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The Birth of a Nation: Media and Racial Hate

Faculty Research Working Paper Series

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November 2020 (Updated July 2022)

RWP20-038

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The Birth of a Nation: Media and Racial Hate

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July 22, 2022

Abstract

This paper documents the impact of popular media on racial hate by examining the first American blockbuster: 1915's *The Birth of a Nation*, a fictional portrayal of the KKK's founding rife with racist stereotypes. Exploiting the film's five-year "roadshow", I find a sharp spike in lynchings and race riots coinciding with its arrival in a county. Instrumenting for roadshow destinations using the location of theaters prior to the movie's release, I show that the film significantly increased local Klan support in the 1920s. Roadshow counties continue to experience higher rates of hate crimes and hate groups a century later.

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*“I had no idea that *The Birth of a Nation* might be used to revive the old Klan... A terrific power lies in the motion picture. It’s a power that is only too leanly recognized in these days. I’m constantly amazed and sometimes almost terrified by it.”*

— *D.W. Griffith (director of *The Birth of a Nation*)*

I Introduction

In recent years, the rising prevalence of hate crimes, mass shootings and far-right extremism has led to significant public debate about the role of entertainment media. While discussing the 2018 Parkland school shooting, former President Trump lamented how “the video games, the movies, the internet stuff is so violent...it’s hard to believe that doesn’t have a negative impact on [a child’s] thought process.” Similarly, fears that antiquated and racist stereotypes of African-Americans have become “commonplace in American society” led WarnerMedia to temporarily remove the 1939 film *Gone with the Wind* from HBO Max, a decision it argued was necessary “to create a more just, equitable and inclusive future.”

Nonetheless, popular media’s impact on racial animus is theoretically and empirically ambiguous. On the one hand, correlations between media consumption and racial prejudice may simply reflect self-selection among individuals predisposed to those beliefs. In line with this, Dahl and DellaVigna (2009) examine modern violent film releases and find a reduction in same-day assaults due to the “voluntary incapacitation” of would-be offenders. On the other hand, persuasion effects have been documented across a number of different settings, from slanted news (DellaVigna and Kaplan, 2007) to government propaganda (Adena et al., 2015) and educational media (La Ferrara, 2016). However, with the exceptions of work by Yanagizawa-Drott (2014) examining government-sponsored radio broadcasts during the Rwandan genocide and recent studies on the spread of social media (Burszтын et al., 2019; Müller and Schwarz, 2020, 2021), little evidence exists of media’s ability to incite extreme behaviors like racial violence. This ambiguity is heightened when considering popular *entertainment* media – where any impacts are likely to be unintended by-products of audience demand for amusement (DellaVigna and La Ferrara, 2015).

A more fundamental issue also remains. Studies examining modern settings are often only able to identify short-run effects on audiences who have already experienced significant exposure to racist and violent content. In 1993, the American Psychological Association estimated that the average American witnessed nearly 200,000 violent acts on television by the time they turned 18. This number has likely only increased with the exponential growth of the internet and social media. Given evidence of desensitization to repeated media stimuli (Krahé et al., 2011), it becomes increasingly difficult to understand media’s aggregate

impact on human behavior and society from studying marginal exposures in modern contexts. This concern is further compounded when considering the most culturally-significant and widely-viewed media events, as increasingly saturated markets mean there are few unexposed individuals from which to construct the counterfactual.

However, history provides a unique opportunity not only to trace out the short- and long-run effects of entertainment media on racial hate but to do so at the advent of the American blockbuster: D.W. Griffith's *The Birth of a Nation* (BON). A revisionist account of the Civil War, Reconstruction and the founding the Ku Klux Klan, the 1915 film was a cinematic spectacle the likes of which the world had never seen. The movie's production, marketing, and popularity "would bring about a revolution in American moviegoing" (Stokes, 2007) and had far more in common with modern tentpoles than the short plotless films of its era. An estimated 10 million Americans – roughly one-fifth of the adult white population – turned out to see the movie in its first two years, paying as much as \$2 a ticket, forty times the going rate. In addition to being the first film ever screened in the White House, *The Birth of a Nation* remains one of the highest-grossing movies of all time.

[Figure I about here.]

As shown in Figure I, the movie's release also marked the beginning of a period of widespread racial violence and unrest across the nation, including a spate of lynchings and race riots. This period also saw the rebirth of the Ku Klux Klan, which had disbanded in the late 19th century only to be revived six months after *The Birth of a Nation*'s premiere. The Second KKK would grow to include over 4 million members by 1924. While historians have long credited the film's racist depictions of African-Americans and overt glorification of the KKK with increased racial tensions and the resurgence of the Klan (Simcovitch, 1972), no empirical evidence corroborating these claims exist.

However, the movie's staggered and incomplete distribution provide an ideal setting for hypothesis testing. Unlike modern films, which are released simultaneously in theaters or streaming platforms nationwide, *The Birth of a Nation* premiered at a time when the movie theater industry was still in its infancy. Fewer than 60% of counties had a theater in 1915 and many of those were too small to house the elaborate projection equipment and orchestra needed to screen the three-hour epic. As a result, many people who wanted to watch *The Birth of a Nation* had no means of doing so. Even those who lived close to a theater often had to wait months or years before seeing the film. This was because the movie was disseminated via an extended roadshow lasting nearly five years, during which time distributors traveled from town to town for limited (and generally sold-out) theatrical runs. Combined with nationwide demand for the movie and a dearth of contemporaneous substitutes, this setting

allows me to cleanly identify the film’s impact while avoiding many of the concerns about substitution effects, mass dissemination, and selective exposure that complicate research in modern media-saturated environments (La Ferrara, 2016).

