression:

$$Y_{ict} = \tau(Censorship_i \times Post\ election_t) + \mu_i + \eta_t + \varepsilon_{ict}, \tag{2}$$

where $Censorship_i$ captures a particular self-reported cost of censorship.

The results in Table 5 suggest that the costs of the social media ban are unlikely to drive the differential changes in NRM support we observe. The odd columns show that respondents that reported experiencing significant costs associated with the social media ban generally did not become less supportive of the NRM. The only exception is among respondents who noted that the ban restricted their access to news. However, in line with the limited correlation with VPN use, adjusting for the interaction between regular VPN use and the post-election survey in the even columns does not significantly alter these estimates. Consequently, these estimates suggest that social media users may have reacted somewhat against government censorship of news, but that this reaction is unlikely to confound the effects of access to social media during the ban.

5 Access to social media six months after an election

Our difference-in-differences estimates show that social media access *during the social media ban* period differentially improved VPN users' attitudes towards the incumbent NRM party. Following the conclusion of the electoral cycle and the President Museveni's inauguration, we augment these results by conducting a randomized intervention that aimed to facilitate social media usage during a non-election period by alleviating the financial costs of access.

5.1 Experimental design

After completing the midline survey in late May/early June 2021, we randomly assigned respondents to receive a financial incentive intended to increase their social media use for three months. Treated participants were compensated for taking the midline survey with payments designed to combat the two main financial barriers preventing participants from using social media as much as they would like: the OTT tax and the cost of mobile data. In June, this entailed: (i) paying the OTT tax for the month (which had a value of around UGX 6,000 or $1.63); and (ii) providing mobile data to encourage usage—1.5GB for the month for Airtel network users (costing UGX 10,000) or 500MB a week for MTN network users (costing UGX 5,000 a week for four weeks).[13] For the remaining eight weeks in July and August, after the OTT tax was replaced by a mobile data tax,

[13] Airtel and MTN are easily the two largest mobile network operators in Uganda, with everyone in our sample using one of the two networks.

Table 5: Differential effects of censorship experience on support for the NRM after the social media ban

	NRM support index (ICW)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Ban affected job × Post election	0.142*	0.140*										
	(0.074)	(0.075)										
Ban affected purchases × Post election			-0.087	-0.086								
			(0.131)	(0.130)								
Ban affected social interactions × Post election					-0.142	-0.151*						
					(0.090)	(0.089)						
Ban affected access to news × Post election							-0.175***	-0.172***				
							(0.065)	(0.064)				
Ban affected affect to entertainment × Post election									-0.063	-0.075		
									(0.086)	(0.087)		
Summed effects of ban × Post election											-0.026	-0.029
											(0.036)	(0.036)
VPN × Post election		0.182**		0.184**		0.189**		0.181**		0.187**		0.185**
		(0.072)		(0.073)		(0.073)		(0.072)		(0.072)		(0.073)
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620
R^2	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62
Ban variable mean	0.40	0.40	0.10	0.10	0.78	0.78	0.58	0.58	0.18	0.18	2.12	2.12

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Lower order terms are omitted to save space. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

treated Airtel users then received 400MB a week (costing UGX 3,500 a week) and treated MTN users received 500MB a week (still costing UGX 5,000 a week).[14] Each transfer was accompanied by an SMS message informing individuals of the payment. Respondents were free to use the mobile data as they liked but, given both the prominence of social media and respondents' financial barriers to access, we anticipated the incentive would increase their usage.

To limit differential attrition risks and mitigate potential income effects, individuals assigned to the control condition were instead compensated with a UGX 6,000 mobile money transfer. Recipients of mobile money can in principle use their transfer to purchase airtime or pay the OTT tax, but we expected them to use this more flexible transfer to make other types of purchases using mobile money or withdraw the funds for other purchases. Our experimental treatment conditions were communicated as a generous reimbursement for study participation necessitated by the impossibility of in-person enumeration.[15]

Using baseline survey data, treatment conditions were block-randomized prior to the midline survey to increase the precision of estimates. For the vast majority of the sample that had been enumerated at baseline ($n = 1,310$), we first stratified at the district $\times$ cell phone network-level before constructing blocks of size 8 within each stratum. We blocked on a vector of predetermined covariates, including measures of their social media usage, COVID-19 knowledge, the extent of their social interactions online and offline, subjective welfare, and attitudes towards the ruling NRM party. For the residual sample who had not been enumerated at baseline and so for whom we lacked covariates observed prior to the midline survey ($n = 145$), we assigned these participants to treatment using complete randomization within strata defined at the district $\times$ cell phone network-level.

To estimate average treatment effects (ATEs) of the treatment with our endline survey data, we estimate pre-registered OLS regressions of the form:[16]

$$Y_i^{post} = \tau Treat_i + \alpha \mathbf{Y}_i^{pre} + \beta_b + \gamma_e + \varepsilon_i, \tag{3}$$

where Y_i^{post} is an endline survey outcome, $\mathbf{Y}_i^{pre}$ is a vector of pre-treatment outcomes (where we use both baseline and midline survey responses, where available), $Treat_i$ indicates receiving our

[14] Mobile data can only be used for using the internet, which predominantly consists of social media use in Uganda. To further encourage social media use, we asked respondents to send examples of interesting content that they encountered on social media via Facebook Messenger or WhatsApp to a project-affiliated WhatsApp account.

[15] Tokens of appreciation for completing surveys are commonplace, typically in the form of small household items or small amounts of cash.

[16] Our full pre-analysis plan is available at www.socialscienceregistry.org/trials/8267. We restrict attention to political outcomes in this paper. We deviate from our pre-specified measures of political support outcomes to match the difference-in-differences analysis, but present similar results for our pre-specified outcome indices in Appendix Figures A4 and A5.

treatment, β_b are randomization block fixed effects, and γ_e are endline enumerator fixed effects. In auxiliary specifications, we add a vector of 'double-selected' predetermined covariates, $\mathbf{X}_i$, following Belloni, Chernozhukov and Hansen (2014) which are selected using LASSO and defined prior to treatment.[17] We use robust standard errors for inference, reflecting the individual-level randomization. When using behavioral measures of WhatsApp usage, for which multiple observations per respondent are available (including during the pre-treatment period), we follow our prior panel analysis by instead including randomization block $\times$ period fixed effects, β_{bt}, to increase estimation precision. Standard errors are then clustered at the respondent level.

Before turning to the results, we first validate the experimental design. First, Appendix Table A7 shows that endline participants assigned to treatment are statistically indistinguishable from those assigned to control across 17 predetermined covariates. Second, Table A8 demonstrates that there is no differential attrition across treatment conditions between the midline and endline surveys three months later, with overall attrition rates very low (around 5%).

5.2 Increases in social media use during a non-electoral intervention period

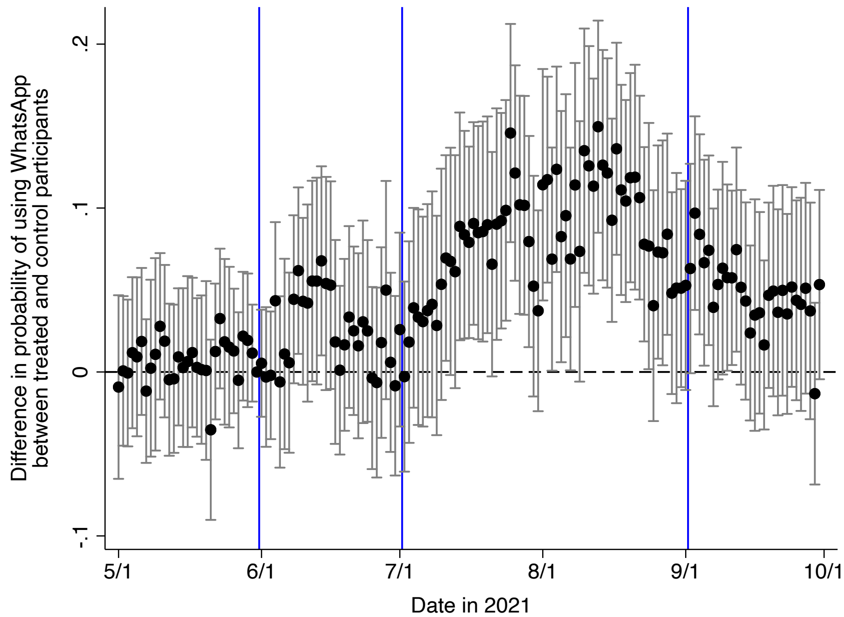
We first assess the extent to which the intervention affected rates of social media usage in the three months following the midline survey. Leveraging the same behavioral WhatsApp data employed in Figure 5, we estimate a panel version of equation (3) in Figure 7. The outcome is again whether a given respondent was "last seen" using WhatsApp on a given date, and we similarly restrict the sample to participants we are able to link to active WhatsApp accounts with publicly-viewable WhatsApp statuses prior to the midline survey.

The day-level estimates demonstrate that WhatsApp usage increased significantly during the treatment period, in contrast with balanced rates of usage in the pre-treatment period. We distinguish the month of June, in which we sent most of our sample a single large data transfer (without reminders) and hence treatment effects dissipated quite quickly, from July and August when we sent smaller weekly data transfers, which led the treatment effects to be more sustained. The persistence of modest, if somewhat dissipating, treatment effects in September provides evidence of sustained behavioral increases in WhatsApp usage following the conclusion of the treatment. In line with prior studies (e.g. Chen and Yang, 2019), this suggests that social media use begets greater demand for further use.

Panel A of Table 6 shows that, pooling across the full treatment period, treated participants were

[17] The superset of all potential covariates, $\mathbf{X}_i^+$, consists all variables from baseline or midline surveys with full data coverage along with trading center fixed effects (which we prespecified as an auxiliary specification in our preanalysis plan). From this superset, $\mathbf{X}_i$ is defined as the union of all covariates selected by LASSO when (1) $Treat_i$ is predicted by $\mathbf{X}_i^+$; (2) Y_i^{post} is predicted by $Treat_i$ and $\mathbf{X}_i^+$. This follows the 'double selection' approach of Belloni, Chernozhukov and Hansen (2014).

Figure 7: Differences in daily use of WhatsApp, by treatment assignment



Notes: Estimates are from equation (3), where the outcome is daily use of WhatsApp. The baseline category is 31 May. Treatment begins around 1 June. Revised weekly treatment begins around 1 July. Treatment ends around 1 September. All bars indicate 95% confidence intervals.

