The Right Reaction? Evidence of Reactive Partisan Media Exposure During the 2020 Black Lives Matter Protests

Elisabeth Anderson
Division of Social Science
NYU Abu Dhabi

Jeffrey L. Jensen
Division of Social Science
NYU Abu Dhabi

Daniel Karell
Department of Sociology
Yale University

July 14, 2023
Word Count: 7907

Abstract

With the growing evidence that Fox News Channel (FNC) influences its viewers' attitudes and behavior, it is important to understand the factors driving demand for FNC and other partisan media. Prevailing explanations suggest that partisans consume media conforming to their preexisting attitudes. Yet, these selective exposure arguments struggle to explain changes in partisan media habits, especially among infrequent consumers. We combine insights from the selective exposure and focusing events literatures to describe a mechanism, reactive partisan media exposure, by which the polarizing nature of some events increases the demand for partisan media on the extensive margin. We then test the mechanism using a census block group-week panel of Black Lives Matter (BLM) protests and FNC ratings during 2020. We find that the local occurrence of a BLM protest subsequently increased the share of the local population watching FNC by 2%. Our argument and findings help explain how real-world events generate demand for partisan media.

Keywords: Partisan Media, Selective Exposure, Black Lives Matter

1 Introduction

Since at least the 1990s, intellectuals and policymakers have worried about how the rise of partisan media affects viewers' political attitudes, beliefs, and behavior (Manjoo, 2008; Berry and Sobieraj, 2013; Sustein, 2017). Scholars have shared this concern, creating a growing literature that often leverages exogenous variation in the supply of partisan media to examine its political and social consequences. While the topic is not yet completely settled, the evidence indicates that watching conservative partisan media, particularly Fox News Channel (FNC), the highest rated cable news channel in the United States (US), changes viewers' attitudes (Broockman and Kalla, 2022) and beliefs about factual information (Levin et al., 2023), increases polarization and support for conservative ideology (Levendusky, 2013), and boosts vote share for Republican Party candidates (DellaVigna and Kaplan, 2007; Hopkins and Ladd, 2014; Martin and Yurukoglu, 2017; Galletta and Ash, Forthcoming).

While this scholarship has advanced our understanding of the effects of partisan media, we know much less about why people consume it. This is particularly true for infrequent consumers, a critical blind spot as this is precisely the type of viewer whose political attitudes, beliefs, and behaviors are most affected by partisan media (Arceneaux and Johnson, 2013; Boxell, Gentzkow and Shapiro, 2017; de Benedictis-Kessner et al., 2019). The longstanding and prevailing explanation of consumers' demand is that they choose to consume partisan media that conforms with their preexisting attitudes and beliefs, a process known as selective exposure (Festinger, 1957; Sears and Freedman, 1967; Lazarsfeld, Berelson and Gaudet, 1968; Stroud, 2008, 2010). However, linking selective exposure theory to the empirical literature demonstrating large political effects of partisan media – especially FNC – raises an important unresolved question. Namely, for partisan media to have their widely observed effects, they must reach viewers who do not already have strongly partisan attitudes and beliefs, and thus are not the kind of viewers envisioned by selective exposure theory. In other words, a limitation of selective exposure theory as it currently stands is that it tells us little about changes in partisan media consumption, in particular the mechanisms that lead infrequent consumers of partisan media to increase their demand for it.

We propose one mechanism that predicts an increase in partisan media consumption, including among low-frequency consumers. We build on the literature on *focusing events*: events that stimulate widespread public interest and media attention, and that have the power to shape the public policy agenda (Kingdon and Stano, 1984; Birkland, 1998; Bishop, 2014; Chaffin, Cooper and Knotts, 2017). Examples include major natural disasters, serious industrial accidents, shocking crimes, and so on. We theorize that there are political events that similarly generate significant public attention and increase demand for political media, but which also have the feature of being perceived fundamentally differently by people on the political left and right. We hypothesize that such an event, which we term a *polarizing focusing event* (PFE), will result in an increase in demand for partisan media specifically. That is, due to the polarized nature of and increased interest induced by a PFE, we expect to see an uptick in the number of people exposing themselves to partisan media (*i.e.*, on the extensive margin), not just an increase in the amount of partisan media people consume (*i.e.*, on the intensive margin). By identifying one way in which partisan media expands their reach, or *reactive partisan media exposure*, we help bridge the gap between selective exposure arguments and the literature demonstrating the effects of partisan media on infrequent consumers.

We test our claim that PFEs result in a rise in partisan media consumption on the extensive margin by analyzing whether the local occurrence of a Black Lives Matter (BLM) protest in the US during 2020 increased the share of people in the area viewing FNC. The BLM movement of 2020 is an ideal case to test our argument for two reasons. First, the protests clearly fit the definition of a PFE. They drew a great deal of attention from the media, the general public, and policymakers, and the reaction they generated was highly polarized. While many Americans on the political left reacted positively to the protests, many on the right responded with anger, fear and anxiety (Rose, 2020; Smith and King, 2021; Field et al., 2022) and those who opposed BLM tended to have high levels of anti-Black racial resentment and to see the movement as racially threatening (Drakulich et al., 2021; Ilchi and Frank, 2021). Meanwhile, negative coverage of BLM, as well as commentary relating to perceived racial threats to the dominant status position of white Americans, were common topics on FNC in 2020 (Klein, 2020; Confessore, 2022). We therefore expect that the local incidence of a BLM protest generated reactive exposure to the channel.

Second, the BLM protests of 2020 present advantages for empirically testing this mechanism, as it provides substantial spatial and temporal variation in the repeated occurrence of a PFE and its consequences for media consumption.[1] We capture the first part of this year-long, countrywide variation – BLM protests – with a measure of whether a protest occurred in any county in each week during 2020 (N = 2,341), constructed using information from the Crowd Counting Consortium (CCC) (Pressman and Chenoweth, 2022). We then use this measure in a linear model with a fixed-effects counterfactual estimator to predict FNC viewership ratings at the census block group (CBG) level.[2] The measure of FNC ratings – the other part of the variation – is a weekly estimate of the percent of the local market population that is viewing a channel licensed from The Nielsen Company. It is thus ideal for analyzing whether the local occurrence of a PFE – a BLM protest – predicts an increase in the number of CBG residents who viewed FNC during the subsequent week (*i.e.*, on the extensive margin).