To estimate the film’s short-run effects on local acts of racial violence, I leverage novel data on the date and location of film screenings drawn from historical newspapers and trade magazines. Using an event study design, I find that *The Birth of a Nation* triggered sharp spikes in racial violence. Counties were four times more likely to experience a lynching in the month after the movie’s arrival. A similar surge is observed for race riots. These effects are corroborated by state-level data on homicide rates, which reveal significant increases in murders of minorities – but not whites – in screening areas. Results are also highly robust to alternative specifications accounting for potential concerns about selection and differential time trends. Randomization inference and a placebo test examining the popular 1918 comedy *Mickey* further confirm *The Birth of a Nation*’s violent aftermath.

To explore longer-run effects, I instrument for whether a county screened BON based on whether it had a theater in 1914, the year before the film’s release. Several pieces of corroborating evidence support the validity of this instrument. First, I show that theater presence in 1914 is orthogonal to past measures of racial prejudice as well as future demographic changes. Second, I show that for counties in Kansas, where *The Birth of a Nation* was banned by the governor, historical theater stock is uncorrelated with future Klan presence. Third, I show that only theaters constructed before the end of BON’s roadshow – and not after it – predict future Klan chapters.

The IV estimates reveal *The Birth of a Nation*’s pivotal role in fueling the revival of the Ku Klux Klan. I find that, at the margin, screening the film increased a county’s likelihood of having a chapter of the Second KKK (“Klavern”) by over 60 percentage points. These results are robust to a host of alternative instruments (ex: using other measures of theater stock and screenings of another movie) and treatment measures (ex: based on alternative data on BON screenings from Esposito et al.) as well as to adjustments for potential selection bias (ex: accounting for geographic differences in historical newspaper availability and employing propensity score matching) and spatial correlation. The film’s impact on the Klan’s growth are also reflected in intensive measures of support, such as Klaverns per capita and state-level membership counts. Consistent with the widespread appeal of both the film and the KKK during this era (Fryer and Levitt, 2012), I find significant impacts on Klavern formation across a diverse range of areas – not simply those that were the most historically racist.

While data constraints preclude a comprehensive interrogation of mechanisms, evidence suggests multiple channels at work. First and most clearly, the role of media imitation is directly reflected in the Second KKK’s adoption of white robes and cross-burning. Neither

practice was actually employed by the First KKK, though both featured prominently in the fictionalized version depicted in *The Birth of a Nation*. Full-count Census data similarly reveal imitation in naming conventions, with white parents in roadshow counties becoming more likely to name their boys after the film’s protagonist. Second, the observed effects on lynchings and Klavern formation – both of which involve large groups of perpetrators – point to potential coordination effects, with local screenings propagating knowledge of the Klan or facilitating coordination among interested individuals (Bursztyn et al., 2019). Finally, while I find little change in contemporaneous voting patterns, more nuanced measures of racial preferences from later decades suggest that the film increased local anti-Black prejudice and Klan approval. These results are consistent with persuasion effects as well as historical accounts of the great emotional sway the film held over its audiences.

The Birth of a Nation’s influence on the landscape of American racial hate can still be seen a century later. Roadshow counties were not only significantly more likely to contain a Klavern of the Third KKK in the 1960s, but are also more likely to contain chapters of the KKK and *other* white supremacist groups in the 2000s. These areas continue to experience higher rates of hate crimes today. Notably, increases in modern hate crimes are driven both by anti-Black incidents and by incidents directed towards other racial, religious and sexual minorities. That these spillovers extend beyond the film’s narrower vilification of African-Americans speaks to the enduring power of the white supremacist organization it unleashed. Indeed, mediation analysis based on Dippel et al. (2020) suggests that the long-run effects are almost entirely explained by the film’s historical galvanization of the Second KKK.

These findings make several contributions. Most directly, I document the lasting legacy of “quite possibly the single most important film of all time” (Everson, 1979). A cinematic precursor to the blockbusters that would come to dominate the movie industry, *The Birth of a Nation* has alternately been credited with the rise of Hollywood, the advent of modern film-making, and the resurgence of the KKK. Together with work by Esposito et al. (2021) – who analyze newspaper text to demonstrate how the movie’s rhetoric came to permeate public discourse around patriotism and North-South reconciliation – this paper provides the first causal evidence of BON’s cultural imprint.¹ I show that the film not only incited racial violence in its immediate wake but also spawned the rebirth of “the most prominent hate-based organization in American history” (Fryer and Levitt, 2012). The historical significance of these effects can hardly be understated. My estimates suggest that film’s roadshow was responsible for the induction of over one million white men into the Second KKK and con-

¹The two papers were conceived and developed independently. However, to further validate the findings of each, the research teams have since coordinated to share their respective BON screenings data. As I will demonstrate, my results are robust to multiple different combinations of the treatment measures.

tinued to shape the geography of racial hate and violence into the 21st century. Even today, the Klan and its regalia remain totems of white supremacist ideology, perpetual reflections of D.W. Griffith’s celluloid vision.