5.1 percentage points more likely to use WhatsApp on a given day, relative to an average probability among the control group of 0.17. This estimated treatment effect is similar in absolute magnitude to that of the VPN-based analysis around the election, though general rates of WhatsApp usage were lower in June, July, and August relative to the election period in January—in spite of the social media ban. The differences for WhatsApp may be small compared to more mobile data-intensive social media platforms. The experimental estimates are robust to the inclusion of trading center $\times$ date fixed effects (column 2), block $\times$ date fixed effects (column 3), and the use of an intensive margin measure of usage (columns 4-6).

While uptake rates are not trivial in magnitude, the WhatsApp auditing data suggests that many treated respondents appear not to have significantly altered their social media use. To identify the respondents who were most responsive to treatment, we used an adaptive LASSO model to select the treatment-by-covariate interactions that best predict WhatsApp usage during the intervention period (always retaining randomization block fixed effects). We then predict individual-level treatment effects by subtracting the model's predicted outcome under control from its predicted outcome under treatment. Although this was not pre-specified, our subsequent analyses will consider how treatment effects vary as the respondents predicted to respond least to treatment (i.e. those with relatively weaker "first stage" effects) are trimmed from our estimation sample.

Table 6: Experimental treatment effects on daily WhatsApp usage

	Used Whatsapp			Number of times used Whatsapp		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.051***	0.053***	0.054***	0.073***	0.075***	0.077**
	(0.016)	(0.017)	(0.019)	(0.026)	(0.028)	(0.030)
N	115654	113960	112112	115654	113960	112112
R^2	0.45	0.54	0.58	0.40	0.50	0.55
Control mean	0.17	0.18	0.17	0.28	0.28	0.28
Control SD	0.38	0.38	0.38	0.69	0.70	0.69
Interactive FEs		TC	Block		TC	Block

Each specification is estimated using OLS, and includes individual and date fixed effects. We exclude all dates following the conclusion of treatment on 1 September. Standard errors clustered by respondent are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Partisan-moderated effects of access to social media on NRM support

Next, we estimate the effects of treatment assignment on the same set of outcomes considered for the election-time social media ban in Table 3. The endline results outside of the election campaign period are shown in Table 7, which reports estimates of τ from equation (3).[18]

In contrast with the difference-in-differences analysis, we find little evidence that increased access to social media during the non-election period induced favorable attitudes towards the incumbent NRM party on average. In panel A, using indexes of attitudes towards parties, we find modestly negative effects on attitudes towards NRM with null effects on attitudes towards opposition parties. In panel B, we fail to reject the null hypothesis of no treatment effects on beliefs about which party cares most about citizen well-being. Panel C reports noisy evidence that respondents feel less warmly about both the NRM party and (less so) about opposition parties. In panel D, we find little evidence of changes in respondents' openness to voting for different political parties in the future.

However, these limited average effects could mask heterogeneous responses. Following our pre-analysis plan, we focus on potential moderation by prior partisanship. Due to greater opposition content on social media in "normal" times—relative to traditional media that is more tightly controlled by the incumbent regime—it is possible that initial NRM supporters may be exposed to more critical content, even outside of election campaigns. As our election-time results for opposition-leaning VPN users show, it is also possible that NRM opponents might also be exposed

[18]We exclude the outcomes from panel D of Table 3, since we did not ask about vote choice in the prior election again.

Table 7: Experimental treatment effects on NRM support

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
Treatment	-0.060	-0.093*	-0.014	0.011	0.005	-0.035
	(0.050)	(0.049)	(0.048)	(0.047)	(0.050)	(0.049)
Observations	1389	1331	1389	1331	1389	1331
Additional controls		✓		✓		✓
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
Treatment	-0.008	-0.028	-0.003	-0.001	-0.006	0.007
	(0.022)	(0.020)	(0.011)	(0.011)	(0.019)	(0.018)
Observations	1389	1331	1389	1331	1389	1331
Control mean	0.72	0.72	0.05	0.05	0.17	0.17
Control SD	0.45	0.45	0.23	0.23	0.38	0.37
Additional controls		✓		✓		✓
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM party		**Opposition parties**		**Difference in thermometer**	
Treatment	-0.200	-0.262*	-0.068	-0.087	-0.146	-0.168
	(0.140)	(0.138)	(0.133)	(0.129)	(0.223)	(0.221)
Observations	1389	1331	1389	1331	1389	1331
Control mean	5.96	5.94	5.41	5.42	0.54	0.53
Control SD	2.79	2.77	2.64	2.64	4.55	4.52
Additional controls		✓		✓		✓
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
Treatment	-0.091	-0.176**	-0.002	0.069	-0.087	-0.157
	(0.073)	(0.072)	(0.071)	(0.068)	(0.105)	(0.102)
Observations	1389	1331	1389	1331	1389	1331
Control mean	3.35	3.35	3.08	3.09	0.27	0.26
Control SD	1.44	1.43	1.43	1.43	1.98	1.98
Additional controls		✓		✓		✓

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Even-indexed columns add LASSO-selected controls following Belloni, Chernozhukov and Hansen (2014). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

to pro-NRM content online as well. We measure NRM partisanship using an indicator defined at midline for whether a given respondent either (i) reported voting for an NRM candidate for MP; or (ii) if they did not disclose (i), then indicated they felt warmer towards NRM than opposition parties and that they overall felt warmly towards NRM. This operationalization assigns 42% of our sample as NRM partisans, with non-NRM partisans split between opposition supporters and non-partisan individuals expressing only weak attitudes towards either the NRM or opposition parties.[19]

Disaggregating the experimental results by respondent partisanship (measured at midline), we find some evidence that effects of access to social media are moderated by partisan allegiance in Table 8. Odd-numbered columns subset to NRM partisans while even-numbered columns subset to non-NRM partisans. Respondents that supported the NRM at midline initially viewed their party more favorably, and the opposition less favorably, for each outcome relative to non-NRM supporters. However, after treatment, panel A shows that treated NRM partisans came to view their party more negatively by 0.14 sd ($p < 0.05$) with more muted effects among non-NRM partisans. Panel C shows that treated NRM partisans then came to view their party more negatively (column 1) and opposition parties more positively (column 3). Moreover, the difference between the two shrank by around half (column 5). Panels B and D suggest a similar, but noisier, pattern of results for NRM partisans across different outcomes.

The moderating effect of prior partisanship becomes starker as we restrict attention to respondents predicted to respond most to the treatment by increasing their social media usage. Figure 8 reports the conditional average treatment effect on NRM attitudes as we trim the estimation sample according to the percentile, between the 0th and 50th, of a given respondent's predicted treatment effect on social media usage. We estimate standardized effects on the full sample (row 1), then split the estimation sample according to partisanship (rows 2-3). Among the full sample, the modest negative effects on NRM attitudes become largest when we trim the bottom 20% of the sample before shrinking again. As the third row shows, negative treatment effects on an index of NRM support become substantially more pronounced among NRM partisans as we exclude respondents less likely to comply with the treatment. This is driven by changes in their perceptions of the NRM caring about welfare (column 2) and their openness to voting for the NRM in the future (column 4). Non-NRM partisans, by contrast, show very modestly negative treatment effects on their attitudes towards the NRM, which remain substantively small and statistically insignificant as we exclude respondents predicted to increase their social media the least in response to the treatment.

Together, these results provide some evidence that experimentally inducing individuals to consume more social media outside of election campaigns moderates their political views. Especially among initial NRM supporters, treated citizens became less favorable toward the NRM and more

[19] As noted in Figure 3, this proportion maps relatively closely to the share of NRM partisans among individuals with internet-connected phones in Uganda.

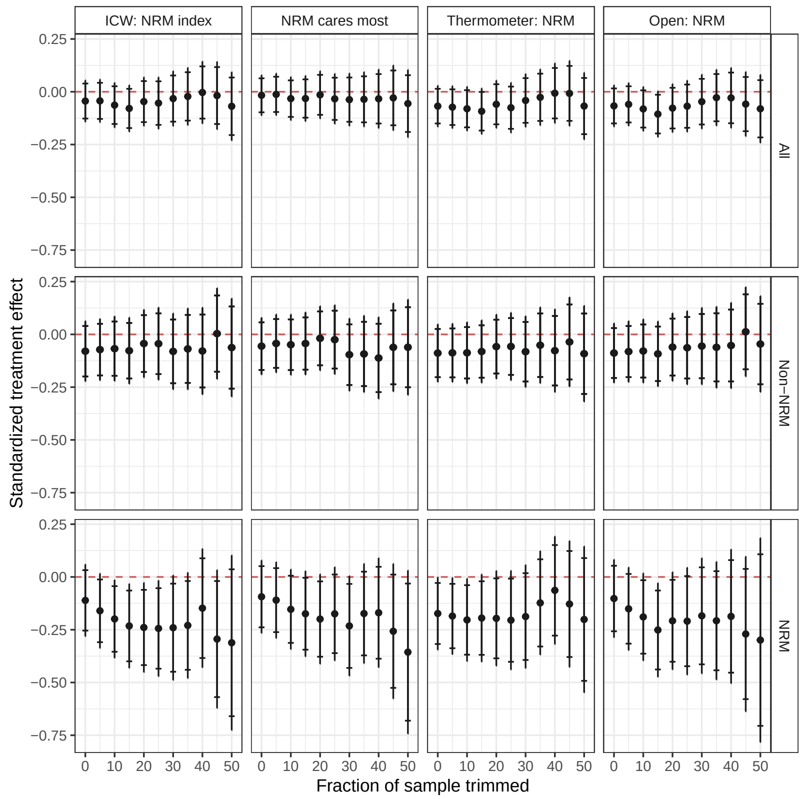
Table 8: Experimental treatment effects on NRM support, subset by partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
Treatment	-0.144**	-0.083	0.056	0.020	-0.111	-0.039
	(0.073)	(0.077)	(0.080)	(0.074)	(0.079)	(0.074)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
Treatment	-0.038	-0.027	0.007	0.008	0.006	-0.005
	(0.036)	(0.033)	(0.018)	(0.017)	(0.028)	(0.031)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	0.80	0.67	0.05	0.06	0.10	0.22
Control SD	0.40	0.47	0.21	0.24	0.31	0.41
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM party		**Opposition parties**		**Difference in thermometer**	
Treatment	-0.417*	-0.272	0.526**	-0.354*	-0.931**	0.164
	(0.218)	(0.195)	(0.252)	(0.196)	(0.378)	(0.341)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	6.45	5.61	4.69	5.92	1.77	-0.32
Control SD	2.65	2.83	2.56	2.58	4.26	4.56
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
Treatment	-0.102	-0.110	0.078	0.009	-0.160	-0.094
	(0.119)	(0.109)	(0.130)	(0.104)	(0.191)	(0.138)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	3.55	3.21	3.00	3.14	0.55	0.07
Control SD	1.36	1.47	1.45	1.42	1.87	2.04

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

The NRM indicator is defined as endline respondents who either: (i) reported voting for an NRM candidate for MP at midline survey; or (ii) if they did not disclose (i), then indicated they felt warmer towards NRM than opposition parties and that they overall felt warmly towards NRM.