We find the occurrence of a BLM protest increased the average FNC ratings by roughly 2% during the following week. This suggests that if a BLM protest had occurred in each county across the US in any given week, more than 50,000 additional viewers would have watched the next week (compared to the average weekly FNC viewership of roughly 2.4 million). These findings offer evidence in support of our proposed mechanism explaining

[1]The countrywide 2020 BLM protests followed the unanticipated murder of George Floyd by Minneapolis police on May 25, 2020 (and other prominent police killings), and by some accounts was the largest protest movement in American history (Buchanan, Bui and Patel, 2020).

[2]A CBG is the second lowest level of spatial aggregation used by the US Census. Each CBG typically contains between 600 and 3,000 residents.

changes in the demand for partisan media consumption, particularly among non-regular consumers.

Two features of our data and analysis allow us to interpret these results causally under reasonable assumptions. First, our results take into account likely confounders. The inclusion of CBG fixed effects adjusts for time-invariant factors, such as demographic characteristics that might lead to greater FNC viewership (*e.g.*, share of population who are non-college educated, white, elderly). The inclusion of week fixed effects adjusts for temporal variation in cable news viewership (*e.g.*, rising viewership due to proximity to the 2020 election). In addition, the results are robust to the inclusion (and omission) of time-varying lagged values of FNC ratings (*i.e.*, lagged outcome variables), areas' histories of BLM protests and comparable PFEs (*i.e.*, counties' cumulative counts of BLM and antifascist ("antifa") protests up to and including the week prior to treatment), and an indicator of whether an antifa protest occurred during the same week at the BLM treatment event. The results are also robust to including the interaction of CBG and week fixed effects, which adjusts for differential trends across CBGs over time (Angrist and Pischke, 2009). The second feature is that we obtain consistent results when using a matrix completion (MC) estimator, which helps account for unobserved time-varying confounders by approximating and estimating the unobservable attributes (Athey et al., 2021; Liu, Wang and Xu, 2023). In addition to these features, diagnostic tests indicate that our preferred model of FNC ratings meet assumptions of spatial panel fixed-effect designs (Liu, Wang and Xu, 2023).

We additionally conduct two placebo tests to help assess our claim that we are capturing the effects of a local BLM protest on changes in FNC viewership. First, we perform a placebo outcome test in which we re-conduct the main analysis with a different outcome variable, in this case CNN ratings (Eggers, Tuñón and Dafoe, N.d.). If BLM simply generated greater general political interest, rather than reactive partisan media exposure, we should see a similar increase in viewership for the more mainstream cable news competitor, CNN.[3]

[3]While some perceive CNN as partisan media, its content is relatively moderate and non-partisan compared to FNC and MSNBC (Budak, Goel and Rao, 2016).

Instead, we find that the occurrence of a local BLM protest has no effect on CNN viewership. These results indicate that we are not simply capturing a general increase in cable news viewership due to local protests, but rather an increase in partisan media specifically. Second, we implement a placebo treatment test in which the main models are replicated with a different treatment variable. The placebo should be similar to the focal treatment – *i.e.*, also a focusing event that both generates lots of public interest locally (because it is perhaps threatening) and varies across space and time – but which would not affect our outcome (FNC ratings) through the mechanism we propose (reactive partisan media exposure in response to PFEs). If we find evidence that the placebo treatment predicts FNC viewership, this is evidence that our design may be flawed (Eggers, Tuñón and Dafoe, N.d.). For our placebo treatment, we selected natural disasters and the environmental threat they pose, and specifically the sustained local presence of hazardous smoke caused by the geographically widespread and devastating forest fires of 2020 (Anguiano, 2020; Burke et al., 2021). We find no effect on FNC ratings of local wildfire smoke, and instead a substantial local increase in ratings for CNN. The two placebo tests offer further evidence for our proposed mechanism: reactive partisan media exposure is conditional upon whether a political event is polarizing. That is, when events that increase political interest *and* are polarizing, people turn to media that accords with their predispositions.

Our study contributes to a large literature on the determinants and consequences of partisan media consumption. We build on research research showing that political events can affect the consumption of news media (Kim and Kim, 2021; Tyler, Grimmer and Iyengar, 2022) and the content of partisan media (Vandeweerdt, 2023) by emphasizing how events influence partisan media consumption among infrequent users. Specifically, we introduce a mechanism – reactive partisan media exposure in response to a PFE – by which those who have been shown to be most affected by partisan media, infrequent consumers, increase their consumption of it. This focus on explaining changes in partisan media consumption begins to reconcile the tensions between the demand-side selective exposure argument and the supply-side literature showing that partisan media has meaningful effects on attitudes, beliefs, and behaviors.

2 Polarizing Focusing Events and Reactive Partisan Media Exposure

Scholars have been thinking about consumers' media choices for decades (Klapper, 1960; Lazarsfeld, Berelson and Gaudet, 1968), but the issue has become more salient since the 1990s with the proliferation of partisan news media outlets on cable television and the internet. Journalists and intellectuals have raised concerns about the emergence of echo chambers and propaganda ecosystems, wherein media users are exposed to slanted news and commentary that validate and amplify their pre-existing political beliefs, exacerbating political polarization and undermining public discourse (Manjoo, 2008; Berry and Sobieraj, 2013; Sustein, 2017; Benkler, Faris and Roberts, 2018).

There is a wealth of research examining the effect of partisan media on political views. Exposure to liberal or conservative media messages contributes to political polarization on both the political left and the right, respectively (Taber and Lodge, 2006; Stroud, 2010; Arceneaux and Johnson, 2013; Levendusky, 2013). Exposure to FNC, specifically, affects viewers' attitudes and factual beliefs (Levendusky, 2013; Broockman and Kalla, 2022; Levin et al., 2023) and increases support for conservative ideology and vote share for Republican Party candidates (DellaVigna and Kaplan, 2007; Hopkins and Ladd, 2014; Martin and Yurukoglu, 2017; Galletta and Ash, Forthcoming). Yet, while offering valuable insights into the effects of partisan media consumption, this "supply-side" approach tells us little about the factors influencing the demand for partisan media among potential viewers.

