

After Parler: Alternative Social Media and Exposure to Extremist Content

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Abstract

Does social media increase exposure to misinformation? I study this question in the context of the 2020 “Stop the Steal” movement, using the rapid growth of Parler, an alternative social media platform with minimal content moderation, as a source of variation in platform adoption. Drawing on URL-level web-browsing data from Comscore covering approximately 2.3 million panelists across desktop and mobile devices, I implement a difference-in-differences design that tracks what happens to users’ browsing behavior after they first access Parler. I find that Parler adopters increase their time spent on websites identified by the House January 6th Committee as sources of Stop the Steal content by roughly fourfold relative to the pre-treatment mean. These effects are robust across multiple heterogeneity-robust estimators and show no evidence of differential pre-trends. The results suggest that social media can have substantial effects on political information consumption, particularly on niche platforms that cater to ideologically motivated users during periods of political turmoil.

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1 Introduction

On January 6, 2021, a violent mob stormed the United States Capitol in an attempt to prevent the certification of the 2020 presidential election. The attack, which the House Select Committee later characterized as the culmination of a coordinated effort to overturn the election results, left several people dead, over 140 law enforcement officers injured, and raised urgent questions about the fragility of American democratic institutions (Select Committee to Investigate the January 6th Attack, 2022). In the aftermath, investigators traced the threads of radicalization back to a constellation of websites and online platforms where false claims of electoral fraud proliferated and calls for violent action went largely unchecked. Platforms such as Parler, 8kun, and TheDonald.win featured prominently in the Committee’s account of how ordinary citizens were drawn into an ecosystem of election denialism and mobilized toward real-world violence.

These findings have renewed longstanding concerns about the role of social media in spreading misinformation and facilitating political extremism (Sunstein, 2001; Lazer et al., 2018; Benkler et al., 2018). Yet the empirical evidence on whether social media actually changes users’ information consumption and political behavior remains surprisingly inconclusive. A large body of experimental and observational work finds that the effects of social media on political attitudes and beliefs are small, often null, and highly context-dependent (Allcott et al., 2020; Guess et al., 2023b; Nyhan et al., 2023; Allcott et al., 2024). Similarly, research on misinformation exposure suggests that false content reaches a relatively small share of the population and is heavily concentrated among a narrow subset of ideologically extreme users (Guess et al., 2020; Allcott et al., 2019; González-Bailón et al., 2024). These findings have led some scholars to conclude that social media’s political influence may be more modest than popularly assumed.

There are two key challenges in the literature in identifying the causal effect of social media. First, because platform adoption tends to occur rapidly and at massive scale, researchers have been largely restricted to studying the effects of temporarily removing users from platforms they already frequent, or experimentally manipulating specific features such as algorithmic feeds or content exposure (Allcott et al., 2020; Levy, 2021; Guess et al., 2023b; Asimovic et al., 2021). These designs are valuable but necessarily limited in scope. They can capture the marginal effects of reducing or modifying existing use, not the potentially larger effects of transitioning from non-use to active participation on a new platform. The distinction matters because the mechanisms through which a platform shapes behavior may differ at the extensive margin (when a user first encounters a community and its norms) versus the intensive margin of incremental changes in exposure. Second, a modest average effect may conceal the effects of social media among demographics whose usage and propensity for real-world harm is particularly high. Given that most online misinformation is consumed by a small subset of extreme users, scholars have argued that effective interventions should take into consideration “tail-end” risks (González-Bailón et al., 2024; Budak et al., 2024). Yet these extreme users are small in number by definition and difficult to observe.

This paper argues that social media can have substantial effects on political information consumption when the right conditions are met: a platform that caters to ideologically motivated users, content that is difficult to encounter through mainstream channels, and a political context that provides a focal point for radicalization. This paper studies Parler’s rapid expansion during the second half of 2020. Parler, a relatively unknown social media platform founded in 2018 with minimal content moderation, experienced two major waves of growth that year. The first followed conservative commentator Dan Bongino’s announcement of an ownership stake in June 2020, and the second occurred after the November presidential election, as mainstream platforms began en-

forcing content moderation policies against election misinformation and prominent conservative voices migrated to Parler (Aliapoulios et al., 2021). The platform was subsequently identified by the House Select Committee as one of several online venues where plans for the January 6th attack were discussed and organized (Select Committee to Investigate the January 6th Attack, 2022).

I use URL-level web-browsing data from Comscore, covering approximately 2.3 million panelists across both desktop and mobile devices throughout 2020, to track when individual users first access Parler and to observe their subsequent browsing behavior. I implement a difference-in-differences design that exploits the staggered timing of Parler adoption across users. Because Parler adoption is rare—roughly 7,000 users in my sample ever access the site—I first use principal component analysis on browsing patterns across 248 national media websites to estimate each user’s political ideology, and restrict the comparison group to the top 5% most conservative users. This ensures that the control group consists of ideologically similar individuals who were plausibly might have heard about Parler but had not yet done so.