Second, this paper adds to a large literature in media economics. One broad set of studies focuses on entertainment and “edutainment.” Examining a variety of television and movie media, researchers have shown that an important by-product of audience demand for entertainment are normatively positive changes in attitudes and behaviors that align with Western values (Jensen and Oster, 2009; Chong and Ferrara, 2009; La Ferrara et al., 2012; Kearney and Levine, 2015; Bursztyn and Cantoni, 2015) or development goals (Banerjee et al., 2015; Ravallion et al., 2015). A separate set of studies examining political media has documented the persuasive effects of radio propaganda and slanted news on racist, nationalist, and xenophobic sentiments (Enikolopov et al., 2011; DellaVigna et al., 2014; Blouin and Mukand, 2019).² This paper bridges and extends those two bodies of work by demonstrating the socially-harmful spillovers of entertainment media. While millions of Americans sought escape in *The Birth of a Nation*’s dramatic scenes, many came away from the experience having internalized its dream of an Aryan empire, ultimately manifesting a carbon-copy of its Klan into reality. Though public concern has often centered on the increasingly blurry line between real and fake news, these findings speak to the destructive and distortionary consequences of even purely fictional forms of entertainment.

Third, this paper contributes to work in economic history exploring the deep roots of cultural values and attitudes. Existing studies document how historical features like agricultural practices (Alesina et al., 2013), plague-era pogroms (Voigtländer and Voth, 2012), and frontier geography (Bazzi et al., 2017) predict gender norms, anti-Semitism and “rugged individualism” centuries later. In contrast to those studies, which focus more on the long-run persistence of cultural values than on their causal origins, this paper leverages a plausibly exogenous historical shock to explore the latter. This allows me not only to estimate the causal impacts of the first American blockbuster on contemporaneous measures of racial hate but also to trace those effects into the present-day. In doing so, my findings provide new insight into the catalyzing forces behind the nation’s oldest and most infamous hate group, and corroborate the role of social institutions in transmitting racial prejudice and conflict across generations (Jha, 2013; Satyanath et al., 2017).

The remainder of this paper is organized as follows. Section II provides background information about *The Birth of a Nation* and the Ku Klux Klan, Section III describes the

²As one recent example, Wang (2020) finds that local availability of Father Charles Coughlin’s populist radio broadcasts in the 1930s significantly increased the presence of pro-Nazi groups. In another, Durante et al. (2019) examine the roll-out of Berlusconi’s media network in Italy on support for his right-wing party.

data, Section IV presents the event study model and contemporaneous effects on racial violence, Section V describes the instrumental variables strategy and results on historical Klan support, Section VI discusses mechanisms, Section VII examines long-run effects on hate crimes and hate groups, and Section VIII concludes. The Online Appendix contains additional figures and tables noted in the text.

II Background

A *The Birth of a Nation*

Adapted from the 1905 novel “The Clansman”, *The Birth of a Nation* premiered at a time when the film industry was dominated by short, slapstick comedies that played in small nickelodeons – converted storefronts that catered to working-class audiences with five cent tickets. In stark contrast to other pictures of the era, *The Birth of a Nation* was over three hours long and boasted high production values, modern editing techniques, and violent action scenes. These features combined to tell a “red-blooded tale of true American spirit” following two fictional white families – the Northern Stonemans and the Southern Camerons – throughout the Civil War and Reconstruction. After the deaths of several Stoneman and Cameron children in the war, the central arc of the film follows protagonist Benjamin Cameron as he forms the Ku Klux Klan to resist the Northern rule of Austin Stoneman, a Republican abolitionist modeled after Thaddeus Stevens.

Throughout the movie, African-Americans are portrayed as lustful and uncivilized predators. In one infamous scene, Gus, a freedman played by a white man in blackface, chases Flora Cameron through a forest while professing his desire for marriage. To escape his clutches, Flora leaps off a cliff to her death. In another scene, Black legislators are shown eating fried chicken and propping their bare feet up on desks. Simultaneously, the film glorified the Reconstruction-era KKK. Klan members are portrayed as fearless saviors fighting an unjust government and are described – in an intertitle quoting Woodrow Wilson – as “a veritable empire of the South” roused by “a mere instinct of self-preservation.” The climax of the film occurs when Benjamin Cameron leads a group of Klansmen to rescue Elsie Stoneman from her kidnapper, a mixed-race man named Silas Lynch. As discussed by Esposito et al. (2021), such tropes served to propagate *Lost Cause* narratives, which cast the Confederacy as heroic defenders of Southern culture and blamed “the bringing of the African to America [as] the first seed of disunion.”

The Birth of a Nation’s overt racism spurred numerous efforts by the NAACP to ban or censor the film. After the National Board of Censorship approved the movie (and cheered

Griffith when he appeared before them), the nascent civil rights organization turned to state and local campaigns, claiming that “in no locality where [BON] is exhibited is the relation between the races the same afterward.” However, outside of Kansas, all recorded efforts to ban the film ultimately failed.³ Throughout this furor, the film’s director, D.W. Griffith, insisted on the film’s historical accuracy, reportedly offering \$10,000 to anyone who could identify a factual error. Griffith was also steadfast in his claim that the purpose of the film was not to inflame racial tensions, writing in the film’s title card that he had “no wish to offend with improprieties or obscenities.”

Despite these controversies, *The Birth of a Nation*’s reception far surpassed anything else in cinematic history. The movie was “was the first American made film to be seen by a heterogeneous (if largely white) national audience” (Stokes, 2007). As evidence of its broad appeal, newspaper reports estimated that nearly 50% of adults in Boston, Baltimore and New Orleans saw the film. While exact ticket sales are unknown, the film is predicted to have grossed between \$20 and \$100 million during its initial run. Adjusted for inflation, the upper end of that range would place the movie as the third highest-grossing of all time – trailing only *Avatar* and *Avengers: Endgame*.

Contemporaneous accounts indicate that regard for the film was near universal among white Americans. The Baltimore Reporter wrote “the movie possessed a peculiar sway over the emotions of all classes and degrees of men,” while Billboard Magazine claimed “there is absolutely nothing in the film that a sane mind can object to.” In summarizing these accounts, Stokes (2007) states that “wherever the film was shown observers agree that storms of applause greeted the climax of the film - the Klan rides” and that “in reality, Northern spectators cheered the Klan on just as enthusiastically as Southerners did.”