A prevailing explanation of viewers' demand, mostly coming from communication and media studies, is selective exposure. Selective exposure refers to people's tendency toward confirmation bias, or consuming congenial media messages that confirm their preexisting attitudes and beliefs while avoiding uncongenial messages that challenge those attitudes and beliefs (Lazarsfeld, Berelson and Gaudet, 1968). Research suggests that individuals prefer to consume media with messages, or from sources, that align with their predispositions when it comes to all kinds of topics, from sports to race relations. However, they are most likely to engage in partisan selective exposure when the topic is politically salient (Stroud, 2008; Iyengar and Hahn, 2009).[4]

While insights from the selective exposure scholarship are critical for understanding partisan media consumption, two features of this literature – one theoretical and one methodological – limit a complete picture of the determinants of the demand for partisan media. First, selective exposure theory suggests that individuals will avoid sources that are inconsistent with their preexisting attitudes. Yet, how can partisan media meaningfully shift attitudes, beliefs, and behaviors if only partisans – those with already-developed politicized and polarized attitudes, beliefs, and behaviors – are engaged in selective exposure? Thus, on its own, selective exposure is insufficient to explain increases in demand for partisan media among infrequent users. This is important in light of the evidence that infrequent users are the ones most affected by partisan media (Arceneaux and Johnson, 2013; Boxell, Gentzkow and Shapiro, 2017; de Benedictis-Kessner et al., 2019).

The second limitation is methodological. Much of this research is based on experiments in which research subjects are given a forced choice of what information to consume (*e.g.*, Taber and Lodge, 2006; Garrett, 2009; Hart et al., 2009; Iyengar and Hahn, 2009; Knobloch-Westerwick and Kleinman, 2012). This raises questions of external validity, since choices made under the artificial pressures of the lab do not necessarily predict voluntary choices in the real world (*i.e.*, the opportunity costs of consuming political media is difficult to incorporate into experimental designs). Indeed, in the real world, many people choose not

[4]This does not mean that people always avoid media messages that conflict with their preexisting beliefs and attitudes. Under certain conditions, they are in fact more likely to consume such messages than to consume attitude-confirming content (*e.g.*, Knobloch-Westerwick, 2014). See Kim and Kim 2021 and Tyler, Grimmer and Iyengar 2022 for evidence that partisans decrease consumption in the event of non-congenial news.

to consume much if any political news or commentary whatsoever. Other studies rely on survey data to analyze selective exposure and are therefore able to get at voluntary media consumption (*e.g.*, Stroud, 2008; Hoffner et al., 2009). However, this work often relies on self-reports of media consumption and is therefore vulnerable to response bias. Moreover, existing studies do not tell us whether particular kinds of real-world events might stimulate demand for partisan (as opposed to mainstream) news media, specifically.

We introduce a mechanism that combines the concepts of selective exposure and focusing events to explain why non-habitual users of partisan media increase their demand for it. The term the mechanism reactive partisan media exposure, defined as an increase in selective exposure to partisan media that occurs in response to an external stimulus or trigger. The trigger, as earlier mentioned, is a polarizing focusing event (PFE). A focusing event is an incident – often unanticipated – that captures popular and media attention, and brings certain issues to the forefront of public awareness and policy discourse (Kingdon and Stano, 1984; Birkland, 1998). In theory, a focusing event could elicit a positive response in the media and general public, but existing research generally concentrates on events widely perceived as negative (Birkland, 1998). Major accidents, shocking crimes, or incidents of public disorder expose problems and spur calls to "do something" about them. As a result, focusing events can push problems to the forefront of the policy agenda (Kingdon and Stano, 1984; Birkland, 1998; Bishop, 2014; Chaffin, Cooper and Knotts, 2017).

We conceive of PFEs as a kind of focusing event that is perceived differently by people on the left and the right. We posit that PFEs create demand for partisan news, thereby causing reactive partisan media exposure not only among habitual partisan media consumers, but also among those who consume partisan media less frequently.[5] Thus, in response to a given PFE, we expect to see an uptick in the number of people exposing themselves to partisan

[5]There are at least two sets of processes – one cognitive (*e.g.*, cognitive dissonance (Festinger, 1957), the other emotional (*e.g.*, fear, anxiety (Shoemaker, 1996; Valentino et al., 2009) – through which PFEs might trigger reactive exposure to partisan news media. Of course, cognition and emotion occur in tandem, influencing each other reciprocally. For example, cognitive dissonance triggers negative emotions, which may subsequently drive individuals to seek out confirming messages that reduce dissonance (Jean Tsang, 2019).

media, not just an increase in the amount of partisan media people consume. In other words, in the wake of a PFE, we expect more people to consume partisan media than otherwise would if the event had not occurred.

While we theorize that PFEs would affect the consumption of partisan media on both the left and right, our data only allow us to evaluate our argument about reactive partisan media exposure in the context of conservative partisan media, or, more specifically, FNC viewership. Therefore, we conclude our discussion of our mechanism by considering how our chosen PFEs, BLM protests in US during 2020, should have affected FNC ratings. Given that BLM protests generated strongly negative reactions among those on the political right (Rose, 2020; Drakulich et al., 2021; Ilchi and Frank, 2021),[6] and that these reactions aligned with FNC coverage at the time (Klein, 2020; Confessore, 2022), we expect that significant reactive exposure to conservative media occurred in a given area after a local BLM protest, and for this to be observable as an increase in the localized extensive margin of FNC ratings following a nearby BLM protest.

3 Data

To test the hypotheses that exposure to conservative partisan media will increase on the extensive margin in reaction to a local BLM protest, we construct a CBG-weekly panel (N = 3,372,600) of the focal PFE occurrences (*i.e.*, BLM protests) and partisan media consumption (*i.e.*, FNC viewership ratings). We describe these and other variables used in this section. All variable sources and summary statistics can be found in SI Tables S1.

[6]See also a relevant report from the Pew Research Center: https://www.pewresearch.org/short-reads/2021/09/27/support-for-black-lives-matter-declined-after-george-floyd-protests-but-has-remained-unchanged-since/.

3.1 Protest events

To record whether a BLM protest took place in a county during a given week of 2020, we used data from the CCC (Pressman and Chenoweth, 2022). The CCC is a publicly available database of protest events in the US, compiled through crowdsourced event detection and various sources, such as online news sites and social media. Once an event is nominated for inclusion in the CCC database, the project's co-directors, research assistants, and numerous volunteers review the event's information and update the database accordingly (Fisher et al., 2019). We also used the database to construct a measure of antifa protests, which we use as a covariate in some models to adjust for alternative PFEs while estimating the relationship between BLM protests – the PFEs we have focused on and explained – and FNC viewership.[7]

We manually identified BLM and antifa protests in the database using information on their participants; if participants were BLM or antifa groups, we coded the event as such. For example, actors like "Black Lives Matter Seattle-King County" and "Black Lives Matter DC" led to the protest being designated as "BLM". "Salt Lake City Antifascist Coalition", "Antifa Equity Outreach", and similar participants resulted in an "antifa" label. Labels are not mutually exclusive. Once all events during 2020 were labeled as "BLM" or "antifa" (or neither), we created a binary variable indicating whether a county experienced a BLM or antifa protest during each week of 2020. We then constructed related variables summing the number of BLM or antifa protests a county had experienced up to a given week for each week during 2020. The former is our main predictor. The latter variables, capturing counties' "histories" of protest, serve as time-varying controls. We identified 2,341 BLM and 10 antifa protests, respectively.

