

The Political Economy of Propaganda: Evidence from US Newspapers*

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Abstract

This study examines whether racial outrage in the media can result from the incentives of elites who seek to divide society for political gain, focusing on the late 19th century Southern United States. At this time, the Democratic Party had strong political incentives to incite racial outrage, and local Democratic elites exercised considerable influence over newspapers, the only mass media at the time. Using high-frequency historical newspaper data and difference-in-differences and triple-difference estimation strategies, we find evidence consistent with Democratic elites orchestrating a racist propaganda campaign to sway white voters. Supporting analyses are consistent with Democrats benefiting from anti-Black propaganda in the subsequent elections. (D72, J15, L82, N91, P, Z1)

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1 Introduction

Emotional media content, particularly when negative or polarizing, can significantly influence political and social behavior. For instance, anti-Islamic tweets by Donald Trump have been linked to an increase in hate crimes against Muslims (Müller and Schwarz, 2023). Similarly, studies have documented the impact of media-induced outrage on voting behavior, discrimination, and violence (e.g., Yanagizawa-Drott, 2014; Adena et al., 2015; Wang, 2021; Ang, 2023). While these works have substantially advanced our understanding of the *effects* of emotion-inducing media content, this article contributes by offering empirical evidence of its *origins*. We provide evidence that racial outrage in mass media can stem from the incentives of political elites who seek to divide society to advance their electoral goals.

Political parties may sometimes benefit from a society divided along racial or sociocultural lines. Consequently, theoretical work has highlighted the exacerbation of these divisions as a political strategy (Glaeser, 2005). In this context, stoking emotions such as outrage can become a powerful tool for politicians, with mass media serving as a key medium for their dissemination.¹ For example, stoking outrage about one racial group may incense members of the other group, persuading them to vote against their economic interests.² While this mechanism has been studied in theory, empirical evidence is scarce. In contemporary settings, identifying whether media outrage is driven by supply-side incentives is challenging, since these incentives are often not directly observable. Moreover, direct control over media outlets by political actors is uncommon, with content more frequently influenced by consumer demand (Gentzkow and Shapiro, 2010).

We turn to an opportunity in history to study how political parties seek to manufacture racial outrage in the media to advance their electoral goals. Our analysis focuses on the rise and fall of the People’s Party, or the Populist Party, in the 1890s Southern United States, alongside the incumbent Democratic Party’s reaction. The Populists were one of the most successful third parties in US history and the first American party committed to redistribution from rich to poor, regardless of race. Their electoral success among poor white farmers signaled a potential realignment in Southern politics, characterized by an alliance between Republicans, supported by Black voters, and Populists, supported by poor white farmers. This coalition posed a direct challenge to the Southern Democratic Party, which relied upon a coalition of white voters, independent of economic status. Consequently, Southern Demo-

¹A significant literature has emerged on the role of emotions in political and social behavior (for reviews, see Goodwin et al., 2001; Jasper, 2011), including the behavioral effects of moral outrage (Crockett, 2017; Salerno and Peter-Hagene, 2013; Skitka et al., 2004; Tetlock et al., 2000).

²The logic extends to cultural divisions. Frank (2007) vividly describes how cultural issues made salient by Republicans have led poor Americans to vote against their economic interests. Shayo (2009) and Bonomi et al. (2021) show how cultural identity can dominate economic interests in voting.

cratic elites viewed the potential Republican-Populist “fusion” as an existential threat to their political hegemony in the South (Woodward, 1955). Historical accounts suggest that this threat created strong incentives for them to deliberately foment racial outrage to split the nascent cross-racial coalition and reclaim support among poor white voters. We find that, in response to this threat, Democratic-affiliated newspapers increased the number of anti-Black, outrage-inducing stories they published. Our findings are more consistent with this surge reflecting a strategic, supply-side response by local Democratic elites than with a purely demand-driven shift in reader preferences, although we cannot fully rule out demand-side forces. Additionally, we find patterns consistent with this targeted campaign of racial outrage having been an effective political strategy: counties with a stronger Democratic newspapers presence—particularly where Democrats faced the greatest threat from Republican-Populist fusion—experienced a significant recovery in the Democratic vote share in the following elections.

We collect novel, fine-grained data on anti-Black content from the text of several thousand newspapers. As the sole mass media of the time with a highly localized readership, newspapers serve as the ideal source for measuring variations in the supply of anti-Black media content both across space and over time. This anti-Black content frequently manifested in stories alleging attacks by Black men on the white community, often involving rape, murder, and other crimes. As Wells (1892) famously reported, these incendiary allegations often served as pretexts to justify the lynching of Black men in the South. Guided by these historical facts, we construct monthly measures of anti-Black propaganda at the newspaper level. Our first measure counts pages on which the terms ‘rape’, ‘murder’, or ‘crime’ co-occur with ‘negro’. A manual review of a random sample of over a thousand articles corroborates the effectiveness of these keywords in identifying anti-Black stories.³

While this keyword-based approach is transparent and straightforward, it has known limitations. It relies on specifying a set of terms that consistently capture anti-Black content across outlets and over time, and the co-occurrence of keywords on the same page does not guarantee that they appear in the same article or in a clearly propagandistic context. To address these concerns, we complement the keyword counts with a supervised, page-level classification approach based on Sentence-BERT (SBERT) embeddings—a recent advance in natural language processing (NLP). Using the subset of newspapers for which full text is available, we manually label pages that contain at least one anti-Black propaganda article and train an embedding-based classifier to predict this label from the full page text. This

³Hereafter, we use the terms “anti-Black content”, “anti-Black stories”, “anti-Black outrage”, and “anti-Black propaganda” interchangeably. While we measure content, we show that these stories likely are indeed propaganda.

procedure allows us to detect anti-Black propaganda even when it is phrased differently from our keywords.⁴

We construct a county-level measure of ‘political threat’ based on the 1892 presidential election results. At the time, Populists and Republicans openly discussed forming electoral “fusions” to challenge Democratic dominance. For example, in Louisiana, the two parties formally agreed on a joint ticket shortly before the 1892 election, setting a precedent that Democratic elites feared could spread to other states, as their explicit goal was to “break the solid South” (White, 1918). Although the presidential election was not decided at the county level, county returns nonetheless carried valuable information for local elites about their vulnerability, and they provide us today with a natural proxy for where elites plausibly felt threatened in future contests. However, Democratic elites were likely to feel threatened only where they did not have a secure majority. To capture this, we define as threatened those counties in which the Democrats received a below-median vote share, which is 60% in our sample.⁵ To validate this measure, we show that, after the 1892 election, newspapers in threatened counties exhibit marked increases in coverage of “fusion” and “opposition” and in the co-occurrence of these terms with threat-related language, relative to newspapers in non-threatened counties. This threat measure is intended to capture cases where Democratic elites were particularly vulnerable to local support for a potential fusion and, thus, faced stronger incentives to incite racial outrage to mobilize poor white voters (Du Bois, 1935; Woodward, 1955).

As a transparent, model-free first step, we plot a simple time series of anti-Black propaganda intensity for newspapers in threatened and non-threatened counties and show that, while threatened counties initially feature less anti-Black propaganda, their propaganda rises more sharply after 1892, closing and temporarily reversing the gap. We then estimate difference-in-differences and triple-differences specifications in both dynamic (event-study) and static forms. We first use an event-study difference-in-differences specification to trace the evolution of anti-Black content in threatened versus non-threatened counties over a sixteen-year period, spanning eight years before and eight years after the 1892 presidential election. This regression compares anti-Black content in counties where Democrats faced greater vulnerability to Populist–Republican fusion with content in safer counties, before and after the 1892 election. To ensure that we are not simply capturing dynamics typical of any

⁴As an additional robustness check, we construct a semantic-similarity-based measure of anti-Black content that is also less sensitive to the precise set of keyword combinations. Specifically, we measure the semantic similarity between the content of each newspaper page and a set of reference statements explicitly related to alleged crimes by Black individuals. These reference statements were compiled through manual review and coding of newspaper articles identified by our keyword approach.

⁵We use the median vote share to ensure an even split. Our results remain robust to alternative plausible cutoff choices (Appendix A.4.1).

election cycle in certain counties, we augment the event-study design with a triple-difference comparison of the 24 months before and after the 1892 election to the 24 months before and after the earlier 1888 presidential election, when the Populist Party had not yet emerged as a political force. All specifications include newspaper fixed effects to absorb time-invariant newspaper characteristics (such as ideological leanings), month fixed effects to capture seasonal and other common shocks, and state-period fixed effects to account for state-specific strategies pursued by Populists and Republicans, as well as Democratic responses to them. We also include time-varying controls for local demographic and urbanization characteristics and, when using the keyword-based outcome, controls for mentions of (i) ‘rape’, ‘murder’, or ‘crime’ and (ii) ‘negro’ to net out shifts in general newspaper coverage of crimes and Black individuals that our measure could otherwise inadvertently capture.⁶

The event-study estimates reveal a clear and politically meaningful pattern. We find no systematic pre-trends in the months leading up to November 1892, lending empirical support to the parallel-trends identifying assumption. A gap in anti-Black propaganda between threatened and non-threatened counties opens gradually and peaks before the 1894 midterms and the 1896 presidential election—on the order of 13% for the keyword-based outcome (approximately 12% of its standard deviation), with somewhat smaller but clearly positive effects for the classifier-based measure—before tapering off as Populist-Republican fusion ceases to pose a serious challenge. In the triple-difference framework, we observe similar patterns for both measures, suggesting that the dynamics are not driven by generic electoral-cycle forces. Here, the effect peaks before the 1894 midterms at around 16% for the keyword-based measure (roughly 14% of its standard deviation). Aggregating these dynamics, our preferred static triple-difference estimates imply that anti-Black propaganda increased on average by about 4–5 percent per month in counties where Democrats received less than 60 percent of the 1892 presidential vote (or 5% of its standard deviation).⁷

The main concern with interpreting of these estimates as reflecting the political supply of propaganda is that the rise of the Populist Party—and the resulting political threat to the Democrats—was not a random event. Recent studies show that the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s (Eichengreen et al., 2019; Klein et al., 2020). This raises the possibility that local economic factors might have simultaneously shaped political preferences and influenced the demand for racist media content.

⁶We also show that the mentions of these terms themselves increases in threatened counties (Appendix Figure A8), motivating their inclusion as controls.

⁷We replicate the event-study analysis aggregating voting returns to the congressional district level, the level at which legislative representation was determined. We find nearly identical dynamic patterns, confirming that our results are not an artifact of defining threat at the county level (see Appendix A.5.2).

We conduct five additional exercises to assess this concern. First, we include interactions of period fixed effects with county and newspaper-level characteristics. Specifically, we control for the economic factors identified by [Eichengreen et al. \(2019\)](#) and [Klein et al. \(2020\)](#) as correlated with Populist vote shares, including railroad penetration, interest rates, and the change in the value of local agricultural portfolios. Additionally, we control for proxy measures of pre-existing local racial animus, including newspapers’ average frequency of anti-Black stories prior to 1892. Adding these pre-determined controls, if anything, increases our estimates.

Second, we conduct a placebo test using newspapers from counties outside the South, which also experienced economic volatility but lacked comparable racial tensions, and where the threat of Populist-Republican fusion was absent. We find no significant relationship between the political threat to Democrats and the prevalence of anti-Black stories in these regions, reinforcing our conclusion that the increased number of stories in the South was specifically tied to the racial dynamics there.

Third, we examine heterogeneity in the impact on anti-Black stories by considering pre-existing racist sentiments and economic inequality across Southern counties. Contrary to a demand-driven explanation, our analysis, using several proxies for local racist sentiments, does not indicate that the effect on anti-Black content was stronger in initially more racist counties. This finding counters expectations that newspapers in counties with higher initial racial biases would experience more pronounced effects, suggesting that local demand for anti-Black stories was not the primary driver of the observed increase. Additionally, we find no meaningful heterogeneity related to economic inequality, further suggesting that local economic grievances did not drive this propaganda.

Fourth, we broaden the analysis by examining whether the post-election increase in propaganda extended to other minority groups. We collect counts of joint mentions of “Asians”, “Chinese”, “Japanese” and “Mexicans” with our crime-related terms. Across all groups, we find no evidence of a systematic increase in such content after the 1892 election. This suggests that the effect was highly specific to the joint framing combining Black people and crime, in line with the political dynamics of the time, and is more consistent with a targeted partisan supply response than with a generalized rise in demand for racist or xenophobic content.

Fifth, by linking the newspaper data to two digitized newspaper directories with information on newspapers’ political affiliation in presidential elections ([Gentzkow et al., 2011, 2015](#)), we can differentiate between newspapers that supported the Democrats and others that endorsed the Republicans, the Populists, or were politically independent. We find that only Democratic newspapers showed a significant increase in anti-Black stories following the 1892

election. Moreover, we detect no corresponding effect in politically independent newspapers, a finding difficult to reconcile with a general demand shock. This further undermines the demand-side explanation and supports a supply-driven model of propaganda influenced by political partisanship. We also incorporate county-period fixed effects to directly address potential time-varying confounders at the county level, allowing us to conduct a within-county comparison across newspapers with different political affiliations. Although this specification is more demanding and faces limitations in statistical power, the resulting estimates remain remarkably consistent with our earlier findings. This consistency reinforces our previous results and demonstrates robustness to potential unobserved county-level variation.

In summary, while we cannot completely rule out demand-side forces, our findings favor a supply-side interpretation over a demand-driven model for the increase in anti-Black outrage. This aligns with studies documenting the intense partisanship of Southern newspapers in this period ([Gentzkow et al., 2015](#); [Hirano and Snyder, 2020](#)), underscoring political motivations as a plausible driver of media content.

In our concluding analysis, we find evidence that the anti-Black propaganda benefited the Democratic Party at the polls. Using changes in Democratic vote share in congressional elections between 1888 and 1896, we find that, on average, the local share of Democratic-affiliated newspapers is not systematically related to changes in Democratic vote share across Southern counties. However, in counties where Democrats were politically threatened by the Populist–Republican fusion, Democrats clawed back support in the years following the 1892 election, and this recovery was substantially stronger in counties with a higher share of Democratic-affiliated newspapers. We do not observe comparable patterns in earlier election cycles or in safer counties. These findings suggest that propaganda was most closely associated with bolstering Democratic power in precisely those areas where the Populist–Republican challenge was strongest, although our evidence is observational and does not identify a causal effect of newspaper control on voting outcomes.⁸

Related literature.— Our study contributes to multiple strands of existing literature. First, we extend the empirical research on media’s influence on political outcomes (reviewed by [DellaVigna and Gentzkow, 2010](#); [Enikolopov and Petrova, 2015](#); [Zhuravskaya et al., 2020](#)). Previous research has predominantly focused on how media exposure affects intergroup conflict and political behavior (e.g., [Enikolopov et al., 2011](#); [DellaVigna et al., 2014](#); [Yanagizawa-Drott, 2014](#); [Adena et al., 2015](#); [Ochsner and Roesel, 2017](#); [Bursztyn et al., 2019](#); [Blouin and](#)

⁸We also examine the effect of political threat on lynchings of Black people. We do not observe an effect on the number of lynchings. This suggests two possible interpretations: the infrequency of lynchings means that there is not enough data to detect an effect, or it could imply that despite the increase in anti-Black propaganda, such content did not lead to lynchings.

Mukand, 2019; Melander, 2021; Müller and Schwarz, 2021; Wang, 2021; Ang, 2023; Esposito et al., 2023; Müller and Schwarz, 2023). By contrast, our work provides evidence that links political incentives to the dissemination of divisive media content, particularly anti-Black propaganda. In this sense, we focus on the determinants of propaganda rather than its consequences.⁹

The post-Reconstruction South offers an especially well-suited environment for studying how political incentives shape media content: a near-monopoly medium (the local press), a clearly timed political shock (the Populist challenge in 1892), and stark, well-documented racial hierarchies. By contrast, in contemporary democracies, direct political control of media is rare; content is often demand-driven (Gentzkow and Shapiro, 2010), and the proliferation of media outlets makes sustained political control difficult (Enikolopov et al., 2025). We combine this setting with newly collected, large-scale historical newspaper data and modern NLP methods to measure anti-Black propaganda at high frequency. By linking this content to election returns and exploiting variation in local political vulnerability, we provide a systematic empirical analysis of how political competition shapes the *supply* of racially charged media narratives.

Second, we contribute to theoretical discussions on the dynamics of social divisions. Murphy and Shleifer (2004) and Glaeser (2005) have emphasized political supply factors, while Shayo (2009) and Bonomi et al. (2021) have highlighted demand factors. Our findings offer empirical support for the supply-side perspective, reinforcing the arguments presented in recent studies on the political impacts of propaganda.

Third, our work has implications for the literature on race and the repression of Black people in the United States (e.g., Du Bois, 1935; Woodward, 1955; Zinn, 1980; Margo, 1982; Williams, 1994; Foner, 1997; Acemoglu and Robinson, 2008; Acharya et al., 2016; Williams, 2017; Cook et al., 2018; Logan, 2020; Logan and Parman, 2017; Masera and Rosenberg, 2021; Suryanarayan and White, 2021; Albright et al., 2021; Masera et al., 2022; Bernini et al., 2023; Testa and Williams, 2025). By quantifying anti-Black stories in thousands of historical newspapers, we provide empirical support to the historical narratives about race and class politics in the United States, particularly the Democrats’ response to the Populist Party and the threat of a Populist-Republican fusion. Two concurrent studies complement our work. Masera et al. (2022) explores the increase in anti-Black media and violence in the post-Civil War era as a response to fears about racial integration. Our analysis adds to this discussion by focusing on a supply-side mechanism of anti-Black media. Testa and Williams (2025) investigate the relationship between lynchings, anti-Black media, and close electoral

⁹A notable recent exception is Zhang et al. (2025), which shows that politicians’ involvement in online debates after mass shootings increases polarization on social media.

races involving the Democratic Party from 1880 to 1900. Together, these studies provide a comprehensive understanding of the interplay between media, racial politics, and electoral outcomes during this period in American history.

We also contribute to the literature studying media capture by political actors and their effects on media content. [Szeidl and Szucs \(2021\)](#) exploit the case of Hungary to document that politicians use advertising to affect media content, expressly to limit coverage of corruption. Similarly, [Tella and Franceschelli \(2011\)](#) document that newspapers in Argentina that were more reliant on government advertising were less likely to report about government corruption on their front page. [Qian and Yanagizawa-Drott \(2017\)](#) document that even in full democracies such as the United States, the government can influence media content. Closer to our setting, [Gentzkow et al. \(2015\)](#) and [Hirano and Snyder \(2020\)](#) document the intense partisanship of newspapers in the late nineteenth-century United States, and [Galvis et al. \(2016\)](#) show that this affected media content: partisan newspapers covered political scandals selectively during this period. We contribute to this literature by showing that political media capture specifically affects media content when and where political elites are threatened.

Finally, our paper complements a growing body of work on populism, both in modern (e.g. [Norris and Inglehart, 2019](#); [Noury and Roland, 2020](#); [Autor et al., 2020](#); [Funke et al., 2020](#); [Rodrik, 2021](#); [Guriev and Papaioannou, 2022](#)) and historical times ([Klein et al., 2020](#); [Eichengreen et al., 2019](#); [De Bromhead and O’Rourke, 2023](#)). Our work examines the response of political incumbents to the emergence of the first Populists, thereby enriching our understanding of populism’s historical roots and impacts.

2 Historical Background

This section outlines four key elements of the Southern political and media environment in the early 1890s that underlie our analysis: (i) the sudden and locally heterogeneous rise of the Populist Party in 1892; (ii) the potential for a biracial Populist–Republican electoral fusion; (iii) the resulting threat to Democratic political control; and (iv) the highly partisan nature of Southern newspapers, which allowed political elites to influence media content.¹⁰

¹⁰We purposefully restrict the scope of this section to the historical features that are key to our research question and the empirical analysis. [Hicks \(1931\)](#) and [Goodwyn \(1978\)](#) provide excellent histories of the Populist Party. [Beeby \(2012\)](#) offers a more recent account focusing on North Carolina. [Du Bois \(1935\)](#), [Woodward \(1955\)](#), and [Hahn \(2003\)](#) trace the history of the political struggle of Black people in the US. A large literature discusses the political role of Black people during the time of the Populist Party, including [Abramowitz \(1953\)](#), [Meier \(1956\)](#), [Shapiro \(1969\)](#), and [Saunders \(1969\)](#).

2.1 The Rise of the Populist Party

The People’s Party—commonly known as the Populists—emerged from farmer protest movements during the agricultural depression of the 1880s. Initially, these organizations attempted to work within the existing two-party system, and many Southern leaders resisted the idea of forming a new party for fear of splitting the South’s white Democratic vote. Only after years of frustration with the major parties’ inaction on farmers’ grievances did the movement consolidate into an independent political party in the early 1890s (Hicks, 1931; Goodwyn, 1978).

The 1892 presidential election marked the Populists’ first major test. Their candidate, James Weaver, won 8.5% of the national vote and drew substantial support from the South, especially among poor white farmers suffering from declining crop prices, heavy debts, and discontent with local Democratic rule. Populist vote shares varied widely across Southern counties (see Appendix Figure A1), providing a natural gauge of the local threat to Democratic incumbents: areas with high Populist support signaled greater vulnerability for local Democratic elites. Prior research finds that this cross-county variation in Populist success was driven by economic distress and agrarian grievances (Klein et al., 2020; Eichengreen et al., 2019), rather than by pre-existing racial animus.

2.2 The Populist Platform and Its Biracial Appeal

The Populists advocated a platform of economic reform and redistribution aimed at alleviating the hardships of farmers and other debtors. Key planks included expanding the money supply (for example, via free coinage of silver), imposing stricter regulation on railroads, introducing a progressive income tax, and curbing the power of banks and large corporations (World-Herald, 1892). These proposals had broad appeal to economically struggling voters across racial lines.

Crucially, Populist organizers in the South also made overtures toward biracial political cooperation. In some localities, Black farmers and activists were invited to Populist meetings and even nominated on Populist tickets (Du Bois, 1935; Woodward, 1955). Some Populist leaders explicitly framed poor white and Black citizens as sharing common economic interests. Although such efforts were uneven and faced opposition from white supremacists within their own ranks, they set the party apart from the Democrats, who by the 1890s were openly committed to restoring white-only rule after Reconstruction. The combination of broad economic appeals and tentative interracial mobilization created the possibility of an electoral coalition—or “fusion”—between the Populists and the Republicans. Since the Republican Party’s remaining base in the post-Reconstruction South was largely Black, a

Populist–Republican fusion could unite a majority coalition against Democratic candidates.

2.3 The Political Threat to Democrats

Southern Democratic elites viewed the rise of Populism as a direct threat to their grip on power. The Populists had encroached on a key Democratic constituency (impoverished white farmers) and showed a willingness to collaborate with Republicans, raising the specter of biracial fusion tickets. Indeed, after 1892, fusion campaigns between Populists and Republicans gained traction in several Southern states (Dew, 1957). In Alabama, Arkansas, Louisiana, and elsewhere, combined Populist–Republican tickets mounted serious challenges to Democratic candidates, underscoring the vulnerability of Democratic control at the local and state level. In many counties the combined Populist–Republican vote exceeded 50%, highlighting how a fusion could potentially unseat the Democrats. The worst fears of Democratic leaders materialized in 1894, when a Populist–Republican alliance won power in North Carolina. Once in office, the fusionists expanded voting access and raised taxes to fund public education, reflecting key Populist priorities (Beeby, 2008). Such outcomes demonstrated that Democratic hegemony in the South could indeed be broken by a Populist–Republican alliance. As White (1918) later noted, the fusion strategy threatened to crack the Democrats’ Solid South control of the region.

Nationally, the Democratic Party moved to blunt the Populist insurgency by co-opting some of its agenda (most notably via William Jennings Bryan’s adoption of the free-silver platform in the 1896 presidential election). In the South, however, Democratic operatives responded with more coercive tactics. The Populist–Republican fusion gave them a potent opportunity to foment racial fear. By allying with the Republican Party—perceived by many white Southerners as the party of Lincoln and Reconstruction—the Populists became vulnerable to the charge that they would bring about “Negro rule.” Democratic newspapers eagerly trumpeted this prospect to rally white voters against the fusion (Woodward, 1955). Notably, such racially charged propaganda was largely unnecessary outside the South, where no comparable alignment of a third-party movement with Black voters existed. In the Southern context, however, stoking racial resentment proved to be a highly effective political weapon for the Democrats.

2.4 Partisanship and Political Influence on Southern Newspapers

Several studies argue that newspapers in the late-19th-century South were often highly partisan, or even under the direct control of political parties. For example, McGerr (1988, p.17) writes, “During elections, papers demonstrated their loyalty to their party by running the

names of its candidates each day on the masthead. A paper failing to do so risked immediate censure from party members.” [Hirano and Snyder \(2020\)](#) systematically measure the partisan behavior and content of newspapers since 1880 and find “patterns (...) consistent with the conventional wisdom that newspapers exhibited a substantial amount of partisan behavior during the late-19th century”. Examining the effect of party control of state government on the economic performance of newspapers in that period, [Gentzkow et al. \(2015\)](#) show that a transition from Republican to Democratic control was associated with a substantial increase in the daily circulation share of Democratic newspapers. While the withdrawal of support for the Republican press played a role, some of the effect likely derived from Democrats exploiting control of the state to suppress Republican newspapers and provide patronage to Democratic papers.¹¹ The authors also note that the South during and after Reconstruction (1865-1900) “stands out (...) for its combination of uniquely powerful political incentives and greatly weakened market discipline.” Consistent with this view, [Petrova \(2011\)](#) finds in a sample of nineteenth-century newspapers that advertising revenue partly explains the rise of the independent press. Advertising revenues in the South, however, were low. Finally, [Masera and Rosenberg \(2021\)](#) shows that newspapers’ pro-slavery content declined in counties that lost their comparative advantage in slave labor. This finding also indicates that elites wielded some control over local newspapers.

3 Data and Measurement

Our empirical strategy requires county-level measures of political threat and of anti-Black stories in newspapers over time. The following section explains how we measure the county-level political threat to the Democratic elites from election data. Then we describe the data source for newspaper content, details our approaches to measuring anti-Black propaganda, and presents its descriptive patterns. Finally, we introduce the other relevant variables used in the analysis. Further details on all variables used in this paper, including their sources and construction, can be found in [Appendix A.1](#).

3.1 Political Threat

The first key empirical challenge is the measurement of local Democratic elites’ perceptions of electoral vulnerability due to the rise of the Populist Party. To this end, we use data on electoral outcomes across counties in the 1892 presidential elections provided by ICPSR

¹¹Patronage may take the form of direct subsidies to newspapers, purchases of newspaper issues by state offices, or jobs and contracts. [Eli and Salisbury \(2016\)](#) and [Folke et al. \(2011\)](#) illustrate the broader prevalence of political patronage in our sample period.

(Clubb et al., 2006). The data set reports county-level vote shares for the Populist Party, the Republican Party, and the Democratic Party in 1892.¹² Of course, U.S. presidential elections were not decided at the county level. However, county returns carried valuable information at the time. Local Democratic elites used them as signals of their vulnerability in subsequent state, congressional, and numerous local elections. In other words, what mattered was not the outcome of the presidential contest itself but what the returns revealed about local electoral preferences and whether fusion tickets between Populists and Republicans might plausibly overturn Democratic dominance in future contests. Hence, county-level presidential returns provide a natural proxy for where elites plausibly felt threatened.¹³

Guided by this logic, we define our political threat measure as an indicator equal to one if the Democrats received a below-median vote share in the 1892 presidential election, which is 60% in our sample (Appendix Figure A2).¹⁴ a lower Democratic vote share implies higher combined support for Populists and Republicans, so threatened counties are precisely those where a fusion ticket would have posed a realistic electoral challenge. Formally, we define

$$\text{Political threat}_c = \mathbb{1}(\text{Democrats}_c < 60\%)$$

for each county c . Figure 1 illustrates this political threat variable for the counties with newspaper data. There is substantial variation in political threat across states and also within states, including between neighboring counties.

Using the newspaper data described in Section 3.2, we also provide direct evidence that this indicator captures meaningful variation in perceived vulnerability to Populist-Republican fusion. In Appendix A.5.1, we show that, after the 1892 election, newspapers in threatened counties exhibit marked increases in coverage of “fusion” and “opposition”, and in the co-occurrence of these terms with words from a threat dictionary, relative to newspapers in non-threatened counties, whereas no comparable patterns arise around the 1888 election. These patterns support our interpretation of the political-threat indicator as capturing locations where Democratic elites plausibly perceived a serious threat of fusion.

3.2 Measurement of Anti-Black Propaganda

Data sources To investigate the occurrence of anti-Black propaganda across newspapers and over time, we draw on text data from *newspapers.com* and *newspaperarchive.com*, two

¹²For Alabama, we augment the data with vote shares from Wikipedia. See Appendix A.1 for more details.

¹³As a robustness check, we replicate the analysis at the congressional district level (Appendix A.5.2) and find nearly identical results.

¹⁴All results are robust to alternative plausible thresholds (Appendix A.4.1).

Political threat indicator  0 1

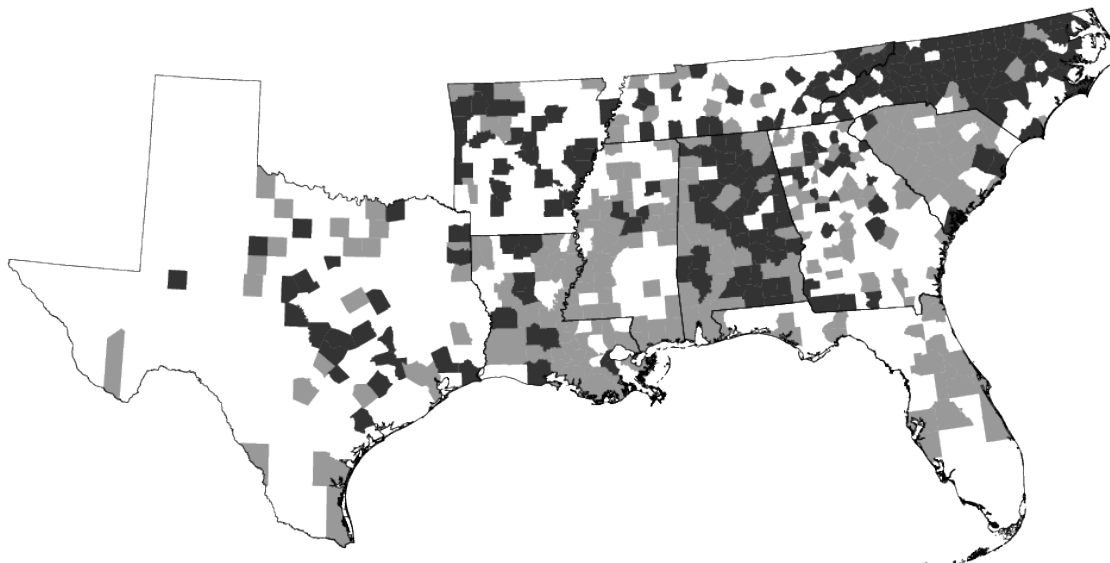


Figure 1: Distribution of political threat to Democratic elites

Notes: The map shows our political threat measure, defined as an indicator equal to one if the Democrats received less than 60% of the county presidential vote in 1892. Darker grey indicates threatened counties. Counties in white do not have newspapers in our data and are not part of the analysis. See Appendix A.1 for details on the data sources and variable definitions.

digital archives of historical and current newspapers. The providers scan newspapers and generate text using optical character recognition (OCR). The databases are the most comprehensive digital newspaper archives currently available: they contain more than one billion pages from over 30,000 newspapers—ranging from big-city dailies to rural weeklies—and continue to grow. They also provide newspaper metadata, including the date of publication and the place of publication for each newspaper. Based on this information, we match each newspaper to a county using 1900 borders. Circulation was often highly local, typically limited to a single county, so we interpret newspaper location as a proxy for newspaper coverage.¹⁵

¹⁵The databases do not contain the universe of US newspapers. When comparing the characteristics of counties with and without newspapers in the databases, we find that counties with newspapers are more likely to be urban, have a higher population share of Black people, and have more manufacturing output per capita (unreported). However, there is almost no association with our political threat indicator. The correlation coefficient between an indicator equal to one if the county is part of the newspaper sample and the treatment indicator of political threat is $\rho = 0.042$ ($p = 0.213$). Moreover, not all titles have a complete run of issues digitized. Some titles only have one issue, while others have thousands. This lack of balance may cause problems for our estimation strategy if selective entry or attrition of newspapers is systematically

Our measurement strategy is shaped by database access restrictions. For *newspapers.com*, we have access only through an automated script that identifies pages containing specified keywords or combinations of keywords. For *newspaperarchive.com*, we have access to the complete full-text data. Consequently, we employ three measurement strategies: one using keyword combinations applied across both databases, and two embedding-based measures that are limited to the full-text data from *newspaperarchive.com*. In the main analysis, we treat the keyword-based and classifier-based measures as our primary outcomes, and use the semantic-similarity measure as an additional robustness check.