Such overwhelming praise would follow the film for decades. D.W. Griffith would go on to be called “the father of film” by iconic stars like Charlie Chaplin. Mary Pickford, co-founder of the Motion Picture Academy, claimed *The Birth of a Nation* was “the first picture that really made people take the motion picture industry seriously.” More recently, the American Film Institute listed the movie as the 44th best film of the past century, while *The New Yorker* claimed “the worst thing about *The Birth of a Nation* is how good it is.”

³The NAACP protested showings of the movie in Ohio, Chicago, Boston, and West Virginia among other places. However, in contrast to Kansas, protests served only to delay, not prevent, the film’s showing in those areas. As Stokes (2007) writes, “The NAACP’s campaign against *The Birth of a Nation* essentially failed. Only in Ohio [where state censors blocked the film for a year and a half] and Kansas had the film been banned for any length of time; elsewhere, attempts on the part of local authorities to suppress the film were usually rapidly circumvented by the courts.”

B Distribution and Reception

The film’s eventual acclaim was foretold by early praise from advance screenings in Los Angeles, New York and Washington, DC. In February 1915, *The Birth of a Nation* became the first movie ever screened in the White House when Woodrow Wilson hosted a private exhibition at the behest of his college classmate, the author of “The Clansman.” As a sign of how fiction would soon come to warp reality, President Wilson is reported to have said that the film was “like writing history with lightning. My only regret is that it is all so terribly true.” The next day, the movie was “cheered and applauded throughout” a private screening attended by numerous members of Congress and the Supreme Court, including Chief Justice Edward White, a former Klansman himself.

While little national media existed at the time, the hype generated by initial reviews and exaggerated claims of the film’s \$500,000 production budget led to widespread demand throughout the country. Advertisements pitched the movie as “the mightiest spectacle ever produced” and a “colossal production” (Stokes, 2007). In order to accommodate its “two projecting machines”, “various other pieces of electrical equipment [that] need to be installed at the correct distance from the screen” and “stage and orchestra pit [which need to be] rearranged to accommodate the apparatus necessary to produce realistic battle and other sound effects,” Epoch Producing Company, the film’s primary distributor, targeted newer, upscale movie theaters as well as large, traditional playhouses and opera theaters. The best seats cost as much as \$2, an unprecedented price at the time, comparable to tickets for live theater performances and equivalent to nearly \$50 in 2020 dollars.

Given the nascent state of the movie theater industry, Epoch decided that the most profitable way of distributing the film to eagerly-awaiting audiences was through a massive roadshow, similar to those employed by successful Broadway plays. While Epoch managed the roadshow in select major cities, distribution rights and profit-sharing agreements were sold to roadshow companies in other parts of the country. Each company consisted of between fifty and a hundred people and included its own manager, projectionist and technicians as well as a full orchestra, which performed the film’s musical score live.

Despite these efforts, it took several years for *The Birth of a Nation* to tour the country. As *The New York Evening Mail* explained to its anxious readers, “the elaborateness of the mounting and accessories forbids its multitudinous reproduction.” This impacted not only where the film could be screened but also how long each screening took. These supply-side constraints were often bemoaned in local newspapers. For example, the *Denver Times* wrote, “With New York 1,934 miles to the east of us...it seems probable the \$2 admission price will have a long, slow, tedious and tiresome time reaching Denver.”

Once *The Birth of a Nation* arrived in an area, tickets sold out quickly with the best

seats often reserved in advance. Newspapers described the film as a “once-in-a-lifetime” experience and reported on the notable luminaries and out-of-towners who attended nearby showings. In many cases, local demand remained unsated by the time the film left town. Advertisements would count down the number of remaining showings, urging readers to view the spectacle while they could, and distributors frequently brought the film back to the same theater months or years after its initial run for sold-out return showings. Ultimately, the end result of this slow and tedious process was the largest roadshow to that point in history, reaching over 600 counties in every state but Kansas by the end of 1919.

C Relation to the Ku Klux Klan

In addition to its enduring cinematic legacy, *The Birth of a Nation* is often linked to the rebirth of the Ku Klux Klan. At the time of the movie’s release, the KKK did not exist. The Reconstruction-era Klan depicted in the film was founded by former Confederate soldiers as a clandestine insurgent movement targeting Black political leaders and white sympathizers. That version of the Klan operated only in the South and claimed to include, at max, half a million members (Baudouin, 2011). It dissolved not long after the passage of the Enforcement Act of 1871, which outlawed many of the Klan’s intimidation tactics.

Six months after *The Birth of a Nation*’s premiere, William J. Simmons founded the Second KKK by burning a cross atop Stone Mountain, Georgia. Inspired by the recent lynching of Leo Frank and newspaper articles about the film, the Methodist preacher declared himself Imperial Wizard of the Invisible Empire on Thanksgiving Day 1915 (Stokes, 2007). Simmons chose the day strategically in anticipation of the film’s opening in Atlanta the following week. He placed newspaper ads next to movie showtimes to recruit for “a high class order for men of intelligence and character” and marched to the premiere with his fellow Klansmen in tow. Though membership stagnated until the end of the film’s roadshow, it grew rapidly after the creation of the Klan’s Propagation Department in June 1920 (Fryer and Levitt, 2012). Over the next several years, the Second KKK would establish Klaverns in every state and grow to include nearly 10% of the nation’s white male adult population.