The CCC database includes information on events' number of participants (Fisher et al., 2019; Sobolev et al., 2020), which could be used to estimate the effect of protest size on FNC and CNN viewership. Unfortunately, 63% and 50% of BLM and antifa protests, respectively,

[7]Antifa protests are likely a good candidate for another set of PFE occurrences, but the CCC database records only 10 of these incidents, so we instead focus on BLM protests.

are missing size information. There are other protest databases available for use, such as the well-known Armed Conflict Location & Event Data Project (ACLED), although these are not as accessible and transparent as the CCC. While we know of no systematic comparison of CCC, ACLED, and other options, recent research on protest in the US during 2020 report very similar results when using either CCC or ACLED (Karell et al., 2023).

3.2 Cable news ratings

We licensed weekly FNC and CNN ratings from 2020 from The Nielsen Company. (Recall that we use CNN ratings in a placebo outcome test.) Nielsen defines a channel's ratings as the portion of an area's population that is viewing that channel in a given period (in our case, weekly) (Policy and Guidelines, 2020). This gives us a measure of FNC and CNN viewership on the extensive margin, as opposed to, for instance, a measure of per capita amount of television watched.

Nielsen can provide weekly cable news ratings for the entire US during 2020. However, the cost for the complete countrywide and year-long dataset exceeded our available resources. Therefore, we obtained FNC and CNN ratings for a sample comprising 67,452 CBGs (28% of all CBGs) and 28,732 tracts (35% of all tracts), consisting of 31% of the total US population. Specifically, we licensed data consisting of the weekly zip code level FNC and CNN ratings for 20 large Designated Market Areas (DMAs), which are formed by multiple counties.[8] Our sample includes counties – and constitutive CBGs and tracts – in the following DMAs: Atlanta, Austin, Boston, Chicago, Dallas and Fort Worth, Detroit, Kansas City, Los Angeles, Louisville, Memphis, Nashville, New York City, Norfolk (Virginia), Philadelphia, Phoenix, San Francisco, Seattle, St. Louis, Tampa, and Tucson. We then apportioned the zip code level ratings into CBGs and tracts using 2019 crosswalks provided by the US Department of Housing and Urban Development.

While we cannot be sure that the spatial units in our sample of 20 DMAs perfectly

[8]Nielsen divides the US into DMAs, each based on a large population center.

represent all DMAs across the US, we take confidence from the fact that they are located in each region of the country and capture roughly a third of the total population. Moreover, the counties captured by our sample are very similar to all US counties across a range of sociodemographic characteristics, as well as voting behavior in the 2016 US presidential election. Specifically, the mean values of sampled counties' sociodemographic and political characteristic are all within one standard deviation of the population's means (see Supplemental Information (SI) Table S1), including the frequency of BLM protests. (We compare county-level distributions of characteristics because that is the geography with a wide range of sociodemographic data available from the US Census's 2019 American Community Survey.)

3.3 Wildfire Smoke

For the placebo treatment test, we selected the sustained local presence of hazardous smoke caused by the geographically widespread and devastating forest fires of 2020 (Anguiano, 2020; Burke et al., 2021). We construct this indicator using a database of county-day estimates of PM2.5 levels, or particulate matter in the air that are two and a half microns or less in width (and are particularly hazardous to humans) due to wildfires across the US during 2020 (Childs et al., 2022). If these levels corresponded to air quality levels of "unhealthy", "very unhealthy", or "hazardous" (*i.e.*, PM2.5 readings greater than 150) for three or more days during a week, we coded the week as experiencing a wildfire smoke event. We use a threshold of three days to ensure that we are not measuring brief variances in wind direction and thus short, less consequential PM2.5 exposure.

3.4 Dataset

With the measures of protests, and cable news ratings, and wildfire smoke, we construct two panel datasets: CBG-week (N = 3,372,600) and tract-week (N = 1,436,600). We conduct our primary analysis with the CBG panel and a secondary analysis with the tract panel.

Using both geographies helps guard against potential biases resulting from aggregating social phenomena into spatial units (Schutte and Kelling, 2022). Note, however, that checking the robustness of results with larger geographies – in our case, tracts relative to CBGs – poses a harder test of statistical relationships because there are fewer observations. The tract dataset reduces our observations by 57%.

4 Analytical strategy

In this section, we explain our primary models, a series of diagnostics and robustness tests, and two placebo tests. We estimate all the described models and implement the diagnostic tests using the software for fixed effects counterfactual (FEct) estimator introduced in Liu, Wang and Xu 2023.

4.1 Main Analysis

The main analysis examines whether the weekly FNC ratings in CBGs (tracts) tend to increase after a BLM event occurring in the CBGs' (tracts') counties any time during the preceding week. Rather than using contemporaneous weeks, we estimate the relationship between a PFE occurrence and the subsequent week's FNC ratings because the records of ratings from Nielsen are at the week level. Therefore, using the PFEs from the week preceding ratings ensures that we are not modeling ratings as a function of an event that happened *after* some of the ratings were measured. We would risk this error if we modeled events and ratings from the same week. Nevertheless, some of our robustness check models adjust for week-level contemporaneous events and ratings; when doing so, we obtain results consistent with our main results.

Our primary approach is to estimate the average treatment effect on the treated (ATT) using the FEct estimator (Liu, Wang and Xu, 2023). Counterfactual estimators like FEct draw on data under the untreated condition to build models, then use these models to impute counterfactuals of the treated observations. Doing so helps avoid negative weights – observations of (treated) early adopters never serve as controls for late adopters – and corrects biases resulting from treatment effect heterogeneity, both of which are problems with conventional two-way fixed effects (TWFE) models that have recently generated concern (Blackwell and Glynn, 2018; Imai and Kim, 2019; Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021).