Keyword-based measure Our keyword-based approach measures whether the words ‘rape’, ‘murder’, or ‘crime’ co-occur with the words ‘negro’ on the same page. This keyword selection is guided by the accounts of historians (Woodward, 1955) and journalists (Wells, 1892). It is also consistent with the approach in Glaeser (2005), who uses similar keywords to measure anti-Black stories in the *Atlanta Constitution*. We aggregate these counts to the newspaper-month level by summing across pages and use the logarithm of this count (plus one, to avoid dropping observations with no mentions) as our main outcome variable.

Three limitations with this measure warrant attention. First, the unit of observation (or documents) in both databases is the newspaper page rather than the individual article. This implies potential measurement error, as our keywords could co-occur on a page without necessarily appearing within the same article. Second, the databases do not permit a search for keywords within specific types of newspaper content, such as editorials or letters to the editor. Hence, the resulting measure is a combination of reporting of (local and distant) rapes that occurred, their amplification by the local press, op-eds, letters to the editors, and fabrications. Third, although the selected keyword combinations are guided by historical accounts and prior research, our approach inevitably misses synonyms and other related terms associated with anti-Black content.

We address these concerns in two steps. First, to assess the reliability of our method, a research assistant reviewed 1,003 newspaper pages identified by keyword search (see Appendix A.2.1). About 20% (200 of 1003) of pages insinuate a connection between the keywords in

related to our outcome and treatment. In Appendix A.4.3, we document that newspaper exit or entry is not significantly correlated with our treatment indicator after the 1892 election, and that exiting or entering newspapers did not differ in their propaganda intensity in threatened counties compared to non-threatened counties after the 1892 election. We also replicate our analyses in the balanced sample. Finally, Beach and Hanlon (2022) document that for Alabama, the only Southern state they consider, *newspapers.com* covers 36% of all newspapers listed in the 1910 newspaper directory, and *newspaperarchive.com* covers 3%. Additionally, in Alabama, Democratic newspapers are not more likely than Republican newspapers to be digitized by *newspapers.com*.

the same article, and the vast majority (83%, 166 of 200) of these, in turn, are reports of actual or alleged crime, of which again almost half (49%, 81 of 166) explicitly state that the perpetrator was Black and the victim white. Given the simplicity of our method, we consider these rates acceptable. Appendix Figure A5 shows four examples of newspaper articles in our dataset. We also document in Appendix A.5.3 that detection rates do not differ systematically with newspaper affiliation and treatment.

Classifier-based measure Second, for the subset of newspapers in *newspaperarchive.com* where we observe full text, we construct a supervised, page-level classifier to detect anti-Black propaganda. We begin by drawing a sample of pages and manually coding whether each page contains at least one anti-Black propaganda article, using the same conceptual criteria as in our keyword validation exercise (Appendix A.2.1). We then embed each page using Sentence-BERT (SBERT) embeddings (Reimers and Gurevych, 2019) computed from its full text and train a binary classifier to predict the human-coded label from the embedding.¹⁶ Applying the trained model to all available full-text pages yields a predicted probability that each page contains anti-Black propaganda. As with the keyword-based measure, we aggregate these predictions to the newspaper-month level by summing the predicted probabilities across pages, yielding the expected number of propaganda pages in a given newspaper-month. Beyond providing an independent signal, this classifier-based measure mitigates concerns that newspapers with different leanings may use different wording for similar content, since SBERT embeddings capture similarity in meaning even when surface keywords differ.

Semantic-similarity measure Third, using the same SBERT embeddings, we construct a simpler semantic-similarity measure that we use as a robustness check. Starting from pages identified by the keyword search, we compile a set of short reference statements that explicitly describe alleged crimes committed by Black individuals (Appendix A.4.6). We embed each reference statement using SBERT and then, for every newspaper page, compute its semantic similarity to this reference set. This yields a continuous page-level score indicating how closely the language on that page resembles the prototypical anti-Black crime narratives in our reference set. Aggregating these scores to the newspaper-month level and re-estimating our main specifications, we find very similar patterns to those obtained with the keyword-based and classifier-based measures, indicating that our results are not driven by the specific choice of keywords.

¹⁶Appendix A.2.3 describes the training sample, model specification, and performance metrics in detail. In out-of-sample tests, the classifier displays good predictive performance.

Complementarity between the measures The correlation between the keyword-based measure and the classifier-based (semantic-similarity-based) measure is 0.65 (0.40). Correlations of this magnitude are natural in this setting. First, all measures are noisy and target related but not identical constructs. The keyword-based measure is a narrow, transparent indicator of a very specific type of content (pages where ‘negro’ co-occurs with ‘rape’, ‘murder’, or ‘crime’). The classifier and semantic-similarity measures are deliberately broader: they capture pages whose overall language resembles hand-coded racist crime stories, even when the exact keywords are absent. Each measure therefore picks up some instances that the others miss, and all suffer from outcome noise. In such a case, we do not expect correlations anywhere near one, even if all measures proxy the same underlying latent concept of ‘anti-Black propaganda.’ Second, wording differences across outlets further reduce the correlation. Newspapers with different audiences and leanings may use different phrases to express similar ideas, making raw keyword counts less comparable across outlets and strengthening the case for embedding-based measures that rely on the full text. In sum, while the keyword-based measure provides a transparent and historically grounded indicator of one important type of anti-Black story, the classifier-based and semantic-similarity measures leverage the full text to capture broader rhetorical strategies and alternative phrasings. Crucially, we do not aim to validate one measure against another in levels, but rather to show that inferences about the effects and dynamics of political threat are robust across quite different measurement strategies. The value of these complementary methods lies in the robustness of the estimated treatment effects, not in achieving a near-perfect correlation in levels.

Geography and trends of anti-Black propaganda We now describe the patterns of anti-Black propaganda across counties and over time. We begin with geography. Figure 2a maps the average of the keyword-based measure of anti-Black propaganda across Southern counties between November 1884 and October 1900.¹⁷ Darker colors indicate more intense propaganda; no data are available for counties in white. The map reveals pronounced differences both across and within states, including between neighboring counties. In the next section, we will use this spatial variation, together with differences in political threat, to examine how vulnerability to Populist-Republican fusion affected the intensity of anti-Black propaganda.

As a first step towards our main analysis in the next section, we study the temporal dynamics of anti-Black propaganda in threatened and non-threatened counties. In Figure

¹⁷To account for heterogeneity in page volume, general coverage of crimes and Black individuals that our measure could otherwise capture, and fluctuations in propaganda over time, we residualize the raw measure as described in the figure notes. We define Southern states as those displayed in the map below. These states are the former Confederacy without Virginia, where Populism “never took root” (Link, 1979).

2b, we plot the monthly averages of our residualized keyword-based outcome for politically threatened and non-threatened counties from November 1888 to October 1896. Importantly, this figure does not include any fixed effects or other controls; it simply compares the average residualized outcome over time for high- versus low-threat counties.

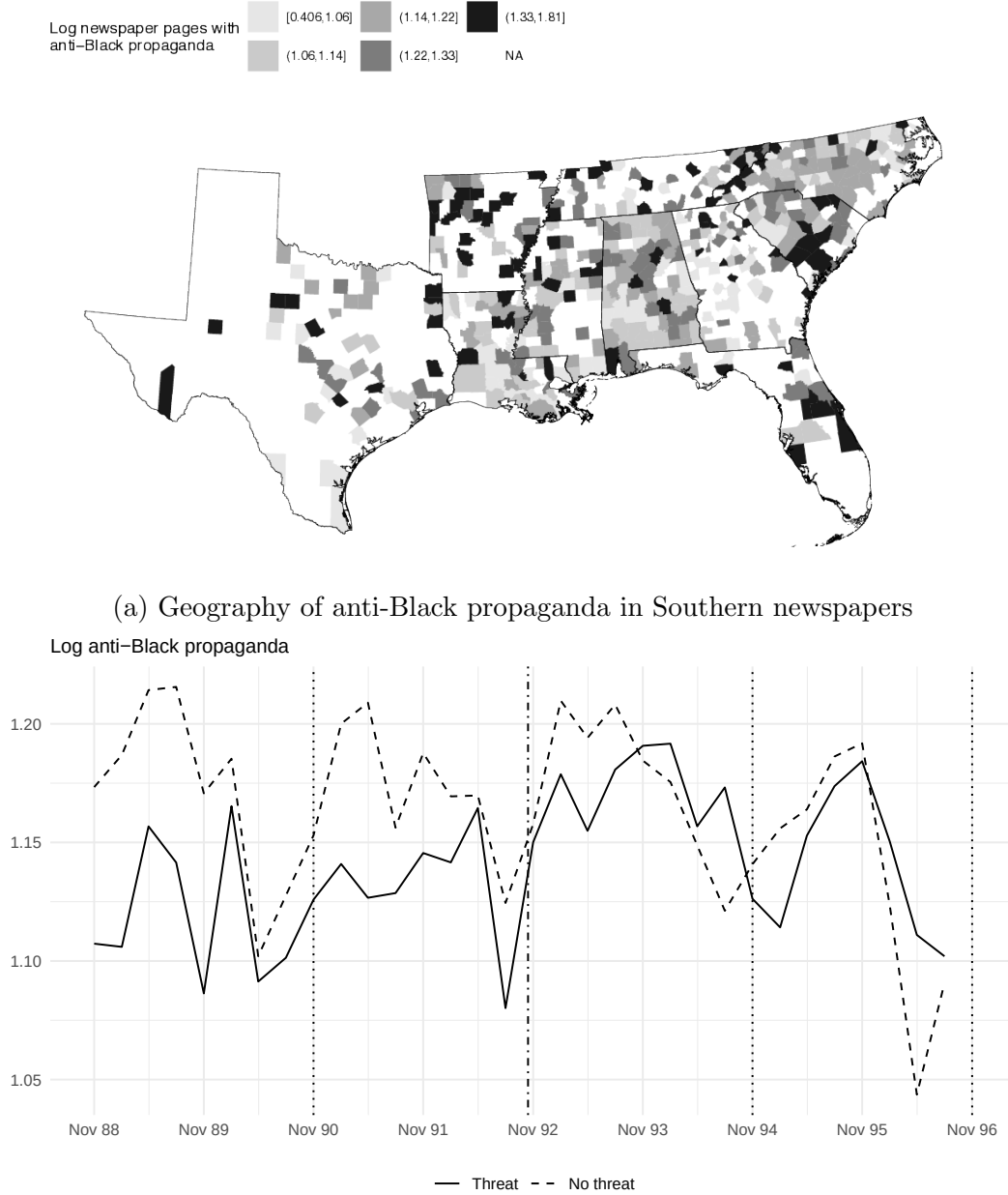
The pattern is informative. Before the 1892 election, including in the run-up to the 1890 midterms, the line for threatened counties (solid line) lies systematically below that for non-threatened counties (dashed). After 1892, this relationship changes: in the run-up to the 1894 midterms and the 1896 presidential election, anti-Black propaganda in threatened counties increases more strongly than in non-threatened counties, so that the gap closes and, for several election months, reverses. This simple visualization thus anticipates our formal estimates: counties that were initially less prone to anti-Black propaganda become relatively more propaganda-intensive once they are politically threatened by Populist–Republican fusion.¹⁸

To summarize, the (minimally residualized) raw data offer preliminary support for our hypothesis. There is substantial variation in average anti-Black propaganda even across neighboring counties, and during the period when the Populists posed a threat to Democrats, anti-Black propaganda was elevated in counties in which the Democrats received below-median vote shares in the 1892 presidential election.

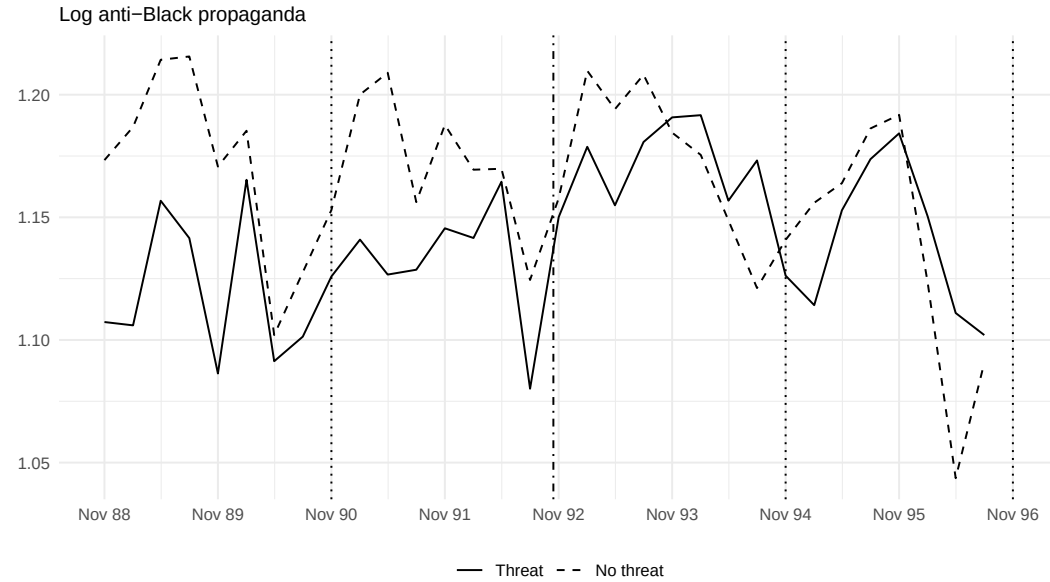
3.3 Other Data

We use several other data sources in our analysis. First, we access county-level socioeconomic characteristics from the 1890 United States census, lynchings from the Historical American Lynching (HAL) database, and counties’ railway miles per square mile in 1890 (Donaldson and Hornbeck, 2016). We also compute changes in counties’ agricultural portfolio from 1888 to 1892 following the method in Eichengreen et al. (2019). Second, we draw on Gentzkow et al. (2011) and Gentzkow et al. (2015), who digitized newspaper directories to provide information about newspapers’ political affiliations in presidential elections. We link this information to our data set of newspaper content to distinguish between newspapers that supported the Democratic Party and those that endorsed other parties or were independent. We identify the political affiliations for the newspapers not covered by these sources and

¹⁸This pattern is not specific to the keyword-based measure: we construct an analogous figure for our classifier-based outcome and report it in Appendix Figure A6. The classifier-based graph shows the same qualitative pattern: threatened counties start with distinctly lower levels of anti-Black propaganda, but their propaganda rises more steeply after 1892, substantially narrowing the gap relative to non-threatened counties during the mid-1890s. In Appendix Figure A7, we further extend both raw-data series back to 1884 and forward to 1900; while these longer series are naturally noisier, we again do not see comparable breaks around earlier elections.



(a) Geography of anti-Black propaganda in Southern newspapers



(b) Evolution of anti-Black propaganda in US newspapers by counties' threat status

Figure 2: Spatial and temporal patterns of anti-Black propaganda

Notes: Top panel: The map shows the cross-county distribution of average keyword-based anti-Black propaganda between November 1884 and October 1900 in the South. To account for heterogeneity in page volume, general coverage of crimes and Black individuals that our measure could otherwise capture, and fluctuations in propaganda over time, we residualize the raw measure by (i) the logarithm of total page count, (ii) the logarithm of the number of crime mentions, (iii) the logarithm of the number of 'negro' mentions, and (iv) year-month mixed effects. Darker colors indicate above-average anti-Black propaganda in a particular county. No newspaper data are available for counties in white. Bottom panel: The figure shows the time variation in keyword-based anti-Black propaganda for threatened (solid line) and non-threatened (dashed) counties from November 1888 to October 1896. The raw measure is residualized by (i) to (iii). See Appendix A.1 for details on the data source and variable definition.

discuss the procedure in Appendix A.2.2. This additional information enables us to test whether all newspapers spread more anti-Black propaganda after November 1892 or whether the effect is limited to newspapers affiliated with the Democrats.

Appendix A.1 provides details on the sources and construction of all variables employed in the analysis. Appendix Table A1 reports the summary statistics of the full sample, and Appendix Tables A2 and A3 report the summary statistics of the subsamples used in difference-in-differences and triple-difference analysis, respectively.

4 The Impact of Political Threat on Propaganda

In this section, we lay out our empirical strategy and present our main findings. We find that anti-Black propaganda increased differentially in Southern counties where Democrats were politically threatened by the Populist–Republican fusion, particularly in the run-up to the 1894 midterm and the 1896 presidential elections. This pattern is distinct from general trends in other areas or periods and does not extend to similar coverage targeting other minority groups. The estimated effects are robust to flexibly controlling for local confounders and to a range of alternative specifications. Taken together, these results and our heterogeneity analyses are more consistent with a politically motivated, supply-side response by Democratic elites than with a purely demand-driven shift in reader preferences, although we cannot fully rule out a demand-side interpretation.

4.1 Estimating Equations

Our empirical strategy builds on difference-in-differences and triple-differences designs, estimated in both dynamic (event-study) and static forms. We first use event-study specifications to trace the evolution of anti-Black propaganda in threatened versus non-threatened counties around the 1892 presidential election, and then summarize the core of these dynamics using static two-period DD and TD estimators.

Event-study difference-in-differences around the 1892 election. Let y_{it} denote one of our newspaper-month outcomes for newspaper i in month t from November 1884 to October 1900 (typically the logarithm of the number of pages containing anti-Black propaganda, as defined in Section 3.2). We define $\text{Political threat}_{c(i)}$ as an indicator equal to one if county $c(i)$, where newspaper i is located, is classified as politically threatened based on the 1892 presidential returns (Section 3.1). We index three-month bins relative to the November 1892 election by τ_t (e.g., $\tau_t = 0$ for November 1892 to January 1893, $\tau_t = 1$ for February to April

1893, etc.). We use three-month bins rather than months to reduce noise and increase the precision of the estimates.

Our baseline dynamic difference-in-differences specification is

$$y_{it} = \sum_{k \neq -1} \beta_k \left[\text{Political threat}_{c(i)} \times \mathbf{1}(\tau_t = k) \right] + \alpha_i + \alpha_{s,t} + X_{i,t} + X_{c(i)} \times \alpha_t + \varepsilon_{it}, \quad (1)$$

where we omit $\tau = -1$ (August to October 1892) as the reference period. Newspaper fixed effects α_i absorb time-invariant newspaper characteristics (such as baseline partisanship) and state-period fixed effects $\alpha_{s,t}$ allow for state-specific strategies and policies over time, such as the continued rise of ‘Jim Crow’ throughout this period (Althoff and Reichardt, 2024). X_{it} includes time-varying newspaper-month controls: the logarithm of total page count and—when the keyword-based measure is the outcome—the logarithms of the number of mentions of crime-related keywords and of ‘negro’. The former accounts for any mechanical relationship between our outcomes and higher newspaper volume, and the latter net out shifts in general reporting on crimes or Black people that our keyword-based measure could otherwise inadvertently capture.¹⁹ Finally, $X_{c(i)}$ is a vector of time-invariant pre-determined county characteristics (log population, population density, and the urban population share based on the 1890 Census) interacted with month fixed effects (α_t) to allow the evolution of propaganda to differ flexibly by local demographics and urbanization. Standard errors are clustered at the county level; Appendix A.4.4 reports inference under alternative clustering choices.

The coefficients β_k trace the evolution of the difference in anti-Black propaganda between threatened and non-threatened counties over time. The identifying assumption is that, in the absence of the Populist-Republican threat, newspapers in threatened and non-threatened counties would have followed parallel trends in propaganda. We formally assess this assumption by examining the pre-election coefficients β_k for $k < 0$: lack of systematic pre-trends supports the validity of the design.

Event-study triple-differences using the 1888 election as a benchmark. To ensure that we are not simply capturing dynamics typical of *any* election cycle in certain counties, we augment the event-study design with a triple-difference comparison to the 1888 presidential election, when the Populist Party had not yet emerged as a national force. We pool data from two election cycles: a four-year window around the 1888 election (November 1886 to

¹⁹We find that both types of coverage increase in threatened counties after the 1892 election, and that these increases persist when we compare the 1892 cycle to the earlier 1888 cycle (Appendix Figure A8).

October 1890) and a four-year window around the 1892 election (November 1890 to October 1894). We define an indicator $\mathbf{1}(1892 \text{ cycle}_t)$ equal to one in the 1890–1894 window and zero in the 1886–1900 window. We then estimate

$$y_{it} = \sum_{k \neq -1} \left[\beta_k (\text{Political threat}_{c(i)} \times \mathbf{1}(\tau_t = k) \times \mathbf{1}(1892 \text{ cycle}_t)) \right. \\ \left. + \theta_k (\text{Political threat}_{c(i)} \times \mathbf{1}(\tau_t = k)) \right] + \text{Political threat}_{c(i)} \times \mathbf{1}(1892 \text{ cycle}_t) \\ + \alpha_i + \alpha_{s,t} + X_{it} + X_{c(i)} \times \alpha_t + \varepsilon_{it}, \quad (2)$$

where τ_t is again defined relative to the November election in each cycle (with $\tau = -1$ omitted in both). The β_k coefficients capture how the dynamics of propaganda in threatened versus non-threatened counties differ in the 1892 cycle relative to the 1888 cycle. If earlier political challengers (such as the Greenback Party or the Farmers' Alliance) had already prompted similar responses in the same counties, we would expect to see comparable post-election patterns in the 1888 cycle; the triple-difference coefficients β_k allow us to test this directly. Standard errors are clustered at the county level as in the DD specification; Appendix A.4.4 reports inference under alternative clustering choices.

Static two-period DD and TD specifications. The event-study estimates provide our most transparent view of the timing and dynamics of propaganda. For ease of interpretation and to summarize the overall magnitude of these effects, we also estimate corresponding static DD and TD models that collapse the post-election period into a single indicator.

For the 1892 cycle, our static difference-in-differences specification is

$$y_{it} = \beta^{DD} [\text{Political threat}_{c(i)} \times \mathbf{1}(\text{Post election}_t)] + \alpha_i + \alpha_{s,t} + X_{it} + X_{c(i)} \times \alpha_t + \varepsilon_{it}, \quad (3)$$

where $\mathbf{1}(\text{Post election}_t)$ equals one in the 24 months following the November 1892 election and zero in the 24 months before. As before, the unit of observation is a newspaper-month in the 1890–1894 window. The coefficient β^{DD} summarizes the average post-election divergence in propaganda between threatened and non-threatened counties.

To compare the 1892 and 1888 election cycles in levels, we estimate a static triple-difference model:

$$y_{it} = \beta^{TD} [\text{Political threat}_{c(i)} \times \mathbf{1}(\text{Post election}_t) \times \mathbf{1}(1892 \text{ cycle}_t)] \\ + \gamma_1 [\text{Political threat}_{c(i)} \times \mathbf{1}(\text{Post election}_t)] + \gamma_2 [\text{Political threat}_{c(i)} \times \mathbf{1}(1892 \text{ cycle}_t)] \\ + \alpha_i + \alpha_{s,t} + X_{it} + X_{c(i)} \times \alpha_t + \varepsilon_{it}. \quad (4)$$

Here the sample includes both the 1888 and 1892 election cycles, using the same four-year windows as in the dynamic specifications. The triple-difference coefficient β^{TD} captures how much more (or less) propaganda increased in threatened versus non-threatened counties after the 1892 election, relative to the analogous difference around the 1888 election.

4.2 Results

Panels (a) and (b) of Figure 3 plot the event-study difference-in-differences estimates from equation (1) for the keyword-based and classifier-based outcomes, respectively. All specifications include our baseline controls introduced above. For both measures, the pre-1892 coefficients fluctuate around zero, indicating no systematic pre-trend. There is no visible break at the November 1892 election itself: coefficients just before and just after the election are essentially zero. Only several months later do the estimates turn positive and begin to rise. The gap between threatened and non-threatened counties then builds gradually, peaking before the 1894 midterms and the 1896 presidential election—on the order of 13% (0.12 log points) for the keyword-based outcome, with somewhat smaller but clearly positive effects for the classifier-based outcome—before declining again towards the end of the decade as Populism ceases to pose a serious challenge. Overall, the dynamics exhibit a gradual build-up and eventual tapering off.

In the triple-difference specification, we extend this design by comparing the 1892 election cycle to the earlier 1888 cycle. Panels (c) and (d) of Figure 3 report the corresponding event-study coefficients from equation (2) for a 24-month window around each election (24 months before and after November 1892, compared to the analogous window around the 1888 election). For both outcomes, the pre-election coefficients are statistically indistinguishable from zero, and the effects become clearly positive from approximately six months onward, with the largest effects observed between 9 and 24 months after the 1892 election. The effect peaks before the 1894 midterms at around 16% (0.15 log points) for the keyword-based measure and 9% (0.08 log points) for the classifier-based outcome. We do not see a comparable pattern around the 1888 election, indicating that the post-1892 build-up is not driven by generic electoral-cycle dynamics or by earlier political challengers that were already strong in the same counties.

Table 1 reports the static difference-in-differences and triple-differences estimates corresponding to equations 3 and 4. In the difference-in-differences specification, our baseline estimates for both the keyword-based and classifier-based measures imply that anti-Black propaganda increased on average by about 3% (0.028–0.034 log points) per month in threatened compared to non-threatened counties. In the triple-difference framework, the corre-

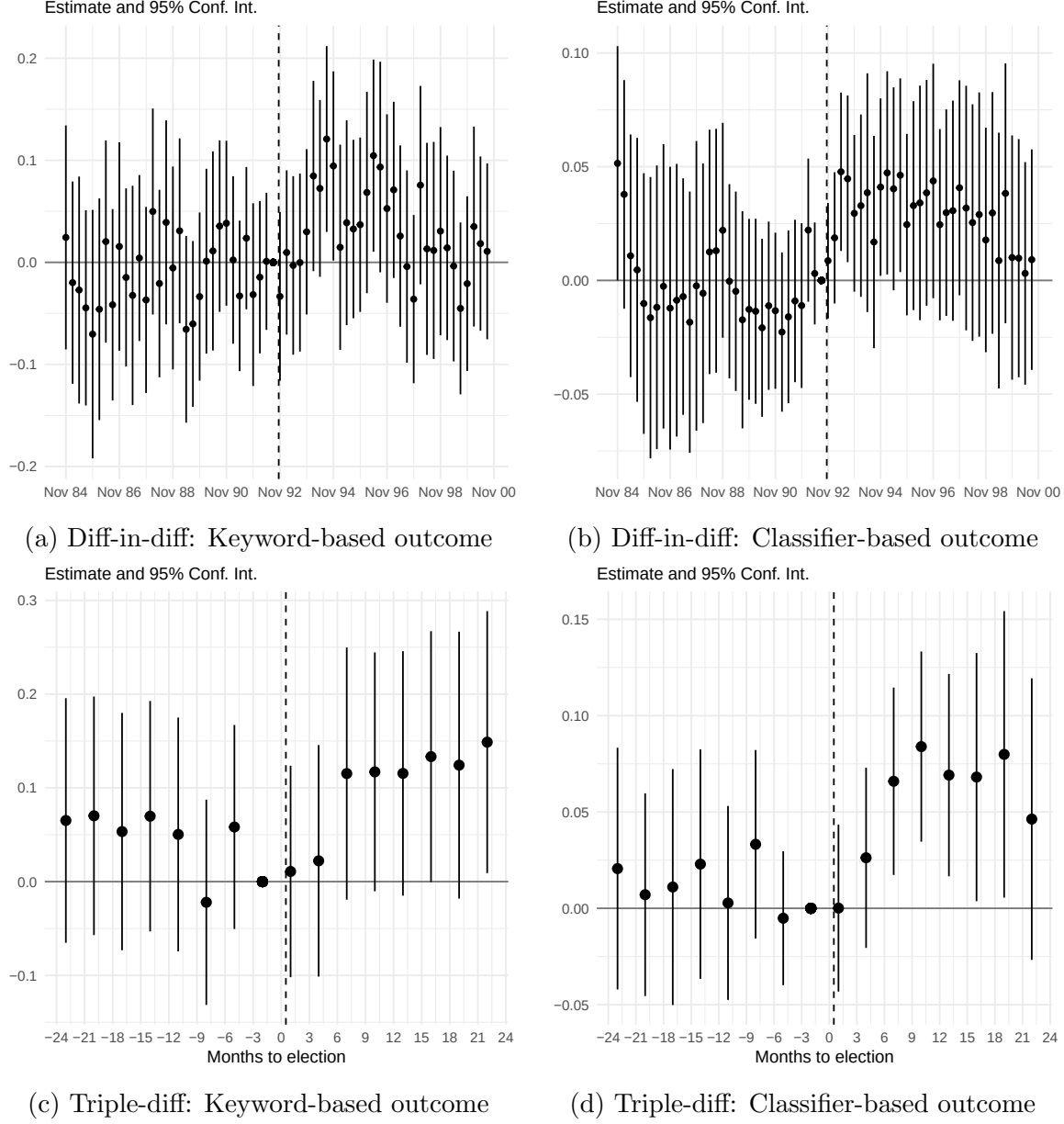


Figure 3: Event-study estimates

Notes: This figure shows that perceived political threat increased the frequency of anti-Black propaganda in newspapers after the 1892 election. It depicts the estimates for β_k and confidence intervals at the 95% level from equation (1) in Panels (a) and (b), and from equation (2) in Panels (c) and (d). The outcome in Panels (a) and (c) is the logarithm of one plus the number of newspaper pages containing anti-Black propaganda in a given month. In Panels (b) and (d), it is the logarithm of one plus the sum of classifier-based probability scores for anti-Black propaganda in a given month (see Section 3.2 for details). The main independent variable is the interaction of (i) Political threat $_{c(i)}$, a county-level dummy indicating that the Democratic Party received less than the Southern sample median vote share in the 1892 presidential election (60%; see Section 3.1 for details), with (ii) indicators for three-month bins k . For the difference-in-differences event-studies in Panels (a) and (b), these indicators cover the period from November 1884 to October 1900. The triple-difference event-studies in Panels (c) and (d) interact these with another indicator, (iii) $\mathbf{1}(1892 \text{ cycle}_t)$, which enables a comparison of the four years around the 1892 election cycle with the four years around the 1888 election cycle. The omitted bin is the 3-month period immediately before each election ($k = -1$). The samples and control variables are described in more detail in the text. Standard errors are clustered at the county level.

sponding increase is roughly 4–5% (0.043–0.052 log points).