In addition to spurring the creation of the Second KKK, *The Birth of a Nation* inspired many of its most infamous practices. After Flora Cameron’s suicide, the film shows Klan members burning a cross drenched in her blood before lynching her pursuer. By all historical accounts, the Reconstruction-era KKK never burned crosses. The first recorded instance of cross-burning in the United States was at the founding of the Second KKK. While Simmons described the act as symbolic of “a service of love and sacrifice to our age and generation,” it had no actual history in the Klan and was in fact drawn from the Scottish traditions of

The Clansman’s author, Thomas Dixon Jr. (Simmons, 1923).

Similarly, the Klansmen in *The Birth of a Nation* are adorned in white robes and hoods. While some members of the First KKK wore white hoods (often over black robes), others wore animal horns, wizards caps, or even flour sacks (Kinney, 2016). Given that the group operated in violation of federal law, its members did not want to be identified with a standard uniform. Nonetheless, as with cross-burning, the film’s white robes were ascribed historic significance in Klan mythology with Simmons claiming that their purpose “was to keep in grateful remembrance the intrepid men who preserved Anglo-Saxon supremacy in the South during Reconstruction” (Simmons, 1923).

Both practices continue to this day. As does the film’s central prominence in Klan history. Simmons himself claimed “*The Birth of a Nation* helped the Klan tremendously...It made a tremendous impression on everybody.” In later years, Grand Wizard David Duke was known to show the film to new initiates, while modern histories of the KKK claim “sooner or later just about every Klansman worthy of his robe sees the silent film classic” (Baudouin, 2011).

III Data

A Movie Screenings

To determine where and when *The Birth of a Nation* was shown, I collected data on newspaper advertisements for screenings of the movie from three online repositories of digitized historical newspapers: *newspapers.com*, *newspaperarchive.com* and the Library of Congress’ Historic American Newspapers database (*loc.gov*). Together, the databases contain over 30,000 unique newspapers and half a billion digitized pages. In each database, I searched for the phrase “Birth of a Nation” in articles dated from 1914 to 1919 – the end of the film’s roadshow. This resulted in over 100,000 hits.

A pair of research assistants visually inspected each document to determine if it contained an advertisement for a showing of the film.⁴ Because most of the film’s marketing was handled by Epoch Producing Co. and a small number of other roadshow companies, the vast majority of ads included an easily recognizable logo: a black circle with the words “Birth of a Nation” inscribed within. In a small share of cases, advertisements took a different form. In total, research assistants identified 6,266 ads. Appendix Figure A.I contains an example advertisement. It is worth noting that the advertisements made no mention of the racial undertones of the film. Instead, they focused on the movie’s extravagant production, which

⁴The remainder of the articles were general news items about the movie or its actors and spurious hits related to other topics.

“took 8 months to produce” and involved “3,000 horses”, as well as its captivating plot with “gripping heart interest and soul stirring emotions.”

From each ad, research assistants captured information on the name and location of the theater as well as the screening dates. To supplement the newspaper data, research assistants also searched for “Birth of a Nation” in digitized copies of *The Moving Picture World* from 1915 to 1919. *The Moving Picture World* was an influential quarterly trade magazine and contained dispatches from movie theaters, roadshow companies, and distributors about the film screenings around the country. This process recovered an additional 362 showings.

[Figure II about here.]

Figure II visualizes the *The Birth of a Nation*’s roadshow, color-coded by the date of the film’s first arrival across the 621 screening counties.⁵ This information comprises my primary treatment variation. However, results are also robust to using alternative data on BON screenings collected by Esposito et al. (2021). Importantly, the film’s roadshow was not limited to former Confederate states, but spanned counties across the U.S. Nor does there appear to be any clear regional pattern with regards to the timing of the film’s arrival in different parts of the country. As I will discuss later, market factors such as population size, distance to major cities, and theater capacity more strongly predict film screenings than do measures of historical racism.

B Movie Theaters

To examine long-run effects, I compare counties that received the film during its roadshow to those that did not. I instrument for whether a county received the film using information on the historical stock of theaters prior to BON’s release. Geocoded information on the location of movie theaters in 1910 comes from data compiled by Jeffrey Klenotic from digitized copies of *Billboard Magazine* and displayed on mappingmovies.unh.edu. I supplemented this data with information on theaters opened after 1910 from cinematreasures.com, an online compendium containing data on over 35,000 current and historical movie theaters drawn from archival research and crowd-sourced information. From the two datasets, I created an indicator variable for whether a county had theater in 1914, the year before BON’s premiere.

From cinematreasures.com, I was also able to collect information on founding year and seating capacity of theaters. For counties where information on founding year was missing, I searched historical editions of *The Julius Cahn-Gus Hill Theatrical Guide and Motion Picture Directory* as well as other online sources, such as newspaper archives and historical

⁵Appendix Figure A.II provides a histogram of the first screening dates.

registers. This allows me to create alternative instruments examining the intensive margin of theater capacity: theaters per 1,000 white residents, the maximum seating capacity of a theater in a county, and the year a county first opened a theater. These measures are used for robustness analysis and exogeneity checks.

C KKK Chapters

Data on the Second Ku Klux Klan come from Kneebone and Torres (2015) and was compiled from a variety of sources including Klan records and publications. The data contain the location of Klan chapters (“Klaverns”) as well as the date each chapter was first mentioned. While this provides an upper-bound on when a Klavern was formed, the data do not contain information on years active, dates of dissolution, or membership levels. Thus, my primary outcome is an indicator variable for whether a Klavern of the Second KKK had been formed in a county by 1930. This data is plotted in Panel B of Figure II and demonstrates the breadth of the Klan’s reach. In contrast to its Southern predecessor, the Second KKK operated in 972 counties across every state, with particular strongholds in the Midwest.