I find that users who begin accessing Parler substantially increase their subsequent consumption of content from websites identified by the January 6th Committee as sources of “Stop the Steal” misinformation, including 8kun, 4chan, Infowars, and related domains. The estimated effect is an increase of approximately 6 minutes per bi-weekly period in time spent on these sources, or roughly a fourfold increase relative to the pre-treatment mean of 70 seconds. These estimates are unchanged to a set of alternative plausible specifications and heterogeneity-robust estimators for staggered designs, and show no evidence of differential pre-trends.

These findings contribute to several literatures. First, they provide direct causal evidence that a social media platform can meaningfully shift users’ information diets toward extremist content. While the recent wave of large-scale experiments on Facebook and Instagram finds mostly null

effects of social media on attitudes and beliefs (Guess et al., 2023b; Nyhan et al., 2023; Allcott et al., 2024), those studies examine the effects of marginal changes on mainstream platforms with billions of users. The case of Parler is fundamentally different: a niche platform with minimal content moderation that attracted a self-selected community of ideologically motivated users during a period of acute political crisis. The results suggest that social media effects may be most consequential not on the platforms where most people spend their time, but in the smaller, less-regulated spaces where radicalization is most likely to occur. Second, the findings speak to ongoing debates about the consequences of content moderation on mainstream platforms, suggesting that when users migrate to alternative platforms with fewer guardrails, their exposure to extremist content can increase dramatically (Horta Ribeiro et al., 2023; Rogers, 2020).

The remainder of the paper is organized as follows. Section 2 reviews the related literatures on social media effects, misinformation, and content moderation. Section 3 provides background on the political context of 2020 and Parler’s expansion. Section 4 describes the data sources. Section 5 presents the empirical approach, and Section 6 reports the results. Section 7 concludes.

2 Social media and extremism

This paper sits at the intersection of three related literatures: research on online misinformation and extremism, studies of content moderation and platform migration, and the broader body of work on the political effects of social media. I discuss each in turn.

Misinformation and online extremism

Concerns that the internet would expose citizens to increasingly extreme and misleading content have motivated a large and growing research agenda. Early theoretical work warned that digital media could create “echo chambers” or “filter bubbles” in which users encounter only information that reinforces their prior beliefs (Sunstein, 2001; Pariser, 2011). The empirical evidence on these predictions is mixed. Studies of online news consumption find that while ideological segregation exists online, it is generally no greater than segregation in offline media environments (Gentzkow and Shapiro, 2011; Barberá et al., 2015). At the same time, the concentration of misinformation consumption among a small fraction of users is well established: research on the 2016 U.S. election finds that visits to untrustworthy websites and sharing of false content were overwhelmingly driven by a narrow segment of older, ideologically extreme individuals (Guess et al., 2020, 2019; Grinberg et al., 2019; Allcott and Gentzkow, 2017).

A separate strand of research examines the relationship between online platforms and political extremism. Much of this work has been qualitative or descriptive, with a historical focus on foreign threats such as jihadist recruitment on social media (Berger and Morgan, 2015; Conway, 2017). The domestic context has received growing attention since 2016, with scholars documenting how far-right movements have used platforms to organize, recruit, and spread conspiratorial narratives (Benkler et al., 2018; Freelon et al., 2020). Müller and Schwarz (2023) provide one of the few quantitative studies in this space, showing that Twitter activity predicted anti-minority hate crimes in the United States.

Content moderation and platform migration

A second literature examines the effects of content moderation policies on user behavior. The dominant finding is that banning communities, removing content, or deplatforming individual users tend to reduce the visibility and activity of targeted content on the platform where the action is taken (Chandrasekharan et al., 2017; Jhaver et al., 2019, 2021; Cima et al., 2024). Reddit’s 2015 ban of hate-speech subreddits, for instance, led to a measurable decline in hateful language across the site (Chandrasekharan et al., 2017).

However, a growing body of work points to the possibility of spillover effects. When users are banned from mainstream platforms, some migrate to alternative platforms with fewer content restrictions (Rogers, 2020; Urman and Katz, 2022; Buntain et al., 2023). Horta Ribeiro et al. (2023) study the aftermath of Amazon Web Services removing Parler from its hosting after the Capitol Attack and find that Parler users did not reduce their overall activity on fringe social media, suggesting that deplatforming displaced rather than eliminated extremist participation. Buntain et al. (2023) similarly find that this deplatforming of thousands of accounts lead people to use alternative platforms like Gab and Rumble.