A remaining concern is that our estimates could reflect omitted local factors that are correlated both with political threat and with changes in anti-Black propaganda, rather than a supply-side response by Democratic elites. Specifically, the same variables that reduced Democratic vote shares in 1892 might also have shifted readers’ demand for anti-Black media content. For example, [Eichengreen et al. \(2019\)](#) and [Klein et al. \(2020\)](#) show that the Populists were more successful in counties that suffered from the economic downturn in the 1880s and 1890s. Such economic distress might have increased demand for anti-Black stories among the readership, to which newspapers responded. Alternatively, pre-existing differences in anti-Black attitudes or newspaper content could have affected all parties’ vote shares in 1892 and generated differential dynamics in anti-Black sentiment after the election.

We present several pieces of evidence that run against this interpretation. First, the triple-differences event studies in panels (c) and (d) of [Figure 3](#) show that the post-1892 build-up in threatened counties is not mirrored around the 1888 election, suggesting that the dynamics are not driven by generic electoral-cycle forces or long-standing local trends. Second, in Columns 2–4 of [Table 1](#), we show that some of our estimated coefficients become larger and more precisely estimated when we augment the baseline model with rich interactions between period fixed effects and a set of county-level characteristics ($X_{c(i)}$) and newspaper-level characteristics (X_i) that partially capture local economic conditions and pre-existing racist sentiment (see the table notes for details). [Appendix Figure A10](#), which replicates the event-study plots in [Figure 3](#) using this extended control set, likewise shows clearer dynamics and flat pre-trends. Finally, in [Section 4.4](#) we exploit additional variation—including coverage of other minority groups and the behavior of non-Democratic newspapers—to provide further evidence that the observed patterns are more consistent with a politically motivated supply-side response than with a purely demand-driven shift.

Benchmarking these magnitudes against the existing literature is not straightforward, as most empirical work studies the effect of media content on behavior, whereas we focus on an upstream “first stage”: how political incentives shape media content. Still, two papers allow for useful comparisons. First, [Testa and Williams \(2025\)](#) estimate that anti-Black media content increases by roughly 30–90% in counties where the Democratic Party narrowly loses local elections between 1880 and 1900, primarily in Democratic-affiliated newspapers. Their setting focuses on the aftermath of razor-thin defeats—arguably moments of extreme political vulnerability. Our results, by contrast, capture the average response to a forward-looking threat (the rise of Populist–Republican fusion) across all counties with below-median Democratic support. It is therefore natural that our estimates are smaller than the spikes they find after tightly fought, high-stakes losses. Second, the closest parallel in spirit—though

Table 1: Estimates of the average effect of political threat on anti-Black propaganda

	Anti-Black Propaganda			
	(1)	(2)	(3)	(4)
<i>Panel A: Difference-in-differences</i>	Dep. var: Log Keywords (Sample mean = 1.13, sd = 1.04)			
Political threat $\times$ Post election	0.028 (0.023)	0.038 (0.024)	0.059** (0.025)	0.061** (0.025)
Observations	18,949	17,349	17,345	16,594
<i>Panel B: Difference-in-differences</i>	Dep. var: Log Classifier Score (Sample mean = 2.16, sd = 0.82)			
Political threat $\times$ Post election	0.034*** (0.013)	0.032*** (0.012)	0.025** (0.012)	0.027** (0.012)
Observations	6,306	5,754	5,750	5,518
<i>Panel C: Triple differences</i>	Dep. var: Log Keywords (Sample mean = 1.13, sd = 1.04)			
Political threat $\times$ Post election $\times$ 1892 election cycle	0.052* (0.031)	0.063** (0.031)	0.083** (0.033)	0.081** (0.032)
Observations	36,430	33,273	33,254	32,397
<i>Panel D: Triple differences</i>	Dep. var: Log Classifier Score (Sample mean = 2.14, sd = 0.83)			
Political threat $\times$ Post election $\times$ 1892 election cycle	0.043* (0.022)	0.039* (0.022)	0.042* (0.023)	0.043* (0.022)
Observations	12,325	11,268	11,249	11,006
Newspaper fixed effects	✓	✓	✓	✓
State-Period fixed effects	✓	✓	✓	✓
County demographic controls $\times$ Period fixed effects	✓	✓	✓	✓
County economic condition controls $\times$ Period fixed effects		✓	✓	✓
County racism controls $\times$ Period fixed effects			✓	✓
Newspaper racism control $\times$ Period fixed effects				✓

Notes: This table shows that political threat increased the frequency of anti-Black propaganda in newspapers after the 1892 election. An observation is a newspaper-month from November 1890 to October 1894 in Panels A and B, and from November 1886 to October 1894 in Panels C and D. In Panels A and C, the outcome is the logarithm of one plus the number of newspaper pages containing anti-Black propaganda in a given month. In Panels B and D, it is the logarithm of one plus the sum of classifier-based probability scores for anti-Black propaganda in a given month (See Section 3.2 for details). The main independent variable is the interaction of (i) Political threat_{ct}, a county-level dummy indicating that the Democratic Party received less than the Southern sample median vote share in the 1892 presidential election (60%; see Section 3.1 for details) with (ii) 1(Post election)_t, a dummy that equals one in the months after the November 1892 election. The sample for the static difference-in-differences estimations is the newspaper-months from November 1890 to October 1894. The static triple-difference estimations in Panels C and D extend the sample back to November 1886, and include another indicator, (iii) 1(1892 cycle)_t, which is one for the 1890 to 1894 window and zero otherwise. All columns control for the logarithm of the number of pages in a newspaper-month, as well as newspaper- and state-period-fixed effects, and interactions of period fixed effects with time-invariant pre-determined county demographic controls (log population, log population density, and the urban population share based on the 1890 Census). In Panels A and C, we include the logarithms of one plus the number of mentions of crime-related keywords and of 'negro' in all columns. Column 2 adds period fixed effects interacted with the following controls for county-level economic conditions: Black population share, average occupation score, literate population share, the percentage change in the value of counties' agricultural portfolio from 1888 to 1892, the average indebtedness (i.e., the ratio of mortgage on farms or homes to their values), the average interest rate on mortgages, log per capita output in manufacturing and agriculture, log railway miles per square mile, log average farm size, farm-size Gini inequality index, the shares of cotton and tobacco acreage to total farm acreage. Column 3 further includes period fixed effects interacted with two county-level proxies for racism: counties' Democratic vote share in the 1888 presidential election, and the number of lynchings before 1892. Column 4 additionally includes period fixed effects interacted with a newspaper-level proxy for racism: the average level of anti-Black propaganda before the election. See Appendix A.1 for more details on these control variables and their construction. The standard errors are clustered on counties and reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

not in setting—is [Tella and Franceschelli \(2011\)](#), who study how Argentine newspapers adjust corruption coverage in response to government advertising. They find that a one-standard-deviation increase in government advertising reduces front-page reporting on corruption by about 0.18 standard deviations. In our case, moving from an unthreatened to a threatened county (a change corresponding to a sizable shift in Democratic vote share) leads to an average monthly increase in anti-Black propaganda of up to 5% of its standard deviation, and it peaks at around 16% before the 1894 midterm according to the keyword-based measure. Therefore, our effect sizes are of similar magnitude to those found in other settings.

It is important to emphasize that newspaper propaganda was only one of several tools Democratic elites used to respond to political competition, alongside more coercive tactics such as voter intimidation, ballot fraud, and political violence against Black citizens and Populist candidates ([Testa and Williams, 2025](#); [Logan, 2020](#)). In [Appendix A.5.4](#), we explore lynching data as an additional outcome; these estimates are imprecise and thus not very informative.

4.3 Robustness

In [Appendix Section A.4](#), we provide a thorough robustness analysis of our findings and offer a summary here. First, the results hold when using different criteria to define political threat by varying the threshold Democratic vote share below which a county is considered vulnerable, as detailed in [Appendix A.4.1](#). We also find some evidence suggesting that the effect is driven by counties in which Populist–Republican fusion could realistically overturn Democratic dominance, rather than in Democrat or opposition strongholds ([Appendix Figure A11](#)). Second, we show robustness to alternative formulations of our dependent variables that are not log-transformed ([Appendix Figure A18](#)) and the semantic-similarity-based measure of anti-Black propaganda ([Appendix A.4.6](#)). Third, [Appendix A.4.3](#) shows that our results are unlikely to be driven by selective entry or exit of newspapers, which we show by restricting attention to the balanced sample of newspapers ([Appendix Figure A19](#)) and examining the correlations between exit and entry rates and both our treatment and propaganda outcomes ([Appendix Tables A5 and A6](#)). Finally, our main coefficient interest and the standard errors are remarkably robust to allowing for different clustering of standard errors ([Appendix A.4.4](#)) or dropping each state from the sample one at a time ([Appendix A.4.5](#)).

4.4 Additional Evidence Consistent with Supply of Propaganda

We have established that the 1892 election uniquely corresponds with a surge in anti-Black media content, particularly in the run-up to the next midterm and presidential elections, a

trend not explained by observable factors at the county and newspaper levels or by unobservable state-period variations. This section builds on those results, offering additional evidence supporting the hypothesis that this spike in anti-Black media content reflects the politically incentivized *supply of propaganda*, or in other words, that local Democratic Party elites manufactured this outrage. First, we employ a placebo exercise to address concerns that local confounders drove both the Populists’ success in 1892 and increased demand for anti-Black propaganda. We find no evidence of a similar effect in non-Southern counties, which speaks against such confounders at the county level driving our results. Instead, our results suggest that the spike in anti-Black propaganda in threatened relative to non-threatened counties was specific to the post-1892 election period in the US South. Second, results from heterogeneity analyses across Southern counties are inconsistent with a demand-driven explanation. We find no evidence that the effect on anti-Black propaganda is stronger in counties that were initially more racist. Third, we find no comparable effects on newspaper content targeting other minorities—Chinese, Japanese and Mexicans—underscoring that the main results are not driven by a general rise in xenophobic or anti-minority demand. Finally, we examine the role of newspaper political affiliations. Our findings indicate that newspapers aligned with the Democratic Party in our data were primarily responsible for the effect on propaganda, even when compared to other newspapers within the same counties. In contrast, we do not find a significant effect on anti-Black propaganda among politically independent newspapers, which would have been expected if the results were driven by local demand changes.

Placebo Analysis in Non-Southern Counties A potential concern in our analysis is that the observed post-1892 election effect on anti-Black propaganda could be attributed to specific, time-varying confounders in counties that also supported the Populists and Republicans. To address this, we conduct a placebo exercise informed by the historical context.

We replicate our analysis using data from newspapers outside the South. Considering the Populist Party’s significant success in the Midwest and West, where the Black population was minimal during this period, there would have been little incentive for local elites to propagate anti-Black content to fend off a Populist-Republican fusion. Furthermore, outside the South, the natural coalition partner for the Populists was the Democratic Party, which ultimately led to the nationwide Democrat-Populist fusion in the 1896 presidential election. Thus, our treatment measure should not reflect a local political threat to Democratic elites outside the South, many of whom were not in power anyway. If our treatment measure were correlated with unobservable factors driving an increase in anti-Black propaganda after the 1892 election, we would expect to observe similar effects outside the Southern context. Our results, presented in Appendix Figure A3, show no significant increase in keyword-based

anti-Black propaganda in non-Southern counties in response to our political threat measure (an indicator equal to one if the Democratic vote share is below its sample median), as evidenced in both the triple-difference and the difference-in-differences analyses around the 1892 election.

These findings suggest that our results are not driven by the Populists’ or Republicans’ success *per se*. Instead, what seems to have mattered was that the Populists’ success coincided with a racially divided electorate and the threat of a Populist-Republican fusion.

Heterogeneity within the South Speaks Against Demand The evidence we have gathered so far leads to an important implication: for unobservable, time-varying factors to invalidate our findings, they would have to be unique to the aftermath of the 1892 presidential election and confined exclusively to the South. To further assess the likelihood of such confounders, we explore various aspects of heterogeneity across Southern counties, examining whether these aspects align with a demand-driven explanation for the observed surge in anti-Black propaganda. Our investigations focus on economic inequality and latent racism. Consistently, we find no evidence supporting a demand-based interpretation.

Appendix Figure A4 plots the estimates of the heterogeneity analysis of the specifications (3) and (4) using various proxies for local economic inequality and latent racism across counties. For each proxy, we create an indicator variable identifying counties above the sample median and fully interact this indicator with our treatment variables and the period fixed effects. The figure plots the estimated coefficients on the interaction between this indicator and our main treatment measure.

The first three rows of the top and bottom panels reveal no systematic evidence that the effects were stronger in economically more unequal counties. To proxy local economic inequality, we consider the average farm size, a farm-size inequality index constructed following Vollrath (2013), and the share of farms employing sharecropping in each county. All data sources for these variables are detailed in Appendix A.1. We observe no sizable or statistically significant heterogeneity consistently associated with any of these variables across specifications (difference-in-differences and triple differences) and outcomes (keyword-based and classifier-based).

The remaining three interaction variables serve as proxies for local latent racist sentiment. Once again, we fail to identify any consistent, meaningful, sizable, or statistically significant heterogeneity across all proxies and panels. To measure local latent racist sentiment, we use three indicators: the Democratic vote shares in the 1888 presidential election, the intensity of anti-Black propaganda in newspapers before the 1892 election, and Black population share. We examine whether counties above the median of these variables experienced a

greater increase in anti-Black propaganda after 1892 compared to below-median counties. Our analysis reveals no such pattern.

No Effects on Newspaper Content Targeting Other Minorities Another possible concern is that the surge in propaganda in response to political threat following the 1892 election might reflect a generalized rise in hostile rhetoric toward minority groups, rather than a targeted political strategy aimed at splitting the Black-white coalition that fusion politics relied upon. To address this possibility, we examine joint mentions of other ethnic groups with crime-related terms.

In particular, we replicate our difference-in-differences analysis for the combinations “Asians”, “Chinese”, “Japanese”, and “Mexicans” with the crime-related keywords, shown in Panels (a)–(d) of Appendix Figure A12. Importantly, in all specifications we separately control for the number of mentions of the ethnic group and for crime-related terms, so that the results do not simply reflect variation in overall reporting intensity.

Nationwide, Asians and, in particular, Chinese were common targets of (newspaper) xenophobia in this period. Consistent with historical accounts that anti-Asian sentiment at the time was concentrated in the West, especially in the aftermath of the 1882 Chinese Exclusion Act, we find no evidence of an increase in anti-Chinese or anti-Japanese propaganda after the election. Panel (a) of Figure A12 shows the null effect for “Asians”, and Panels (b) and (c) show the corresponding null effect for “Chinese” and “Japanese”, respectively. In Panel (d), we further extend the analysis to “Mexicans,” who were a relevant group in the Southern context of the 1890s. We find no evidence of increased propaganda linking Mexicans to crime. The null effects are precise enough to rule out sizable increases comparable to those observed for Black citizens. We show that the same takeaway holds in triple-differences in Appendix Figure A13.

Taken together, these placebo analyses suggest that the surge in propaganda was not a broad-based phenomenon directed against racial or ethnic minorities in general. Instead, the evidence indicates that the increase was specific to framing Black people with crime. This specificity is consistent with the political context of the time, in which Democratic elites sought to mobilize racial divisions to counter the threat of a Populist-Republican fusion. It thereby strengthens our interpretation that the post-election propaganda reflected the politically motivated supply of racially charged media content rather than a generalized increase in public demand for anti-minority content.

Democratic Newspapers Drive the Entire Effect We present additional evidence indicating that the surge in anti-Black media content post-1892 was not merely informational

but rather *propaganda*, predominantly propagated by partisan Democratic newspapers rather than Republican, Populist or independent ones, even within the same counties. This assertion is grounded in the historical context and empirical evidence suggesting that late 19th-century newspapers, especially in the South, were often highly partisan or directly influenced by political parties (see Section 2.4).

Leveraging data from newspaper directories digitized by Gentzkow et al. (2011, 2015), which includes information on newspapers’ political affiliations or endorsements during presidential elections, we differentiate between Democrat-aligned newspapers and those endorsing Republicans, Populists, or being politically independent.²⁰ In our samples, about 89% of newspapers are Democrat-affiliated, around 1% are independent, and the remainder either endorse other parties (such as the Republicans) or have no explicit political affiliation.

In Table 2, we interact our static difference-in-differences and triple-difference equations with newspaper affiliation dummies. Column 1 replicates the baseline specifications (3) and (4) in the subsample of newspapers for which we observe political affiliation. Column 2 adds a fully interacted ‘Run-up to next election’ indicator, equal to one in the twelve months before the next midterm or presidential election (i.e., splitting the post-election period into two equal-sized halves), to focus on the months with the strongest effects and increase precision. Column 3 further includes a fully interacted indicator equal to one for Democrat-affiliated newspapers and zero otherwise, while Column 4 replaces this with a set of fully interacted indicators for Democratic-affiliated, independent, and other newspapers.²¹ While some of the estimates are noisy, particularly in the smaller difference-in-differences sample, the results consistently point to Democratic newspapers as driving the entire spike in anti-Black propaganda after the 1892 election.

These findings challenge the predominant view in the literature, as exemplified by seminal work by Gentzkow and Shapiro (2010), that readers’ demand drives newspaper content. Importantly, while this holds true for the period from 1972 to 1998 in the US, Gentzkow et al. (2015) found an exception in the South from 1860 to 1900, where state-level politics significantly influenced newspaper circulation and affiliation. This political control of newspapers might render a supply-side interpretation of our main result plausible.

Finally, we observe no significant effect among politically independent newspapers. This absence of an effect could be partly attributed to their limited representation in our sample. However, a more telling observation emerges when considering the economic motivations of these independent newspapers. Such newspapers are typically more aligned with local

²⁰See Appendix A.1 for details on how we link the data sets.

²¹This analysis is restricted to the keyword-based measure because there is no treatment variation in Democrat-affiliated versus other newspapers in the smaller sample in which we can construct the classifier-based measure.

Table 2: Heterogeneity: Democrat-affiliated versus other newspapers

	Anti-Black propaganda				
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Difference-in-differences</i>					
	Dep. var: Log Keywords (Sample mean = 1.17, sd = 1.05)				
Political threat $\times$ Post election	0.035 (0.024)	0.003 (0.029)	0.017 (0.064)	0.028 (0.078)	
Political threat $\times$ Run-up to next election		0.070** (0.028)	0.012 (0.066)	0.031 (0.077)	
Political threat $\times$ Run-up to next election $\times$ Democrat			0.063 (0.066)	0.043 (0.078)	0.161*** (0.046)
Political threat $\times$ Run-up to next election $\times$ Independent				-0.060 (0.111)	
Observations	16,794	16,794	16,794	16,794	11,371
<i>Panel B: Triple differences</i>					
	Dep. var: Log Keywords (Sample mean = 1.17, sd = 1.05)				
Political threat $\times$ Post election $\times$ 1892 election cycle	0.059* (0.032)	0.028 (0.035)	-0.002 (0.098)	-0.025 (0.117)	
Political threat $\times$ 1892 election cycle $\times$ Run-up to next election		0.067 (0.041)	-0.142 (0.094)	-0.141 (0.110)	
Political threat $\times$ 1892 election cycle $\times$ Run-up to next election $\times$ Democrat			0.219** (0.094)	0.218* (0.112)	0.259** (0.112)
Political threat $\times$ 1892 election cycle $\times$ Run-up to next election $\times$ Independent				-0.006 (0.162)	
Observations	32,181	32,181	32,181	32,181	21,517
Newspaper fixed effects	✓	✓	✓	✓	✓
State-Period fixed effects	✓	✓	✓	✓	
County demographic controls $\times$ Period fixed effects	✓	✓	✓	✓	
Affiliation-Period fixed effects			✓	✓	✓
County-Period fixed effects					✓

Notes: The table demonstrates that the effect of Political threat on anti-Black propaganda, specifically during the run-up to the next congressional or presidential elections, is stronger in Democratic newspapers compared to independent and other newspapers. Column 1 replicates the diff-in-diff and triple-diff specifications of Column 1 in Table 1 in the sample of newspapers with information on political affiliation. Column 2 includes a fully interacted dummy equal to one for the second half of the 24-month post-election period, and zero otherwise. Column 3 adds a fully interacted indicator equal to one for newspapers affiliated with the Democrats, and zero otherwise. Column 4 replaces this indicator with a fully interacted indicator for Democrat-affiliated, independent and other newspapers. Column 5 includes the dummy from Column 3 and adds county-period fixed effects. See Appendix A.1 and the note to Table 1 for details on the control variables. The standard errors are clustered on counties and reported in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

demand due to their economic incentives (Petrova, 2011). If shifts in readers' demand primarily drove the increase in anti-Black content, we would have expected a corresponding rise in such content in independent newspapers. The lack of clear evidence for an increase in anti-Black propaganda among these publications reinforces a supply-side interpretation of our findings.²²

Finally, we address the potential issue of time-varying confounders within counties, which,

²²A limitation of this specification is that it cannot fully rule out the possibility that racially conservative readers demanded more anti-Black content precisely when and where Democrats were threatened, and that Democratic newspapers, but not independent newspapers, responded to this demand. However, this possibility is less consistent with our findings in Appendix Figure A4, which shows that the effect is not stronger in counties and newspapers that were racially less conservative.

if present, would need to be specific to the period following the 1892 election and limited to Southern counties to impact our findings. To mitigate this concern, we introduce county-period fixed effects in column 5 of Table 2. This adjustment allows us to compare Democrat-affiliated newspapers with other newspapers within the same county, controlling for any month-specific shocks that might affect each county. This approach represents a highly stringent (triple- and quadruple-differences, respectively) specification. Our analysis now hinges on variation within counties that have at least one Democrat-affiliated newspaper and at least one non-affiliated newspaper. Despite the rigor of this specification, the observed effects are substantial, statistically significant and broadly align with our previously estimated results. If anything, these estimates suggest larger effect magnitudes.²³

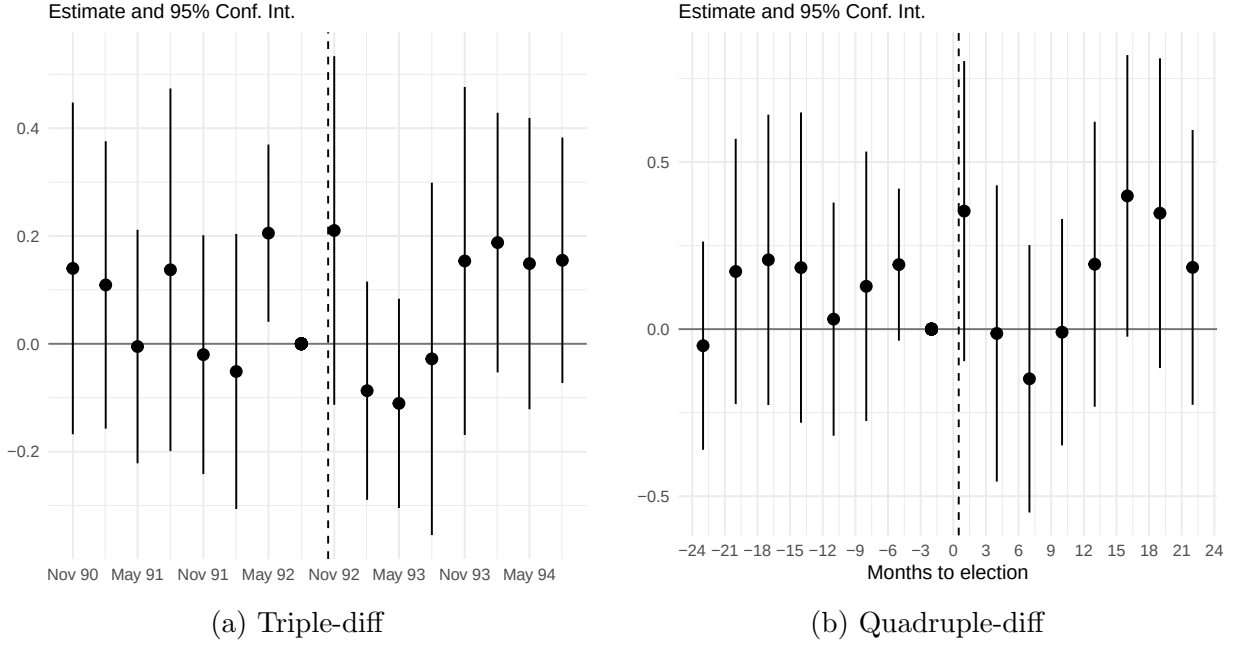
Figure 4 plots event-study estimates for these stringent specifications including county-period fixed effects. Again, we observe no evidence of any pre-trend, and a gradual increase in anti-Black propaganda that peaks in the run-up to the next midterm election, exclusive to Democratic-Party-affiliated newspapers after the 1892 election in counties where fusion threatened the Democrats.

This analysis suggests that Democrat-affiliated newspapers were the primary drivers of the observed increase in anti-Black propaganda. This holds true even in this more narrowly defined comparison, further supporting our interpretation that the spike in propaganda was not due to broader, time-varying factors within counties but rather a targeted effort by these newspapers.

Taking Stock Our analyses collectively indicate that the rise of the Populist Party and the possibility of a fusion with the Republican Party posed a significant threat to the dominance of Southern Democrats, who reacted by ramping up the dissemination of anti-Black propaganda in their affiliated newspapers. While we cannot fully rule out a role for changing reader demand, the evidence strongly suggests that this trend was not driven by unobserved factors simultaneously influencing political preferences and the demand for anti-Black or xenophobic content. Rather, it appears to be a deliberate strategy by the Southern Democrats to supply such content and manufacture racial outrage for political gain. In other words, Southern Democrats used a “sensational press that played up and headlined current stories of Negro crime, charges of rape and attempted rape” (Woodward, 1955) to discredit the Populists in the eyes of poor white voters, thereby sabotaging prospects for a successful fusion. This

²³The within-county comparison also helps disentangle how Democratic elites reacted to the political threat. Instead of directing affiliated newspapers to emphasize distant crimes, they could have instigated local police and judges to accuse, incriminate, and sentence Black men at higher rates, with newspapers subsequently reporting these instances. However, if this were the case, we would also expect other newspapers in the same county to report on the increased accusations and the corresponding legal proceedings.

Figure 4: Event-study estimates with county-period fixed effects



Notes: This figure shows that Democrat-affiliated newspapers drive the effect of political threat on keyword-based anti-Black propaganda in the run-up to the next congressional and presidential elections, even when compared to other newspapers in the *same* county. The estimates are from event-study triple-difference and quadruple-difference specifications reported in column 5 of Table 2. Both panels show coefficients of three-month bins, and the corresponding confidence intervals at the 95% level. Standard errors are clustered at the county level.

tactic was used to ‘divide and rule,’ ensuring the maintenance of their political power in the region.

5 Propaganda and Voting

This section explores whether the divide-and-rule tactics employed by Southern Democrats influenced electoral outcomes, and the role that politically affiliated newspapers played in this process. We present evidence suggesting that propaganda efforts did indeed bolster the Democratic vote share—particularly in the critical period after the 1892 presidential election and in counties where Democratic elites were threatened by a potential Populist-Republican fusion, coinciding with the spike in anti-Black propaganda documented in the previous sections.

To assess this, we calculate changes in Democratic vote share between consecutive election years across all Southern counties. We then regress these changes on the political threat dummy, the share of Democratic newspapers recorded in each county in 1892, and their

Table 3: Voting

	Dem. vote share							
	Δ 1890-1888		Δ 1892-1890		Δ 1894-1892		Δ 1896-1892	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share Dem newspapers	-1.938 (1.253)	-0.788 (1.708)	-1.843 (1.643)	-0.079 (2.021)	0.655 (1.413)	-2.361 (2.210)	2.165* (1.162)	0.033 (1.780)
Political threat		2.307 (1.819)		-8.404*** (2.315)		6.392*** (1.811)		5.279*** (1.623)
Share Dem newspapers $\times$ Political threat		-2.660 (2.495)		-3.527 (3.137)		5.708** (2.870)		3.837 (2.361)
Observations	637	630	610	604	626	619	579	572
R ²	0.096	0.101	0.182	0.224	0.148	0.201	0.064	0.120
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table presents OLS estimates examining how the interaction of political threat and local media control by the Democratic Party shaped Democratic vote shares in congressional elections. The key independent variables are the political threat indicator (equal to one if the Democratic presidential vote share in 1892 was below the sample median), the share of newspapers affiliated with the Democrats in each county in 1892 (Gentzkow et al., 2011, 2015), and interactions between these variables. The dependent variable is the difference in Democratic vote share between given congressional elections. All regressions include state fixed effects, and standard errors are heteroskedasticity robust and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

interaction. To address concerns about state-specific factors influencing the Democratic vote—such as differences in campaign strategies or policy debates confined to particular states—we include state fixed effects in all specifications.

The results, reported in Table 3, reveal several notable patterns. First, the overall share of Democratic newspapers is not associated with greater change in Democratic vote shares in any period. Second, in threatened counties, Democrats experienced a decline of about eight percentage points in their vote share between 1890 and 1892, which is aligned with—and partly mechanical to—our definition of the threat measure. Most importantly, third, Democrats managed to claw back vote share in threatened counties in the years following 1892, and this recovery was strongly amplified by the share of Democratic-affiliated newspapers in those counties (Columns 6 and 8). By contrast, columns 1–4 suggest that outside of this interaction, changes in Democratic vote share were not systematically related to newspaper affiliation.

Taken together, these findings suggest that propaganda was most closely associated with electoral gains in counties where Democrats both were politically vulnerable and enjoyed dominance over the local press.

6 Conclusion

Our study provides empirical evidence suggesting that political parties can successfully manufacture racial outrage in the media. Leveraging a uniquely suited historical setting and

employing both a difference-in-differences and triple-difference estimation strategy, we observe a substantial increase in anti-Black stories in Democratic Party-affiliated newspapers in response to the local political threat posed by a possible Populist-Republican fusion.

It is important to emphasize that these findings do not imply that supply was or remains the only determinant of anti-Black, or generally, divisive media content. An extensive literature on the determinants of media slant demonstrates the importance of demand for such content, and we cannot definitively rule out such demand effects in our context. Nevertheless, a battery of auxiliary results makes it highly likely that the stories were largely propaganda supplied by partisan newspapers. More generally, our findings support the view that political actors will tend to use outrage-oriented propaganda when they have strong political incentives to divide society and exert some influence over media outlets.