To examine Klan presence in later years, I draw on two sources of data regarding the Third KKK, which originated in the 1950s and persists today. Information on the location of active Klaverns in the 1960s come from contemporaneous House Un-American Activities Committee reports compiled by Mazumder (2018). Information on the location of active chapters of the KKK and other hate groups from 2000 to 2019 come from the Southern Poverty Law Center’s Hate Map. Appendix Figure A.III plots the number of Second and Third KKK Klaverns over time.

D Racial Violence

Data on racial violence comes from several different sources. The timing and location of lynchings comes from the Historical American Lynching Data Collection Project (“Project HAL”), which in turn are drawn from archival work by Tolnay and Beck (1995). Because this data focuses primarily on lynchings in the South while *The Birth of a Nation* was shown nationwide, I supplement the data with newly-uncovered information from Seguin and Rigby (2019) on lynchings in other parts of the country. Data on the timing and location of historical race riots comes from archival research gathered by Gilje (1996) and the Red Summer Archive (visualizingtheredsummer.com). While comprehensive historical data on less extreme forms of racial violence does not exist, information on the prevalence of hate crimes in recent years is available from FBI Uniform Crime Reports. I examine data from 2000-2018 cleaned and compiled by Kaplan (2020).

IV Contemporaneous Effects on Racial Violence

A Empirical Strategy

To estimate short-run effects on local lynchings and race riots, I exploit the staggered distribution of *The Birth of a Nation* during its roadshow. Specifically, I employ an event study model to examine changes in the months before and after the movie’s arrival in a county. I focus only on the film’s first showing in a county, as the timing and location of return showings are less likely to be exogenous. Given widespread demand across the country, whether and when the film was first shown in a county was likely determined more by geography and market factors than differences in local racial animus.

[Table I about here.]

As descriptive evidence, Table I presents summary statistics comparing counties that did and did not receive the film. Treatment counties were less likely to be located in former Confederate states than control counties and had similar Black and illiterate population shares as well as similar rates of support for Woodrow Wilson in 1912. While treatment counties were slightly more likely to have a Confederate monument or lynching in the pre-period, these differences swing in the opposite direction on a per capita basis. Instead, the largest differences appear to lie in population size, density and urbanicity as well as newspaper and theater penetration, suggesting that market size and availability of screening spaces were primary predictors of where the film was shown.⁶

Nonetheless, it is important to note that the event study design used to examine short-run effects does not require unobserved factors correlated with racism to be uncorrelated with treatment. The model instead relies on a parallel trends assumption and accounts for level differences between treatment and control areas by examining changes in acts of racial violence before and after the film’s arrival. In particular, I estimate the following equation on weekly panel data from 1913 to 1922 for all counties in the continental U.S.:⁷

$$(1) \quad y_{c,t} = \delta_c + \lambda_{s,t} + \sum_{\tau=-6}^6 \beta_{\tau} Show_{\tau} + \epsilon_{c,t}.$$

⁶Section V provides empirical corroboration for this claim with respect to the extensive margin of treatment (whether a county received BON). Appendix Figure A.IV shows that the timing of the film’s arrival in a county (conditional on treatment) is strongly predicted by observable county and market factors.

⁷In total, the sample includes 3,104 counties. However, the inclusion of state fixed effects effectively drops one county: Washington, DC.

Here, $y_{c,t}$ is an indicator for whether a lynching (race riot) occurred in county c at week t . δ_c are county fixed effects and $\lambda_{s,t}$ are state-week fixed effects that flexibly control for state-wide trends or shocks. The coefficients of interest (β_τ) are on a vector of dummies corresponding to relative months to BON’s arrival in a county.⁸ The omitted period is the last month before the movie’s arrival in a county and $Show_6$ ($Show_{-6}$) is set to 1 for periods 6 or more months after (before) a showing. Standard errors are clustered by state.

B Lynchings and Race Riots

Results from estimation of Equation 1 on lynchings are presented in Panel A of Figure III. I find a sharp increase in the probability of a lynching coinciding with the film’s premiere in a county. The validity of these findings is supported by flat pre-trends. In the six months prior to the film’s arrival, the treatment group of over 600 counties experienced two lynchings combined, none of which occurred within two months of treatment.⁹ In the month after the film’s arrival, treated areas experienced a roughly four-fold increase in the rate of lynchings ($\beta_0=0.0011$; mean=0.0003). Panel B of Figure III examines the effect of BON’s arrival on race riots. Results indicate a sharp increase in the likelihood of a county experiencing a race riot in each of the three months following the movie’s premiere. These estimates again correspond to a four-fold increase over the sample mean ($\beta_0=0.00034$; mean=0.00009).

[Figure III about here.]

It is important to note that the lynching and riot effects are highly imprecise and are not statistically significant at conventional levels ($p = 0.06$ for $\beta_0^{lynching}$; $p = 0.30$ for β_0^{riot}). This is largely due to the scarcity of lynchings and riots. From 1913 to 1922, a period spanning the Red Summer and the peak of race riots in the U.S., there were fewer than 20 race riots and 60 lynchings per year nationwide. As such, the effects are driven by a small number of events – 15 treatment counties experienced a lynching or race riot in the year after the film’s arrival. Nevertheless, as I demonstrate in the next subsection, the sharp increase in racial violence is preserved across a host of robustness checks, and randomization inference yields highly significant p-values ($p < .001$ for lynchings and $p = 0.01$ for riots).

⁸Months correspond to five-week intervals centered on the week of the film’s arrival, not calendar months. Thus, if BON premiered during the week of January 16, 1916, the week of February 6, 1916 is coded as $month = 0$ while the week of January 9, 1916 is coded as $month = -1$.