This raises an important question that existing research has not fully addressed: what happens to users’ broader information consumption when they first adopt alternative platforms? If these platforms serve as gateways to a wider ecosystem of extremist content, then the net effect of platform migration may extend well beyond activity on the destination platform itself. My paper provides direct evidence on this question by tracking the full browsing behavior of users who adopt Parler.

Political effects of social media

The broadest literature relevant to this study concerns the political effects of social media, which has produced a remarkably mixed set of findings. Several recent large-scale experiments conducted in partnership with Meta provide some of the most credible evidence to date. Allcott et al. (2020) find that deactivating Facebook for four weeks before the 2018 midterm elections reduced political news consumption and polarization but had no significant effect on political knowledge or vote choice. A suite of follow-up experiments around the 2020 election find that removing re-shared content, altering algorithmic feeds, and reducing exposure to like-minded sources had at most modest effects on political attitudes and beliefs (Guess et al., 2023b,a; Nyhan et al., 2023; González-Bailón et al., 2023). Allcott et al. (2024) find that deactivating Facebook and Instagram before the 2020 election reduced news consumption but did not meaningfully change political polarization, knowledge, or voting behavior. Outside the U.S., Asimovic et al. (2021) find that Facebook deactivation reduced interethnic hostility in Bosnia and Herzegovina, though the effects were modest and context-specific.

Several features of this experimental literature are worth noting. First, the studies focus exclusively on mainstream platforms like Facebook and Instagram that serve billions of users with diverse political orientations. The effects of niche platforms that attract ideologically homogeneous user bases may differ substantially. Second, the experimental designs capture the effects of *reducing* existing usage, not of *initiating* use of a new platform. Going from no exposure to active participation may involve qualitatively different mechanisms, including exposure to novel content, social reinforcement from like-minded users, and the formation of new online identities.

This paper addresses these gaps. By studying the adoption of a niche, ideologically oriented

platform during the most contentious period of the 2020 election cycle, I examine precisely the conditions under which social media effects are most likely to be large. The combination of passively measured browsing data, a credible comparison group constructed via ideology estimation, and a staggered difference-in-differences design allows me to move beyond the existing literature’s reliance on deactivation experiments and survey-based measures.

3 Background

The empirical setting for this paper is the second half of 2020, a period defined by an unprecedented confluence of political polarization, contested election claims, and the rapid growth of alternative social media platforms. This section provides context on the political environment, the origins and spread of the “Stop the Steal” movement, and the specific events that drove Parler’s expansion.

The 2020 election and Stop the Steal

In the months leading up to the November 2020 presidential election, President Donald Trump and allied media figures repeatedly cast doubt on the integrity of the electoral process, with particular focus on mail-in ballots. As early as the spring of 2020, Trump publicly questioned whether expanded mail-in voting, which were adopted by many states in response to the COVID-19 pandemic, would lead to widespread fraud. These claims were amplified by a significant ecosystem of right-wing media outlets and online commentators, despite the absence of evidence supporting them.

After major news organizations projected Joe Biden as the winner of the election on November 7, 2020, these claims escalated into what became known as the “Stop the Steal” movement. The

movement drew on a loose coalition of Trump supporters, far-right media figures, and conspiracy communities united by the belief that the election had been stolen through coordinated fraud. The House Select Committee’s investigation later documented how this movement was organized and amplified through a network of websites, social media platforms, and alternative media sources, ultimately culminating in the attack on the Capitol on January 6, 2021 (Select Committee to Investigate the January 6th Attack, 2022).

Content moderation and migration to Parler

The growth of Stop the Steal coincided with a series of content moderation decisions by major technology companies. Throughout 2020, platforms including Facebook, Twitter, and YouTube implemented increasingly aggressive policies to address election-related misinformation. Twitter began labeling tweets containing disputed claims about the election, while Facebook removed groups associated with Stop the Steal and QAnon. These actions provoked a backlash among many conservative users, who viewed the moderation policies as politically motivated censorship.

Parler, founded in 2018 as a “free speech” alternative to mainstream social media, became a primary beneficiary of this backlash. The platform’s growth occurred in two distinct waves (Aliapoulios et al., 2021). The first wave followed Dan Bongino’s announcement in June 2020 that he had acquired an ownership stake in the platform. Bongino, a prominent conservative media personality with a large following, actively promoted Parler as a refuge from what he characterized as censorship on mainstream platforms. This endorsement brought a substantial influx of new users, many of whom were drawn from the broader conservative media ecosystem.

The second and much larger wave of adoption occurred after the November 2020 election.