Figure 8: Conditional average treatment effect, by midline NRM partisanship



Notes: The estimates in Figure 8 derive from equation (3) estimated in the full sample (row 1) and partisan subsamples (rows 2-3), where we trim varying proportions of the sample from the estimation according to the percentile of their predicted treatment effect on social media usage. Column 1 uses an ICW index of the variables from columns 2-4. 95% confidence intervals plotted.

favorable toward opposition parties after three months of elevated social media access. This finding more closely aligns with the hopes of some that social media might buttress opposition movements in electoral authoritarian regimes. There is some, but far weaker, evidence to suggest that opposition supporters also moderates their view, as young opposition-leaning VPN users did during the social media ban. These findings chime with earlier evidence from Ghana (Conroy-Krutz and Moehler 2015) and Kenya (Brierley, Kramon and Ofosu 2020) that counter-attitudinal exposure can moderate—rather than polarize—opinions in relatively new democracies in the Global South.

6 Mechanisms connecting social media access with political attitudes

Both our difference-in-differences and experimental analyses suggest that access to social media can shape Ugandans' political attitudes, but in seemingly different ways. Predominantly non-NRM VPN users became relatively more favorable towards the NRM than non-VPN users after experiencing differentially greater social media exposure during the election-time social media ban. In contrast, individuals—especially NRM supporters—became slightly less favorable toward the NRM after three months of elevated exposure to social media outside of an election campaign.

Having earlier shown that the election-time results are not simply driven by sanctioning of government censorship, we next consider two classes of explanation for the different findings across interventions. The first is that VPNs and OTT/mobile data payments induced different types of people to consume more social media, and these people reacted differently to the same types of online content. The second relies on differences in the content encountered on social media between the election-time social media ban and "normal" times.

6.1 Differences in the types of people affected by each intervention

One potential reason for the differences in effects across interventions relies on different groups of "compliers". Even though we focus on the same sample of respondents and find effect sizes on WhatsApp usage of comparable magnitude across the designs, the *types* of people that were induced to consume more social media by possessing VPNs in the difference-in-differences analysis might differ from the types of people that were induced to consume more social media by alleviating financial barriers in the experimental analysis. If so, differences in effects could reflect heterogeneity in the effect of social media exposure by respondent type.

First, we consider whether variation in the estimation sample between the two research designs might explain the differences in the results. To explore this possibility, Appendix Table A10 restricts the experimental sample to consist either of: (i) the subset of endline respondents who also feature in the difference-in-differences analysis (i.e. those who also completed the baseline as well as the midline survey); or (ii) the subset of endline respondents who were regular VPN users prior to the baseline survey. In each case, we find similar patterns of results. This suggests that the more muted average effect of access to social media outside the election period is not driven by changes in sample composition.

Second, we consider the extent to which the characteristics of compliers from the first research design overlap with those from the second. We do this by assessing how the cross-sectional imbalances in VPN usage described in Table 1 *also* apply to respondents in the experimental sample predicted to respond most strongly to the treatment according to the adaptive LASSO exercise. In Appendix Figure A6, we find some differences in their demographic characteristics—for example, younger respondents were more likely to increase their use of social media during the ban relative to during the experimental intervention, while men were relatively more responsive to the randomized intervention but no more responsive during the social media ban. We find little difference in terms of their education or perceived living standards. Further, across our key outcome measures, the differences between those induced to increase their social media usage across the designs tend to covary: across both designs, compliers are likely to have been using social media slightly more prior to each treatment and to have had slightly worse attitudes towards the government.

Third, while this exercise suggests that complier characteristics are reasonably similar along pertinent dimensions, we assess the extent to which residual variation in observable complier characteristics can reconcile the effect estimates.[20] In the spirit of Angrist and Fernandez-Val (2013), Aronow and Carnegie (2013), and Hotz, Imbens and Mortimer (2005), we do this by reweighting our treatment effect estimates on positive attitudes toward the NRM by the inverse of the strength of the "first stage" effect on a given respondents' social media usage, such that we downweight participants who responded strongly to the treatment and upweight participants who responded weakly.[21] We construct the weights using the two adaptive LASSO measures we describe above, each of which can be interpreted as the predicted change in a given respondents' social media usage either due to the social media ban (in the difference-in-differences design) or due to the social media subsidy treatment (in the field experiment). Since these predicted changes are sometimes of different directions, we weight using the inverse of the percentile of the predicted change in social media usage. Appendix Figure A7 reports the resulting estimated treatment effects on our ICW index of NRM support and its subcomponents. Overall, reweighting the treatment effects to account for variation in respondents' propensities to increase their use of social media across the two designs has little effect on the resulting estimates.

Together, these exercises imply that differences in the estimated effects on attitudes towards the incumbent party are because access to social media across the two time periods of the study represents a fundamentally *different treatment*, rather than a similar treatment impacting different

[20] In principle, variation in *unobservable* characteristics predicting selection into increased social media usage might also account for variation in the treatment effects on political attitudes (Mogstad and Torgovitsky, 2018). We consider this to be relatively unlikely in our setting: our *observed* complier characteristics across designs, along with the substantive magnitude of our "first stage" coefficients, are far more similar than other empirical settings where variation in unobserved characteristics have proven useful for reconciling treatment effect estimates (Kowalski, 2019).

[21] Such an exercise only recovers the average treatment effect assuming full compliance under strong assumptions—that treatment effects are constant across individuals with the same observed characteristics, or that treatment effects vary but individuals do not self-select into compliance based on their unobserved gain from social media use (Brinch, Mogstad and Wiswall, 2017)—and so we only use the weighted estimates as an intuitive way to control for compliance propensities rather than to accurately extrapolate our treatment effects.

parts of our sample.

6.2 Differences in social media content across interventions

A more substantive explanation as to why election-time access to social media may have produced different results from access outside of election campaigns is due to differences in the content users were exposed to. Such differences could naturally arise around elections or be consequences of the election-time social media ban. We consider two salient ways in which content during the social media ban may have been distinctive from earlier and later content: degree of favorability toward the NRM; and coverage of the election campaign and election malpractice.

6.2.1 Favorable content about the NRM

By preventing non-VPN users from using social media and creating the threat of potential sanctions for violators (likely expected to be imposed differentially by partisanship), exposure to Facebook and WhatsApp content that was favorable toward the NRM could have increased during the social media ban. Relative to the normal flow of opposition-skewed social media content, this may have in turn increased positive perceptions of NRM governance or the risks of opposition rule.

We start appraising this possibility by examining trends in political content on Facebook.[22] As noted above, we collected publicly-accessible Facebook posts from individual and group accounts between the formal beginning of the election campaign on November 8, 2020 and the beginning of endline survey enumeration on September 3, 2021. Since we do not observe activity on small accounts, this sample is not necessarily representative of all the content on Facebook. To address this limitation, we focus on examining changes in account activity and the content of such activity over time, comparing our two intervention periods with other periods.

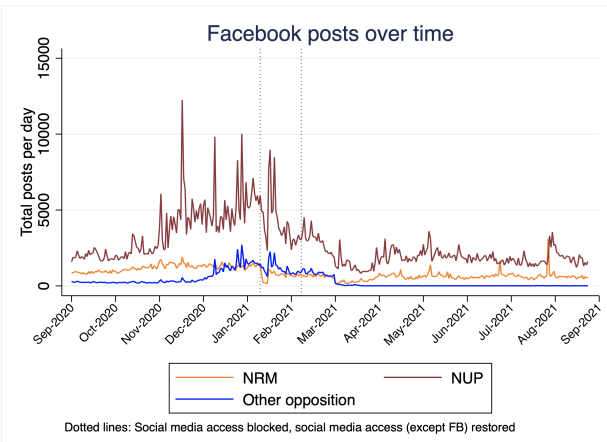
Figure 9 reports trends in Facebook post counts, views of posted videos, and interactions with posts such as comments or likes sourced from Crowdtangle.[23] The descriptive data unsurprisingly indicate that activity of all kinds dipped during the social media ban, but the drop varied by account partisan affiliation. NUP and other opposition Facebook accounts became less likely to post content during the ban than government and NRM accounts, while views and interactions with such content

[22] The content of posts on WhatsApp are not publicly accessible.

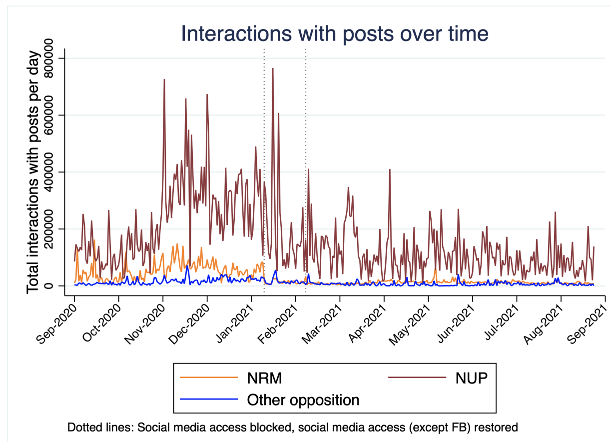
[23] Crowdtangle tracks all Facebook accounts with more than 25,000 followers or public accounts otherwise specifically added to Crowdtangle by researchers. We show data for pages and groups affiliated with (1) the incumbent NRM party ($n = 74$), (2) the currently strongest opposition party, NUP ($n = 163$); and (3) other opposition parties ($n = 18$), each with at least 1,000 Facebook followers. In panel d) we show data for the 127 MP candidates who have tracked public Facebook pages which anyone can follow and thus arguably have the greatest reach. We are also in the in the process of scraping the remaining accounts.While non-representative, we consider the Crowdtangle corpus to cover a reasonably comprehensive set of the major Facebook accounts of relevant political actors in Uganda.

also declined in relative terms. Compared with the average government/NRM account, the average opposition account posted 13 fewer times, received more than 2,000 fewer views, and received around 700 fewer interactions per day—these differentials constitute 4-10% of the baseline mean. In contrast, pro-government and pro-opposition posts and engagement had largely reverted to stable pre-election campaign levels by June 2021. This suggests that the quantity of social media content engaged with likely became less skewed toward the opposition during the social media ban, but had returned to normal by the time of the experimental intervention.