Our main analysis consists of a set of three models, each regressing FNC ratings on the BLM protest indicator variable with week and CBG (tract) fixed effects. The models differ by their additional specification. First, we fit a TWFE model with CBG (tract) and week fixed effects and no other covariates. Second, we add three time-varying adjustments to the TWFE: the occurrence of an antifa protest during the preceding, or treatment, week, counties' histories of BLM events (up to an including the week preceding the treatment week), and counties' histories of antifa events (also up to an including the week prior to the treatment week). This second model is our preferred model.

The third model further adds lagged outcomes to the second specification. Including the lagged outcomes could be redundant because, as controls, they would adjust for many of the factors already accounted for by the unit and time fixed effects. However, since they vary over time, adjusting for lagged outcomes potentially helps also account for unobserved time-varying confounders. Because of these advantages and disadvantages, as well as recent insights into the "bracketing relationship" between TWFE and lagged dependent variable (LDV) approaches, which indicate that TWFE and LDV models can provide upper and lower bounds of the true expected ATT (Angrist and Pischke, 2009; Ding and Li, 2019; Marsh, 2022; Xu, 2022), we see our third model as a way to gain useful information about the ATT's lower bounds.

We fit all three models using both the CBG and tract panel datasets. Units have staggered adoption and can switch between treatment and control conditions. All models equally weight observations when computing the ATT because the propensity to receive treatment varies over both units and time. We estimate uncertainty by using non-parametric block bootstrap clustered at the unit level.

4.2 Diagnostics and Robustness

Using the preferred model, we conduct two diagnostic assessments of the modeling assumptions (*i.e.*, functional form, exogeneity, and meeting the feasibility condition) (Liu, Wang and Xu, 2023). The first is a diagnostic placebo test. For this test, we assume that the occurrence of a BLM protest happened two weeks earlier than it in fact did. We then use the FEct estimator to obtain an overall ATT estimate for these pretreatment periods (up to and including the actual treatment period, for a total of three weeks before the outcome is observed). This placebo ATT should not be statistically different from zero.

We examine whether this placebo ATT is different from zero by using two one-sided tests (TOST) to check whether we can reject the null hypothesis that the placebo ATT falls outside a prespecified range (in our case, ± 0.36 multiplied by the standard deviation of the residualized untreated outcome (Hartman and Hidalgo, 2018)). This procedure is a modified equivalence test developed by Liu, Wang and Xu (2023), and has the advantage of being robust to potential biases due to outliers or confounders.

The second diagnostic assessment is a carryover test, or an evaluation of whether BLM protests continue to exert an effect on FNC viewership after they cease. While a continued effect does not threaten our arguments, a lack of carryover aligns better with modeling assumptions (Liu, Wang and Xu, 2023). To test for carryover, we use the same framework as the placebo test, except we compute ATTs for the three weeks after the treatment ends. Once again, if there is no continuing effect, we should not observe evidence of BLM protest increasing FNC or decreasing CNN. Successful tests, or no evidence of placebo or carryover effects, offer evidence that the modeling assumptions are met.

We evaluate the robustness of our results by fitting three supplemental models, each using the preferred model specification. First, we add two more adjustment variables: indicators of whether a BLM or antifa event occurred in a county during the same week the FNC and CNN ratings are measured. These contemporaneous protest variables help address an alternative explanation of why BLM protest may cause an increase in FNC viewership and (potentially) a decrease in CNN viewership. Namely, it is possible that a BLM (or antifa) protest is so distasteful to conservatives, they are more likely to stay home to avoid the activity and thus consume more television, including FNC. Meanwhile, liberals are more likely to leave the home to join the protests, leading to lower ratings for the channels they tend to watch, potentially including CNN. Adjusting for contemporaneous protests with our first robustness check model helps us rule out the possibility that our estimates are capturing the physical avoidance of (or participation in) active BLM (antifa) protests rather than the consequences of post-PFE reactive partisan media exposure.

Our second robustness check model adds an interaction of CBG (tract) and week fixed effects to the preferred model, which accounts for differential trends across spatial units over time (Angrist and Pischke, 2009). Our third check is a model using a matrix completion (MC) estimator. The MC estimator, a generalization of factor-augmented models, helps us account for unobserved time-varying confounders by seeking to approximate and estimate the unobservable attributes (Athey et al., 2021; Liu, Wang and Xu, 2023). The MC tuning parameter is selected using k-fold cross validation (Liu, Wang and Xu, 2023). As with the three main models, we conduct these three robustness checks at both the CBG and tract levels. The models equally weight observations when computing the ATT and use non-parametric block bootstrap clustered at the unit level to estimate uncertainty.

4.3 Placebo Outcome and Treatment Tests

Finally, we conduct two placebo tests to evaluate the soundness of this study's design and our interpretation of the results. We first perform a placebo outcome test in which we estimate the same models as in the main analysis but with a different outcome variable, in this case, CNN ratings (Eggers, Tuñón and Dafoe, N.d.). This tests whether we are capturing the effects of BLM on FNC ratings through the mechanism we propose, reactive partisan media exposure to a PFE, rather than other mechanisms, such as a general increase in cable news viewership. In addition to replicating the main models, we conduct the diagnostic tests and robustness checks using the placebo outcome.

In the second placebo test, a placebo treatment test, the main models are replicated with a different predictor variable. Ideally, the placebo should be a *non-polarizing* focusing event. That is, it should be a localized event that generates significant public interest and demand for media, but not one that elicits radically different responses from viewers based on their existing partisan attachments. It should also be similar to our chosen PFE in that the event varies substantially across time and space. If we find evidence that the placebo treatment predicts the outcome, this is evidence that the design may be flawed or that the interpretation of results may be unsupported (Eggers, Tuñón and Dafoe, N.d.).

Our placebo treatment is the sustained local presence of hazardous smoke caused by the geographically widespread forest fires of 2020 (Burke et al., 2021; Anguiano, 2020). In this scenario, individuals experience the smoke as a local, immediate threat – similar to a protest they dislike or fear – but would turn to media in a way that is largely independent of their existing political ideology. Instead of considering their partisan attachment, they would likely seek out accurate, useful information (Hart et al., 2009) about, in this case, the origin of the smoke event, which could be fires in another part of country (hence an interest in a national news source), the duration of the smoke event, and how to avoid or protect themselves against smoke inhalation.