Mainstream platforms intensified their enforcement of election integrity policies by suspending accounts that promoted Stop the Steal content and flagging and fact-checking President Trump’s claims as false. In response, Parler experienced an enormous surge in downloads and new registrations. In the week following the election, Parler briefly became the most-downloaded app in the United States. The platform’s minimal content moderation meant that claims of election fraud, calls for protest, and increasingly extreme rhetoric circulated with little restriction. The House Select Committee documented how Parler, along with other alternative platforms, served as a venue where plans for the January 6th rally and subsequent attack were openly discussed (Select Committee to Investigate the January 6th Attack, 2022).

4 Data

My analysis draws on three primary data sources: (i) URL-level web-browsing data from Comscore, (ii) a catalogue of websites associated with “Stop the Steal” content drawn from the House January 6th Committee report, and (iii) a collection of national media sources used to estimate user-level political ideology.

Web-browsing data

The main dataset consists of URL-level browsing records from Comscore’s panel of internet users in 2020. The panel includes approximately 2.3 million unique machines (a combination of desktop computers, laptops, and mobile devices) and records the URL, timestamp, and estimated duration of each web visit. I observe browsing activity across both desktop and mobile platforms, including Android, iPhone, and iPad devices.

For each browsing session, I estimate the duration of the visit. Following the recommendations of Clemm von Hohenberg et al. (2024) for working with passively collected web-browsing data, I cap estimated session durations at 5 minutes to address the well-known problem of inflated duration estimates caused by idle browser tabs. The resulting dataset provides a granular, minute-by-minute picture of each panelist's online activity throughout the year. Basic demographic information like gender, age group, household income bracket, and race (whether the respondent identifies as Black or Hispanic) is also available for a subset of panelists.

January 6th sources

To measure exposure to extremist content, I construct a list of websites and webpages associated with the Stop the Steal movement using the House January 6th Committee's final report (Select Committee to Investigate the January 6th Attack, 2022). The Committee's investigation provides extensive documentation of the online ecosystem that facilitated the planning and execution of the January 6th attack, including detailed analysis of specific platforms, domains, and individual webpages where Stop the Steal content was disseminated.

I catalogue all domains and individual webpages identified in the report as associated with Stop the Steal content. The resulting list includes Parler itself, stopthesteal.us, 8kun, 4chan (specifically the /pol/ board), Infowars, Telegram, the personal websites and streaming platforms (DLive) of prominent figures such as Nicholas Fuentes, and the subreddit r/The_Donald. In practice, the websites with sufficient traffic to be measured in the Comscore panel are primarily Parler, 8kun, 4chan, Infowars, and Reddit. I use these domains to construct the outcome variable: the total time (in seconds) that each user spends on January 6th-associated sources in each bi-weekly period.

National media sources

To estimate the political ideology of users in the Comscore panel, I draw on a list of 248 national media websites compiled from the Media Cloud project.¹ These websites span the U.S. media landscape and include major national outlets across the ideological spectrum. I aggregate each user’s total time spent on each of these 248 websites across the sample period to construct a user-by-website matrix of media consumption. This matrix serves as the input for the principal component analysis described in the next section, which I use to estimate latent dimensions of media consumption and to construct an appropriate comparison group for the difference-in-differences design.

5 Empirical Approach

This study aims to estimate the effect of adopting Parler on users’ subsequent consumption of extremist content. Two key challenges must be addressed. First, Parler adoption is endogenous—users who join Parler are likely to differ from non-users in ways that are correlated with their consumption of extremist content. To address this, I implement a difference-in-differences design that exploits the staggered timing of Parler adoption across users. Moreover, to mitigate concerns of reverse causality, I focus on people who access Parler after Dan Bongino’s announcement. Some users joined Parler during the Bongino wave in mid-2020, while others did not adopt until after the election. Although Parler adoption is clearly driven by political preferences, media consumption habits, and responsiveness to specific events, the timing of adoption varies across users in ways that can be leveraged for identification.

¹Media Cloud National Media Sources collection: <https://search.mediacloud.org/collections/34412234>.

The second challenge is that Parler adoption is rare, making it difficult to find a suitable control group from the general population. To construct a control group of ideologically similar non-adopters, I estimate the political orientation of each user in the Comscore panel using their media consumption patterns. Because no survey responses are attached to the browsing data, I recover latent dimensions of media consumption through principal component analysis (PCA) on the user-by-website matrix of time spent across 248 national media outlets.

I preprocess the consumption matrix using TF-IDF (term frequency–inverse document frequency) weighting, which dampens the influence of universally popular websites and upweights consumption of more distinctive outlets. I also scale each column to have unit variance before applying PCA. This ensures that the principal components reflect patterns of relative media preference rather than differences in total browsing volume.

The first three principal components admit natural interpretations. The first component separates users who primarily consume national outlets from those who favor local media. The second component separates liberal from conservative media consumers, with outlets such as the New York Times, Slate, and the New Yorker loading on one end and Breitbart, the Daily Caller, and National Review loading on the other. The third component distinguishes consumers of hard news from those who favor soft news and entertainment.