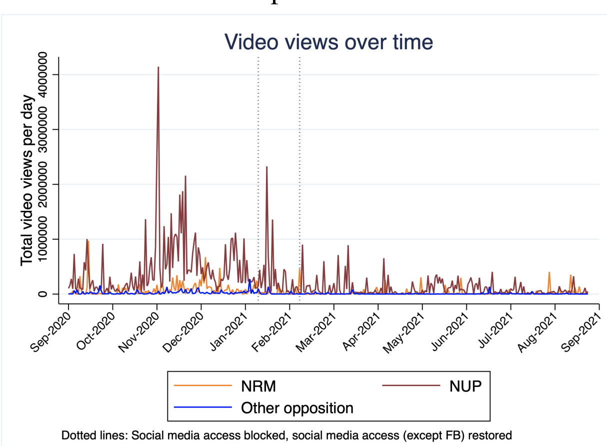
Figure 9: Facebook posts, interactions, and views by accounts



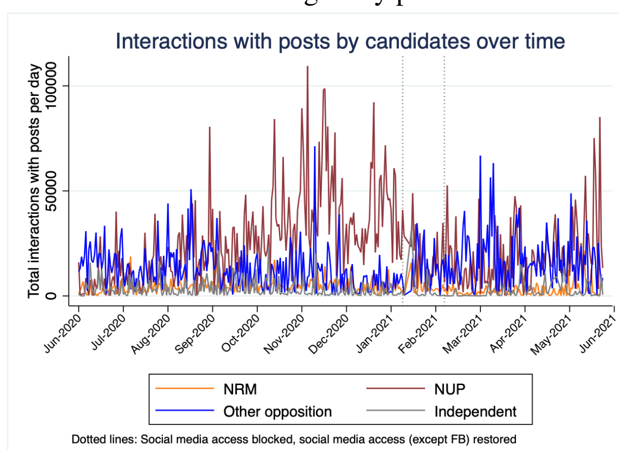
(a) Number of posts by different actors during study period



(b) Number of interactions with posts by different actors during study period



(c) Views of videos posted by different actors during study period



(d) Interactions with posts by MP candidates over time,by partisanship of candidates

While political content on social media likely became relatively more favorable toward the NRM on balance, this does not necessarily imply that citizens were swayed by this content after a brutal election campaign. We next assess whether favorable content persuaded respondents by examining changes in posterior beliefs about NRM government performance. Panel A of Table 9

Table 9: Potential mechanisms driving election-time changes in NRM support

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Assessments of government performance						
	Central government		**District government**		**Subcounty government**	
VPN × Post election	0.051	0.121	0.015	0.017	-0.076	-0.076
	(0.081)	(0.092)	(0.079)	(0.083)	(0.088)	(0.092)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.55	0.61	0.57	0.61	0.54	0.59
Control outcome mean	3.28	3.28	3.15	3.14	3.08	3.08
Control outcome std. dev.	1.15	1.15	1.10	1.10	1.14	1.14
Trading center × Post election FEs		✓		✓		✓
Panel B: Negative perceptions of democracy in Uganda						
	Democracy with major problems		**National government officials**		**Opposition politicians**	
VPN × Post election	-0.029	-0.046	-0.041	-0.022	-0.075**	-0.086**
	(0.031)	(0.032)	(0.037)	(0.040)	(0.033)	(0.035)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.56	0.61	0.57	0.62	0.60	0.64
Control outcome mean	0.55	0.55	0.41	0.41	0.40	0.40
Control outcome std. dev.	0.50	0.50	0.49	0.49	0.49	0.49
Trading center × Post election FEs		✓		✓		✓

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

examines respondent appraisals of government performance on a five-point scale ranging from very bad (1) to very good (5). For central (but not local) government performance, we observe a modest positive effect. When using the adaptive lasso approach to predicting elevated WhatsApp use during the ban, Table A11 shows that the effects on central government performance become statistically significant. Consistent with differences in content driving support for the NRM, Appendix Table A12 shows that we do not observe such an effect among individuals treated during the post-election experiment—if anything, respondents assigned to treatment updated *negatively* about (particularly central) government performance.

If comparatively favorable content is driving political attitudes among individuals that were using social media during the ban, some respondents may be more persuaded by this content than others. In particular, increased support for the NRM is likely to be concentrated among individuals with the least favorable prior beliefs about the government's performance. We evaluate this implication by testing for differential effects across respondents that did and did not already evaluate central government performance favorably. For the difference-in-differences estimates, the results in columns (3) and (4) of Table 10 also provide evidence in line with favorable updating from more pro-NRM content. Specifically, there is a positive interaction effect indicating that access to social media during the election-time significantly increased our index of NRM support among respondents who did not initially view central government performance favorably. By contrast, in panel B we estimate heterogeneous effects of the experimental intervention along the same dimensions. The lack of evidence that individuals with weaker pre-treatment attitudes towards the government experienced the largest treatment effects on their attitudes is consistent with differences in the type of content that social media users were exposed to.

Table 10: Heterogeneity in observational and experimental effects on NRM support by prior beliefs

	NRM support index (ICW)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Difference-in-differences estimates								
VPN × Post election	0.184**	0.198***	0.071	0.077	0.110	0.125	0.074	0.049
	(0.073)	(0.075)	(0.092)	(0.097)	(0.078)	(0.081)	(0.097)	(0.098)
VPN × Post election × Non-good incumbent performance prior			0.215*	0.233*				
			(0.125)	(0.140)				
VPN × Post election × Uganda a flawed democracy prior					0.383**	0.391**		
					(0.191)	(0.191)		
VPN × Post election × Followed opposition politicians							0.195	0.272**
							(0.132)	(0.134)
Observations	2,620	2,612	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.62	0.66	0.62	0.67	0.63	0.68	0.62	0.66
Control outcome mean	0.00	-0.00	0.00	-0.00	0.00	-0.00	0.00	-0.00
Control outcome std. dev.	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Trading center × Post election FEs		✓		✓		✓		✓
Sum of coefficients			0.286	0.311	0.492	0.517	0.270	0.321
			(0.1)	(0.109)	(0.17)	(0.169)	(0.1)	(0.103)
Panel B: Experimental estimates								
Treatment	-0.045	-0.045	-0.090	-0.124	-0.055	-0.055	0.017	0.031
	(0.052)	(0.056)	(0.080)	(0.102)	(0.060)	(0.073)	(0.074)	(0.097)
Treatment × Non-good incumbent performance prior			0.056	0.103				
			(0.111)	(0.138)				
Treatment × Uganda a flawed democracy prior					0.006	0.077		
					(0.137)	(0.186)		
Treatment × Followed opposition politicians							-0.110	-0.046
							(0.114)	(0.142)
Observations	1253	1251	1253	1251	1253	1251	1253	1251
R^2	0.38	0.47	0.40	0.60	0.39	0.59	0.40	0.58
Control mean	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
Control SD	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Additional controls				✓		✓		✓
Sum of coefficients			-0.034	-0.021	-0.049	0.022	-0.092	-0.015
			(0.072)	(0.088)	(0.12)	(0.165)	(0.081)	(0.097)

Notes: Panel A: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. Panel B: Each specification is estimated using OLS and includes block and enumerator fixed effects and pre-treatment controls for lag dependent variable. Heterogeneous effect variables fully interacted with treatment indicator, fixed effects, and controls. Lower order terms are omitted to save space. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Taken together, we thus find some evidence suggesting that relatively greater exposure to social media content speaking favorably of the NRM during the social media ban may have helped shape VPN users' greater support for NRM. In contrast, the relatively greater preponderance of opposition-leaning social media content may instead account for why social media exposure increased opposition to the NRM during "normal" times outside of the election-time social media ban. Next steps in our analyses will further explore changes in the sentiment and topic of the content social media users would likely have been exposed to on Facebook.

6.2.2 Content relating to election integrity

After widely-covered violent repression of opposition protests and multiple arrests of opposition leaders in November and December 2020, respondents may also have expected to have been inundated with reports of electoral malpractice on election day that did not ultimately materialize. While the opposition challenged the integrity of the election (from home confinement in Bobi Wine's case), these claims may have been viewed as surprisingly limited by citizens expecting much worse. In short, citizens on social media during the election-time ban may have inferred that Ugandan elections are more democratic than they had expected.

We assess this interpretation of the election-time results in two ways. First, we examine posterior beliefs and behaviors. Panel B of Table 9 reports that VPN users became about 4 percentage points less likely to say that they believe Uganda is a democracy with major problems or not a democracy at all. While this effect is not significant, respondents did become 10 percentage points less likely to follow opposition politicians on Facebook, Instagram, or Twitter. In contrast, they did not become significantly less likely to follow national government officials. Both findings tentatively suggest that citizens could have become less concerned about the violations of democratic norms frequently highlighted online by opposition politicians.

Second, increased NRM support among regular VPN users after the election is driven by respondents for whom democratic violations were most likely to have been less bad than expected. Columns (5) and (6) of panel A in Table 10 show that increased support for the NRM is four times larger among respondents that believed Uganda was a democracy with major problems or not a democracy at all at baseline. Similarly, the effects of access to social media were greater among respondents that followed opposition politicians on social media at baseline. In contrast, neither interaction holds in the experimental analysis—a time by which the elections were a less salient topic. Together, these findings suggest that favorable updating, albeit from low expectations, about the integrity of the 2021 election process among social media users may also help explain increased support for the NRM among VPN users who were more likely to use social media during the election-time ban.

7 Conclusion

Due to limited barriers to entry, social media has the potential to act as a powerful democratizing force and opposition communication tool in competitive authoritarian regimes. Particularly in the face of government control of traditional media, it can provide opposition parties with a voice and megaphone. However, it can also serve as a distraction or even a tool for autocratic regimes, especially when combined with censorship and the implicit or explicit threat of sanctions for violating rules concerning political speech. These more pessimistic perspectives may be particularly pertinent during sensitive political moments, such as elections.

Leveraging natural and field experiments, this study explored the potential of each function of social media in Uganda—both during and long after the contentious 2021 election campaign. Our findings provide mixed evidence that social media acts works as a "liberation technology" that increases support for institutionally-disadvantaged opposition parties in practice. On one hand, our experimental estimates show that greater access to social media over three months outside of election campaigns reduced individual support for the ruling NRM party among initial supporters. On the other, VPN-induced access to social media during the month-long social media ban (and continuing Facebook ban) during Uganda's month of elections differentially increased support for the NRM. In contrast with opposition dominance of social media content during normal times, this election-time effect appears to be driven by exposure to content that is more favorable to the government during the election-time social media ban—whether in terms of relatively greater positive coverage of the NRM performance or less information about electoral intimidation than expected. Either way, this finding suggests that ruling parties may be able to control information flows on social media at the times that matter most.