The placebo treatment test consists of estimating eight models. Beginning with the CBG level data, we use the FEct estimator to regress FNC ratings on the indicator of dangerous wildfire smoke levels during the prior week. This model includes CBG and week fixed effects. Next, we fit a second model that adjusts for the BLM treatment and covariates from our preferred model (and also includes unit and time fixed effects). We then use these two model specifications to also estimate the effect of smoke on the following week's CNN ratings.

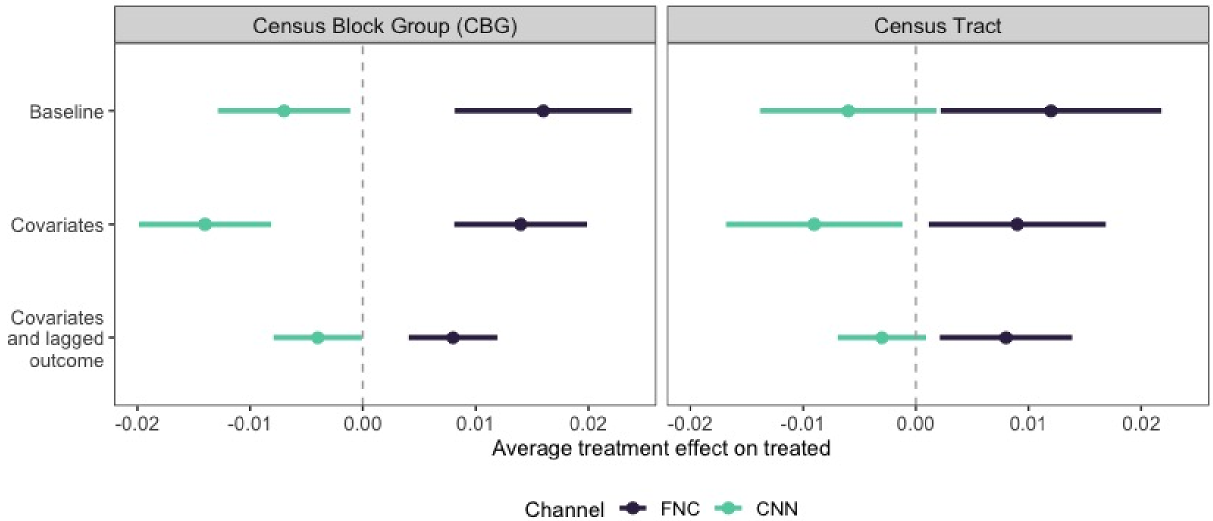

Figure 1: The relationship between local BLM protest and subsequent FNC and CNN ratings during 2020, estimated at the census block group (CBG) and tract levels with three different model specifications. Bars denote 95 percent confidence intervals. Each model includes unit and week fixed effects. Complete results are shown in SI Tables S2 (CBG) and S3 (tract).

And, as with the main analysis, we replicate this placebo treatment test with the tract-level panel dataset. All the eight models compute the ATT by equally weighting observations and estimate uncertainty using non-parametric block bootstrap clustered at the unit level.

5 Results

We present the estimates of the three models from the main analyses in Figure 1, which plots the estimated ATTs and 95 percent confidence intervals obtained by regressing FNC on the BLM protest indicator variable. For ease of comparison and the sake of space, we have included the estimates for the placebo outcome test, CNN ratings, in Figure 1, as well. The left-side panel shows the results when using the CBG panel. Our baseline model, which includes only the outcome and treatment variables and CBG and week fixed effects, indicates that the occurrence of at least one BLM event predicts a statistically significant increase in subsequent FNC ratings ($p < 0.001$).

Our preferred model, the second specification described above, adds three time-varying controls: the occurrence of an antifa protest (or not) during the same week as the treatment BLM protest, the county's history of BLM protest up to the treatment week, and the county's history of antifa events up to the treatment week. This model indicates that a local BLM protest predicts a 0.014 percentage point increase in FNC ratings ($p < 0.001$). Since the mean of our measure of FNC ratings is approximately 0.67 (*i.e.*, Nielsen's mean estimate of the share of people who were viewers in a given week), the estimate suggests that a BLM protest increased the average FNC ratings by more than 2% during the following week.

The third model adds a lagged dependent variable to the preferred model's specification. As explained above, we interpret the third model's estimate as a lower bound of the ATT. We obtain results consistent with the preferred model's results; the model estimates a 0.008 percentage point increase ($p < 0.001$). The complete results for each of these three models are reported in SI Table S2.

Diagnostic assessments of the preferred model's modeling assumptions indicate that the assumptions are met. A diagnostic placebo test suggests that we would observe no effect of a BLM protest if it occurred two weeks earlier than it in fact did. A carryover test shows that the effect "switches off" after the BLM protest. See SI Table S4 for the complete results.

We check the robustness of the results with three supplemental models. First, we adjust for the effect of contemporaneous protests, or BLM and antifa protests occurring during the same week that the ratings are measured. These adjustments help address the possibility that rising FNC ratings are due to an increase in the likelihood that conservatives stay home during the occurrence of a BLM protest and thus watch more television. (For the CNN outcome, this robustness check addresses the possibility that lower CNN ratings are due to liberals being out of the home to participate in protests and thus watching less television.) The second supplemental model adds to the preferred model an interaction of CBG and week fixed effects, which allows us to relax the parallel trends assumption of standard TWFE models and accounts for differential trends across CBGs over time (Angrist and Pischke, 2009). The third supplemental model uses the MC estimator with the preferred modeling specification to help account for unobserved confounding. The results of all three robustness checks are consistent with our main results, and are reporeted in SI Table S5.

Figure 1 also presents the main estimates from the placebo outcome tests (*i.e.*, when FNC ratings are replaced by CNN in each model). Interestingly, we find evidence of a statistically significant negative relationship between a BLM protest and subsequent CNN ratings (also presented in columns 4-6, SI Table S2). Like the FNC results, these results are robust to the supplemental models' alternative specifications, although the model using the MC estimator suggests that the negative relationship may be statistically insignificant (SI Table S5). However, despite these successful robustness checks of the placebo outcome test, the main CNN results – those obtained with the preferred specification – should be interpreted with caution. The diagnostic assessments suggest that its modeling assumptions may not be met (see SI Table S4 for results). Nevertheless, we see the totality of the CNN results as indicating that BLM protests did not increase CNN ratings, and perhaps even decreased them. This is evidence that the main analysis with FNC is not capturing a general increase in cable news viewership, but rather identifying a growing demand for conservative partisan media specifically and offering evidence in support of our argument for reactive partisan media exposure.