I use the second principal component to restrict the sample to the top 5% most conservative users. This yields a comparison group that is ideologically proximate to politically motivated Parler adopters, substantially reducing the concern that estimated effects are driven by baseline differences in political orientation between treated and control units.

Difference-in-differences estimation

I organize the data into a balanced panel at the user–bi-weekly level, spanning 27 bi-weekly periods across 2020. The treatment indicator is equal to one for all periods after a user first accesses `parler.com`. Users who never access Parler during the sample period are coded as never-treated.

The baseline specification is a two-way fixed effects (TWFE) model:

$$Y_{it} = \beta \cdot \text{ParlerJoin}_{it} + \alpha_i + \gamma_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is the total time (in seconds) that user i spends on J6-associated sources during bi-weekly period t , ParlerJoin_{it} is a binary indicator equal to one if user i has accessed Parler by period t , α_i are user fixed effects, and γ_t are bi-weekly period fixed effects. Standard errors are clustered at the user level.

I exclude users who first access Parler prior to June 2020 (the “January–May” cohort), as their adoption predates the two identifiable waves of growth and likely reflects different selection dynamics. The remaining sample includes users who adopt during the Bongino wave (June–October), users who adopt around the election (November–December), and users who adopt in the post-election period, along with never-treated users. Figure 1 plots the treatment status for all sample units over time.

Recent econometric research has shown that TWFE estimators can be biased in settings with staggered treatment adoption and heterogeneous treatment effects, because they implicitly use already-treated units as controls (Goodman-Bacon, 2021; de Chaisemartin and d’Haultfoeuille, 2020). To assess the sensitivity of my results to this concern, I supplement the baseline TWFE estimates with three alternative estimators designed for staggered settings: the interaction-weighted

Unit

Every even week

Under Control Under Treatment

estimator of Sun and Abraham (2021), the doubly-robust estimator of Callaway and Sant’Anna (2021), and the imputation estimator of Liu et al. (2024); Borusyak et al. (2024). Each of these methods restricts comparisons to not-yet-treated and never-treated units, avoiding the “forbidden comparisons” that can bias TWFE estimates. I present dynamic treatment effect estimates from all four approaches to assess the robustness of the results and to evaluate the parallel trends assumption.

6 Results

Who are Parler joiners?

Before turning to the main estimates, I characterize the users in my sample who adopt Parler during 2020. Table 1 reports the top-loading websites for each of the first three principal components, along with the share of variance explained.

The PCA reveals a media landscape organized along three primary dimensions. The first component separates consumers of national outlets (the New York Times, Newsweek, Forbes, Vanity Fair) from those who primarily visit local and regional media (the Rutland Herald, the San Antonio Express-News, the Press-Republican). The second component captures the familiar left-right divide: liberal outlets such as the New Yorker, Rolling Stone, and Slate load on one end, while conservative outlets including National Review, the Daily Signal, Breitbart, and the Daily Caller load on the other. The third component distinguishes hard news consumers (the Atlantic, the New Yorker, the New York Times, the Guardian) from those oriented toward entertainment and lifestyle content (Bustle, E! Online, People, Us Magazine).

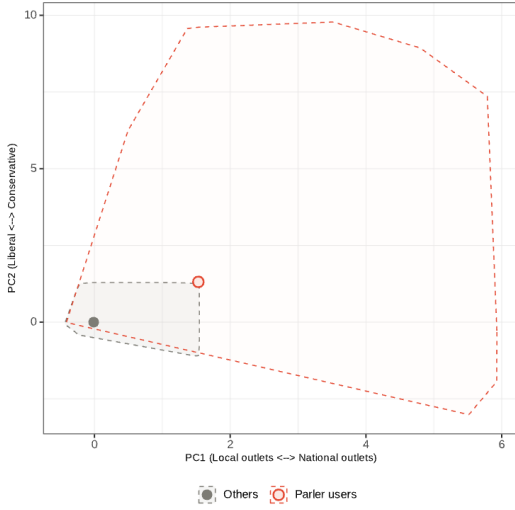
Table 1: Principal Component Loadings: Top and Bottom Media Outlets