In addition to the particularity of the setting, a natural limitation of our study is that we can only measure one means by which politicians exacerbate racial tensions. Historical accounts of our setting highlight various other means, such as voter intimidation and violence targeting politicians from the opposition. We thus view the evidence presented in this paper as highlighting *one* part of a broader political strategy to divide and rule the South.

Future research could extend this work in several directions. One promising avenue is to explore similar supply-side dynamics in other historical or contemporary contexts, examining how political incentives shape outrage in media in different socio-political environments. Another area of interest could involve examining the long-term effects of such propaganda on societal attitudes and intergroup relations.

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A Appendix (for Online Publication)

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A.1 Data Sources and Construction

This section provides sources and details for all variables used in the analysis. Appendix Tables [A1](#), [A2](#) and [A3](#) provide summary statistics.

Main Dependent and Independent Variables

Anti-Black Propaganda – keyword-based measure. Our first measure of anti-Black propaganda is based on counting the number of pages on which the words “rape”, “murder”, or “crime” co-occur with the “negro” on the same page. We aggregate these frequencies to the newspaper-month level. The dependent variable we employ in our baseline analysis is the logarithm of one plus the number of pages in a newspaper-month that contained propaganda.

Anti-Black Propaganda – classifier-based measure. See Appendix [A.2.3](#) for details.

Anti-Black Propaganda – semantic similarity-based measure. See Appendix [A.4.6](#) for details.

Political Threat. We use county-level electoral returns from the 1892 presidential elections sourced from [Clubb et al. \(2006\)](#) to approximate the political threat experienced by Democrats. The baseline measure is a dummy indicating that the Democrats were not guaranteed political control of the county. This dummy is one if the Democrats scored less than their median vote share of 60%. We explore heterogeneity to this margin in Appendix Table [A.4.1](#).

Control Variables – Newspaper Level

Log Number of Pages For *newspapers.com* we approximate the total number of pages per newspaper-month by counting “and”, while for *newspaperarchive.com* we rely on the actual number of pages provided. We compute log one plus these numbers to construct the variable.

Newspapers’ Political Affiliation. We link our newspaper dataset to information on newspapers’ political affiliations in presidential elections, as collected by [Gentzkow et al. \(2011\)](#) and [Gentzkow et al. \(2015\)](#). This allows us to distinguish between (i) newspapers that supported the Democratic Party and those that did not, including those that endorsed other parties or were independent; and (ii) among the latter, between newspapers that were independent and those that were not. More detail is provided in Appendix [A.2.2](#).

Log Average Propaganda before 1892. We residualize all newspaper-month observations of anti-Black propaganda (from November 1890 to October 1892 in the static difference-in-differences sample and from November 1886 to October 1892 in the triple-difference sample) by period fixed effects and then calculate the average across newspapers.

Log “Negro” We count the occurrence of the keyword “negro” in all newspaper-month observations, and use the log of one plus this number to control for reporting about Black people when the keyword-based measure of anti-Black propaganda is the outcome. See also Appendix Figure A8 where we use this variable as outcome.

Log Crime Terms We count the occurrence of the keywords “crime”, “rape”, or “murder” in all newspaper-month observations, and use the log of one plus this number to control for reporting about crime when the keyword-based measure of anti-Black propaganda is the outcome. See also Appendix Figure A8 where we use this variable as outcome.

Control Variables – County Level – Economic Variables

Change in Value of Agricultural Portfolio (1888-1892). We compute percentage changes in counties’ agricultural portfolio between 1888 and 1892 following the method in Eichengreen et al. (2019).

Log Railway Miles Per Square Mile (1890). Data on railway miles per square mile come from Donaldson and Hornbeck (2016).

Farm size Gini Index (1890). Data on the number of farms by size category comes from the Agricultural Census 1890. We follow Vollrath (2013) (Appendix A) in assuming that each farm in a size category (which ranges from 0-9 to >1000 acres) has the midpoint acreage of the category (e.g., 4.5 acres for the 0-9 category, 14.5 acres for the 10-19 category, and so on). For farms of size 1000 acres or larger, we assume an acreage of 1000. We then calculate a Gini index from this data and the number of farms in each size category, as reported in the 1890 Agricultural Census.

Control Variables – County Level – Racism Proxies

Democratic Party Votes Share (1888). We take the Democratic Party’s county-level vote share in the 1888 presidential election from Clubb et al. (2006).

Number of Lynchings before 1892. Data on lynchings comes from the Historical American Lynching (HAL) database, made available online under <http://people.uncw.edu/hinese>. For each county, we calculate the total number of lynchings taking place in the years before 1892. In the analysis of Appendix section A.5.4, we employ a monthly panel based on this data.

Census Variables (1890)

The following variables are taken directly from the Population and Agricultural Censuses of 1890 provided by Haines (2010): average (farm) indebtedness, average (farm) mortgage

interest rate, log per capita output in manufacturing and agriculture, average farm size, the share of sharecropping and tenant farmers, log total population, Black population share, urban population share, average occupation score, and literate population share, and shares of cotton and tobacco acreage to total farm acreage.

Table A1: Summary statistics of the variables used in event-study difference-in-differences analysis

	N	Mean	SD	Min	Max
Outcome variables:					
Anti-Black propaganda (log, keywords)	75456	1.15	1.05	0.00	5.39
Anti-Black propaganda (log, classifier)	25795	2.21	0.84	0.16	5.53
Anti-Black propaganda (semantic similarity)	25795	0.10	0.04	-0.04	0.25
Treatment variables:					
Political threat	75456	0.50	0.50	0.00	1.00
Post election indicator	75456	0.66	0.47	0.00	1.00
Newspaper-level control variables:					
Log page count	75456	3.26	0.99	0.00	6.99
Log “negro”	75456	1.83	1.09	0.00	5.78
Log “rape” or “murder” or “crime”	75456	1.96	1.04	0.00	5.86
Log avg. anti-Black propaganda before 1892 (keywords)	59912	1.13	0.95	0.00	4.68
Log avg. anti-Black propaganda before 1892 (classifier)	20414	2.17	0.83	0.29	5.23
Log avg. anti-Black propaganda before 1892 (semantic similarity)	20414	0.10	0.03	0.01	0.17
County-level control variables:					
Log population	75456	10.01	0.64	6.57	12.40
Log population density	75456	-11.21	0.81	-16.08	-7.66
Share black population	75456	0.05	0.19	0.00	1.00
Share urban population	75456	0.42	0.22	0.00	0.93
Avg. income (occupational score)	74170	6.39	1.52	2.59	11.60
Share literate population	74170	0.55	0.14	0.21	0.98
Change in value of agricultural portfolio from 1888 to 1892	75209	0.01	0.06	-0.16	0.30
Avg. interest rate on mortgages	75118	0.04	0.01	0.01	0.08
Avg. indebtedness	75118	0.45	0.12	0.14	0.97
Log railway miles per square mile	75456	3.78	1.15	0.00	5.77
Log per capita output in agriculture	75448	3.55	0.63	0.83	5.02
Log per capita output in manufacturing	74262	2.91	1.29	0.00	5.43
Log avg. farms size	75448	4.87	0.42	3.37	7.94
Farm size gini inequality index	74452	0.52	0.09	0.25	0.83
Share of tobacco acreage to total farm acreage	71979	0.00	0.01	0.00	0.04
Share of cotton acreage to total farm acreage	75209	0.11	0.09	0.00	0.37
Dem. vote share Presid. elections 1888	75222	64.71	17.87	7.80	100.00
No. of lynchings before 1892	75456	1.10	1.94	0.00	11.00

Notes: This table provides summary statistics for all variables employed in the event-study difference-in-differences analysis from November 1884 to October 1900. Appendix A.1 provides data sources and information on variable construction.

Table A2: Summary statistics of the variables used in static difference-in-differences analysis

	N	Mean	SD	Min	Max
Outcome variables:					
Anti-Black propaganda (log, keywords)	19069	1.13	1.04	0.00	5.08
Anti-Black propaganda (log, classifier)	6348	2.16	0.82	0.22	5.49
Anti-Black propaganda (semantic similarity)	6348	0.10	0.04	-0.02	0.25
Treatment variables:					
Political threat	19069	0.51	0.50	0.00	1.00
Post election indicator	19069	0.50	0.50	0.00	1.00
Democrat-affiliation indicator	16863	0.93	0.25	0.00	1.00
Independent-newspaper indicator	16863	0.02	0.13	0.00	1.00
Newspaper-level control variables:					
Log page count	19069	3.23	0.99	0.00	6.78
Log “negro”	19069	1.78	1.08	0.00	5.52
Log “rape” or “murder” or “crime”	19069	1.95	1.04	0.00	5.68
Log avg. anti-Black propaganda before 1892 (keywords)	18186	1.12	0.92	0.00	4.68
Log avg. anti-Black propaganda before 1892 (classifier)	6084	2.16	0.79	0.29	5.23
Log avg. anti-Black propaganda before 1892 (semantic similarity)	6084	0.10	0.03	0.01	0.17
County-level control variables:					
Log population	19069	10.01	0.66	7.61	12.40
Log population density	19069	-11.19	0.83	-13.99	-7.66
Share black population	19069	0.05	0.19	0.00	1.00
Share urban population	19069	0.42	0.22	0.00	0.93
Avg. income (occupational score)	18695	6.39	1.53	3.52	11.60
Share literate population	18695	0.55	0.14	0.21	0.98
Change in value of agricultural portfolio from 1888 to 1892	18995	0.01	0.06	-0.11	0.30
Avg. interest rate on mortgages	18999	0.04	0.01	0.01	0.08
Avg. indebtedness	18999	0.45	0.12	0.14	0.97
Log railway miles per square mile	19069	3.76	1.16	0.00	5.77
Log per capita output in agriculture	19069	3.53	0.63	0.83	4.81
Log per capita output in manufacturing	18824	2.93	1.30	0.00	5.35
Log avg. farms size	19069	4.88	0.43	3.37	7.94
Farm size gini inequality index	18853	0.52	0.09	0.25	0.83
Share of tobacco acreage to total farm acreage	18238	0.00	0.01	0.00	0.04
Share of cotton acreage to total farm acreage	18995	0.11	0.09	0.00	0.37
Dem. vote share Presid. elections 1888	18994	64.93	17.74	7.80	100.00
No. of lynchings before 1892	19069	1.09	1.95	0.00	11.00

Notes: This table provides summary statistics for all variables employed in the difference-in-differences analysis from November 1890 to October 1894. Appendix A.1 provides data sources and information on variable construction.

Table A3: Summary statistics of the variables used in the triple-difference analysis

	N	Mean	SD	Min	Max
Outcome variables:					
Anti-Black propaganda (log, keywords)	36596	1.13	1.04	0.00	5.08
Anti-Black propaganda (log, classifier)	12383	2.14	0.83	0.19	5.51
Anti-Black propaganda (semantic similarity)	12383	0.10	0.04	-0.04	0.25
Treatment variables:					
Political threat	36596	0.50	0.50	0.00	1.00
Post election indicator	36596	0.51	0.50	0.00	1.00
Democrat-affiliation indicator	32274	0.93	0.26	0.00	1.00
Independent-newspaper indicator	32274	0.02	0.12	0.00	1.00
Newspaper-level control variables:					
Log page count	36596	3.21	0.98	0.00	6.80
Log “negro”	36596	1.79	1.07	0.00	5.52
Log “rape” or “murder” or “crime”	36596	1.93	1.04	0.00	5.68
Log avg. anti-Black propaganda before 1892 (keywords)	32941	1.13	0.94	0.00	4.68
Log avg. anti-Black propaganda before 1892 (classifier)	11202	2.16	0.82	0.29	5.23
Log avg. anti-Black propaganda before 1892 (semantic similarity)	11202	0.10	0.03	0.01	0.17
County-level control variables:					
Log population	36596	10.02	0.64	6.94	12.40
Log population density	36596	-11.18	0.81	-14.63	-7.66
Share black population	36596	0.05	0.19	0.00	1.00
Share urban population	36596	0.42	0.22	0.00	0.93
Avg. income (occupational score)	35938	6.41	1.52	3.31	11.60
Share literate population	35938	0.56	0.14	0.21	0.98
Change in value of agricultural portfolio from 1888 to 1892	36474	0.01	0.06	-0.16	0.30
Avg. interest rate on mortgages	36522	0.04	0.01	0.01	0.08
Avg. indebtedness	36522	0.45	0.12	0.14	0.97
Log railway miles per square mile	36596	3.77	1.16	0.00	5.77
Log per capita output in agriculture	36596	3.54	0.63	0.83	4.81
Log per capita output in manufacturing	36140	2.94	1.29	0.00	5.35
Log avg. farms size	36596	4.87	0.43	3.37	7.94
Farm size gini inequality index	36167	0.52	0.09	0.25	0.83
Share of tobacco acreage to total farm acreage	34940	0.00	0.01	0.00	0.04
Share of cotton acreage to total farm acreage	36474	0.11	0.09	0.00	0.37
Dem. vote share Presid. elections 1888	36506	64.68	17.63	7.80	100.00
No. of lynchings before 1892	36596	1.10	1.95	0.00	11.00

Notes: This table provides summary statistics for all variables employed in the triple-difference analysis from November 1886 to October 1894. Appendix A.1 provides data sources and information on variable construction.

A.2 Additional Data Appendices

In the following, we describe our validation of the newspaper data (section A.2.1) and document how we identify the political affiliation of newspapers (section A.2.2).

A.2.1 Validating the Newspaper Data

To validate the measure of anti-Black propaganda, we asked a research assistant to assess one thousand randomly selected newspaper pages identified by our keyword search. Table A4 reports the results of this assessment, showing that our approach correctly identifies articles about crimes (allegedly) committed by Black men. Approximately 20% of the *pages* that the keyword search returned contain *articles* insinuating a link between “negro” (or “colored”) and “rape” or “crime” or “murder”. Of these, about 80% are instances of reporting, including reports on allegations, lynchings, court proceedings, and so forth. Almost 50% of these explicitly mention a black perpetrator and a white victim.

Note that our regression sample consists of draws on more than 1.5 million newspaper pages, of which 228,630 contain the keyword combination. This renders it unfeasible to assess every page, identify those pages manually where the keyword combination appears in the same article, and finally use the resulting page count as a more precise measure of anti-Black propaganda. If anything, relying on our straightforward measure increases (classical) measurement error, making it more difficult to find statistically significant effects. Further, it helps us capture biased editorial decisions to locate articles about Black people on the same page as articles about crimes unrelated to Black people or in which the race of the offender was not explicitly mentioned.

Table A4: Identifying newspaper articles on pages

Newspaper articles manually assessed	1003
Keywords correctly transcribed	897
Keyword combination in same article	213
Insinuating connection between keywords in article	200
Constituted Reporting	166
Mentions Black perpetrator and white victim	81

Notes: This table documents the results from validating one thousand newspaper pages identified by our approach.

A.2.2 Identifying the Political Affiliation of Newspapers

Gentzkow et al. (2011) and Gentzkow et al. (2015) digitized newspaper directories to pro-

vide information about newspapers’ political affiliations in presidential elections. We link this information from 1888 to 1896 to our data set to distinguish between newspapers that supported the Democratic Party and those that endorsed other parties or were independent. For 489 newspapers for which the political endorsement is missing or unknown from these sources, we manually identify whether the newspaper showed the election ticket of any party on its header or stated that it was, e.g., a Democratic-affiliated newspaper, issues close to the 1892 election. If a newspaper did not show such information in the election year, we looked for indications in earlier or later elections. For the remaining newspapers, we rely on the title of the newspapers and use the keywords “DEMOCRAT”, “REPUBLICAN”, “POPULIST” or “PEOPLE” to identify newspaper affiliation accordingly.

A.2.3 Details on our Classifier-based Measure

Our second measure of anti-Black propaganda is based on training a classifier in an annotated subsample of the corpus and apply this classifier to predict whether a page in the wider corpus contains anti-Black propaganda. Here, we describe the details of the training sample, model specification, and performance metrics.

Training and Labeling Strategy We train a logistic regression classifier on the SentenceBERT embeddings of each page, with five-fold cross-validation for tuning. For this, we construct a labeled sample of 353 pages (87 positives, 266 negatives) as follows: We first identified the 500 pages with the highest scores under our semantic-similarity measure described in Appendix A.4.6 as a pool of likely positives. Second, for each such page, we then selected up to five pages from the same newspaper, published shortly before or after it, that scored below the 75th percentile of the similarity measure. These serve as likely negatives, while keeping the newspaper, time, and general topic mix comparable. Third, from this pool of likely negatives, we drew a 10% random subsample and asked a research assistant to code whether each page in fact contained a racist article linking Black people with crime, rape, or murder. Fewer than 5% were labelled as false negatives. Finally, the same research assistant then hand-coded the likely positive pages. Roughly 55% were labelled as true positives; around 40% mentioned race and crime separately but did not link them; and about 5% contained neither Black nor crime-related content (both treated as false positives). This yields a labelled set with a realistic mix of true propaganda pages and “near misses,” which is particularly useful for training a classifier.

Model and Performance Using this labelled sample, we train a logistic regression classifier on the SBERT embeddings of each page, with five-fold cross-validation for tuning. This

corresponds to a supervised model built on top of frozen embeddings rather than end-to-end fine-tuning of the transformer weights, which is better suited to our small but carefully curated training set and helps avoid overfitting. All performance statistics reported below are out-of-sample (cross-validated). For the downstream task of detecting anti-Black propaganda pages, the classifier reaches an accuracy of 78.2%, an F1-score of 66.5%, recall of 86.1%, and precision of 54.4%. We interpret this out-of-sample performance as strong, especially given the noisy OCR text and the absence of article-level segmentation.

A.3 Additional Figures and Tables

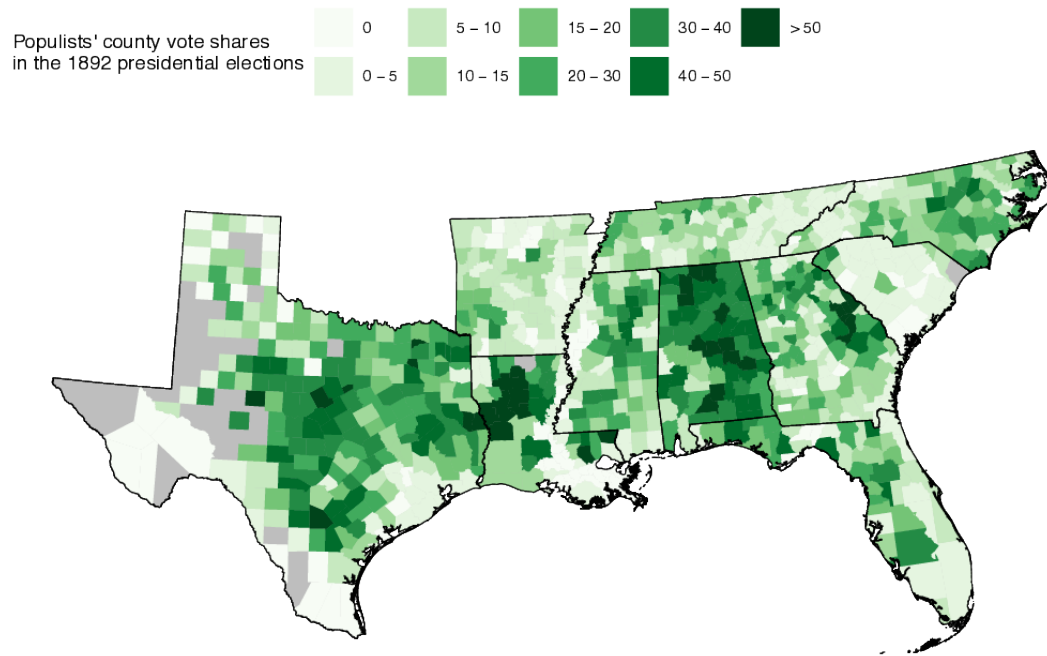


Figure A1: Vote share for the Populist Party in the 1892 presidential elections.

Notes: The map shows the county-level vote share for the Populist Party in the 1892 presidential election. Data comes from [Clubb et al. \(2006\)](#), except for Louisiana and Alabama. For Louisiana, where no separate data on election returns is available for 1892, we use the Populist vote share in the 1894 Congressional election. In Alabama, the vote shares for the Populists are missing or erroneously close to zero. We draw on Wikipedia to fill this gap and cross-check with [Burnham \(1955\)](#).

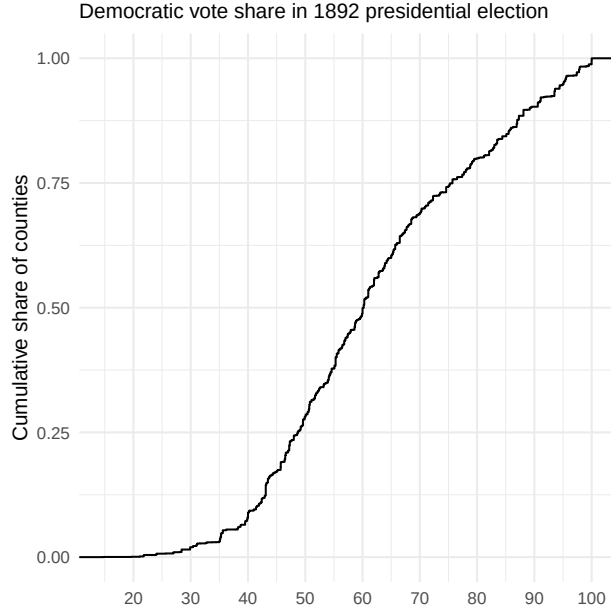


Figure A2: Cumulative distribution of Democratic vote shares in the 1892 presidential elections

Notes: This figure shows the cumulative distribution of Democratic vote shares across counties in the 1892 presidential Election. The median vote share is 60%.

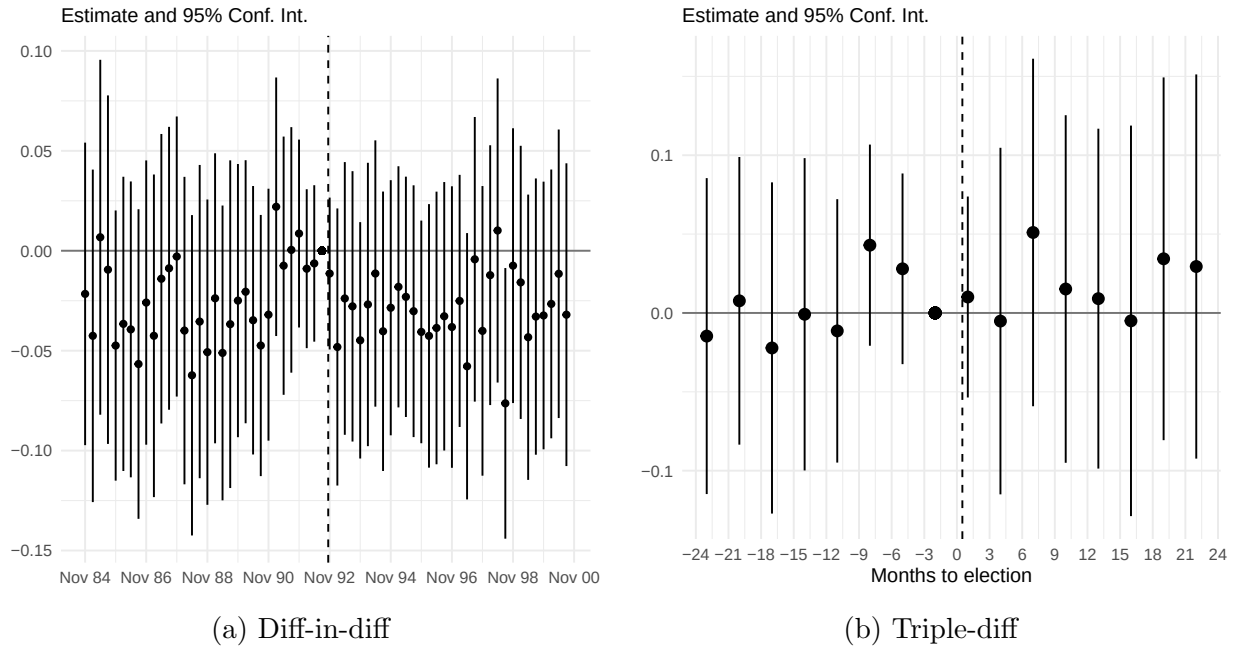


Figure A3: Placebo test: Non-Southern states

Notes: The figure shows the estimates for β_k and confidence intervals at the 95% level from equations (1) and (2) using the keyword-based in the sample of newspapers located outside the South outcome. The political-threat indicator equals one if the Democratic vote share is below its sample median: 38.8% in Panel (a), and 36.7% in Panel (b). Standard errors are clustered at the county level.

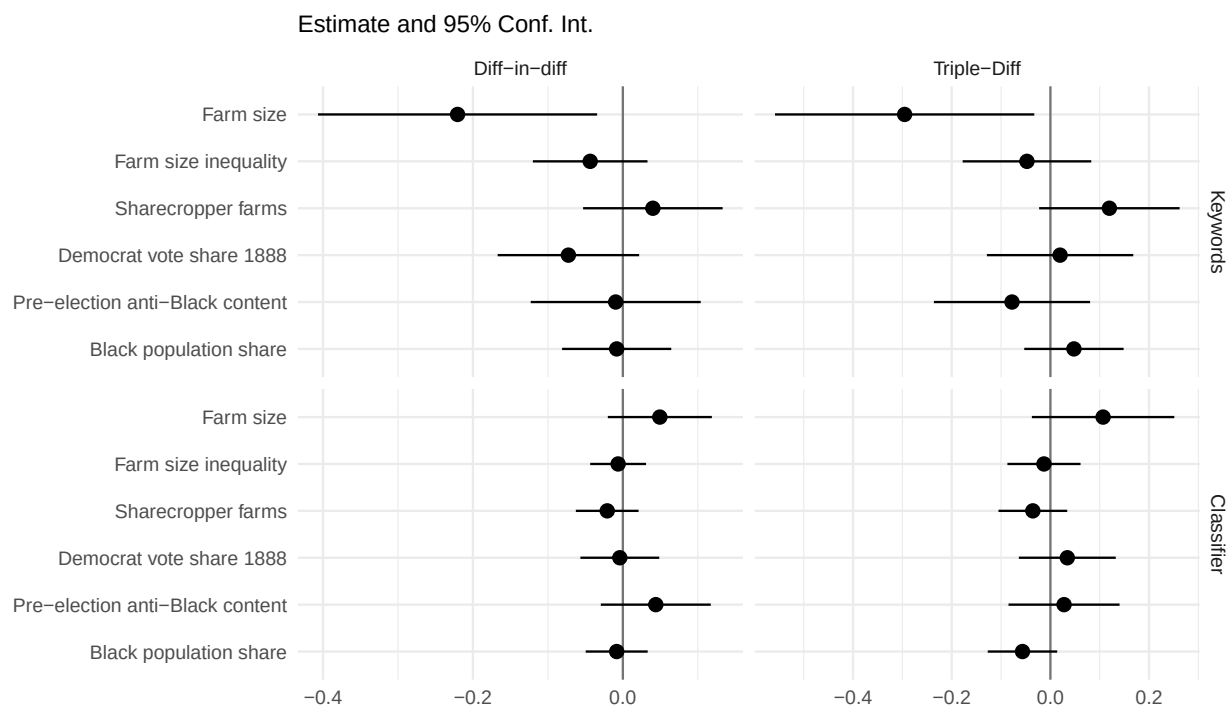


Figure A4: Heterogeneity: Economic inequality and latent racism

Notes: This figure shows that our baseline static coefficients do not vary systematically with economic inequality or latent racism. The figure depicts estimates and 95% confidence intervals from interacting equations (3) and (4) with above-median dummies for several proxies of economic inequality and latent racism across counties, as indicated. The left panel presents estimates of interactions with the static difference-in-differences specification, while the right panel presents estimates of the static triple-differences specification. The panels above show estimates using the keyword-based outcome measure, while the two panels below show estimates using the classifier-based outcome measure. See Appendix A.1 for details on the data source and variable definition.