It is possible that our results reflect the choice to use the CBG level. As shown in the right-side panel of Figure 1, we therefore replicate our analysis at the next highest level of spatial aggregation, the census tract level (N = 1,436,600). Unsurprisingly, given the greater than 50% reduction in the number of observations, our estimates become less precise. Yet, the relationship between BLM protests and subsequent FNC ratings remains positive and statistically significant at conventional levels in each model. See SI Table S3 (columns 1-3) for complete results. Our preferred model of FNC once again passes the tests of modeling assumptions (SI Table S4), and the supplemental models offer consistent results (SI Table S6).

As also shown in Figure 1 (right-hand panel), each model off CNN ratings at the tract level obtains negative ATT estimates, and our preferred model's estimates remain statistically significant. These results are robust to the supplemental models' alternative specifications and MC estimator (although the MC model's results are once again not statistically significant at conventional levels) (SI Table S6). However, the tests of modeling assumptions suggest that the preferred model may not meet the assumptions, leading us to interpret the tract-level CNN results similarly as the CBG-level CNN results. Namely, BLM protests do not appear to have increased CNN viewership, and perhaps even decreased viewership. The main conclusion, though, is that the results of the census-tract analysis provide us with confidence that our CBG-level findings are not biased due to the effects of aggregating social phenomena into specific spatial units (Schutte and Kelling, 2022).

Finally, the results of our placebo treatment test, in which we use the environmental threat posed by sustained wildfire smoke in place of our main treatment, BLM protests, indicate that the placebo treatment does not predict FNC viewership. When using the CBG panel, the ATT is negative but not statistically significant at conventional levels (SI Table S7). When using the tract panel, the ATT is positive and statistically insignificant (SI Table S8). Interestingly, we find a positive and statistically significant relationship between sustained wildfire smoke and CNN viewership (SI Tables S7 and S8). These findings lend support to our study's design and further increase confidence in our argument that BLM protests increased FNC viewership because people sought media that confirmed their partisan reactions to the movement, and not because it was understood as a kind of general topic of interest or threat. Furthermore, the strongly positive results for CNN ratings are consistent with findings that in the event of non-polarizing focusing event people may select more accurate rather than partisan news sources when they require factual information to accomplish an immediate goal (Hart et al., 2009).

6 Discussion

Despite the substantial progress made in recent decades in the literature studying the causes and consequences of partisan media, the various agendas addressing its primary questions across communications, media studies, psychology, and political science often advance on separate, disconnected fronts. As a result, our understanding of the processes through which the proliferation of partisan media outlets leads to greater polarization of the electorate remains disjointed and incomplete. There is often little attempt to reconcile theories focused on the demand side, such as selective exposure, with research that analyzes how the supply of partisan media affects viewers' political attitudes and behaviors. Similarly, the literature often conflates consumption of political media, both mainstream and partisan, on the intensive and extensive margins. But if demand for partisan media (and the content therein) simply reflects the already-existing predispositions of habitual consumers of political media, then there is little role for partisan media to play in changing attitudes and behaviors, particularly among infrequent consumers. In order to identify the effects of partisan media, we believe these facets need to be more clearly delineated in both theoretical frameworks and research designs.

With this study, we advance the literature on several of these points. First, we combine concepts from two different literatures – selective exposure from communication and media studies, themselves building partly on psychology, and focusing events from public policy and political science – to propose a mechanism by which partisan media consumption increases on both the intensive and extensive margins. We suggest that reactive partisan media exposure may occur among infrequent users in response to a polarizing focusing event (PFE). Concentrating on how PFEs affect partisan media consumption on the extensive margin, we directly test our argument using a CBG-weekly panel of BLM protests and FNC ratings. Our results, which can be interpreted causally under reasonable assumptions, demonstrate that a BLM protest resulted in an increase in the share of the local population watching FNC during the subsequent week. In sum, we find substantial evidence that certain types of political events, specifically PFEs, can lead to an increase in non-regular viewers' consumption of partisan media.

Our proposed mechanism and evidence also highlight several areas where our knowledge needs further development. For one, our evidence does not allow us to examine at the individual level whether the new consumption of conservative partisan media following a BLM protest led to longer-term shifts in cable news viewing habits. We also do not empirically study or theorize whether reactive partisan media exposure due to a PFE affects the political attitudes, beliefs, and behaviors of previously low-frequency viewers. We can only draw on the robust literature on the consequences of consuming partisan media to speculate that the initial exposure to partisan media caused by a PFE may ultimately result in some meaningful change in infrequent viewers' political perspectives and actions. In other words, while our study sheds light on why people may choose to consume partisan media and other work explains the consequences of this decision, future research can trace the entire sequence, from novel demand and consumption to the political outcomes of this consumption.

Future research could also build on our study to explore more widely the relationship between politicized and polarizing events and partisan media consumption. Our evidence supports the importance of these kinds of events for political media outcomes, joining a few recent studies (Tyler, Grimmer and Iyengar, 2022; Vandeweerdt, 2023). However, further research is needed to assess the scope of such events, such as how systematic and lasting their effects can be, as well as to identify the conditions under which media habits change in the absence of such events. In addition, while our mechanism also suggested that we should see similar effects of PFEs on the consumption of leftist partisan media, we did not have the data – in particular, a measure of liberal partisan media consumption – to test this claim. Future research should examine whether PFEs have a similar effect across the political spectrum on partisan media consumption or whether PFEs asymmetrically influence consumption on the political right. More broadly, we think theory and designs focused on jointly examining demand for, supply of, and consequence of partisan media consumption across the political spectrum are fertile ground for future research.

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Supplementary Information

for

The Right Reaction? Evidence of Reactive Partisan Media Exposure During the 2020 Black Lives Matter Protests

July 2023

Table S1: Summary Statistics

	All counties				Counties in sample			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Population	101,868	327,345	66	10,081,570	179,088	409,388	395	5,198,275
Median household income (USD)	52,648	14,990	12,441	142,229	57,132	16,402	25,283	116,100
White (non-Hispanic) share	0.74	0.23	0	1	0.76	0.18	0.09	1
Median age of adult white males	43.47	5.39	22	68.6	42.28	4.87	26.7	59.6
Non-citizen residents share	0.03	0.04	0	0.33	0.03	0.03	0	0.23
Share with bachelors degree	0.15	0.07	0	0.55	0.16	0.08	0.06	0.47
Share with internet service subscription	0.29	0.05	0.08	0.42	0.29	0.04	0.17	0.42
Share of adults not in labor force	0.34	0.07	0.14	0.78	0.33	0.07	0.2	0.61
Gini coefficient	0.44	0.04	0.3	0.71	0.44	0.04	0.33	0.61
Republican vote share (2016)	0.67	0.16	0.04	0.97	0.63	0.19	0.1	0.95
Total BLM protests	0.14	0.71	0	10	0.32	1.15	0	10
Total antifa protests	0.01	0.06	0	2	0.01	0.09	0	2
N	3220				542			

Note: County-level descriptive statistics. Data from the 2019 American Community Survey and MIT Election Data and Science Lab.