		Highest loadings	Lowest loadings
PC1	Local $\leftrightarrow$ National	univision.com, rutlandherald.com, lexisnexis.com, pressrepublican.com, gazettenet.com, elnuevodia.com, recorder.com, norwichbulletin.com, mysanantonio.com, ledger-enquirer.com	thewrap.com, nytimes.com, thedailybeast.com, newsweek.com, qz.com, rollingstone.com, newyorker.com, forbes.com, vanityfair.com, theatlantic.com
PC2	Liberal $\leftrightarrow$ Conservative	newyorker.com, rollingstone.com, bustle.com, nytimes.com, theguardian.com, vanityfair.com, slate.com, ew.com, vice.com, mashable.com	nationalreview.com, dailysignal.com, newsbusters.org, breitbart.com, redstate.com, spectator.org, realclearpolitics.com, hotair.com, dailycaller.com, townhall.com
PC3	Hard $\leftrightarrow$ Soft news	theatlantic.com, newyorker.com, nytimes.com, thedailybeast.com, theguardian.com, washingtonpost.com, motherjones.com, vox.com, salon.com, fivethirtyeight.com	bustle.com, eonline.com, hollywoodreporter.com, people.com, usmagazine.com, thewrap.com, rollingstone.com, variety.com, mashable.com, ew.com

Notes: Table reports the ten websites with the highest and lowest loadings on each of the first three principal components, estimated via PCA on TF-IDF-weighted browsing time across 248 national media outlets. The sample includes approximately 1.4 million user-year observations.

Figure 2: Comparing Parler Joiners in Latent Space

(a) Local/National vs Liberal/Conservative scale



(b) Liberal/Conservative vs. Hard/Soft News scale

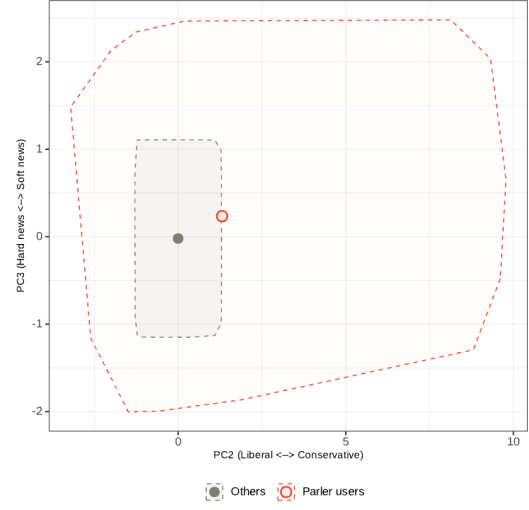


Figure 2 plots the distribution of Parler users and non-users in this PCA space using one-standard-deviation convex hulls. Two patterns are noteworthy. First, Parler adopters occupy a much more dispersed region of the media consumption space than other users, particularly along the conservative dimension (PC2). While the average positions of the two groups are not dramatically different, the variance among Parler users is substantially larger—they include both moderate conservatives and individuals whose media consumption is far to the right of the general panel. Second, Parler users tend to score higher on the national-media dimension (PC1), consistent with a population that pays more attention to national political media. The two groups show no clear separation along the hard-versus-soft news dimension (PC3).

Demographic differences reinforce this portrait. Among the subset of users who report demographic information, Parler adopters are substantially older than non-adopters: 56% are over 55, compared to 26% of non-users. Parler users are also much more likely to access the internet primarily through mobile devices—25% versus 7%—suggesting a user base that is engaged with online content throughout the day rather than only during dedicated browsing sessions.

Table 2: Effect of Parler Adoption on Browsing Behavior

<i>Dependent Variable:</i>	<i>Parler time</i>	<i>J6 source time</i>	<i>J6 source time</i>	<i>J6 source time</i>
Parler adoption	298.5*** (62.65)	341.7*** (63.92)	323.9*** (66.64)	367.9*** (69.41)
<i>Fixed Effects:</i>				
User	✓	✓	✓	✓
Bi-weekly period	✓	✓		
Period × Pre-J6 quintile			✓	
Period × Pre-Bongino quintile				✓
S.E. clustered by	User	User	User	User
N Observation	206,820	206,820	206,820	206,820

Notes: Each column reports estimates from a two-way fixed effects regression. The dependent variable in column (1) is total time (seconds) spent on Parler per bi-weekly period; in columns (2)–(4), it is total time spent on January 6th-associated sources (excluding Parler). The treatment indicator equals one for all periods after a user first accesses Parler. Columns (3) and (4) interact period fixed effects with quintiles of pre-treatment consumption of J6 sources and pre-Bongino consumption, respectively. The sample is restricted to the top 5% most conservative users based on PCA of media consumption. Standard errors clustered at the user level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-tailed test).

Main results

Table 2 presents the main difference-in-differences estimates. I report results for time spent on Parler itself and time spent on J6-associated sources (excluding Parler) across three specifications: the baseline TWFE with user and time fixed effects, a specification with time effects interacted with pre-treatment J6-source consumption quintiles, and a specification with time effects interacted with pre-Bongino consumption quintiles.