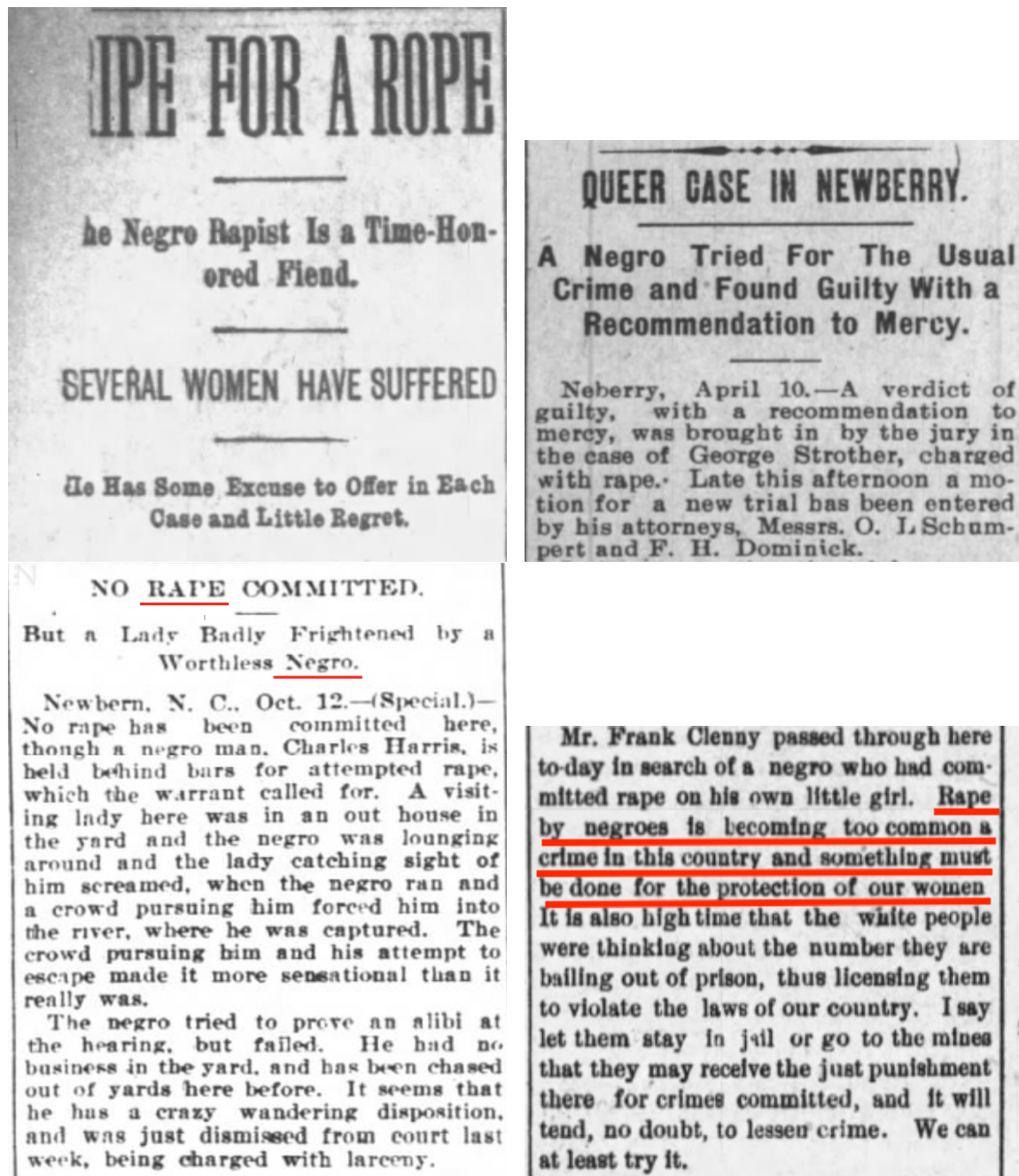


Figure A5: Illustrative examples of newspaper articles associating Black men with rape

Notes: Top left panel: Public Ledger, Memphis (TN), 1893. Top right panel: The Watchmen and Southron, Sumter (SC), 1903. Bottom left panel: News and Observer, Raleigh (NC), 1898. Bottom right panel: Eufaula Daily Times (AL), 1893

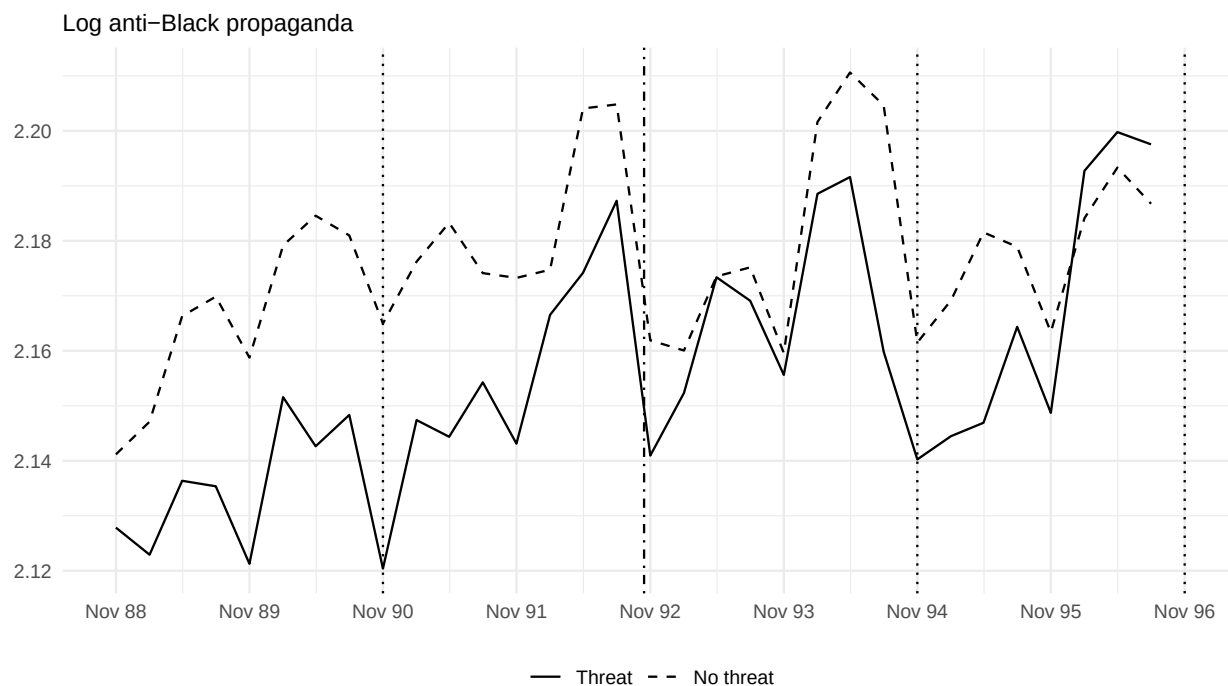
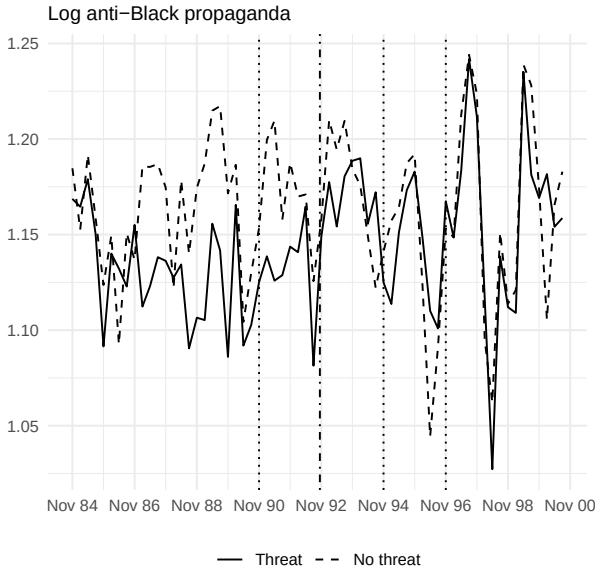
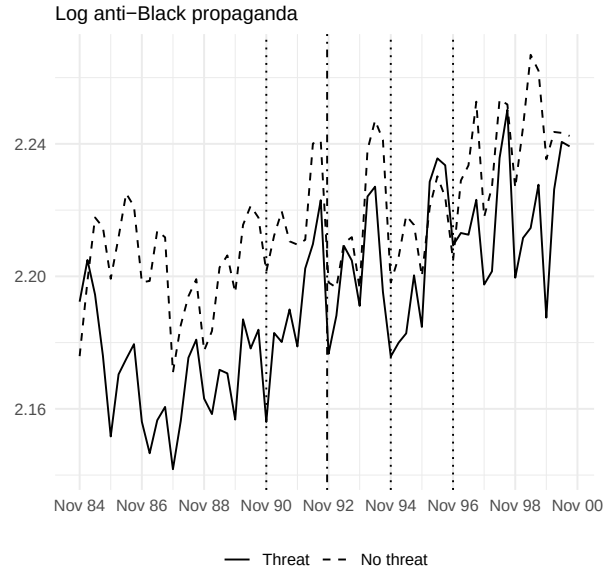


Figure A6: Evolution of classifier-based anti-Black propaganda by counties' threat status

Notes: The figure shows the time variation in classifier-based anti-Black propaganda for threatened (solid line) and non-threatened (dashed) counties from November 1888 to October 1896. To account for heterogeneity in page volume, we residualize the raw measure by the log of total page count. See Appendix [A.1](#) for details on the data source and variable definition.



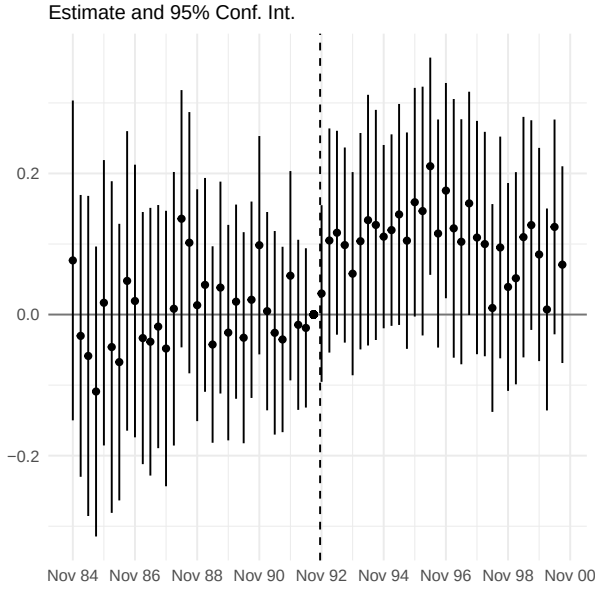
(a) Keywords



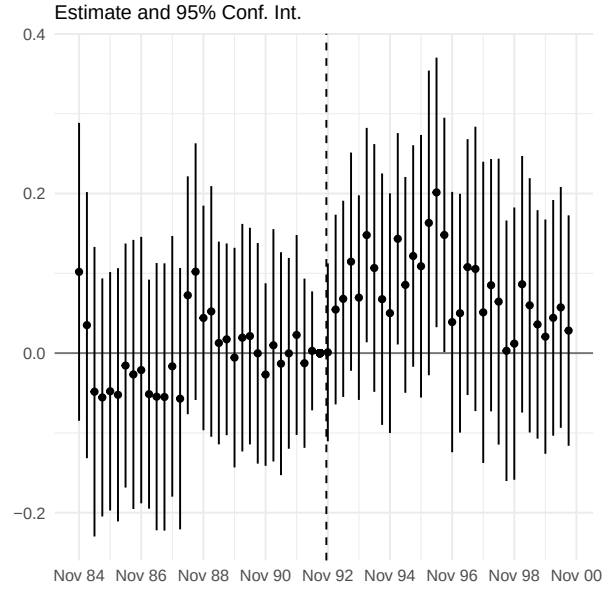
(b) Classifier

Figure A7: Evolution of anti-Black propaganda by counties' threat status (extended time series)

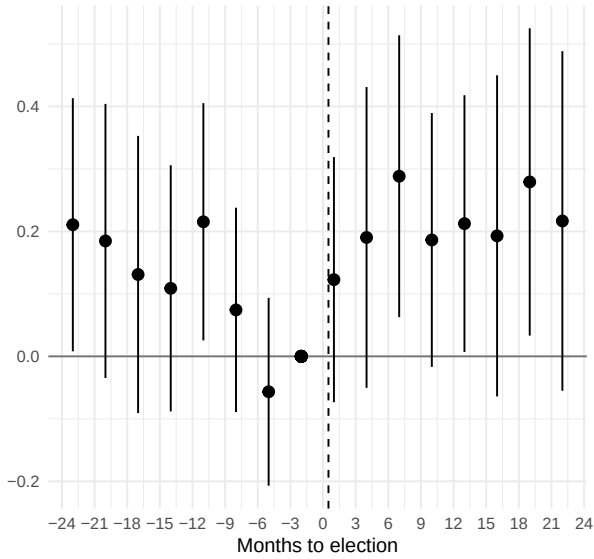
Notes: The figure shows the time variation in anti-Black propaganda for threatened (solid line) and non-threatened (dashed) counties from November 1884 to October 1900. Panel (a) shows this for the keyword-based outcome measure, extending Figure 2b in the main text. Panel (b) shows this for the classifier-based outcome measure, extending Appendix Figure A6. See the notes of Figure 2b for how the raw measures were residualized. See Appendix A.1 for details on the data source and variable definition.



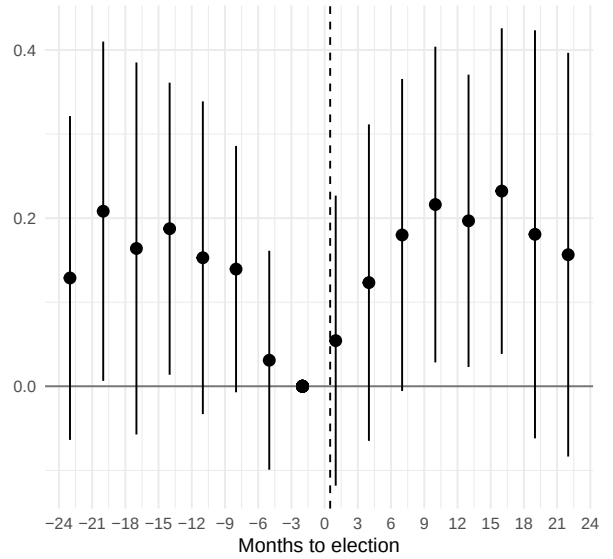
(a) Diff-in-Diff: Reporting about Blacks
Estimate and 95% Conf. Int.



(b) Diff-in-Diff: Reporting about crime
Estimate and 95% Conf. Int.



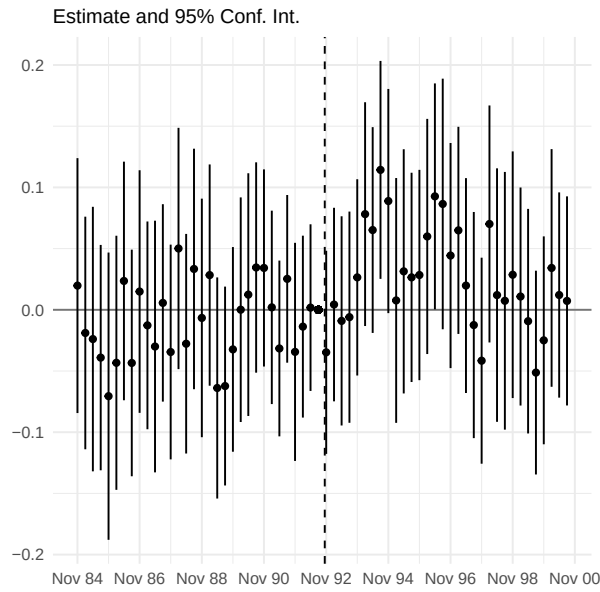
(c) Triple-Diff: Reporting about Blacks



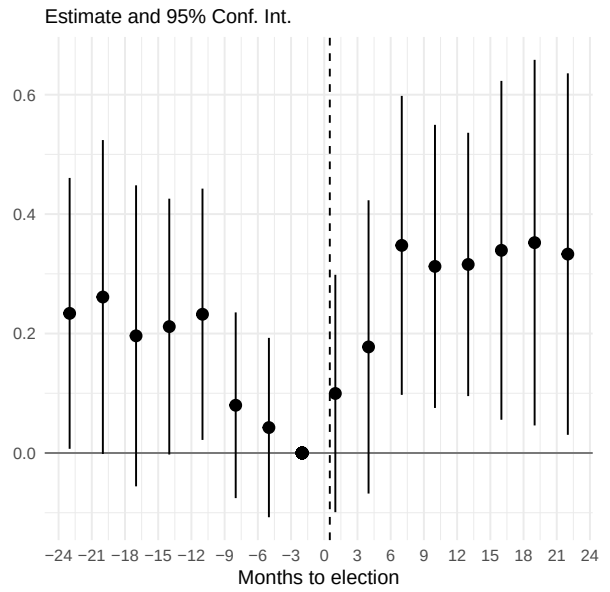
(d) Triple-Diff: Reporting about crime

Figure A8: Mentions of Blacks and crime separately increase after the election

Notes: Top panel: The figures show the results of estimating the difference-in-differences event study described in equation (1) using the log mentions of one plus “negro” (left, Panel a) and “rape”, “murder”, or “crime” (right, Panel b) as the outcome variable. Note that the specification differs from the description of the equation in the text in that we do not include these outcomes in the control vector. Bottom panel: The figures show the results of estimating the triple-differences event study described in equation (2) using the log mentions of one plus “negro” (left, Panel c) and “rape”, “murder”, or “crime” (right, Panel d) as the outcome variable. Different to the baseline specifications, we do not include these outcomes in the control vectors.



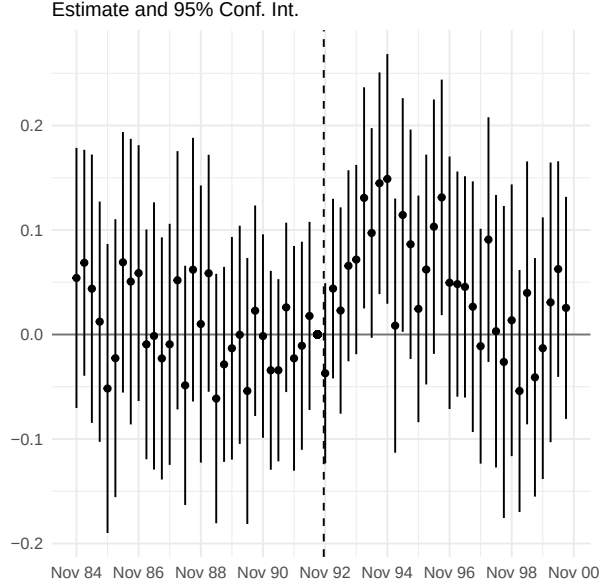
(a) Diff-in-Diff



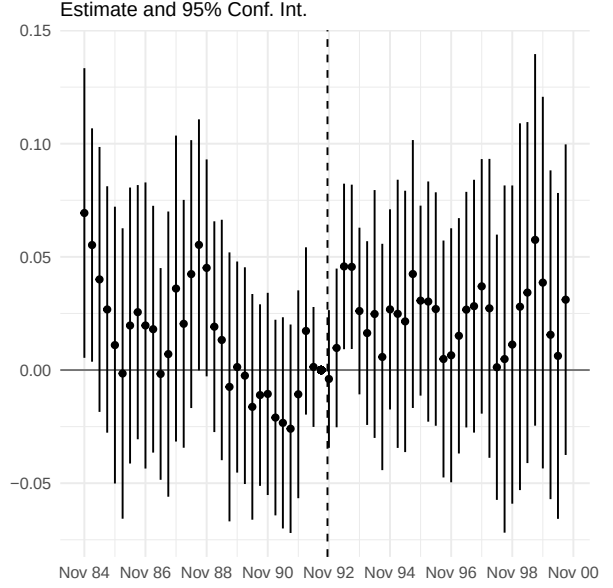
(b) Triple-Diff

Figure A9: Estimates from specifications that do not control for mentions of Blacks and crime

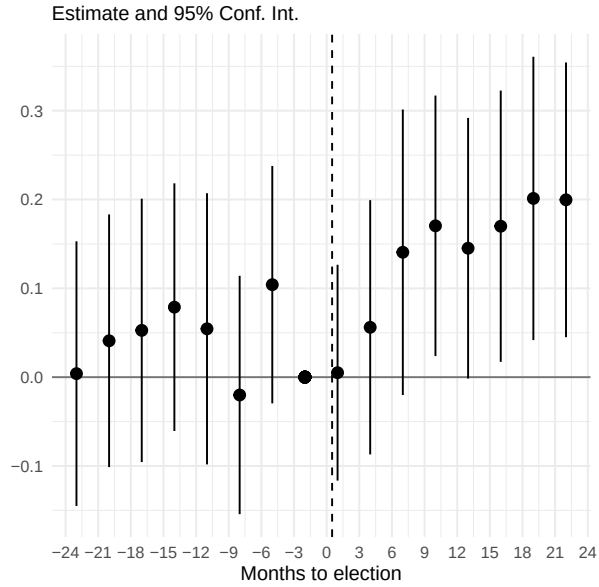
Notes: The figures show the results of equation (1) in Panel (a) and equation (2) in Panel (b) without including the controls for log one plus mentions of “negro” and “rape”, “murder”, “crime”. See Appendix A.1 for details on the data source and variable definition.



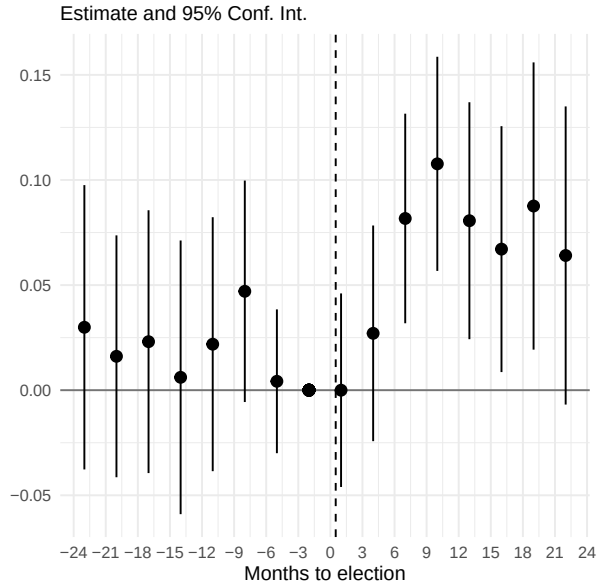
(a) Diff-in-diff: Keyword-based outcome



(b) Diff-in-diff: Classifier-based outcome



(c) Triple-diff: Keyword-based outcome



(d) Triple-diff: Classifier-based outcome

Figure A10: Event-study estimates with extended controls

Notes: This figure shows that extending the set of control variables in the baseline event study yields similar dynamic patterns. The figure follows Figure 3 in the main text, and additionally includes the full set of controls shown in Column 4 of Table 1. The figure depicts the estimates for β_k and confidence intervals at the 95% level from equation (1) in Panels (a) and (b), and from equation (2) in Panels (c) and (d). The outcome in Panels (a) and (c) is the logarithm of one plus the number of newspaper pages containing anti-Black propaganda in a given month. In Panels (b) and (d), it is the logarithm of one plus the sum of classifier-based probability scores for anti-Black propaganda in a given month (see Section 3.2 for details). The main independent variable is the interaction of (i) Political threat $_{c(i)}$, a county-level dummy indicating that the Democratic Party received less than the Southern sample median vote share in the 1892 presidential election (60%; see Section 3.1 for details), with (ii) indicators for three-month bins k . For the difference-in-differences event-studies in Panels (a) and (b), these indicators cover the period from November 1884 to October 1900. The triple-difference event-studies in Panels (c) and (d) interact these with another indicator, (iii) $1(1892 \text{ cycle}_t)$, which enables a comparison of the four years around the 1892 election cycle with the four years around the 1888 election cycle. Standard errors are clustered at the county level.

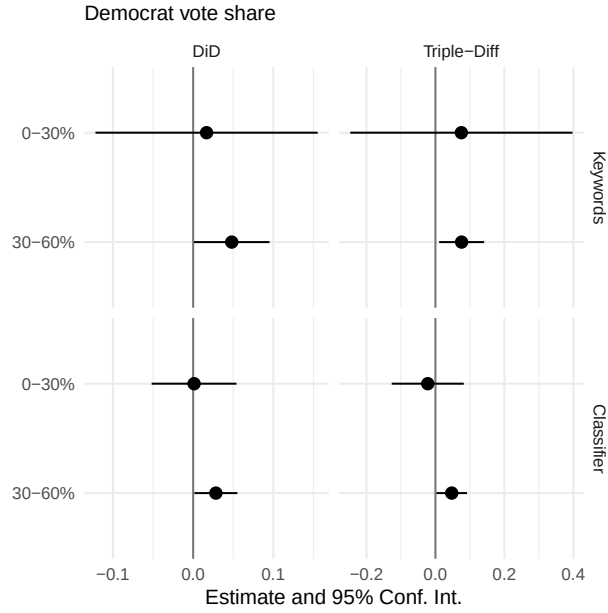
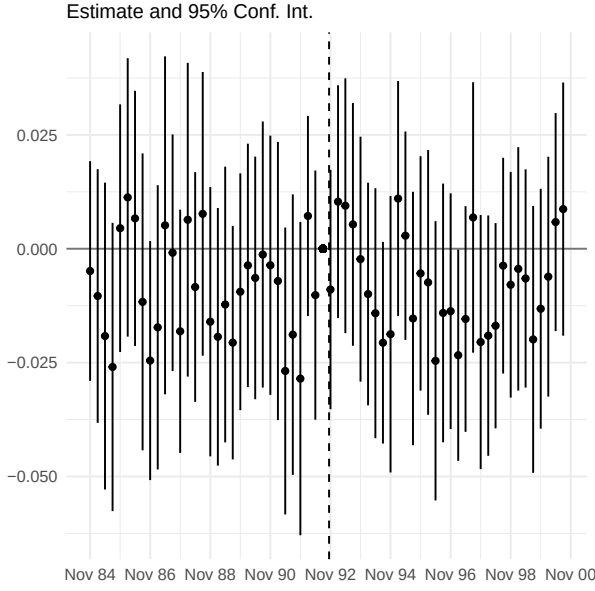
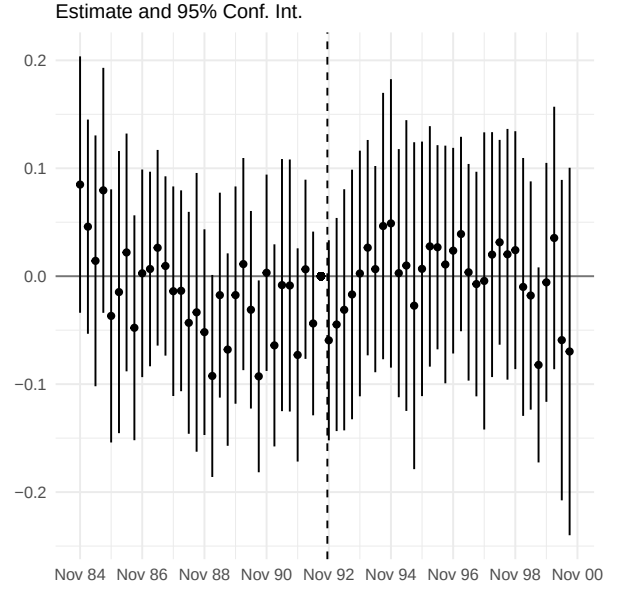


Figure A11: Effect heterogeneity by Democratic vote share

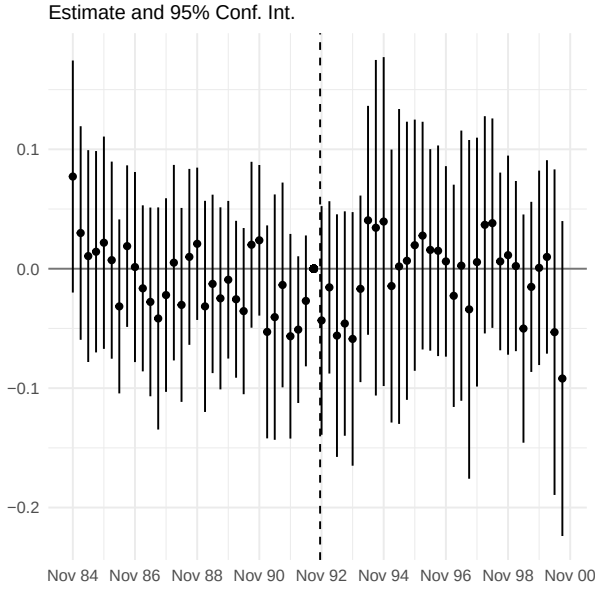
Notes: The figure shows that the effect on propaganda appears to be concentrated in counties where Democrats did not enjoy a secure majority, with no detectable effect in opposition strongholds. The figure depicts the coefficients and 95% confidence intervals from estimating equations (3) on the left, and (4) on the right, with the political-threat indicator replaced by dummies for whether the Democratic county-level vote share in the 1892 presidential election was (i) below 30%, (ii) between 30% and 60%, or (iii) above 60% (the omitted category). The upper panels display these coefficients for the keyword-based outcome measure, while the lower panels show those for the classifier-based outcome measure.



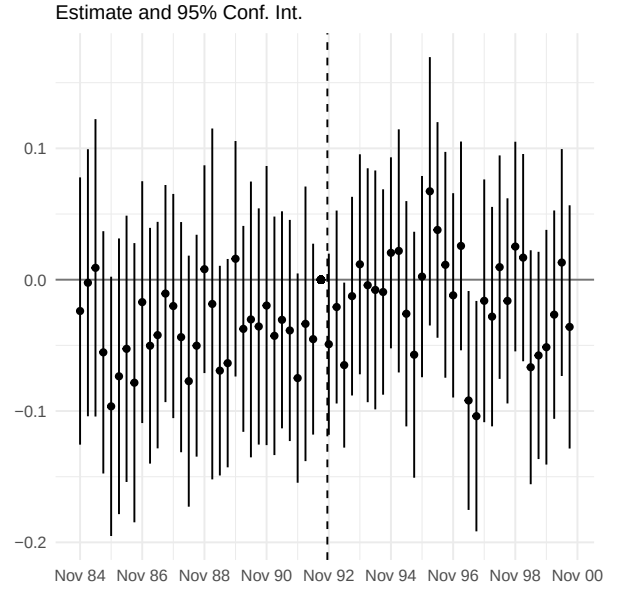
(a) “Asians”



(b) “Chinese”



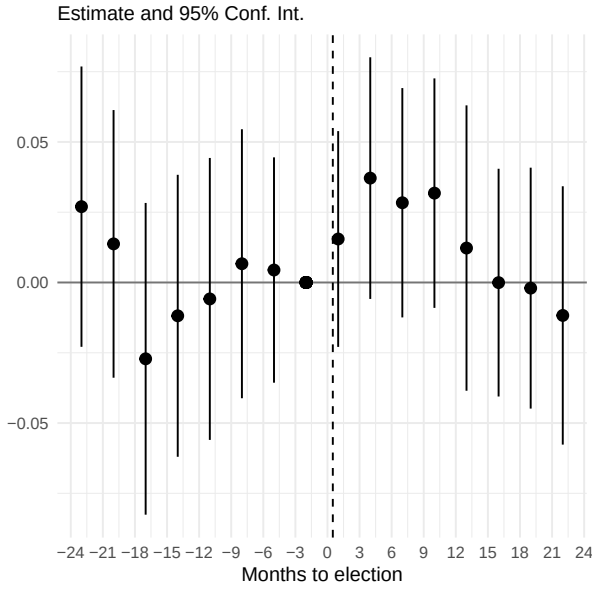
(c) “Japanese”



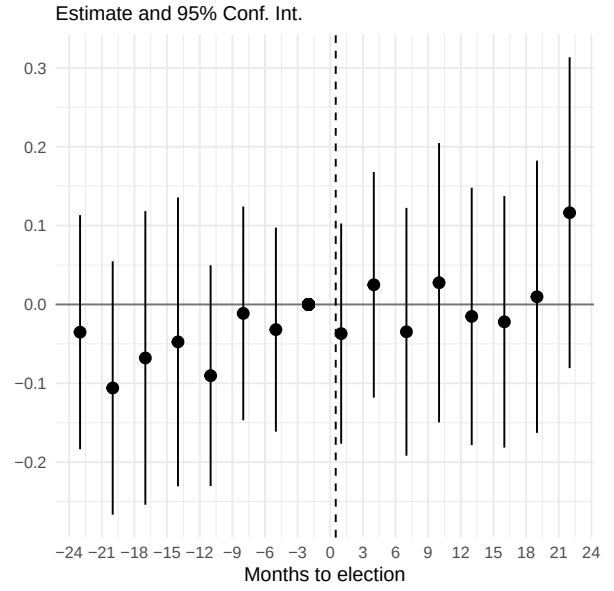
(d) “Mexicans”

Figure A12: Difference-in-differences estimates for other ethnic groups

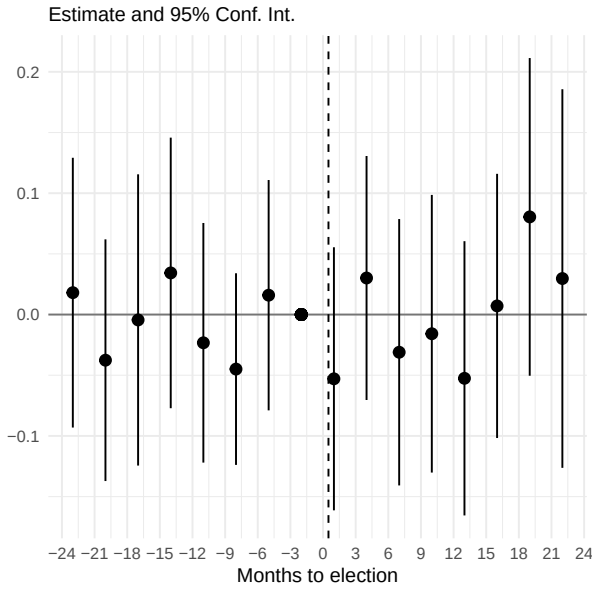
Notes: The figures show the results of the difference-in-differences event study equation (1) with the logarithm of one plus the number of joint mentions of the relevant ethnic group mentioned in the caption of each subgraph with crime-related keywords as dependent variables. All panels control for the log one plus counts of the ethnic term and the crime terms.