Table S2: Effect of BLM Protest on FNC and CNN Ratings, CBG Level

	FNC			CNN		
	(1)	(2)	(3)	(4)	(5)	(6)
BLM protest	0.016	0.014	0.008	-0.007	-0.014	-0.004
	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Covariates	N	Y	Y	N	Y	Y
Lagged outcome	N	N	Y	N	N	Y
CBG FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
Units	67,452	67,452	67,452	67,452	67,452	67,452
Observations	3,372,600	3,372,600	3,305,148	3,372,600	3,372,600	3,305,148

Note: Fixed effects counterfactual estimates of average treatment effect on treated. Units are US Census CBGs; observations are CBG-weeks. Bootstrapped standard errors in parentheses.

Table S3: Effect of BLM Protest on FNC and CNN Ratings, Tract Level

	FNC			CNN		
	(1)	(2)	(3)	(4)	(5)	(6)
BLM protest	0.012	0.009	0.008	-0.006	-0.009	-0.003
	(0.005)	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)
Covariates	N	Y	Y	N	Y	Y
Lagged outcome	N	N	Y	N	N	Y
Tract FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
Units	28,732	28,732	28,732	28,732	28,732	28,732
Observations	1,436,600	1,436,600	1,407,868	1,436,600	1,436,600	1,407,868

Note: Fixed effects counterfactual estimates of average treatment effect on treated. Units are US Census tracts; observations are tract-weeks. Bootstrapped standard errors in parentheses.

Table S4: Results of Diagnostic Tests of the Preferred Model

	CBG		Tract	
	FNC	CNN	FNC	CNN
BLM protest placebo ATT	0.003 (0.003)	-0.014 (0.003)	-0.004 (0.005)	-0.013 (0.004)
TOST p-value	0.000	0.000	0.000	0.000
BLM protest carryover ATT	-0.038 (0.003)	-0.031 (0.003)	-0.050 (0.005)	-0.025 (0.005)
TOST p-value	0.000	0.000	0.000	0.000
Units	67,452	67,452	28,732	28,732
Observations	3,372,600	3,372,600	1,436,600	1,436,600

Note: Diagnostic tests use fixed effects counterfactual estimates of BLM protests' effect on FNC or CNN ratings during the subsequent week obtained with models with the preferred model's specification. Table shows values for average treatment effects on the treated (ATT) and two one-sided tests (TOST). Bootstrapped standard errors in parentheses.

Robustness of main results: Alternative models

Table S5: Alternative Models Estimating the Effect of BLM Protest on FNC or CNN Ratings, CBG Level

	FEct contemporaneous protest		FEct unit time trend		Matrix completion	
	FNC	CNN	FNC	CNN	FNC	CNN
BLM protest	0.015	-0.013	0.020	-0.017	0.011	-0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
Covariates	Y	Y	Y	Y	Y	Y
Contemporaneous protest	Y	Y	N	N	N	N
CBG FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
Unit-specific time trend	N	N	Y	Y	N	N
Units	67,452	67,452	67,452	67,452	67,452	67,452
Observations	3,372,600	3,372,600	3,372,600	3,372,600	3,372,600	3,372,600

Note: Table shows average treatment effect on treated estimated using fixed effects counterfactual (FEct) and matrix completion estimators. The first model FEct model adjusts for contemporaneous BLM and antifa events; the second model includes a unit-specific time trend. Units are US Census CBGs; observations are tract-weeks. Bootstrapped standard errors in parentheses.

Table S6: Alternative Models Estimating the Effect of BLM Protest on FNC or CNN Ratings, Tract Level

	FEct contemporaneous protest		FEct unit time trend		Matrix completion	
	FNC	CNN	FNC	CNN	FNC	CNN
BLM protest	0.010	-0.009	0.014	-0.014	0.011	-0.001
	(0.005)	(0.004)	(0.005)	(0.004)	(0.002)	(0.002)
Covariates	Y	Y	Y	Y	Y	Y
Contemporaneous protest	Y	Y	N	N	N	N
Tract FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
Unit-specific time trend	N	N	Y	Y	N	N
Units	28,732	28,732	28,732	28,732	28,732	28,732
Observations	1,436,600	1,436,600	1,436,600	1,436,600	1,436,600	1,436,600

Note: Table shows average treatment effect on treated estimated using fixed effects counterfactual (FEct) and matrix completion estimators. The first model FEct model adjusts for contemporaneous BLM and antifa events; the second model includes a unit-specific time trend. Units are US Census trcts; observations are tract-weeks. Bootstrapped standard errors in parentheses.

Placebo Treatment Test: Wildfire Smoke

Table S7: Effect of Wildfire Smoke on FNC and CNN Ratings, CBG Level

	FNC		CNN	
	(1)	(2)	(3)	(4)
Wildfire smoke	-0.001	-0.003	0.075	0.078
	(0.014)	(0.014)	(0.021)	(0.022)
CBG FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
BLM treatment and covariates	N	Y	N	Y
Units	67,452	67,452	67,452	67,452
Observations	3,372,600	3,372,600	3,372,600	3,372,600

Note: Fixed effects counterfactual estimates of average treatment effect on treated. Units are US Census CBGs; observations are CBG-weeks. Bootstrapped standard errors in parentheses.

Table S8: Effect of Wildfire Smoke on FNC and CNN Ratings, Tract Level

	FNC		CNN	
	(1)	(2)	(3)	(4)
Wildfire smoke	0.062	0.061	0.103	0.105
	(0.071)	(0.070)	(0.016)	(0.016)
Tract FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
BLM treatment and covariates	N	Y	N	Y
Units	28,732	28,732	28,732	28,732
Observations	1,436,600	1,436,600	1,436,600	1,436,600

Note: Fixed effects counterfactual estimates of average treatment effect on treated. Units are US Census tract; observations are tract-weeks. Bootstrapped standard errors in parentheses.