⁹Additional support for the parallel trends assumption is provided in Appendix A.V, which expands the sample to 1900-1930 and estimates a year/election-level analogue of Equation 1. Due to the scarcity of race riots prior to 1915 (i.e. zero in 1914 and fewer than fifteen in the preceding five years), I focus on lynchings and Democratic vote share and find parallel pre-trends even several years before treatment.

Though the scarcity of incidents prevents rigorous heterogeneity analysis, Appendix Table A.I shows descriptively that areas that experienced racial violence after the film’s showing were less dense, more likely to be located in former Confederate states, and had notably higher Black population shares, Democratic support and historical racial violence than treatment areas that were not induced to violence by the film. While Section VI explores mechanisms in more detail, this provides suggestive evidence that the immediate spikes in racial violence were facilitated by increased coordination among existing racists.

Lynchings and race riots can have potentially deleterious spillovers on economic growth and constitute some the most extreme manifestations of racial hate (Cook, 2014). However, they comprise only a small fraction of all racial violence.¹⁰ Thus, my findings likely underestimate BON’s total impact on racial intimidation. As one salient example, a white moviegoer in Indiana reportedly shot a 15-year-old Black high school student immediately after leaving the theater (Stokes, 2007). Unfortunately, no comprehensive data of historical hate crimes exists. To examine effects on racial violence beyond lynchings and race riots, I instead leverage novel data on state homicide rates by race drawn from *Vital Statistics*. Appendix Figure A.VI presents results from estimation of an analogous state-level event study design. Consistent with increased anti-Black violence, I find that states with greater BON penetration experienced a significant uptick in minority homicides, but no change in white homicides, in the decade after the film’s release. Together, the findings demonstrate the violent racism that *The Birth of a Nation* incited in local communities.

C Robustness

The Appendix contains a host of additional analyses validating the contemporaneous effects on racial violence. Figure A.VII demonstrates robustness to alternative samples: restricting to or excluding control counties that neighbored treatment areas, limiting to screened counties and exploiting only variation in treatment timing, and restricting to counties with at least one digitized paper in 1915. Figure A.VIII instead examines alternative specifications: excluding state-week FEs, controlling for county-specific linear time trends, allowing for differential trends based on historical discrimination, and controlling for the number of digitized papers in a county-year. Meanwhile, Figure A.IX explores effects on alternative measures of racial violence: the number of lynchings and race riots, the number of days with lynchings, the number of riot-related deaths, and per capita lynchings and riots.

To demonstrate robustness to selection, Figure A.X replicates the event study analysis using propensity score matched samples. Propensity scores are calculated from demographic,

¹⁰For example, Tolnay and Beck (2018) estimate that there were as many incidents of threatened lynchings as there were actualized lynchings in the early 1900s.

social capital, media and racism covariates described in the forthcoming instrumental variables model (Section V). Treatment counties are then matched to control counties in the same state across three samples: the full set of control counties, control counties with movie theaters, and control counties without movie theaters. As further corroboration, Figure A.XI estimates the event study model using predicted as opposed to actual treatment, where treatment is predicted from the same area characteristics plus theater presence in 1914.

To ensure that BON's effects are not driven by noise, I run a series of 500 permutation tests, randomly assigning counties placebo treatment dates drawn without replacement from the distribution of actual treatment dates in the same state. Figure A.XII provides a histogram of the placebo coefficients, confirming that the observed increases in lynchings and race riots are unique to the timing of the movie's arrival. For lynchings, none of the 500 placebo estimates at $\tau = 0$ and only one of the 3000 placebo estimates at $\tau > 0$ exceeds the actual treatment estimate at $\tau = 0$. For race riots, 1% (3.7%) of placebo estimates at $\tau = 0$ ($\tau > 0$) exceed the actual treatment estimate at $\tau = 0$.

Finally, to test whether spikes in violence follow screenings of other popular movies, I conduct a placebo test using the 1918 film, *Mickey*. A rags-to-riches comedy about a female orphan, *Mickey* sold over 40 million tickets from 1918 to 1922, a record-breaking figure that stood for nearly two decades.¹¹ Figure A.XIII replicates the event study analysis for *Mickey* screenings, which were determined using the same methods and sources as for BON.¹² I find little evidence of increased violence in the immediate wake of *Mickey*'s premiere, corroborating the unique racial impact of *The Birth of a Nation*.

V Historical Effects on the Second Ku Klux Klan

A Empirical Strategy

I next examine effects on Klavern formation. While the event study allows for valid estimation of short-run effects, the parallel trends assumption may be less plausible over longer time horizons. More practically, there is no pre-period data to estimate a difference-in-differences design as the Second KKK did not exist until after *The Birth of a Nation* premiered and did not gain wide prominence until the early 1920s.

Instead, I leverage an instrumental variables strategy to assess the film's longer-run impacts. Here, I exploit cross-sectional variation between counties that received the film during its roadshow and those that did not. Thus, the primary relationship of interest is:

¹¹As comparison, *The Birth of a Nation* is estimated to have sold 15 million tickets over its initial five-year run. However, the film grossed far more revenue than *Mickey* due to significantly higher ticket prices.