These findings suggest that the decision of whether, and how, to censor social media is a complex one from an incumbent government's perspective. On one hand, first, limiting access to social media might reduce citizens' exposure to persuasive opposition content. In this respect, social media bans appear appealing, especially for risk-averse governments that expect to win. Second, changes in the content on social media around election time may benefit incumbents. This could be because their online election machines only kick into gear at election time or because social media bans produce an indirect chilling effect on opposition content producers. We intend to distinguish the role of social media bans in shaping election-time content from regular political business cycles by comparing social media content within campaigns across the 2016 and 2021 elections. On the other hand, while it was not our focus of this study, other studies suggest that the act of censorship may also prove unpopular with citizens concerned about losing valued content or the implications for democracy (Kronick and Marshall, 2022). These considerations suggest that the public approval returns to censorship are ambiguous.

Finally, it is also important to emphasize that our study predominantly captures partial equilibrium effects. By focusing on changes in access to social media among relatively small groups of people—those that used VPNs to circumvent the social media ban and a small experimental sample—we follow prior studies (Allcott et al., 2020) in estimating relatively atomistic short-term effects of *individual-level* use of social media. Because individuals in this context are largely "content-takers," we primarily capture effects of content exposure relative to what individuals have received from other information sources. If changes in social media use were larger and more geographically concentrated, social mechanisms shaping collective action and social pressure are more likely to be activated. Group-level exposure to social media remains understudied, although we hope to next explore the extent to which the introduction of Uganda's OTT tax impacted communication and coordination between citizens after its introduction.

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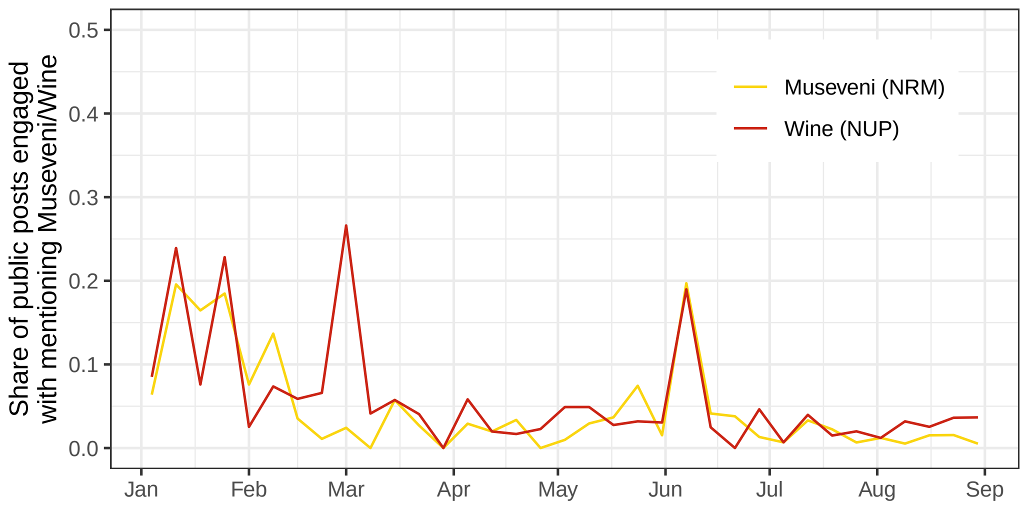
A Appendix

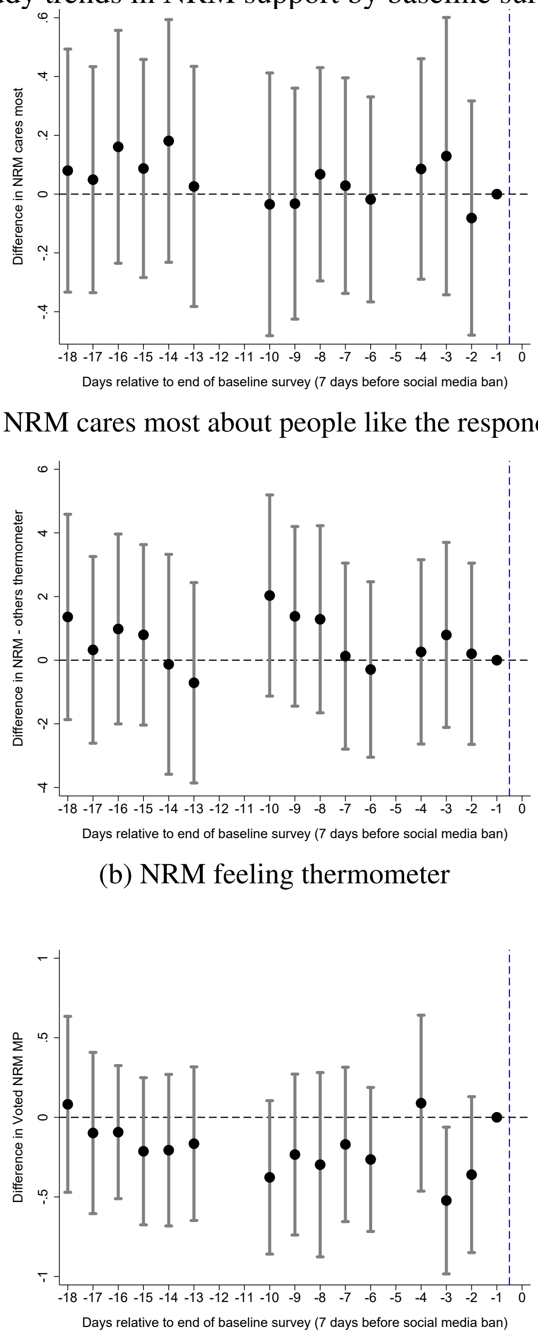
A.1 Figures

Figure A1: Respondents' engagement with political Facebook posts during study period



Notes: Figure plots the proportion of Facebook posts engaged with (i.e. liked, commented, or posted) relating to presidential candidates on a given day by Facebook users with names matching respondents in our sample in Uganda.

Figure A2: Event study trends in NRM support by baseline survey enumeration date



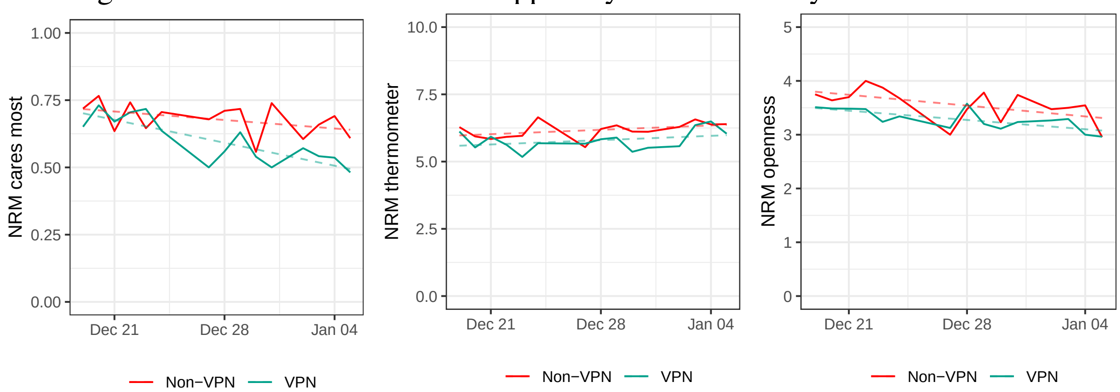
(a) NRM cares most about people like the respondent

(b) NRM feeling thermometer

(c) Openness to voting NRM

Notes: We include all enumeration days where at least 25 surveys were completed

Figure A3: Raw trends in NRM support by baseline survey enumeration date

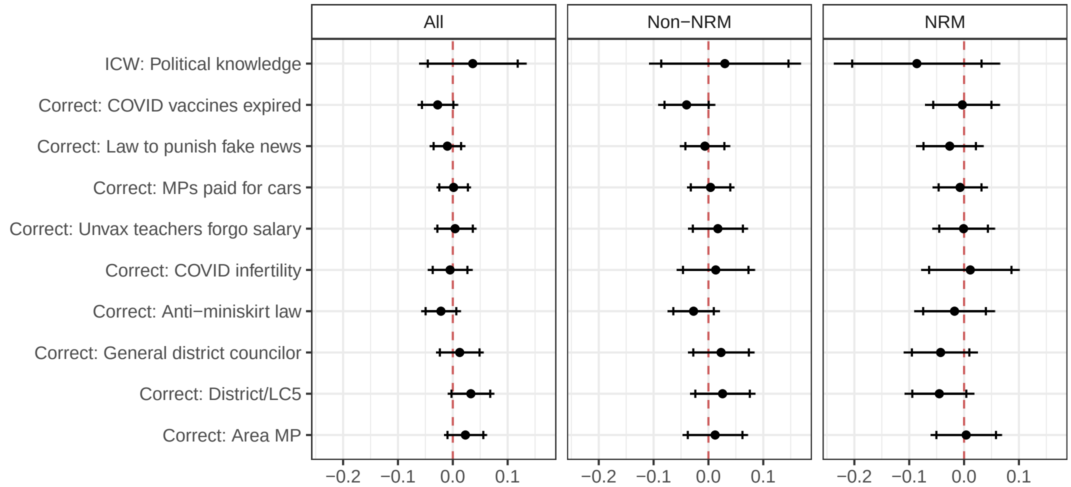


(a) NRM cares most about people like the respondent

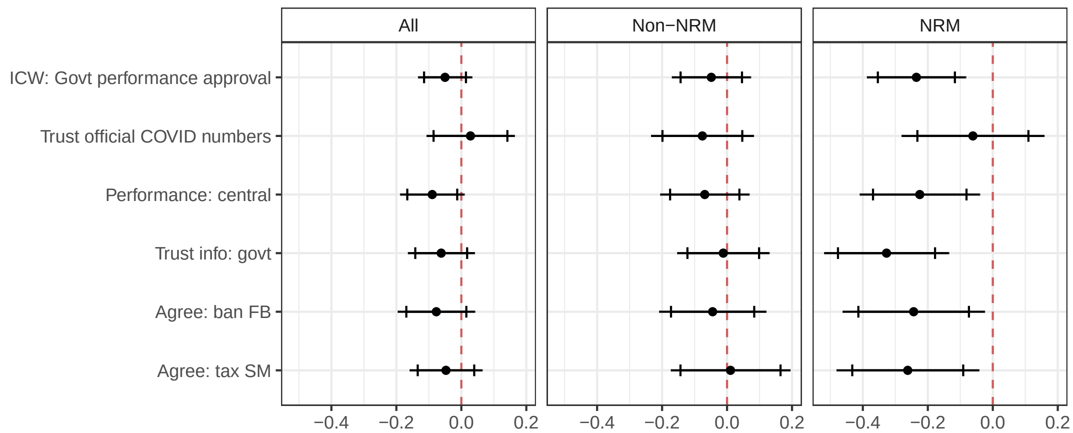
(b) NRM feeling thermometer

(c) Openness to voting NRM

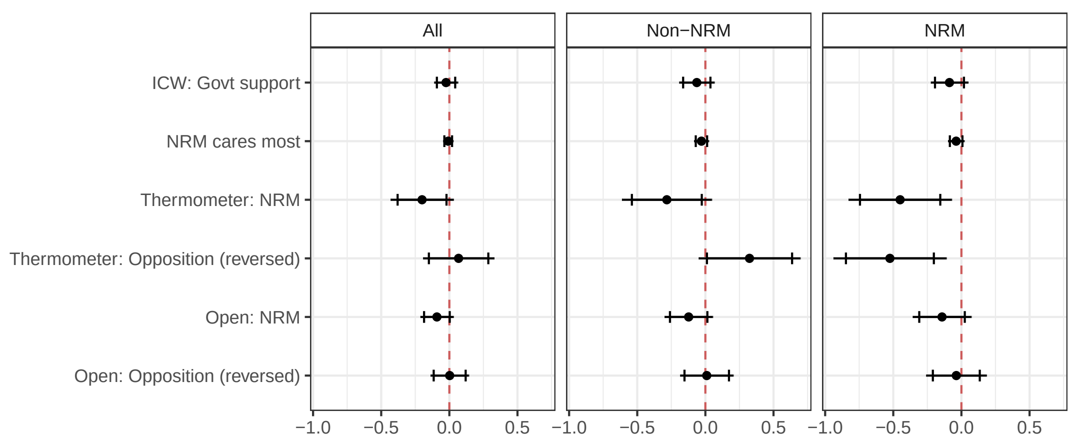
Notes: We include all enumeration days where at least 25 surveys were completed. The dashed lines are linear regression lines.