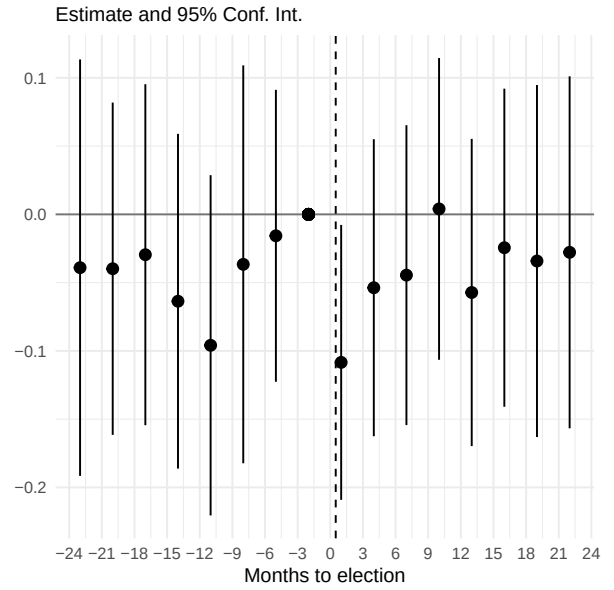
(a) “Asians”



(b) “Chinese”



(c) “Japanese”



(d) “Mexicans”

Figure A13: Triple-difference estimates for other ethnic groups

Notes: The figures show the results of the triple-differences event study equation (2) with the logarithm of one plus the number of joint mentions of the relevant ethnic group mentioned in the caption of each subgraph with crime-related keywords as dependent variables. All panels control for the log one plus counts of the ethnic term and the crime terms.

A.4 Robustness of Core Estimates

This section reports results from various robustness and sensitivity tests of our core difference-in-differences and triple-difference estimates. We document that the baseline result of increased anti-Black propaganda in more threatened counties after the 1892 election is robust to alternative definitions of political threat (section A.4.1), alternative formulations of the dependent variable (section A.4.2), and that the results are unlikely to be driven by the entry or exit of newspapers in our sample (section A.4.3). Furthermore, we show that alternative assumptions about the standard errors (section A.4.4) and excluding one state of the sample at a time (section A.4.5) do not alter the main takeaways, and neither does using the semantic-similarity based measure of anti-Black propaganda as outcome variable (section A.4.6).

A.4.1 Robustness to Alternative Definitions of Political Threat

For our baseline political threat measure, we assumed Democrats were more likely to perceive political threat when their vote share was below the sample median of 60% in the 1892 presidential election. We test the sensitivity of this assumption using alternative cutoffs and other definitions in all panels of Figures A14 to A17. All these figures follow the same structure: In Panel (a), we define counties with Democratic vote shares below the 60th percentile of the sample in the 1892 presidential election as threatened. Panel (b) lowers this threshold to the 55th percentile, and Panels (c) and (d) lower it further to the 45th and 40th percentiles, respectively. Panel (e) instead uses a very narrow definition of competitive counties, treating counties as threatened when the Democrats receive between 40 and 60% of the vote share. Finally, Panel (f) interacts the baseline measure with the continuous “fusion” vote share of the Populist and Republican parties.

Figures A14 and A15 present the difference-in-differences results, and Figures A16 and A17 show the triple-differences results, for the keyword-based and classifier-based outcome measures, respectively. All specifications are fully saturated and include our baseline controls as outlined in the main text.

Throughout, we observe similar qualitative patterns to those in the baseline Figure 3: no evidence of a pre-trend before the 1892 election and a slow buildup that peaks over the subsequent elections.

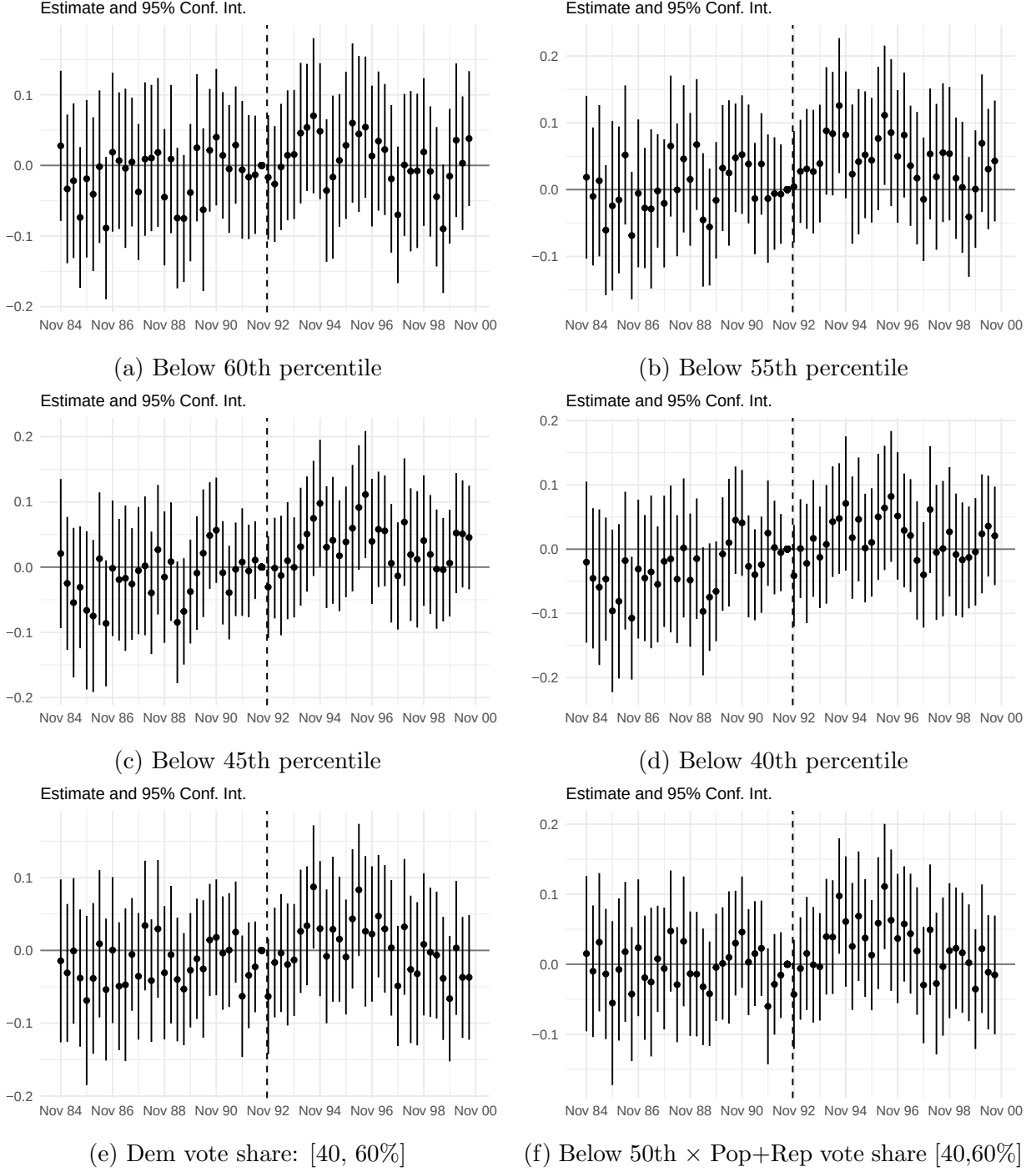
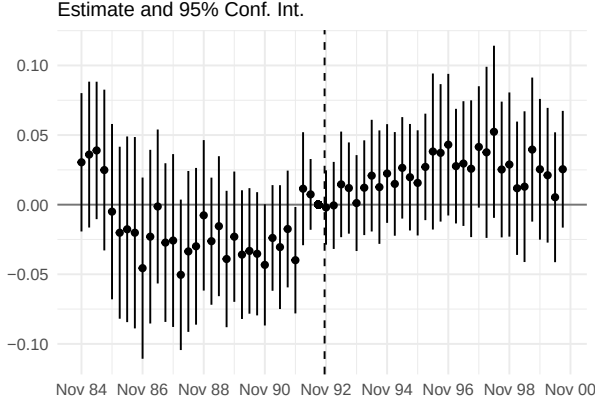
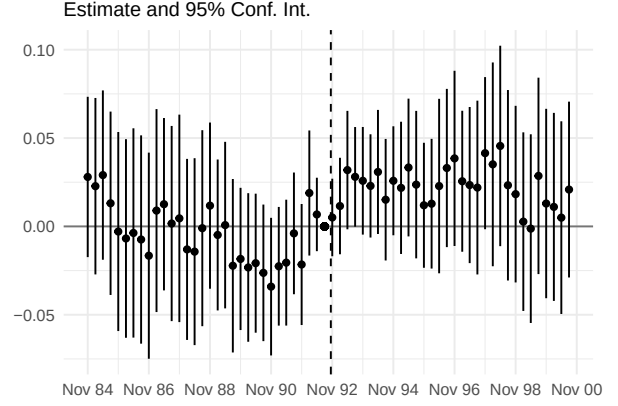


Figure A14: Alternative definitions of political-threat indicator (Diff-in-diff; keywords)

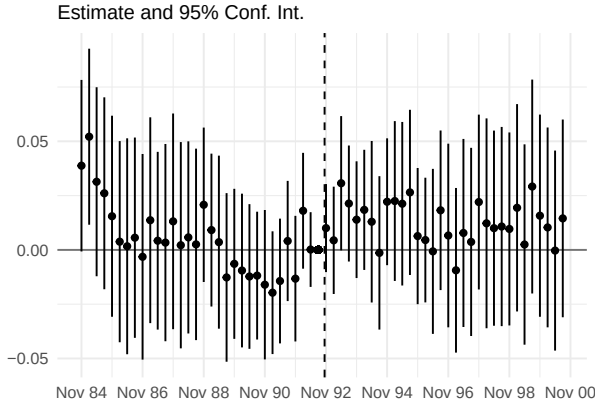
Notes: This figure illustrates that using alternative definitions of the political threat measure, as indicated in each panel label and described in the text, yields similar patterns to those displayed in the baseline Figure 3. This figure shows estimates from the event-study difference-in-differences specification (equation (1) in the main text) using the keyword-based outcome. See the main text and Appendix A.1 for further details on the specification and variable definition.



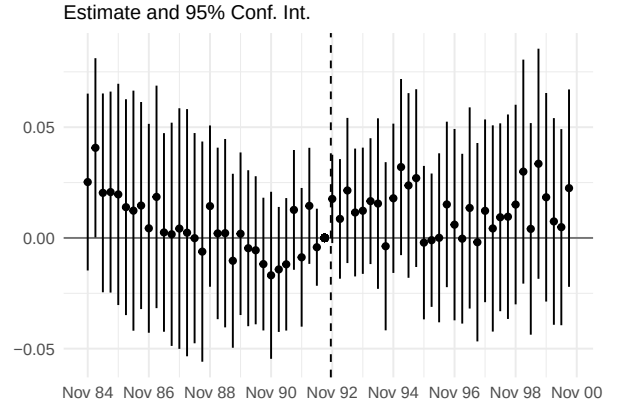
(a) Below 60th percentile



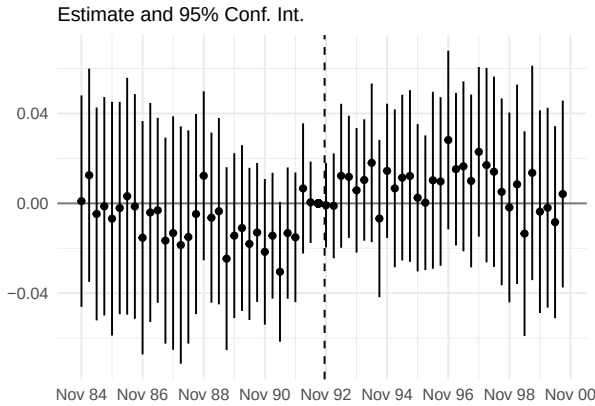
(b) Below 55th percentile



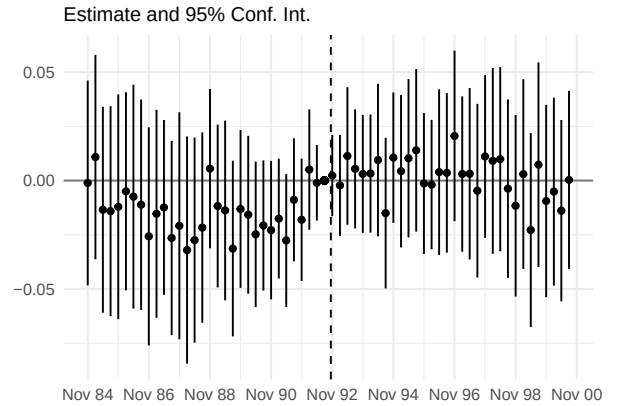
(c) Below 45th percentile



(d) Below 40th percentile



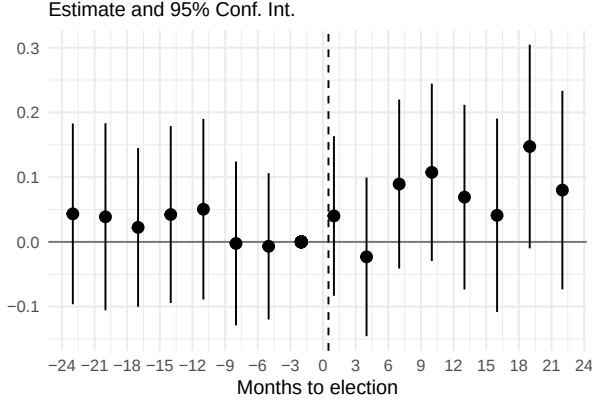
(e) Dem vote share: [40, 60%]



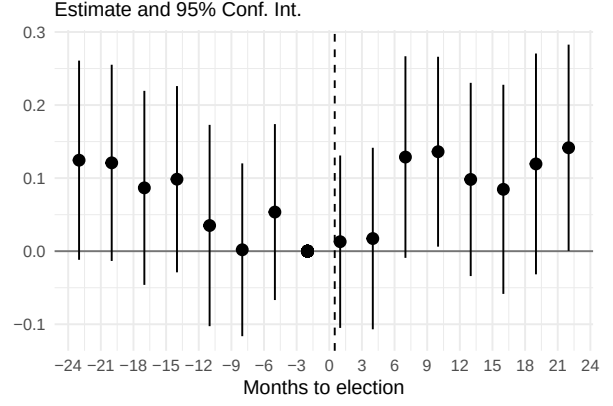
(f) Below 50th $\times$ Pop+Rep vote share [40,60%]

Figure A15: Alternative definitions of political-threat indicator (Diff-in-diff; classifier)

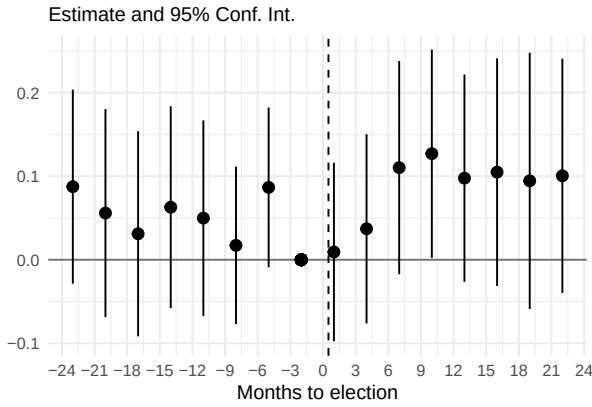
Notes: This figure illustrates that using alternative definitions of the political threat measure, as indicated in each panel label and described in the text, yields similar patterns to those displayed in the baseline Figure 3. This figure shows estimates from the event-study difference-in-differences specification (equation (1) in the main text) using the classifier-based outcome. See the main text and Appendix A.1 for further details on the specification and variable definition.



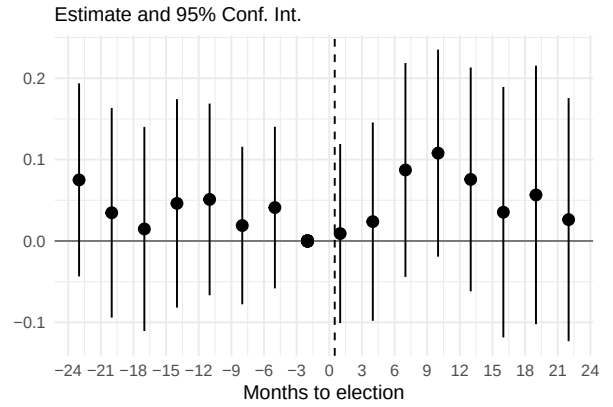
(a) Below 60th percentile



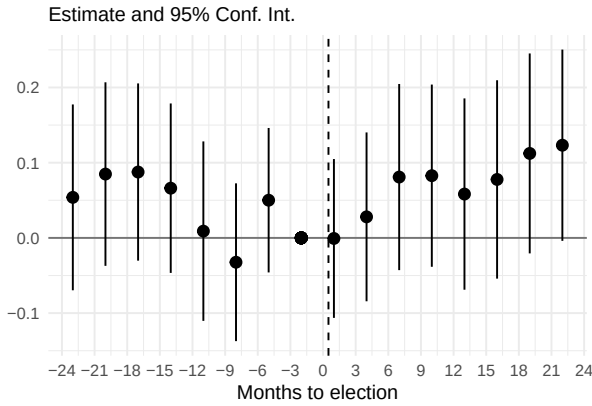
(b) Below 55th percentile



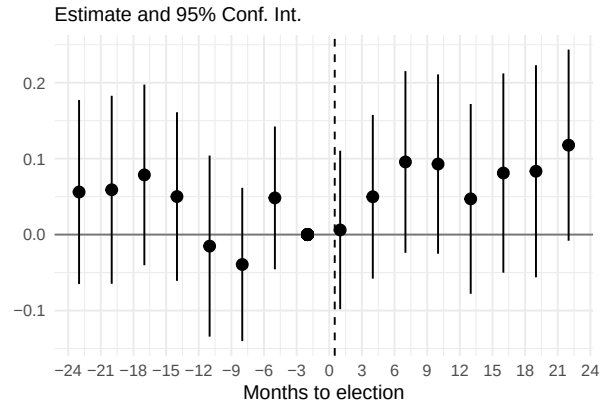
(c) Below 45th percentile



(d) Below 40th percentile



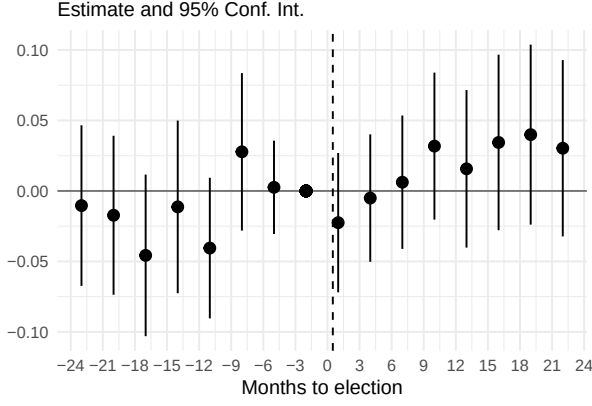
(e) Dem vote share: [40, 60%]



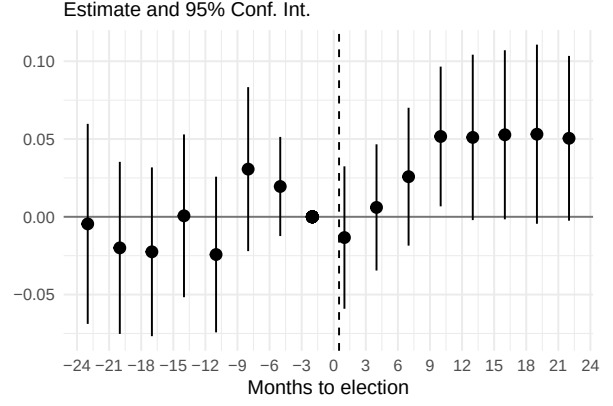
(f) Below 50th $\times$ Pop+Rep vote share [40,60%]

Figure A16: Alternative definitions of political-threat indicator (Triple-diff; keywords)

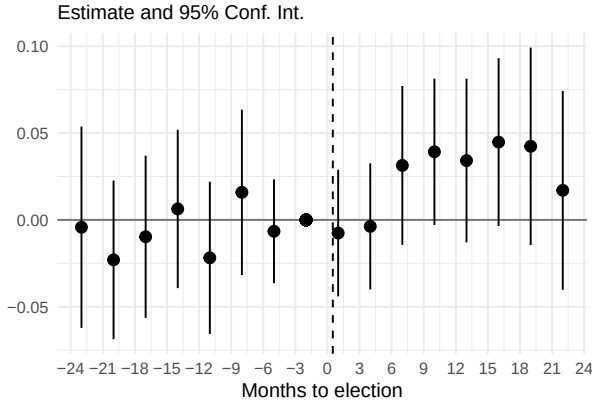
Notes: This figure illustrates that using alternative definitions of the political threat measure, as indicated in each panel label and described in the text, yields similar patterns to those displayed in the baseline Figure 3. This figure shows estimates from the event-study triple-differences specification (equation (2) in the main text) using the keyword-based outcome. See the main text and Appendix A.1 for further details on the specification and variable definition.



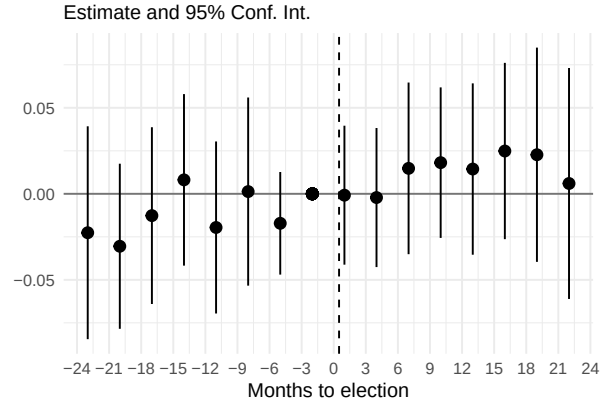
(a) Below 60th percentile



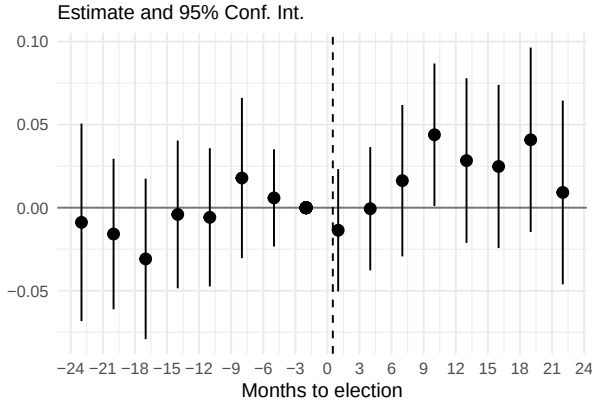
(b) Below 55th percentile



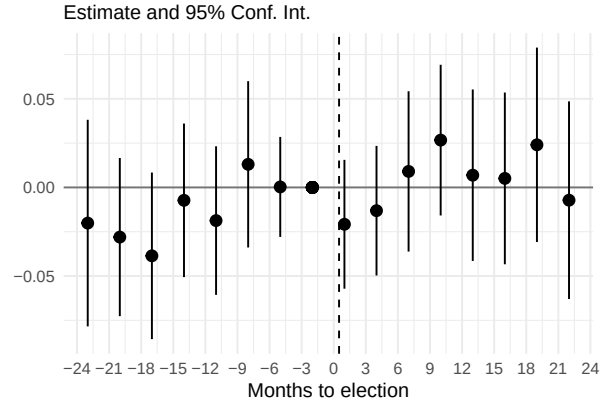
(c) Below 40th percentile



(d) Below 45th percentile



(e) Dem vote share: [40, 60%]



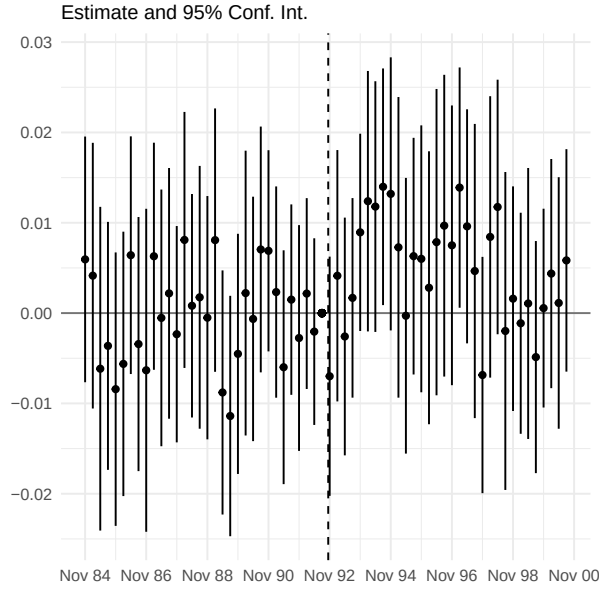
(f) Below 50th $\times$ Pop+Rep vote share [40,60%]

Figure A17: Alternative definitions of political-threat indicator (Triple-diff; classifier)

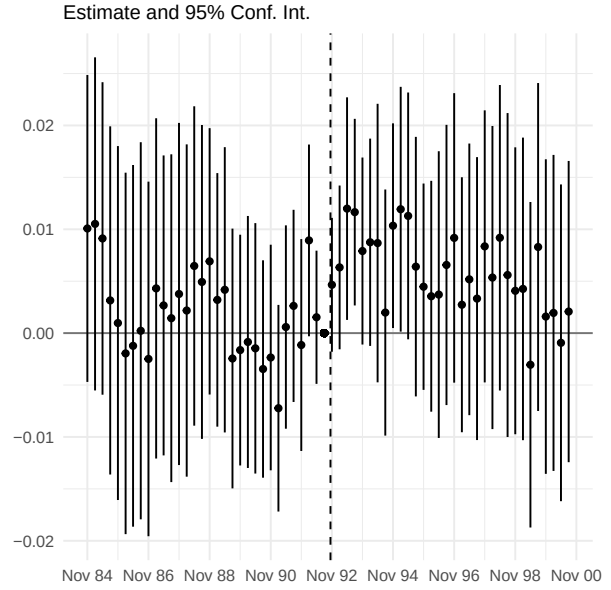
Notes: This figure illustrates that using alternative definitions of the political threat measure, as indicated in each panel label and described in the text, yields similar patterns to those displayed in the baseline Figure 3. This figure shows estimates from the event-study difference-in-differences specification (equation (2) in the main text) using the classifier-based outcome. See the main text and Appendix A.1 for further details on the specification and variable definition.

A.4.2 Alternative Dependent Variable Formulation

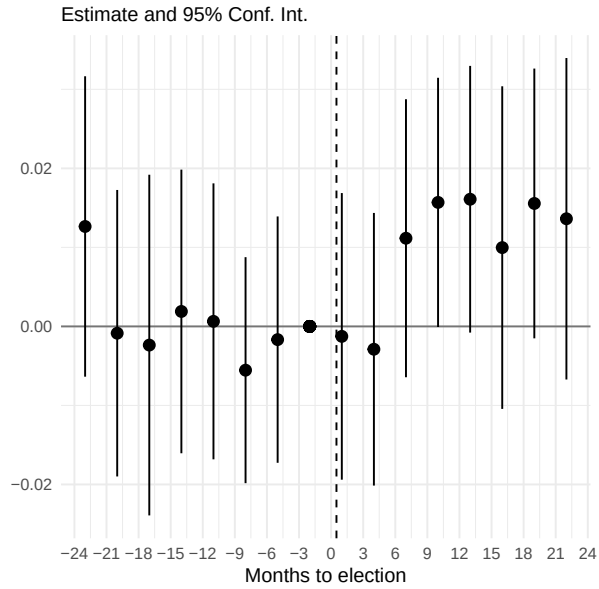
Our baseline dependent variables are the logarithm of one plus the number of joint mentions of the keywords “rape,” “murder,” or “crime” with “negro” and the logarithm of one plus the sum of classifier scores in a newspaper-month. We control throughout for the logarithm of one plus the number of pages. In Appendix Figure [A18](#), we instead use, as outcomes, the share of newspaper pages containing these keyword combinations and the sum of classifier scores divided by page count in each newspaper-month. The results are comparable in size and statistical significance.



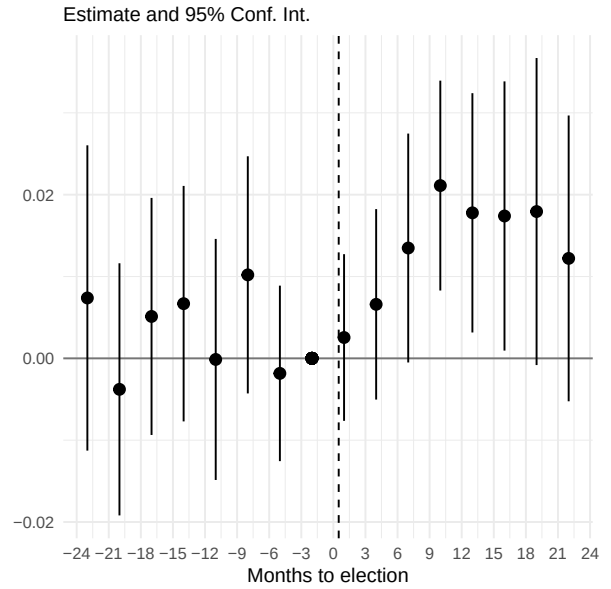
(a) Diff-in-diff: Keyword-based outcome



(b) Diff-in-diff: Classifier-based outcome



(c) Triple-diff: Keyword-based outcome



(d) Triple-diff: Classifier-based outcome

Figure A18: Event-study estimates using the share of pages containing propaganda as the outcome

Notes: This figure shows that using the share of newspaper pages containing propaganda and the sum of classifier scores divided by page count as the outcomes yields dynamics similar to those in Figure 3. Specifically, we use, as outcomes, the share of pages with keyword-based propaganda (untransformed counts of pages with the relevant keyword combinations divided by total pages) and the share of pages with classifier-based propaganda (the sum of classifier probabilities divided by total pages) in each newspaper-month. All specifications include the baseline set of fixed effects and, for the keyword-based outcome, control for the share of pages mentioning race- and crime-related terms. See the main text and Appendix A.1 for further details on the specification and variable definitions.

A.4.3 Entry and Exit of Newspapers

The newspaper database is highly unbalanced. While some newspapers are available for many years, most are only for short periods. A highly unbalanced panel may cause problems for the estimation if the entry and attrition of newspapers are systematically related to both the outcome and the treatment. To address this concern, we conduct two separate analyses. First, we show that when we restrict the sample to a balanced panel of continuously observed newspapers, the keyword-based difference-in-differences and the classifier-based estimates display dynamic patterns broadly comparable to those in the full sample, albeit with wider confidence intervals. For the keyword-based triple-difference specification, the balanced-panel estimates become considerably noisier and no longer very informative (Appendix Figure A19). Second, we examine whether newspaper exit or entry was more likely in threatened counties than in non-threatened counties after the 1892 election, and we assess whether exiting or entering newspapers contained more propaganda in threatened counties compared to non-threatened counties. We find that exit and entry rates do not differ systematically between threatened and non-threatened counties after the 1892 election (Appendix Table A5). Newspapers that enter or exit between 1892 and 1894 are not systematically more or less propaganda-intensive in threatened counties than elsewhere (Appendix Table A6). Any composition effects we can detect are small and statistically imprecise, suggesting that it is unlikely that exit or entry systematically inflates our baseline estimates.