The interacted specification is motivated by a natural concern is that the estimated effect reflects reverse causality: prior consumption of extremist content may drive Parler adoption, rather than the other way around. This specification allows users with different baseline levels of extremist content consumption to follow different time trends, absorbing the possibility that pre-existing interest in conspiracy content predict Parler adoption and drives subsequent consumption growth through channels unrelated to Parler itself.

The first panel confirms that Parler adoption is persistent. Users who first access the site continue to spend substantial time on it in subsequent periods. The baseline estimate indicates an increase of approximately 299 seconds (roughly 5 minutes) per bi-weekly period in time spent on Parler.

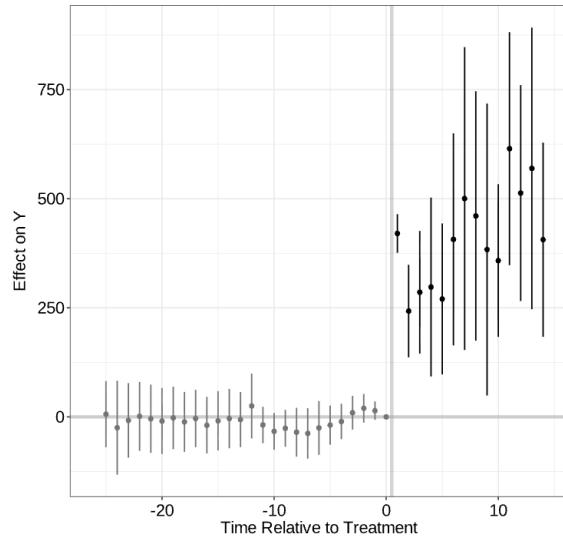
The second panel presents the key results. Adopting Parler is associated with an increase of approximately 342 seconds per bi-weekly period in time spent on J6-associated sources. Given a pre-treatment mean of 70 seconds (with a median of zero), this represents roughly a fourfold increase in exposure to Stop the Steal content. The estimates are stable across the three specifications: interacting time trends with pre-treatment quintiles of J6-source consumption yields an estimate of 324 seconds, while interacting with pre-Bongino consumption quintiles yields 368 seconds. The similarity of these estimates across specifications is reassuring, as it suggests that the results are not driven by differential trends associated with prior levels of extremist content consumption. Finally, it's worth noting that these estimates likely reflect a lower bound in exposure to content, given that I restrict tracking to websites that are mentioned in the House January 6th report.

All estimates are statistically significant, with standard errors clustered at the user level. The total sample includes 206,820 user-period observations, reflecting the restricted sample of the top 5% most conservative users across 27 bi-weekly periods.

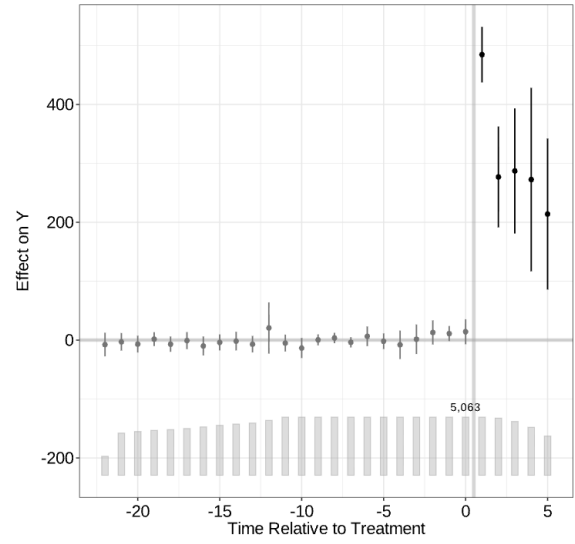
Figure 3 presents dynamic treatment effect estimates from four estimation approaches: the standard TWFE event study, the fixed-effects counterfactual estimator, and two the interaction-weighted estimators by Sun and Abraham (2021) and Callaway and Sant'Anna (2021).

Three features of the event study plots are notable. First, there is no evidence of differential pre-trends in any specification. In the periods leading up to Parler adoption, the estimated co-

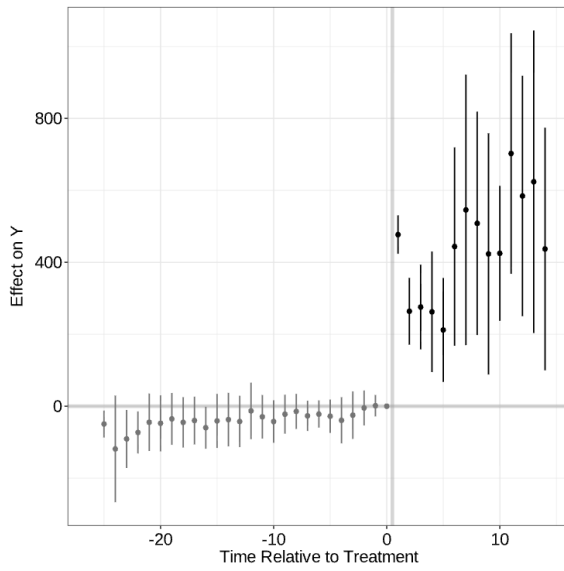
Figure 3: Dynamic Events Study Plot Across Heterogeneity-Robust Estimators