(a) Political knowledge



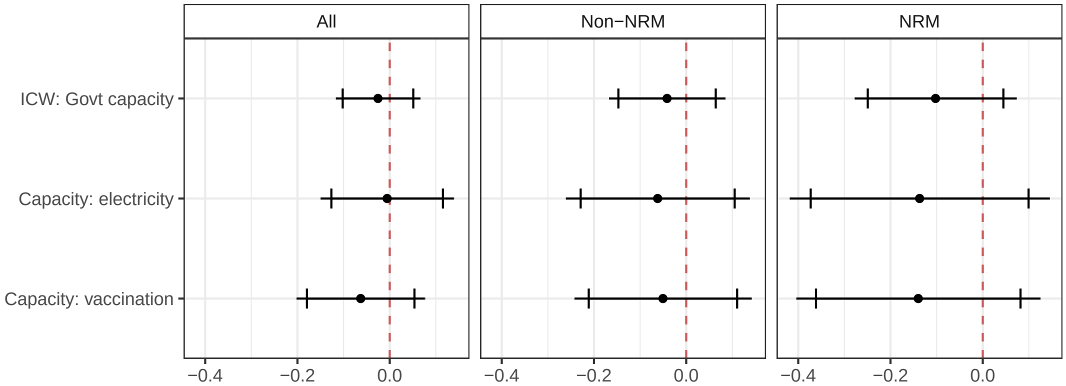
(b) Government performance approval



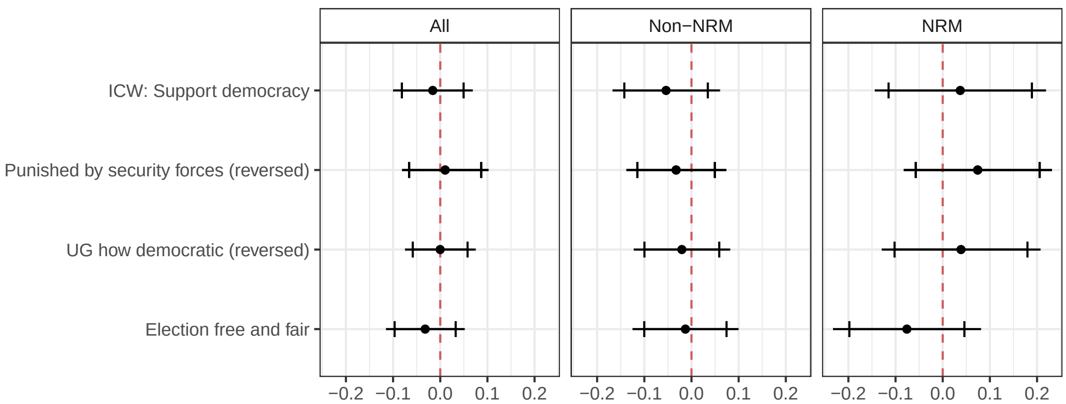
(c) Government support

Figure A4: Treatment effects on pre-registered indices

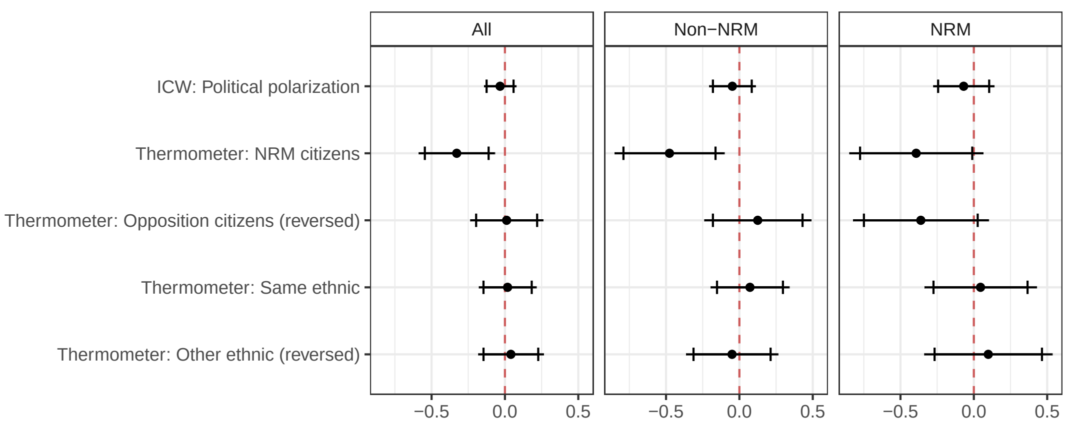
Notes: The estimates derive from equation (3) estimated in the full sample (left panels) and subsamples according to partisanship (middle and right panels). Index outcomes are standardized; subcomponents are unstandardized. 95% confidence intervals plotted.



(a) Perceived state capacity



(b) Perceptions of democracy



(c) Political polarization

Figure A5: Treatment effects on pre-registered indices

Notes: The estimates derive from equation (3) estimated in the full sample (left panels) and subsamples according to partisanship (middle and right panels). Index outcomes are standardized; subcomponents are unstandardized. 95% confidence intervals plotted.

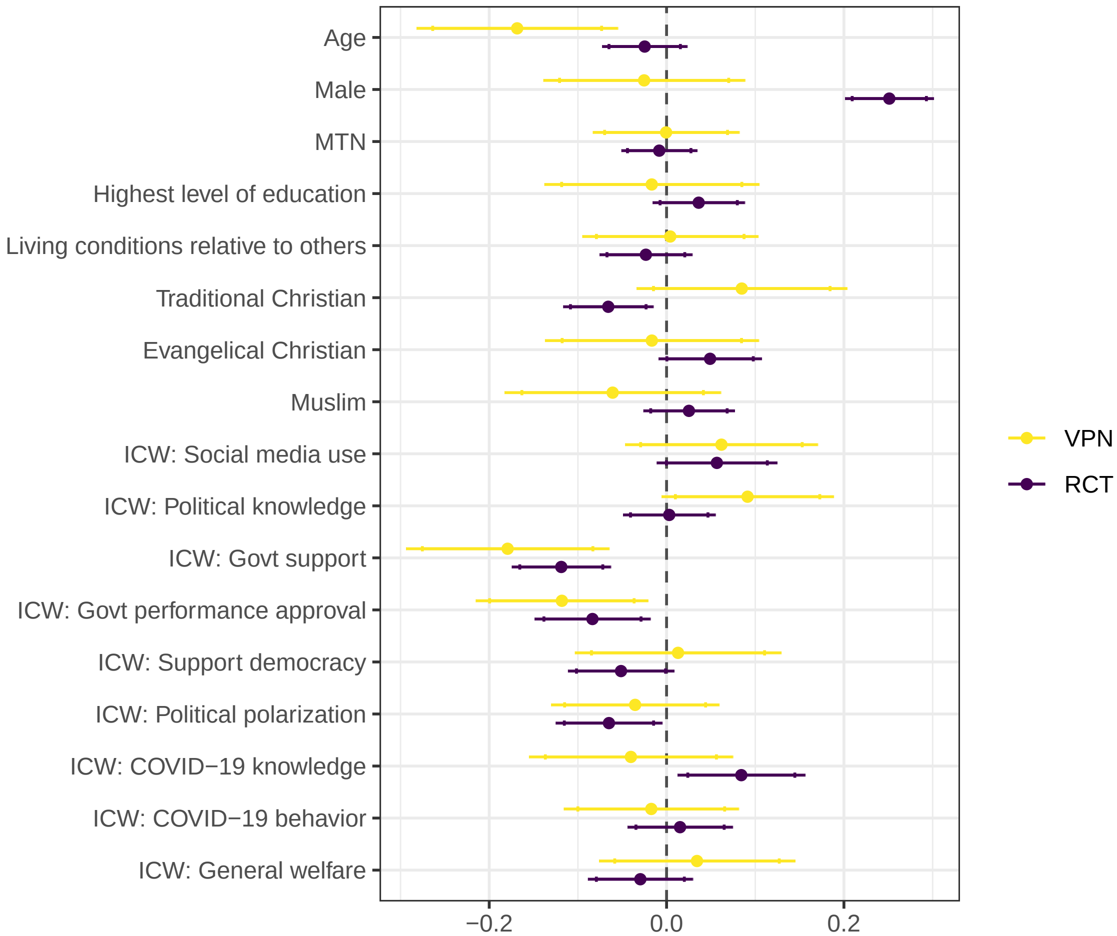


Figure A6: Correlation between baseline characteristics and VPN use/predicted treatment uptake

Notes: Each specification is estimated using OLS including trading center fixed effects, where standardized baseline covariates differ by row. ‘VPN’ regresses each standardized covariate onto an indicator for the respondent being a regular VPN user as per panel B of Table 1. ‘RCT’ regresses each standardized covariate onto their predicted “first stage” effect of the treatment on social media usage in the experimental period. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