Balanced Sample We replicate the analyses using only the subsets of newspapers for which we have a complete run of newspaper-period observations. Figure A19 below presents the resulting dynamic estimates. For the keyword-based outcome, the balanced-panel difference-in-differences estimates are qualitatively similar but noisier than in the full sample: coefficients remain close to zero before 1892 and turn positive afterwards, but with wider confidence intervals and somewhat smaller magnitudes—unsurprising given the sizeable reduction in sample size. By contrast, the triple-difference estimates are basically flat with wide bands. They do not contradict the baseline, but they are not very informative either. For the classifier-based outcome, the dynamic pattern in the balanced sample is very close to the baseline estimates, again with reduced precision. We therefore view the balanced-panel results as partially supportive but not decisive: they do not overturn our findings, but the loss of precision means we rely primarily on more targeted survival tests described next.

Treatment, Outcome, and Newspaper Survival The core concern is that the survival of newspapers in our dataset could be systematically related to either the treatment status or the intensity of anti-Black propaganda, in ways that would inflate or even drive our main

Table A5: Exit and Entry as Outcome in Difference-in-Differences and Triple-Differences

	(1)	(2)	(3)	(4)
Dep. Var.:	Exit	Entry	Exit	Entry
Political Threat $\times$ Post Election	-0.000 (0.005)	-0.002 (0.004)	0.005 (0.004)	-0.001 (0.005)
Political Threat $\times$ Post Election $\times$ 1892 Election Sample			-0.003 (0.006)	-0.002 (0.007)
Political Threat $\times$ 1892 Election Sample			-0.001 (0.005)	-0.005 (0.005)
Observations	18949	18949	36430	36430
R ²	0.238	0.210	0.196	0.177

Notes: This table reports estimates of difference-in-differences (columns 1 and 2) and triple-differences regressions (columns 3 and 4). The outcome variable is a dummy for whether a newspaper exited (odd columns) or entered (even columns) in a given month. All specifications include newspaper and state-period fixed effects. Robust standard errors clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

results. For example, if less propaganda-intensive newspapers in threatened counties were more likely to cease publication after the 1892 election, or if more propaganda-intensive newspapers were disproportionately likely to enter in threatened counties after the election, then the estimated increase in propaganda intensity could reflect compositional changes rather than true responses to the election within the same newspaper. We therefore complement our balanced-panel analysis above with direct tests of whether survival correlates with treatment and with the outcome variable. Specifically, we test whether treatment predicts exit or entry, and whether survival is correlated with propaganda intensity, especially in treated counties. We begin by examining whether newspaper exit and entry are correlated with the treatment status. Specifically, we use monthly exit and entry indicators as our main outcome variables in the difference-in-differences and triple-differences regressions. Table A5 shows the estimation results. Columns 1 and 2 document that, if anything, newspapers in threatened counties were slightly less likely to exit or enter after the 1892 election, but the estimated coefficients are statistically insignificant and economically small, also in light of the overall low exit and entry probability during the 1890 to 1894 period of around 1% each month. Columns 3 and 4, extending this approach to our triple-difference sample period from 1886 to 1894, document that there was also no difference in exit and entry in threatened counties after the 1892 presidential election compared to the earlier 1888 one. These findings suggest that the treatment itself did not differentially affect newspaper survival or entry.

Next, we directly examine whether newspapers that exited (entered) in threatened counties relative to non-threatened counties after the 1892 election displayed lower (higher) levels of anti-Black propaganda. We focus on the core period from 1890 to 1894 for this and define

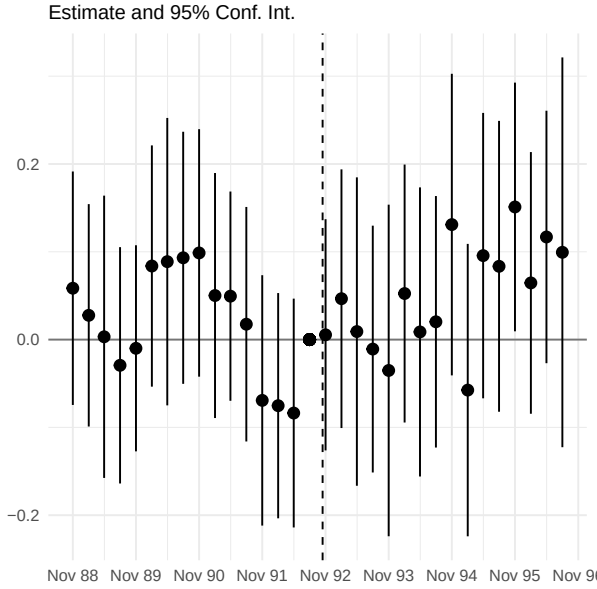
Table A6: Propaganda, Exit, Entry, and Threat in the Newspaper Cross-section

	(1)	(2)	(3)	(4)
Dep. Var.: Propaganda	Keyword-based	Classifier-based	Keyword-based	Classifier-based
Political Threat	-0.045 (0.122)	-0.222 (0.171)	-0.049 (0.118)	-0.170 (0.168)
Exit after 1892 Election	0.303* (0.177)	-0.056 (0.311)		
Political Threat $\times$ Exit after 1892 Election	-0.059 (0.248)	0.355 (0.534)		
Entry after 1892 Election			0.217 (0.185)	0.282 (0.270)
Political Threat $\times$ Entry after 1892 Election			-0.072 (0.291)	0.039 (0.495)
Observations	817	254	817	254
R ²	0.033	0.050	0.030	0.053

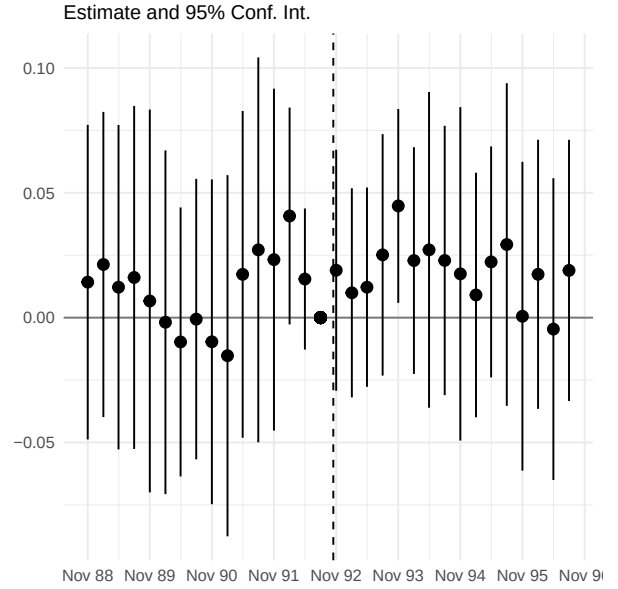
Notes: This table reports estimates of regressing propaganda on a dummy of whether a newspaper exited (columns 1 and 2) or entered (columns 3 and 4) between the 1892 and 1894 elections, and their interaction with a dummy of whether a county was treated with political threat. The sample includes newspapers from November 1890 to October 1894. The dependent variable is the average of propaganda, residualized with page count and the number of separate mentions of “negro” and “rape,” “murder,” or “crime”, and period dummies, and standardized to mean zero and standard deviation one. Odd columns use the keyword-based propaganda measure, and even ones use the classifier-based one. All columns include state fixed effects. Robust standard errors clustered at the county-level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

exit and entry as separate dummies at the newspaper level indicating whether a newspaper exited and/or entered in the period between the 1892 presidential election and the 1894 midterm elections. The core outcome variable is propaganda intensity, standardized to mean zero and standard deviation one, and residualized with respect to page count, the number of separate mentions of “negro” and “rape,” “murder,” or “crime,” and period dummies – to ensure that over-time variation does not drive our cross-newspaper results. All regressions are at the newspaper level and include state fixed effects. Table A6 reports results. Odd columns use the keyword-based propaganda measure, and even columns use the classifier-based measure. The first two columns look at newspaper exit. Column 1 suggests that exiting newspapers were 0.3 std-dev. more propaganda intensive, and that the level of propaganda was weakly lower for newspapers exiting in threatened counties (0.06 std-dev.). This could be a concern for our baseline result of more propaganda in threatened counties after the election, since those that left after treatment were less propaganda-intensive. However, the estimates are small, confidence intervals are large, and we show above already that entry per se was not higher in threatened counties after 1892. Furthermore, Column 2 shows that the opposite holds true for our classifier-based measure: if anything, more propaganda-intensive newspapers exit in threatened counties after the 1892 election, which works against us finding our baseline result of more propaganda after the election. Columns 3 and 4 show that propaganda was not significantly higher in newspapers entering in threatened counties compared to non-threatened counties for the keyword-based measure, but was for the classifier-based one.

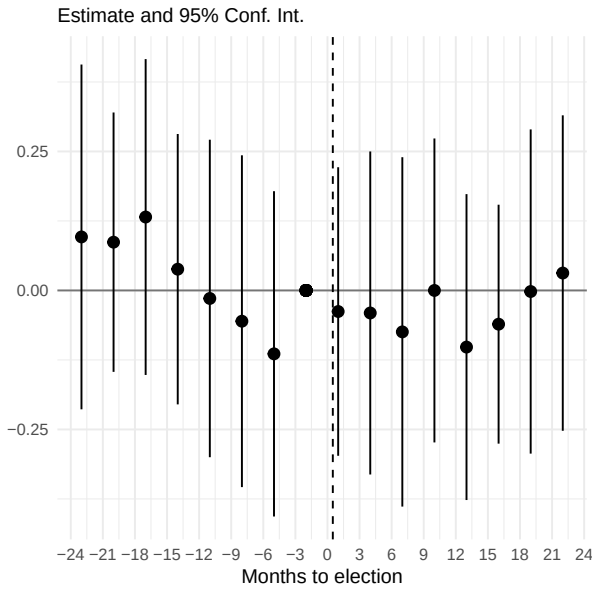
In sum, we find statistically insignificant and quantitatively small evidence that fewer anti-Black newspapers left the marketplace in threatened counties – which would inflate our baseline – counterbalanced by fewer anti-Black newspapers entering – which would attenuate our baseline – for the keyword-based measure. For the classifier-based outcome measure, we find that more anti-Black newspapers exited in threatened counties, which would attenuate our baseline, and more anti-Black newspapers entered, which would inflate the baseline. Hence, while these results suggest that some weak selection bias might exist, it is not clear whether it would inflate rather than attenuate our main estimates, and in any case, the effect sizes are small and not significant. Taken together with the balanced-panel analysis above, these findings indicate that neither the unbalanced nature of the data nor the entry and exit of newspapers drives our baseline results.



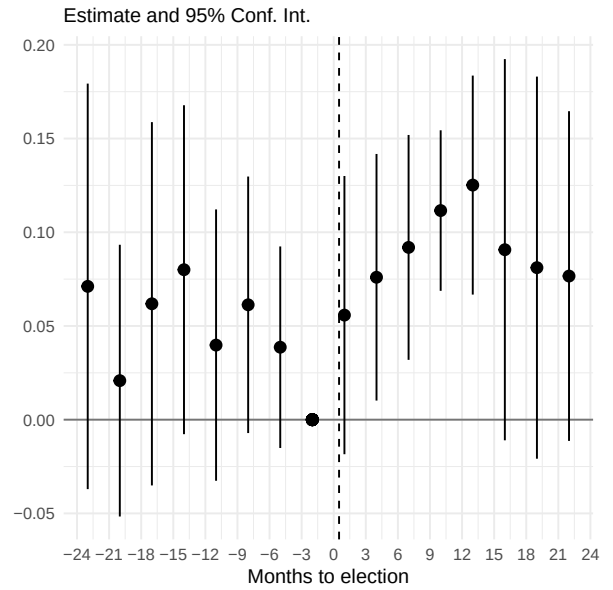
(a) Diff-in-diff: Keywords



(b) Diff-in-diff: Classifier



(c) Triple-diff: Keywords



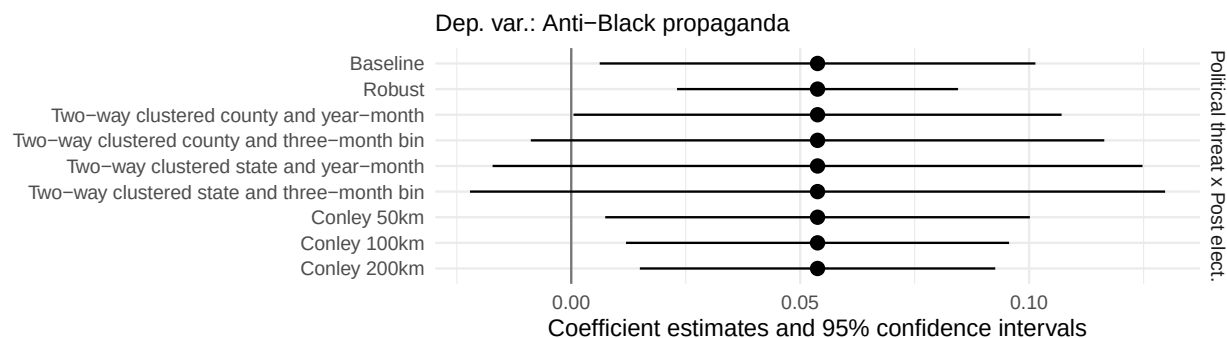
(d) Triple-diff: Classifier

Figure A19: Event-study estimates in balanced data sets

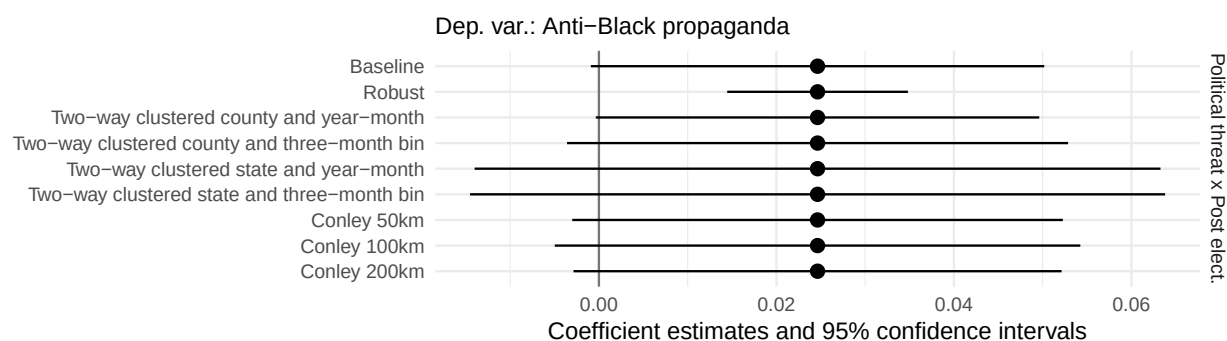
Notes: This figure reports the dynamic difference-in-differences and triple-difference estimates from equations (1) and equations (2). It displays the estimated coefficients and confidence intervals at the 95% confidence level. Panels (a) and (c) show difference-in-difference and triple-difference estimates, respectively, for the keyword-based outcome, while Panels (b) and (d) show difference-in-difference and triple-difference estimates for the classifier-based outcome. Standard errors are clustered at the county level.

A.4.4 Inference

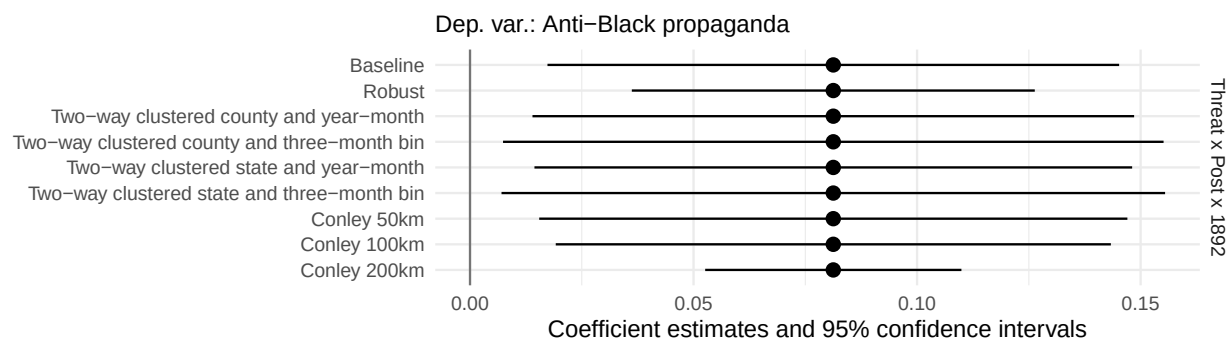
We now assess the sensitivity of our main result to alternative assumptions on standard errors. Figure [A20](#) depicts 95% confidence intervals for alternative assumptions on the standard errors. We conclude that the results are largely robust to alternative clustering choices and spatially clustered standard errors. Note that standard errors become smaller as the distance cutoff increases, which reflects the fact that the additional long-distance pairs added between 100 km and 200 km contribute little additional positive covariance on average.



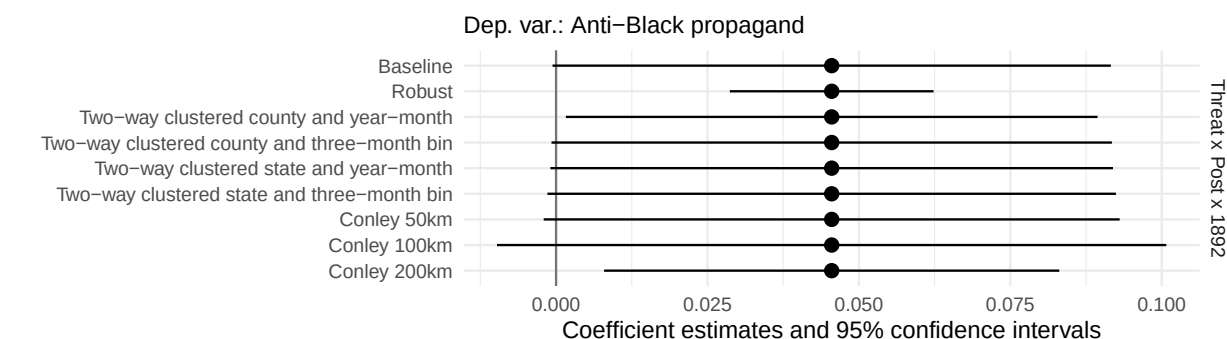
(a) Diff-in-Diff: Keywords



(b) Diff-in-Diff: Classifier



(c) Triple-diff: Keywords



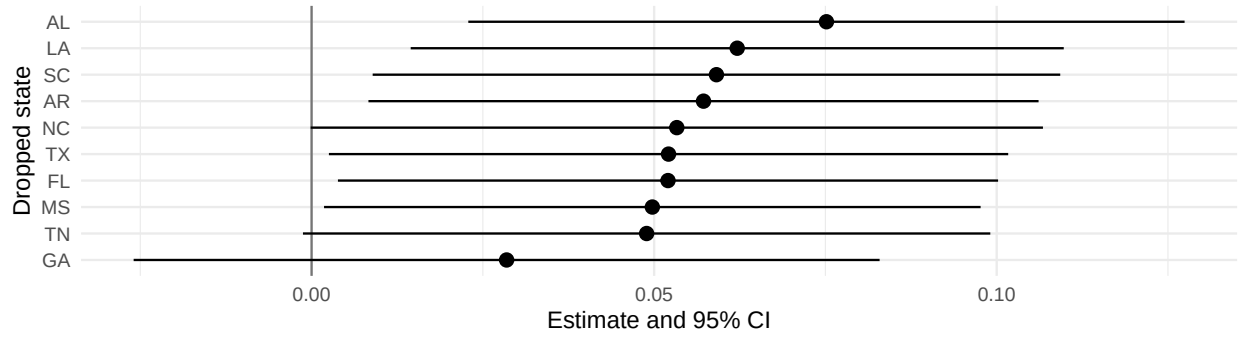
(d) Triple-diff: Classifier

Figure A20: Inference: Robustness to alternative standard errors

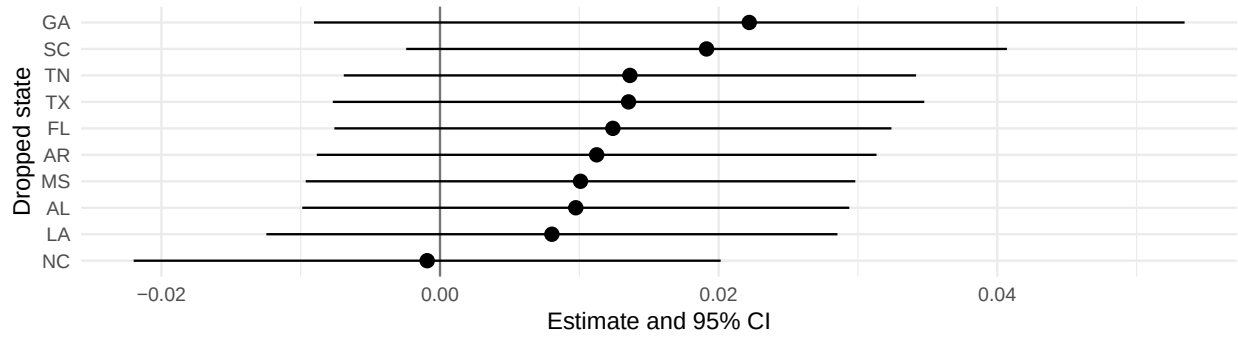
Notes: This figure shows that our static baseline differences-in-differences (Panels a and b) and triple-differences estimates (Panels c and d) are robust to alternative assumptions about the standard errors. The figure shows coefficients and 95% confidence intervals from estimating Column 4 in Table 1 under different assumptions about standard errors, as indicated in the figure.

A.4.5 Dropping one state at a time

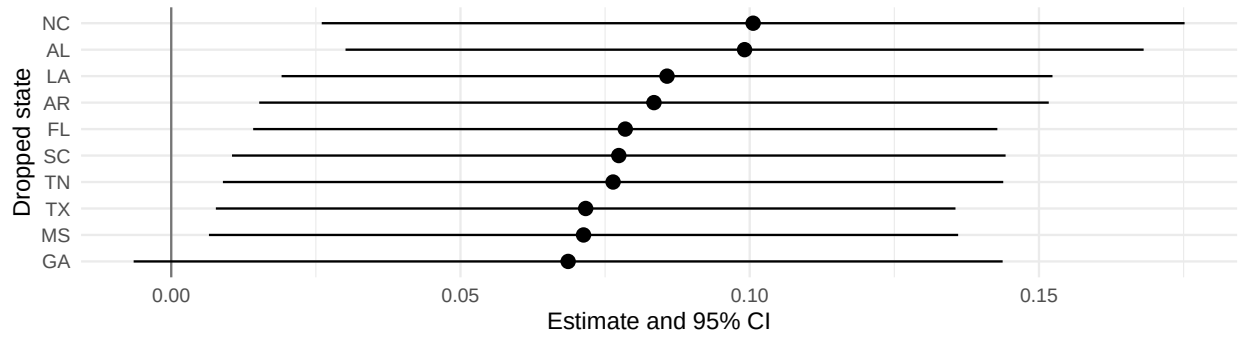
The Populists' and Republicans' success varied across states. So did the Democrats' hold on power, their control over the press, and the economic and social determinants of Populist success. In Appendix Figure [A21](#), we present estimates in which we drop one state at a time from the sample. For the keyword-based outcome, Panels (a) and (c) show that the static difference-in-differences and triple-difference estimates are positive and, in most cases, statistically significant. For the classifier-based outcome, the figure in Panel (d) reports a similar patterns for the static triple-difference estimates, whereas Panel (b) shows that the estimate decreases to zero, turns even negative, when North Carolina, where a Populist-Republican fusion government actually took office in 1894, is excluded.



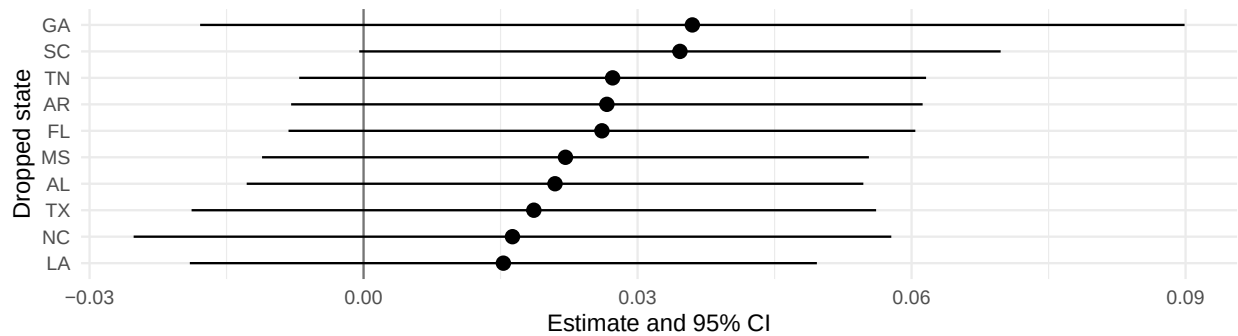
(a) Diff-in-Diff: Keywords



(b) Diff-in-Diff: Classifier



(c) Triple-diff: Keywords



(d) Triple-diff: Classifier

Figure A21: Robustness to dropping individual states

Notes: The figure shows coefficients and 95% confidence intervals from estimating Column 4 in Table 1 while dropping one state at a time. Standard errors are clustered at the county level.

A.4.6 Alternative Outcome Measure: Semantic Similarity

This section replicates our baseline results with a semantic-similarity-based measure of anti-Black propaganda. Below, we describe the construction of the measure, replicate our baseline analyses using it as outcome, and explore robustness to using high-similarity dummies rather than continuous similarity scores.

Details of the Semantic Similarity Measure We measure the semantic similarity between newspaper content and a set of reference headlines explicitly related to alleged crimes by Black individuals using the SBERT embeddings. These reference headlines were identified through manual review and coding of articles flagged by our keyword-based method. For example, on September 24, 1892—roughly one month before the presidential election—a reader of the *Russell Register* in Seale, Alabama, would have encountered the statement: “It seems to be true in every part of the country that rape means rope.” We selected this and four additional similar statements to capture this specific type of anti-Black content. All statements and their sources are listed below.

By calculating cosine similarity scores between each newspaper page—divided into segments to fit within SBERT’s context window—and our curated set of reference headlines, aggregated to the newspaper-month level, we create another measure of anti-Black content. This method provides robustness to the particular keyword combinations used in our main measure, although it is limited to newspapers for which full-text data is available. The correlation between the average cosine similarity scores and our keyword-based measure (across newspapers and months) is around 0.4; this relatively low correlation is expected given the presence of measurement error in both approaches and the fact that the cosine-similarity-based approach potentially captures a broader dimension of anti-Black content.

List of Reference Statements Used

- “It seems to be true in every part of the country that rape means rope.” (*Russell Register*, Seale (Alabama), September 24, 1892)
- “Hemp continues to grow and be grown for the rape fiend. It is quite certain that the supply of hemp will last as long as the rape fiends are to be found. It is the duty of all good men to uphold the majesty of the law. It is also the duty of all good men to protect the home and fireside and the virtue of every woman.” (*The Moulton Advertiser*, Moulton (Alabama), May 26, 1892)
- “There seems to be a regular rape epidemic, the worst on record.” (*The Montgomery Advertiser*, Montgomery (Alabama), July 22, 1897)

- “We have absolutely no patience for the fiend who commits rape, and have no apology to make.” (*The Asheville Weekly Citizen*, Asheville (North Carolina), September 17, 1891)
- “We would yet be compelled to drive the negro out of this country or drive him into the ground.” (*The Goldsboro Headlight*, Goldsboro (North Carolina), September 2, 1897)

Replication of Core Results Figure A22 replicates our event-study difference-in-differences (Panel a) and triple-differences (Panel b) specifications using the semantic-similarity-based measure as the outcome. Both specifications include our baseline set of controls, except for the control for the number of pages.

Comparing these patterns with the baseline ones in Figure 3 reveals that, overall, the dynamics are very similar for this alternative outcome measure: there is no evidence of any pre-trend before the 1892 election, and anti-Black propaganda increases in threatened counties after that election.