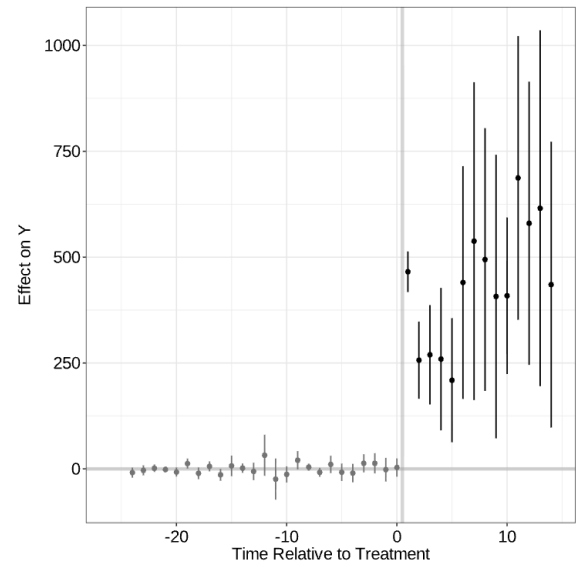
(a) Two-way fixed effect estimator



(b) Imputation estimator



(c) Sun & Abraham Estimator



(d) Callaway & Sant'Anna Estimator

efficients are small, statistically insignificant, and show no systematic pattern. This supports the parallel trends assumption underlying the difference-in-differences design. Second, the treatment effect emerges sharply at the time of adoption and persists throughout the post-treatment period, consistent with a model in which Parler serves as a gateway to a broader ecosystem of extremist content. Third, the estimates are remarkably similar across the four estimation methods. The point estimates from the heterogeneity-robust estimators, which restrict comparisons to avoid the potentially biased “forbidden comparisons” in standard TWFE, differ from the baseline TWFE estimates by less than 10%. This suggests that heterogeneous treatment effects and contaminated comparisons are not a significant source of bias in this setting.

7 Conclusion

This paper provides evidence that adoption of an alternative social media platform can substantially increase users’ exposure to extremist content. Using URL-level web-browsing data and a difference-in-differences design, I find that users who begin accessing Parler during 2020 increase their consumption of websites associated with the “Stop the Steal” movement by approximately fourfold relative to the pre-treatment mean. The effects are immediate, persistent, and robust to a battery of alternative estimators and identification checks.

These findings carry two main implications. First, they suggest that the political effects of social media may be most consequential in precisely the settings in non-mainstream platforms. The dominant paradigm in the literature using large-scale experiments on mainstream platforms has consistently found small or null effects on political attitudes and persuasion. But the conditions under which platforms like Facebook shape political attitudes may differ fundamentally from the

conditions under which niche platforms like Parler shape information consumption. When a platform caters to an ideologically homogeneous user base, imposes minimal content moderation, and operates during a period of political crisis, it can serve as a gateway to a much broader ecosystem of extremist content. The “small effects” consensus may therefore reflect the particular settings in which causal evidence has been most readily available, rather than a general property of social media.

Second, the results speak to the ongoing policy debate over content moderation. When mainstream platforms enforce policies against misinformation and extremist content, some displaced users migrate to alternative platforms with fewer restrictions. My findings suggest that this migration can have real consequences for the information environments of affected users. This does not necessarily imply that content moderation is counterproductive. The welfare effects depend on the relative magnitudes of reduced exposure among the many users who remain on mainstream platforms versus increased exposure among the smaller number who migrate. But it does suggest that the external effects of platform governance deserve careful empirical attention, particularly when it involves a small group of politically active individuals with a high propensity for violence. In short, the consequences of content moderation cannot be evaluated by looking at a single platform in isolation.

Several limitations of this study point toward directions for future research. First, the browsing data capture information consumption but not attitudes, beliefs, or behavior. Whether the increased exposure to Stop the Steal content documented here translated into changes in political views or offline action is an important question that the data cannot address. Second, the Comscore panel, while large, is not a probability sample, and the generalizability of these findings to the broader population of Parler users is uncertain. Finally, the paper studies a single platform during a specific

historical episode. Whether similar dynamics characterize other alternative platforms, in other political contexts, remains an open question. Despite these limitations, the results offer a clear empirical contribution: social media platforms can meaningfully shape what people read online, and the effects can be large when the platform, the users, and the political moment align.

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