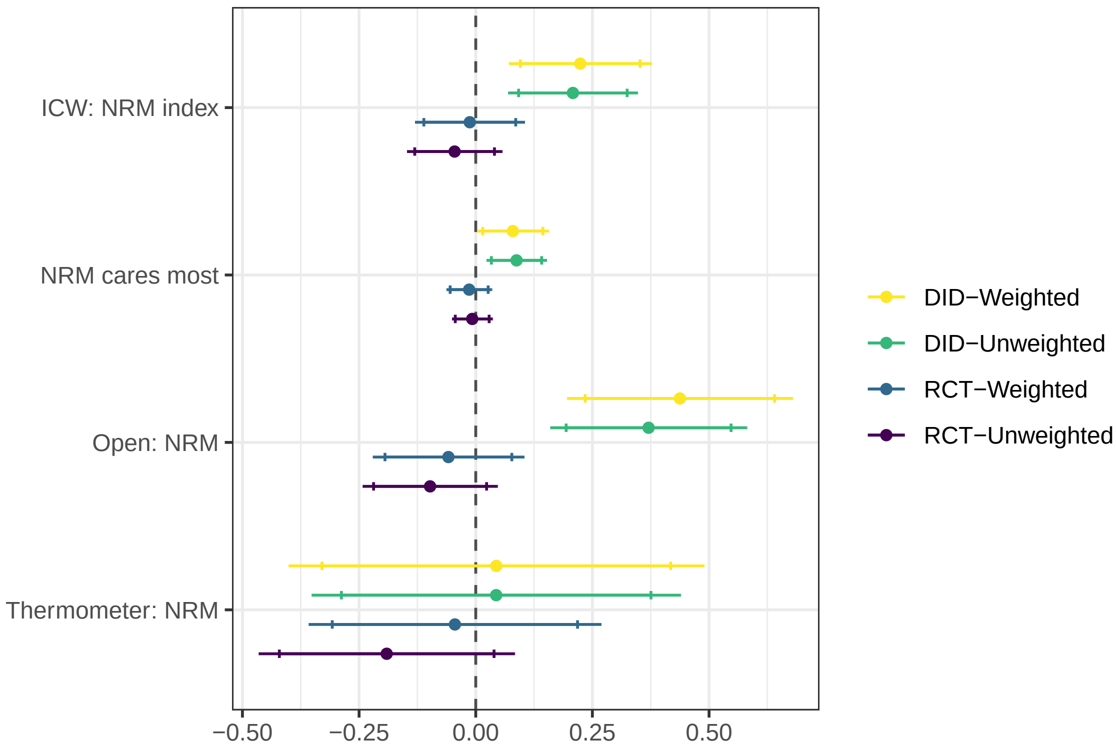


Figure A7: Reweighted estimates from DID and RCT designs

Notes: 'DID' specifications are estimated using equation 1, 'RCT' specifications are estimated using equation (3). We vary the inclusion of inverse weights based on a participant's predicted increase in social media usage due to the ban (VPN) or the treatment (RCT). 90% and 95% confidence intervals plotted (two-sided tests).

A.2 Tables

Table A1: Stated main reasons for using different social media platforms

	Facebook	WhatsApp	Twitter
Share using platform	0.79	0.78	0.17
Entertainment	0.70	0.66	0.53
Catching up with friends and family	0.91	0.95	0.67
Getting news about politics	0.71	0.53	0.58
Getting information about COVID-19	0.70	0.55	0.46
Discussing or solving community problems	0.26	0.26	0.17
Discussing politics and current events	0.25	0.24	0.17

Sample restricted to baseline survey respondents. Reasons for using each platform are conditioned on the participant using that platform. Respondents were asked "Which of the following reasons are your main reasons for using (platform)?"

Table A2: Differences in midline attrition by baseline VPN use

	Outcome: Attrited	
	(1)	(2)
VPN	-0.022	-0.001
	(0.020)	(0.020)
Observations	1,542	1,538
R^2	0.00	0.10
Control outcome mean	0.16	0.16
Control outcome std. dev.	0.37	0.37
Trading center FEs		✓

Notes: Each specification is estimated using OLS and includes the full sample of respondents that completed the baseline survey. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A3: Differences in midline attrition by baseline VPN use

	Outcome: Believe enumerators were sent by...			
	Government		**NRM**	
	(1)	(2)		
VPN × Post election	0.006	0.006	-0.003	0.001
	(0.024)	(0.026)	(0.010)	(0.010)
Observations	2,620	2,612	2,620	2,612
R^2	0.56	0.60	0.52	0.56
Control outcome mean	0.10	0.09	0.02	0.02
Control outcome std. dev.	0.29	0.29	0.13	0.14
Trading center × Post election FEs		✓		✓

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A4: Differential effects of VPN use on support for the NRM after the social media ban, using adaptive lasso to predict changes in WhatsApp use

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
Predicted WA usage during ban × Post election	0.104**	0.111***	-0.156***	-0.179***	0.150***	0.170***
	(0.040)	(0.041)	(0.032)	(0.035)	(0.036)	(0.039)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.62	0.66	0.61	0.65	0.62	0.66
Outcome mean	-0.08	-0.08	0.08	0.08	-0.09	-0.09
Outcome std. dev.	1.01	1.01	1.02	1.02	1.01	1.01
Trading center × Post election FEs		✓		✓		✓
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
Predicted WA usage during ban × Post election	0.053***	0.055***	-0.023***	-0.025***	-0.029***	-0.035***
	(0.019)	(0.019)	(0.007)	(0.008)	(0.011)	(0.012)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.60	0.64	0.60	0.65	0.60	0.63
Outcome mean	0.64	0.64	0.05	0.05	0.16	0.16
Outcome std. dev.	0.48	0.48	0.21	0.22	0.36	0.36
Trading center × Post election FEs		✓		✓		✓
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM parties		**Opposition parties**		**Difference in thermometer**	
Predicted WA usage during ban × Post election	0.177	0.221*	-0.207**	-0.255**	0.384**	0.476***
	(0.115)	(0.113)	(0.098)	(0.101)	(0.160)	(0.164)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.58	0.63	0.56	0.60	0.61	0.66
Outcome mean	5.80	5.79	5.00	5.00	0.80	0.79
Outcome std. dev.	2.72	2.72	2.53	2.53	4.03	4.03
Trading center × Post election FEs		✓		✓		✓
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
Predicted WA usage during ban × Post election	0.090	0.084	-0.125**	-0.147***	0.215**	0.231***
	(0.060)	(0.063)	(0.049)	(0.051)	(0.082)	(0.086)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.55	0.60	0.55	0.59	0.58	0.63
Outcome mean	3.37	3.37	3.08	3.09	0.29	0.28
Outcome std. dev.	1.43	1.43	1.45	1.45	2.02	2.01
Trading center × Post election FEs		✓		✓		✓
Panel E: Indicators for self-reported voting for NRM						
	Voted NRM for MP		**Voted NRM for LC5**			
Predicted WA usage during ban × Post election	0.020	-0.006	0.053***	0.062***		
	(0.034)	(0.029)	(0.020)	(0.020)		
Observations	906	860	1,902	1,884		
R^2	0.64	0.77	0.61	0.65		
Outcome mean	0.44	0.45	0.48	0.48		
Outcome std. dev.	0.50	0.50	0.50	0.50		
Trading center × Post election FEs		✓		✓		

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. Predicted WhatsApp use is based on the (standardized) predictions of an adaptive LASSO model that predicts the individual change in WhatsApp use during the social media ban using baseline survey covariates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A5: Differential effects of VPN use on support for the NRM after the social media ban, 2 or more days of VPN

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
VPN × Post election	0.198***	0.206***	-0.128*	-0.175**	0.174**	0.209***
	(0.072)	(0.075)	(0.073)	(0.077)	(0.074)	(0.076)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.62	0.66	0.60	0.64	0.61	0.65
Control outcome mean	-0.01	-0.01	0.01	0.02	-0.02	-0.02
Control outcome std. dev.	1.00	1.00	1.01	1.01	1.01	1.01
Trading center × Post election FEs		✓		✓		✓
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
VPN × Post election	0.081**	0.080**	-0.015	-0.021	-0.022	-0.030
	(0.033)	(0.034)	(0.015)	(0.017)	(0.024)	(0.026)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.59	0.64	0.60	0.65	0.60	0.63
Control outcome mean	0.67	0.67	0.04	0.04	0.13	0.13
Control outcome std. dev.	0.47	0.47	0.21	0.21	0.34	0.34
Trading center × Post election FEs		✓		✓		✓
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM parties		**Opposition parties**		**Difference in thermometer**	
VPN × Post election	0.029	0.189	-0.489***	-0.582***	0.518**	0.771***
	(0.201)	(0.206)	(0.164)	(0.171)	(0.261)	(0.260)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.58	0.63	0.56	0.60	0.61	0.66
Control outcome mean	5.93	5.93	4.89	4.89	1.04	1.03
Control outcome std. dev.	2.65	2.65	2.48	2.48	4.00	4.00
Trading center × Post election FEs		✓		✓		✓
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
VPN × Post election	0.373***	0.321***	-0.010	-0.054	0.382***	0.374**
	(0.106)	(0.117)	(0.107)	(0.111)	(0.144)	(0.145)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.55	0.61	0.55	0.59	0.58	0.63
Control outcome mean	3.44	3.44	3.02	3.03	0.41	0.41
Control outcome std. dev.	1.41	1.41	1.45	1.45	2.04	2.03
Trading center × Post election FEs		✓		✓		✓
Panel E: Indicators for self-reported voting for NRM						
	Voted NRM for MP		**Voted NRM for LC5**			
VPN × Post election	0.031	0.020	0.036	0.038		
	(0.058)	(0.055)	(0.038)	(0.040)		
Observations	910	864	1,904	1,886		
R^2	0.64	0.77	0.61	0.65		
Control outcome mean	0.49	0.50	0.51	0.51		
Control outcome std. dev.	0.50	0.50	0.50	0.50		
Trading center × Post election FEs		✓		✓		

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A6: Differential effects of VPN use on support for the NRM after the social media ban, 3 or more days of VPN