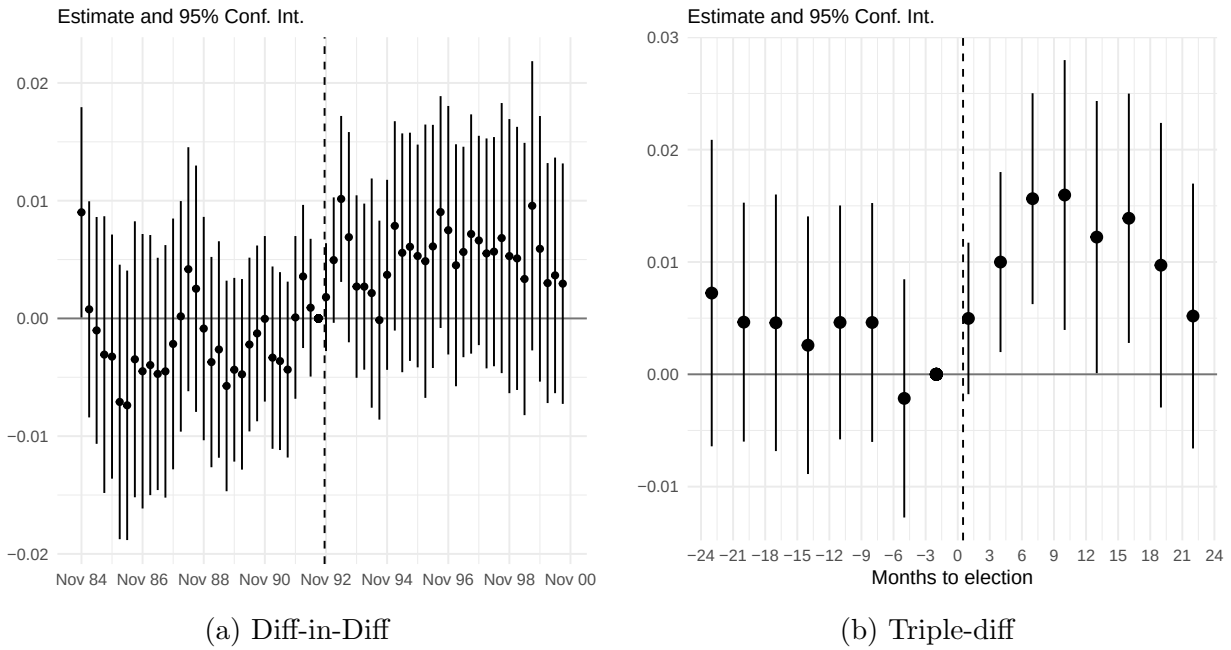
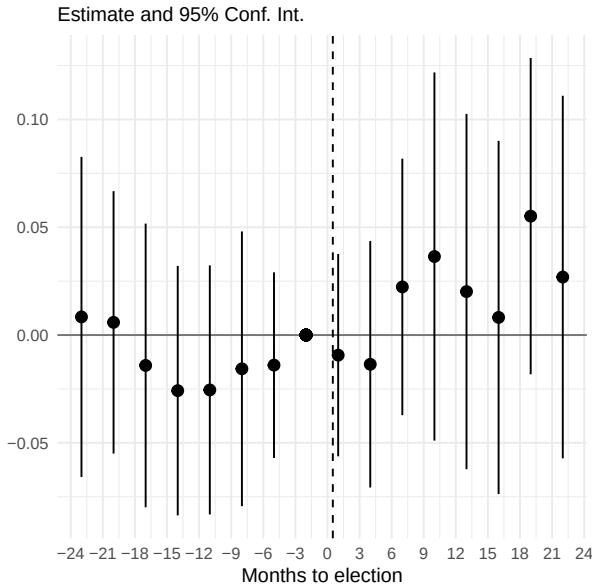


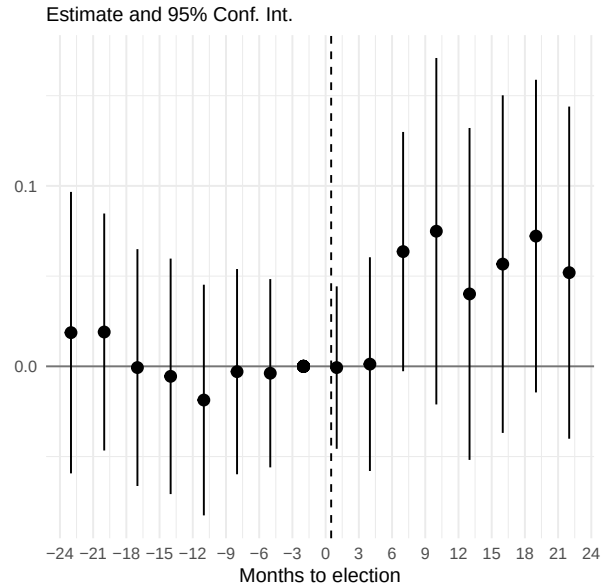
Figure A22: Event-study estimates with semantic similarity as the outcome

Notes: This figure shows the dynamic difference-in-differences and triple-difference estimates of the effect of political threat on anti-Black propaganda based on Equations (1) and (2). It displays the estimated coefficients and confidence intervals at the 95% confidence level. Panel (a) shows difference-in-difference estimates, and Panel (b) shows triple-difference estimates. Both panels use the average semantic similarity of each newspaper-month as the outcome variable and include the same baseline controls as for the classifier-based measure, except that we omit the control for the number of pages, which is not required here. Standard errors are clustered at the county level.

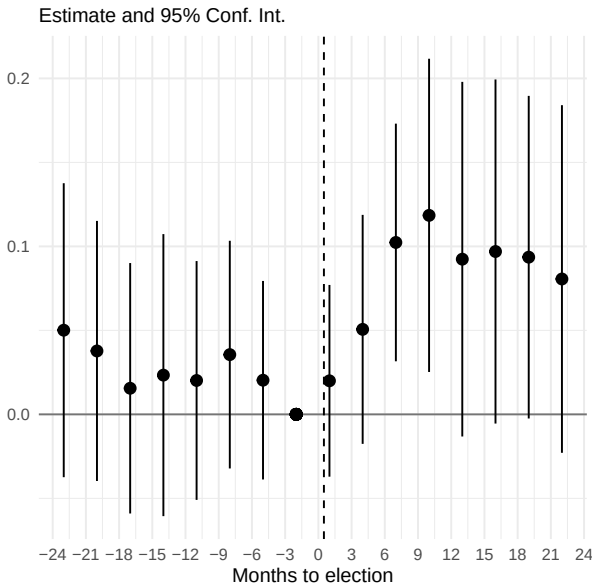
Robustness: Dummies for High Similarity as Outcome Finally, we document that our core results are robust to using binary indicators for high similarity based on the upper tail of the page-level similarity distribution as outcome: a dummy for pages above the 90th, 85th, 80th and 75th percentile of the similarity score, respectively. Appendix Figure [A23](#) presents the corresponding triple-difference event-study estimates using these binary outcomes. Across all cutoffs, the results are very similar to those obtained with the continuous similarity score: the estimated treatment effects remain positive and of comparable magnitude, and the timing of the effects mirrors our main specification (no pre-trends and a build-up after 1892).



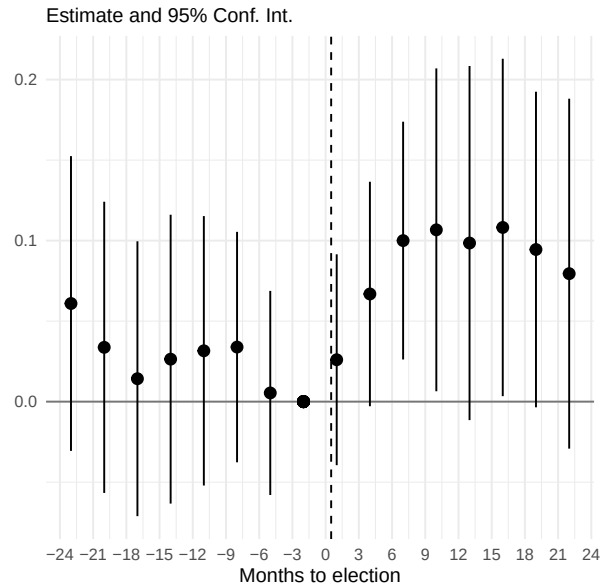
(a) 90% percentile



(b) 85% percentile



(c) 80% percentile



(d) 75% percentile

Figure A23: Triple-difference results using high-similarity indicators as dependent variables

Notes: The figures show the results of Equation (2) with high-cosine similarity indicators as dependent variables. The indicators are defined using various cutoffs of the semantic similarity score as indicated in the panel headers. All specifications include the same baseline controls as for the classifier-based measure, except that we omit the control for the number of pages, which is not required here. Standard errors are clustered at the county level.

A.5 Additional Analyses

A.5.1 Validation of the Political Threat Measure

In this section, we validate our political threat measure using contemporaneous newspaper content. Specifically, we show that, after the 1892 election, newspapers in threatened counties exhibit stronger increases in coverage of “fusion” (referring to Populist-Republican fusion tickets) and “opposition” and in the co-occurrence of these terms with threat-related language than newspapers in non-threatened counties. We also show that similar patterns do not arise around the 1888 election.

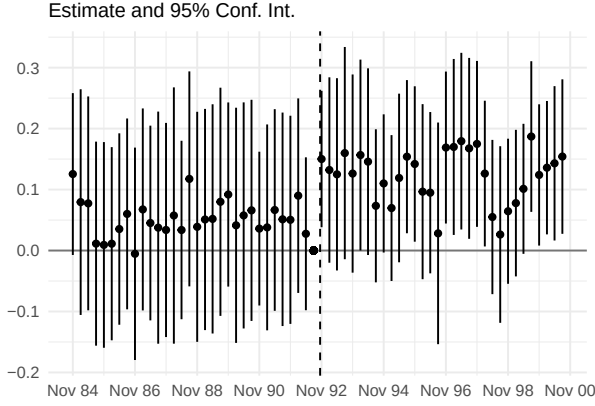
The six panels of Figure A24 report difference-in-differences and triple-difference event-study estimates based on equations (3) and (4). Panels (a)–(b) and (c)–(d) use the number of pages mentioning “fusion” and “opposition”, respectively, as the outcome. In all four panels, we find that, relative to non-threatened counties, threatened counties display a sharp and persistent increase in these political keywords after the 1892 election, but not around the 1888 election. Panels (e) and (f) move beyond raw keyword counts and examine the co-occurrence of “fusion” or “opposition” with any term from the threat dictionary compiled by Choi et al. (2022). These specifications also show pronounced post-1892 increases in threatened counties relative to non-threatened counties; here we rely solely on *newspaperarchive.com* due to the high number of dictionary terms.

Taken together, this evidence suggests that, in the immediate aftermath of the 1892 election, newspapers in counties flagged as “threatened” by our indicator were indeed more likely to discuss fusion, opposition, and political threat. This pattern is exactly what we would expect if our threat variable captures local perceptions of electoral vulnerability rather than being a purely mechanical function of vote shares.

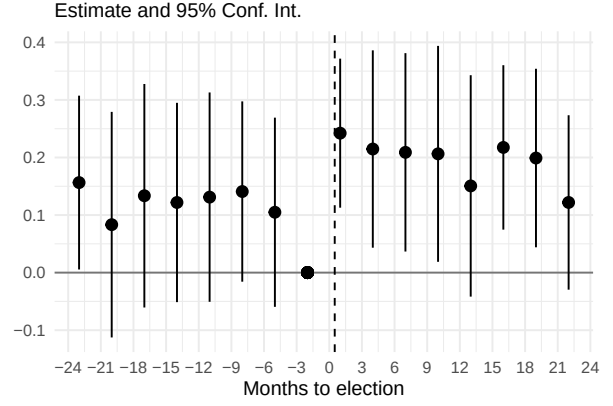
A.5.2 Baseline Analysis at Congressional District Level

In this section, we replicate our baseline analysis with political threat defined at the congressional district level. We assign newspapers to districts of the 53rd Congress using the shapefiles provided by Lewis et al. (2013). We then aggregate county-level election results to congressional districts following the methodology described in Ferrara et al. (2024) and replicate the newspaper-level equations (1) and (2) with this congressional-district-level threat indicator.

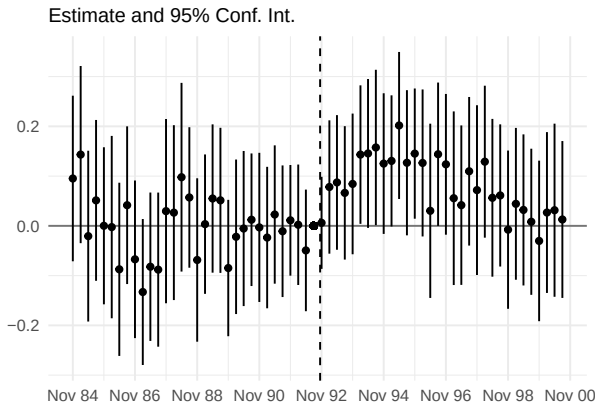
Figure A25 shows the results for our keyword and classifier-based outcomes in Panels (a)–(c) and (b)–(d) respectively. Panels (a) and (b) present the event-study estimates from our difference-in-differences specification, and Panels (c) and (d) from the triple-differences specification. Comparing these to the baseline event-study estimates in Figure 3, we find



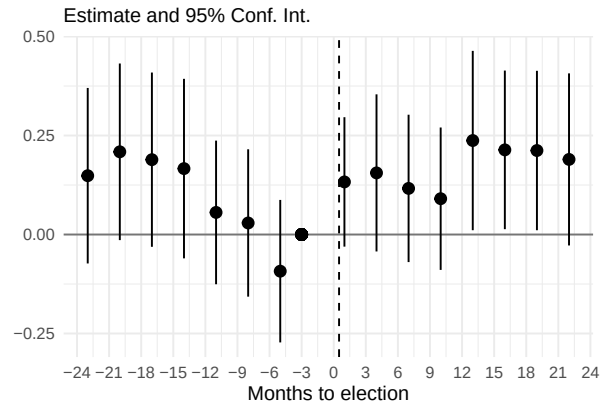
(a) Diff-in-diff: “fusion”



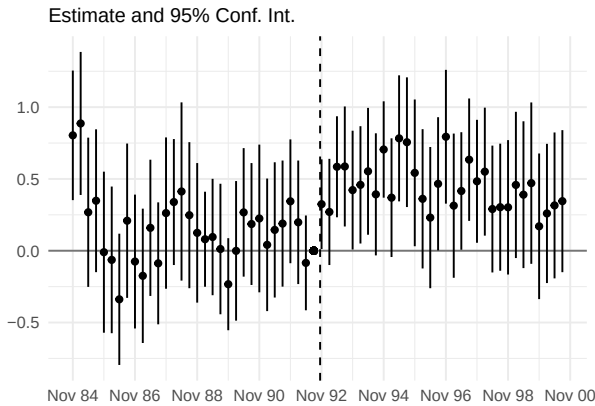
(b) Triple-diff: “fusion”



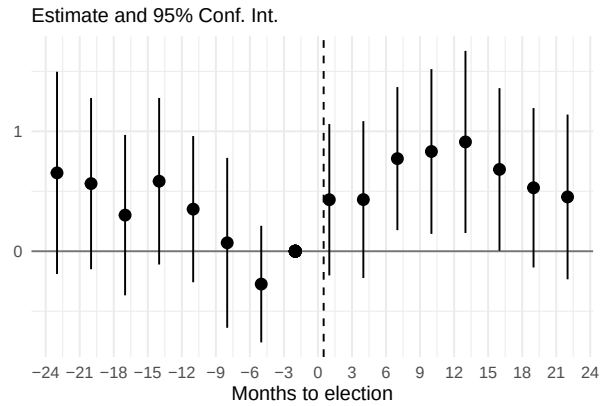
(c) Diff-in-diff: “opposition”



(d) Triple-diff: “opposition”



(e) Diff-in-diff: threat dictionary



(f) Triple-diff: threat dictionary

Figure A24: Validation of the political threat measure with newspaper content

Notes: This figure reports event-study estimates from equations (1) and (2). In panels (a) and (b), the outcome is the number of pages mentioning “fusion”; in panels (c) and (d), mentions of “opposition”; in panels (e) and (f), co-occurrences of “fusion” or “opposition” with any keyword from Choi et al. (2022)’s threat dictionary. All panels include our baseline controls (log page count and further period FE interacted with log population, log population density, urban share above 25k).

that the results are very similar. We again find no evidence of pre-trends, and we observe a pronounced increase in anti-Black propaganda in the run-up to the 1894 midterm and 1896 presidential elections. The baseline coefficients at the congressional district level are highly comparable in magnitude to those from the county-level analysis.

A.5.3 No Systematic Measurement Error in Keyword-based Outcome

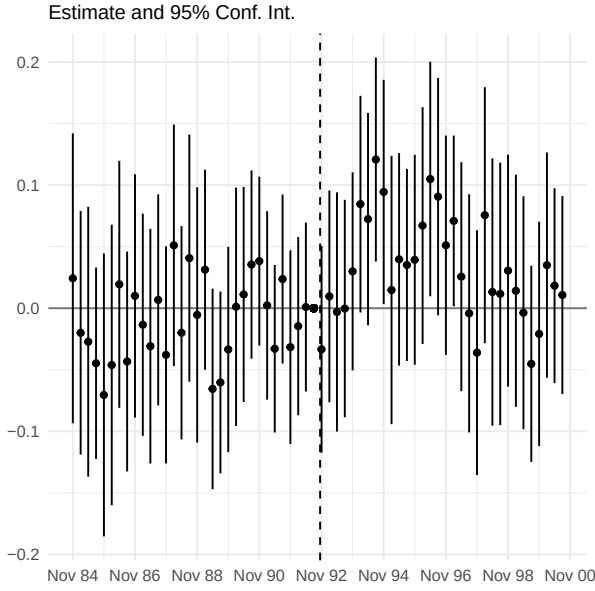
In this Appendix, we document that measurement error in the keyword-based outcome variable does not vary systematically in a way that would threaten our main conclusions.

We build our analysis upon the subset of annotated newspaper pages from Appendix A.2.1 that were located in counties in the South and published between 1890 and 1894. We add meta-information on the year and state of publication, as well as political affiliation of newspapers, and our treatment to the around 800 manually reviewed pages. We define a dummy variable, $1(\text{correct})$, which indicates that a keyword search correctly identifies a page containing an article that mentions and links both keywords (true positive), based on our manual assessment of each page. Note that we have within newspaper variation over time in this measure, since we have manually verified content for pages on 178 newspapers in total.

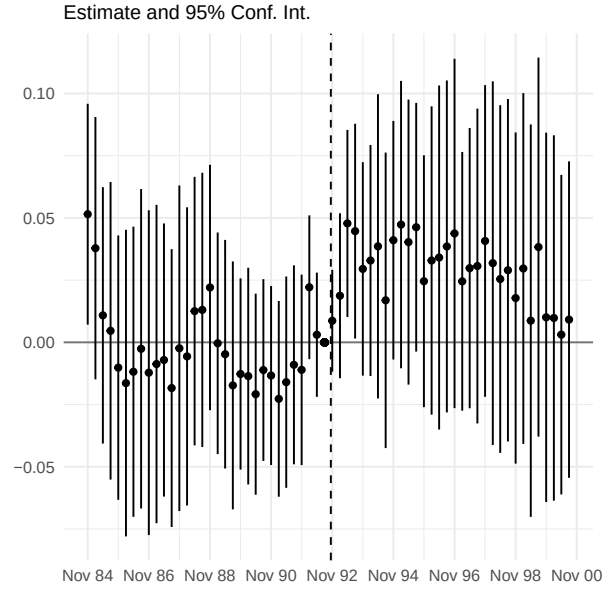
Figure A26 below shows histograms of detection rates by periods and newspapers in panels (a) and (b). For each period (left panel) and newspaper (right), we calculate the share of pages that actually contain propaganda, based on our manual verification. Unsurprisingly, there is plenty of variation along both margins. Panel (c) further shows the average detection rate by year and threat status. Except for a slightly elevated rate in 1893, differences are negligible. Note that the slightly higher detection rate in 1893 cannot account for our baseline results, since our baseline result is driven by higher anti-Black propaganda in 1894, in the run-up to the congressional election, and not in 1893. In any case, detection rates are similarly elevated in both threatened and non-threatened counties.

Panel (d) of Figure A26 shows average detection rates by treatment status (threatened vs. non-threatened counties) before and after the 1892 presidential election. The difference between threatened and non-threatened counties was larger before the election, at 14.7% vs. 19.3%.

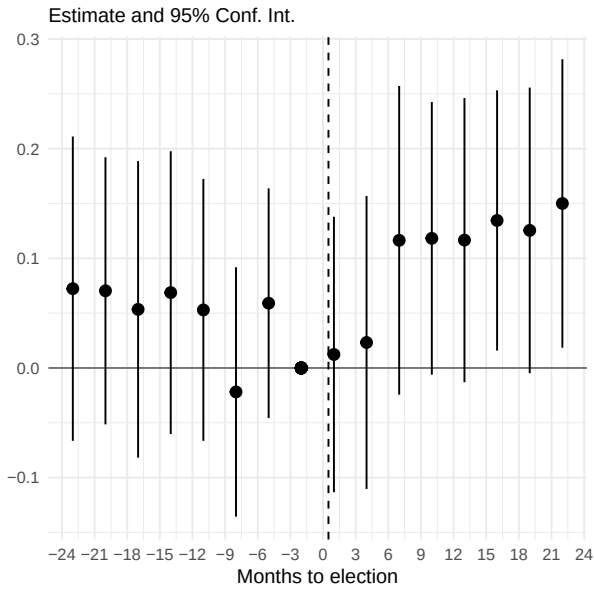
To undermine our baseline result, detection of anti-Black media content would have to be systematically higher in newspapers in threatened counties after the presidential election of 1892. Table A7 below shows that this is not the case. Column 1 documents that while we are 8% more likely to successfully identify anti-Black media content after the 1892 election, we do not have higher detection rates in counties that were treated with political threat in 1892. Column 2 shows that the latter statements hold when including period fixed effects. Column 3 is our main test of whether differential detection rates pose a threat to the baseline



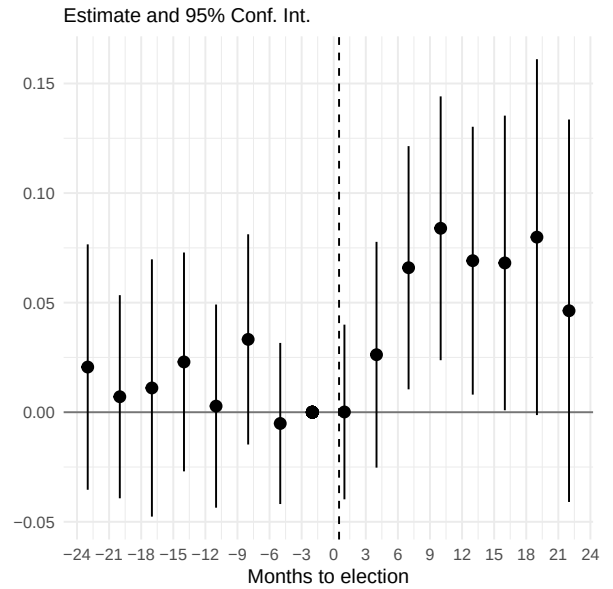
(a) Diff-in-Diff: Keywords



(b) Diff-in-Diff: Classifier



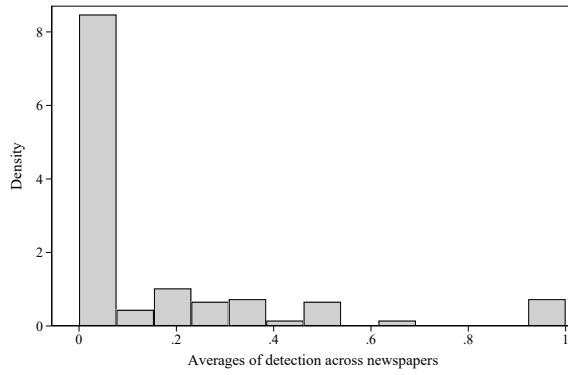
(c) Triple-Diff: Keywords



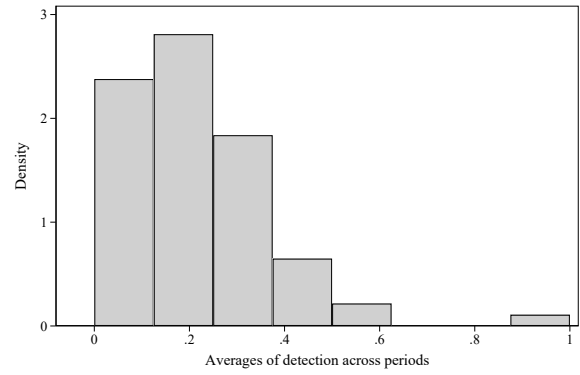
(d) Triple-Diff: Classifier

Figure A25: Replicating the baseline analysis at the congressional district level

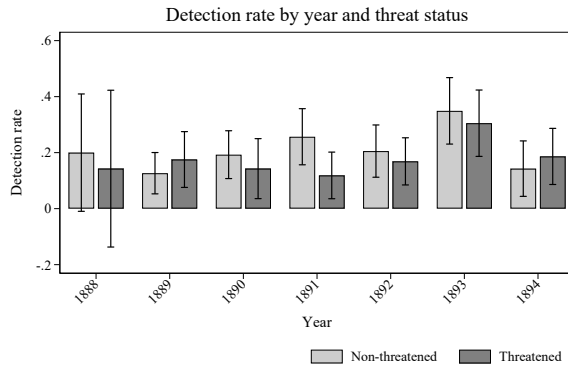
Notes: This figure reports estimates from equations (1) and (2) using a political-threat indicator defined at the congressional district level. Details on how we link newspapers to congressional districts are provided in the text.



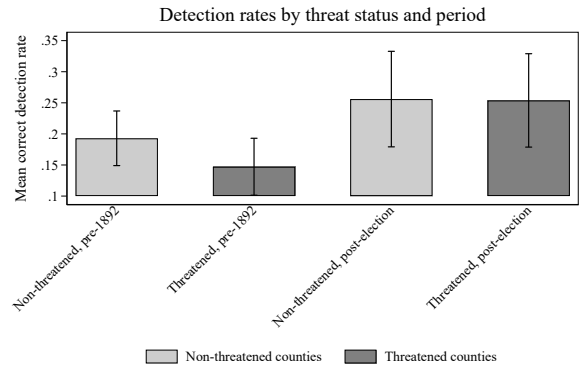
(a) Detection rate across periods



(b) Detection rate across newspapers



(c) Detection rate by year and threat status



(d) Detection rate by period and threat status

Figure A26: Detection rates of true positives of keyword-based anti-Black propaganda

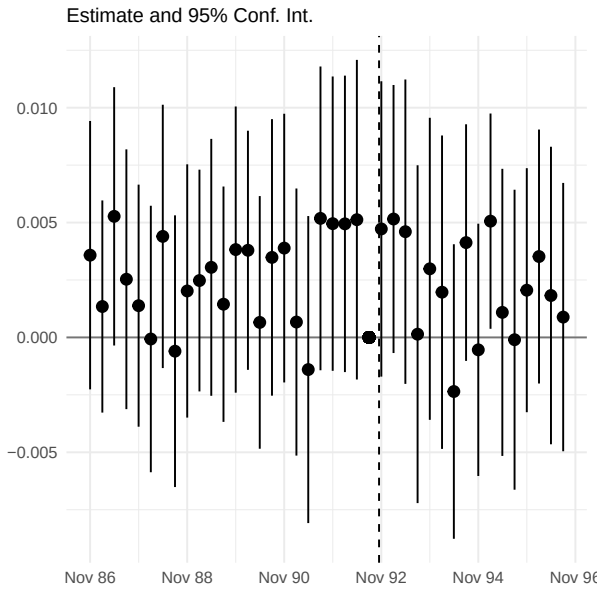
Notes: In the first two panels, the figure show averages across periods (month-year; panel a) and newspapers (panel b) of a dummy variable, 1(correct), equal to one if a keyword search correctly identifies a page containing an article that mentions and links both keywords (true positive). Panel (c) and (d) show detection rates by the threat status of a county and years (panel c) and pre- vs. post-period (panel d). The figures are based on the manually validated sample of pages for which our keyword search indicated anti-Black content, described in more detail in Appendix A.2.1 That sample contains 797 individual pages, published in 178 newspapers and over 74 months in the period from 1888 to 1894 in Southern counties.

result. Here, we test if detection of anti-Black media content was systematically higher in newspapers in threatened counties after the presidential election of 1892. The estimated coefficient suggests that successful detection was 0.65% higher in threatened counties after the 1892 election. This is a very small difference, which is also far from significant. Note, however, that the 95% confidence intervals on the interaction coefficient are rather large (ranging from -0.18 to 0.20), so that we cannot definitely rule out even sizable differences in detection rates after the 1892 election in threatened counties. Column 4 shows that measurement error does not vary systematically or significantly with newspaper political affiliation in general. Finally, Column 5 explores whether detection rates vary based on treatment and newspapers’ political affiliation. To this end, we fully interact with a dummy for whether a newspaper was affiliated with the Democratic Party. While detection rates are higher in Democrat-affiliated newspapers after the 1892 election (by 7.1%), this is not the case in threatened counties in particular, where detection rates are lower (by -8.6%). While confidence intervals are large, the point estimate suggests that our core heterogeneity is also not driven by differential detection rates. In sum, while statistical power in this manually validated subset is limited, point estimates suggest no meaningful differences in detection rates by treatment. In any case, our classifier- and semantic-similarity-based approaches provide additional reassurance, as they are less dependent on the correct identification of individual keywords.

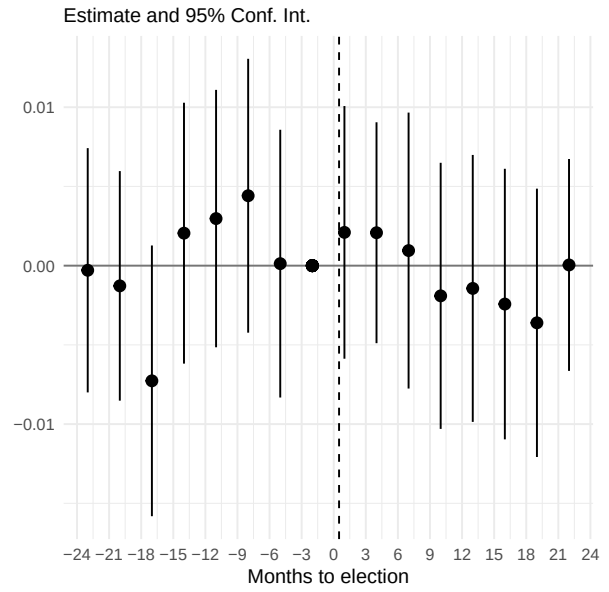
A.5.4 Lynchings

In this section, we present our findings on how the perceived threat from the Populist Party influenced both the actual incidence of lynchings of Black people and the reporting of such events in newspapers. Our methodology involves a dynamic triple-difference framework, focusing on two key outcome variables: actual lynchings and newspaper reports on lynchings. For the former, we measure the number of lynchings of Black individuals in each county-month, using the data sources detailed in Appendix Section A.1. For the latter, we calculate the share of pages that mention the words “lynch” or “lynching” in conjunction with “negro” or “colored.”

Appendix Figure A27 reports the dynamic difference-in-difference and triple-difference estimates for both outcomes. We do not observe an effect on the number of lynchings or on reports of lynchings in newspapers. This suggests two possible interpretations: the infrequency of lynchings means that there is not enough data to detect an effect, or it could imply that despite the increase in anti-Black propaganda, which included reports of lynchings in other locations, such content did not lead to lynchings.



(a) Diff-in-diff



(b) Triple-diff

Figure A27: Event-study estimates of the effect on lynchings

Notes: This figure shows that we fail to detect an effect of political threat on lynching after the 1892 election. Panel (a) and (b) report estimates based on equations (1) and (2), with two adjustments: First, the dependent variable is the logarithm of one plus the number of lynchings of Black people in a county in a month. Second, we omit the newspaper-level controls. Standard errors are clustered at the county level.

Table A7: Measurement error does not vary with treatment or newspaper affiliation

	(1)	(2)	(3)	(4)	(5)
Political Threat	-0.033 (0.028)	-0.029 (0.027)			
Post 1892 Election	0.081** (0.037)				
Political Threat $\times$ Post 1892 Election			0.006 (0.097)		0.079 (0.197)
Dem. newspaper				0.009 (0.049)	
Dem. newspaper $\times$ Political Threat $\times$ Post 1892 Election					-0.086 (0.183)
Dem. newspaper $\times$ Post 1892 Election					0.071 (0.105)
Observations	796	795	718	795	718
R ²	0.010	0.098	0.244	0.097	0.244
Period FE		✓	✓	✓	✓
Newspaper FE			✓		✓

Notes: The main dependent variable is a dummy of whether a newspaper page, identified by our keyword approach, actually contains anti-Black articles. The core independent variables are (i) an indicator of whether the newspaper was in a threatened county (defined as any county in which the Democrats scored below their 1892 median vote share in the South of 60%) and (ii) a dummy for periods after the 1892 election. Column 3 interacts the two variables, resembling our difference-in-differences specification. Column 4 shows that measurement error does not vary systematically or significantly with newspaper political affiliation in general. Column 5 adds another interaction with a dummy indicating whether a newspaper was affiliated with the Democratic Party. All columns after the first include period fixed effects, and columns 3 and 5 further include newspaper fixed effects. Robust standard errors clustered at the county level are displayed in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.