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
VPN × Post election	0.169**	0.203**	-0.090	-0.125*	0.117	0.155**
	(0.077)	(0.079)	(0.072)	(0.075)	(0.076)	(0.077)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.62	0.66	0.60	0.64	0.61	0.65
Control outcome mean	-0.02	-0.02	0.03	0.04	-0.03	-0.03
Control outcome std. dev.	1.00	1.00	1.02	1.02	1.02	1.02
Trading center × Post election FEs		✓		✓		✓
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
VPN × Post election	0.088***	0.096***	-0.022	-0.029*	-0.010	-0.013
	(0.033)	(0.035)	(0.016)	(0.016)	(0.027)	(0.029)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.59	0.64	0.60	0.65	0.60	0.63
Control outcome mean	0.66	0.66	0.05	0.05	0.14	0.14
Control outcome std. dev.	0.47	0.47	0.21	0.21	0.35	0.35
Trading center × Post election FEs		✓		✓		✓
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM parties		**Opposition parties**		**Difference in thermometer**	
VPN × Post election	-0.054	0.082	-0.388**	-0.453**	0.334	0.535*
	(0.210)	(0.212)	(0.171)	(0.173)	(0.284)	(0.290)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.58	0.63	0.56	0.60	0.61	0.65
Control outcome mean	5.93	5.93	4.90	4.90	1.03	1.02
Control outcome std. dev.	2.65	2.65	2.48	2.48	4.01	4.01
Trading center × Post election FEs		✓		✓		✓
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
VPN × Post election	0.305**	0.321**	0.089	0.077	0.216	0.244
	(0.118)	(0.124)	(0.111)	(0.113)	(0.154)	(0.158)
Observations	2,620	2,612	2,620	2,612	2,620	2,612
R^2	0.55	0.61	0.55	0.59	0.58	0.62
Control outcome mean	3.45	3.45	3.04	3.05	0.41	0.40
Control outcome std. dev.	1.40	1.40	1.45	1.44	2.03	2.03
Trading center × Post election FEs		✓		✓		✓
Panel E: Indicators for self-reported voting for NRM						
	Voted NRM for MP		**Voted NRM for LC5**			
VPN × Post election	0.062	0.057	0.045	0.049		
	(0.062)	(0.060)	(0.041)	(0.045)		
Observations	910	864	1,904	1,886		
R^2	0.64	0.77	0.61	0.65		
Control outcome mean	0.47	0.48	0.51	0.51		
Control outcome std. dev.	0.50	0.50	0.50	0.50		
Trading center × Post election FEs		✓		✓		

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A7: Covariate balance (RCT)

	Age	Male	MTN	Education	Better living conditions	Traditional Christian	Evangelical Christian	Muslim	Social media use scale	Political knowledge scale	Support government scale	Approve government scale	Support democracy scale	Political polarization scale	COVID-19 knowledge scale	COVID-19 behavior scale	General welfare scale
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Treat	-0.34	0.00	0.00	-0.01	-0.07	0.01	-0.01	0.00	-0.02	-0.01	-0.04	0.01	0.08	0.01	0.01	-0.06	-0.02
	(0.41)	(0.02)	(.)	(0.09)	(0.05)	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
Observations	1387	1389	1389	1389	1389	1383	1383	1383	1253	1253	1253	1253	1253	1253	1253	1253	1253
R^2	0.30	0.36	1.00	0.20	0.17	0.42	0.25	0.32	0.30	0.33	0.28	0.21	0.23	0.24	0.25	0.29	0.29
Control mean	30.78	0.67	0.46	4.90	3.36	0.59	0.21	0.19	0.00	0.01	0.01	-0.01	0.00	-0.01	0.01	0.01	0.01
Control SD	8.17	0.47	0.50	1.63	0.81	0.49	0.41	0.39	1.01	1.00	1.00	1.01	1.00	0.99	1.00	1.00	1.00

Each specification is estimated using OLS, and includes block fixed effects. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Attrition (RCT)

	Attrited	
	(1)	(2)
Treatment	0.003	0.005
	(0.011)	(0.011)
N	1455	1455
R^2	0.00	0.18
Control outcome mean	0.04	0.04
Control outcome std. dev.	0.21	0.21
Block FEs		✓

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A9: Experimental treatment effects on NRM support, subset by partisanship (with controls)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
Treatment	-0.144**	-0.083	0.056	0.020	-0.111	-0.039
	(0.073)	(0.077)	(0.080)	(0.074)	(0.079)	(0.074)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
Treatment	-0.038	-0.027	0.007	0.008	0.006	-0.005
	(0.036)	(0.033)	(0.018)	(0.017)	(0.028)	(0.031)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	0.80	0.67	0.05	0.06	0.10	0.22
Control SD	0.40	0.47	0.21	0.24	0.31	0.41
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM party		**Opposition parties**		**Difference in thermometer**	
Treatment	-0.417*	-0.272	0.526**	-0.354*	-0.931**	0.164
	(0.218)	(0.195)	(0.252)	(0.196)	(0.378)	(0.341)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	6.45	5.61	4.69	5.92	1.77	-0.32
Control SD	2.65	2.83	2.56	2.58	4.26	4.56
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
Treatment	-0.102	-0.110	0.078	0.009	-0.160	-0.094
	(0.119)	(0.109)	(0.130)	(0.104)	(0.191)	(0.138)
Subset	NRM	Non-NRM	NRM	Non-NRM	NRM	Non-NRM
Observations	583	804	583	804	583	804
Control mean	3.55	3.21	3.00	3.14	0.55	0.07
Control SD	1.36	1.47	1.45	1.42	1.87	2.04

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

The NRM indicator is defined as endline respondents who either: (i) reported voting for an NRM candidate for MP at midline survey; or (ii) if they did not disclose (i), then indicated they felt warmer towards NRM than opposition parties and that they overall felt warmly towards NRM.

Table A10: Experimental treatment effects on party attitudes (baseline and VPN subsets)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Support for NRM and opposition party ICW indexes						
	NRM support		**Opposition support**		**Differential NRM support**	
Treatment	-0.046	-0.067	-0.024	-0.026	0.030	0.028
	(0.052)	(0.075)	(0.049)	(0.071)	(0.052)	(0.074)
Subset	Baseline	VPN	Baseline	VPN	Baseline	VPN
Observations	1253	773	1253	773	1253	773
Panel B: Which party cares most about people like the respondent						
	NRM cares most		**FDC cares most**		**NUP cares most**	
Treatment	-0.009	-0.016	-0.006	-0.006	-0.005	-0.002
	(0.023)	(0.032)	(0.011)	(0.016)	(0.019)	(0.030)
Subset	Baseline	VPN	Baseline	VPN	Baseline	VPN
Observations	1253	773	1253	773	1253	773
Control mean	0.72	0.73	0.06	0.06	0.17	0.18
Control SD	0.45	0.45	0.23	0.23	0.37	0.38
Panel C: Feeling thermometer (0-very cold – 10-very warm)						
	NRM party		**Opposition parties**		**Difference in thermometer**	
Treatment	-0.197	-0.232	-0.023	-0.302	-0.187	0.056
	(0.146)	(0.210)	(0.140)	(0.194)	(0.232)	(0.335)
Subset	Baseline	VPN	Baseline	VPN	Baseline	VPN
Observations	1253	773	1253	773	1253	773
Control mean	5.95	5.95	5.38	5.64	0.57	0.31
Control SD	2.79	2.75	2.64	2.57	4.55	4.50
Panel D: Openness to voting for different party (1-not at all – 5-very open)						
	Openness to NRM		**Openness to opposition**		**Difference in openness**	
Treatment	-0.055	-0.059	-0.024	0.012	-0.024	-0.074
	(0.077)	(0.111)	(0.075)	(0.105)	(0.109)	(0.156)
Subset	Baseline	VPN	Baseline	VPN	Baseline	VPN
Observations	1253	773	1253	773	1253	773
Control mean	3.35	3.25	3.10	3.03	0.25	0.22
Control SD	1.44	1.43	1.44	1.42	1.98	1.93

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

'Baseline' subset restricts to endline respondents also surveyed at baseline; 'VPN' subset restricts to endline respondents who reported being VPN users at baseline.

Table A11: Potential mechanisms, using adaptive LASSO

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Assessments of government performance						
	Central government		**District government**		**Subcounty government**	
Predicted WA usage during ban × Post election	1.741	2.669**	1.484	2.014*	0.129	0.626
	(1.090)	(1.097)	(1.022)	(1.094)	(0.812)	(0.866)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.55	0.61	0.57	0.61	0.54	0.59
Control outcome mean	3.28	3.28	3.14	3.14	3.08	3.08
Control outcome std. dev.	1.15	1.15	1.10	1.10	1.14	1.14
Trading center × Post election FEs		✓		✓		✓
Panel B: Negative perceptions of democracy in Uganda						
	Democracy with major problems		**National government officials**		**Opposition politicians**	
Predicted WA usage during ban × Post election	-0.209	-0.510	-1.258***	-1.047***	-1.876***	-1.944***
	(0.429)	(0.446)	(0.393)	(0.382)	(0.387)	(0.405)
Observations	2,598	2,590	2,598	2,590	2,598	2,590
R^2	0.56	0.61	0.57	0.62	0.61	0.64
Control outcome mean	0.55	0.55	0.41	0.41	0.40	0.40
Control outcome std. dev.	0.50	0.50	0.49	0.49	0.49	0.49
Trading center × Post election FEs		✓		✓		✓

Notes: Each specification is estimated using OLS, and includes individual and period fixed effects. Standard errors clustered by trading center are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table A12: Potential mechanisms, RCT data

	Outcomes vary by panel:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Assessments of government performance						
	Central government		**District government**		**Subcounty government**	
Treatment	-0.140*	-0.153*	-0.055	-0.070	-0.072	-0.057
	(0.080)	(0.087)	(0.066)	(0.074)	(0.064)	(0.071)
Observations	1389	1387	1389	1387	1389	1387
R^2	0.27	0.35	0.29	0.39	0.34	0.41
Control mean	2.98	2.98	2.97	2.97	2.91	2.91
Control SD	1.45	1.45	1.23	1.23	1.25	1.25
Additional controls		✓		✓		✓
Panel B: Negative perceptions of democracy in Uganda						
	Democracy with major problems		**National government officials**		**Opposition politicians**	
Treatment	0.016	0.007	0.039	0.039	0.044*	0.034
	(0.026)	(0.028)	(0.024)	(0.026)	(0.025)	(0.027)
Observations	1389	1387	1389	1387	1389	1387
R^2	0.23	0.34	0.31	0.41	0.27	0.38
Control mean	0.59	0.59	0.63	0.63	0.59	0.59
Control SD	0.49	0.49	0.48	0.48	0.49	0.49
Additional controls		✓		✓		✓

Each specification is estimated using OLS, and includes block and endline enumerator fixed effects as per Equation (3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Panel A: Accounts						
Partisanship	MP candidate	Media	Group	Page	Government	Total
NRM	56	11	39	37	31	174
NUP	22	5	58	103		188
Other opposition	20	12	5	15		52
Independent	31	3	0	0		34
Total	129	31	102	155	31	448

Panel B: Posts (June 1, 2020-May 31, 2021)						
Partisanship	MP candidate	Media	Group	Page	Government	Total
NRM	3,636	135,429	294,092	9,816	7,460	450,433
NUP	3,781	4,386	874,065	24,216		906,448
Other opposition	2,082	81,269	176,770	4,060		264,181
Independent	1,459	37,147	0	0		38,606
Total	10,958	258,231	1,344,927	38,092	7,460	1,659,668

Table A13: Crowdtangle Sample