¹²In total, 897 counties screened *Mickey* from 1918 to 1922.

$$(2) \quad KKK_c = \lambda_s + \beta Screened_c + X'_c \Gamma + u,$$

where KKK_c is an indicator for whether county c had a Klavern by 1930, $Screened_c$ is an indicator for whether the county screened *The Birth of a Nation* from 1915 to 1919, λ_s are state fixed effects and X'_c is a vector of exogenous demographic, social capital, media and racism controls. The concern is that OLS estimates of β could be biased by the potential endogeneity of BON screenings. The direction of this bias is *ex ante* ambiguous. On the one hand, if areas with greater latent racial prejudice were more likely to show the film and more likely to form KKK chapters, estimates would be biased upwards. On the other hand, given high ticket prices and widespread demand, distributors may have directed the movie towards wealthier areas where Klaverns were less likely to form, biasing estimates downwards.

To address this potential endogeneity, I instrument for whether BON screened in a county using the historical presence of theaters prior to the movie's release. Specifically, I estimate the linear probability model:

$$(3) \quad Screened_c = \delta_s + \gamma Theater_c + X'_c \Lambda + v,$$

where $Theater_c$ is an indicator for whether a theater existed in county c in 1914.¹³ In robustness analysis, I demonstrate similar effects using alternative instruments based on the relative prevalence, maximum seating capacities, and opening dates of local theaters as well as the screening locations of *Mickey*. As before, standard errors are clustered by state.

Exogeneity

For the IV strategy to yield valid causal estimates of the impact of *The Birth of a Nation* on Klavern formation, $Theater_c$ must be both relevant and exogenous. The first is obvious, as having a theater is a necessary condition for showing the film. The key concern then is whether my instrument is exogenous to omitted factors correlated with local KKK support.

While I am unable to directly test the exogeneity assumption, I provide several pieces of evidence in support of the instrument's validity. First, I show that theater stock in 1914 is conditionally uncorrelated with historical measures of race and discrimination. Figure IV displays results from estimation of Equation 3 on the number and severity of race riots from

¹³Basu et al. (2017) show 2SLS produces consistent estimates of local average treatment effect (LATE) in settings with binary outcomes and binary treatments, regardless of the rarity of those variables. Angrist and Krueger (2001) argue that LPM are preferable to probit or logit estimation of first-stage regression.

1900 to 1914, Democratic vote share in Congressional elections from 1910 to 1914 (when Democrats were the more racially-conservative party), and the total Black population and share enslaved during the antebellum period. Across all outcomes, estimates are precise zeroes, with magnitudes that never exceed 0.05 standard deviations. In Appendix Figure A.XIV, I also find little relationship between theaters and population measures before or *after* the movie’s release, suggesting that future demographic changes (such as due to the Great Migration or European immigration) are unlikely to bias my estimates.

[Figure IV about here.]

While these tests provide evidence of exogeneity with respect to observed measures of discrimination, unobserved confounds – like latent racial prejudice or social capital – that are correlated both with historical theater presence and future Klan formation could still present concerns. Another potential threat is if theaters had an independent effect on Klavern formation, such as through the screening of other influential films.¹⁴ I provide two pieces of evidence that suggest these concerns are unlikely to bias my results.

First, I leverage the fact that BON was banned in the state of Kansas until the mid-1920s by Governor Arthur Capper, who feared that the film would “excite racial prejudice” against African-Americans. It is worth reiterating that attempts to ban the film were not unique to Kansas, though all other recorded efforts were ultimately unsuccessful. Nor was Kansas an especially progressive state, with rates of Klan membership 50-100% higher than the neighboring states of Nebraska and Missouri (Jackson, 1992). Indeed, Governor Capper’s decision was widely opposed even within the state. The Topeka State Journal called it “a ploy for Negro votes” and the Independence Daily Reporter “a serious mistake.” However, because the ban applied to all counties in Kansas, it allows for a unique test of the exclusion restriction: whether having a theater in 1914 is exogeneous to future Klavern formation, absent *The Birth of a Nation*.

[Table II about here.]

Thus, Table II presents reduced form relationships between Klavern formation in 1930 and theater presence in 1914, separately for counties in Kansas and those in all other states. Column 1 examines my preferred instrument, an indicator for whether a county had a theater in 1914. The remaining columns examine alternative measures of theater stock: the number of theaters per 1,000 white residents (Column 2), the number of seats in the largest theater in a county (Column 3), and the year a county’s first theater was founded (Columns 4

¹⁴However, if screenings of other films (or the impact of those screenings) were themselves dependent on prior viewership of *The Birth of a Nation*, exogeneity would not be violated, in a narrow sense.

and 5). Examining counties outside of Kansas (Panel B), where BON was *not* banned, I find significant positive relationships between future Klavern formation and areas with any theater, larger theaters, and older theaters in 1914. In contrast, these relationships are all insignificant when examining counties in Kansas (Panel A), where BON was banned. The point estimates suggest that, if anything, unobserved confounds associated with theater presence would bias IV estimates towards zero.

I next exploit the timing of theater openings across counties. If theater presence affects future Klan support through some channel other than *The Birth of a Nation* – such as its correlation with unobservable racial preferences or by facilitating exposure to other impactful films – then one might expect to observe similar effects for theaters constructed before and after the film’s roadshow. However, if this relationship is mediated only by BON, theaters that opened after the film’s roadshow should have no impact on Klavern formation.

[Figure V about here.]

Thus, Figure V estimates reduced form relationships between Klan presence in 1930 and a set of indicator variables corresponding to the year a county’s first theater was opened. The omitted category are counties that did not have a theater by 1930. As shown, I find that counties with theaters opened before 1918 are significantly more likely to have a Klavern by 1930, but counties with theaters opened after 1918 are not. This aligns with the timing of BON’s roadshow, which concluded in 1919, and provides further evidence that the relationship between historical theater stock and future Klan support is driven by screenings of *The Birth of a Nation* and not other correlated factors.

B First-Stage Results

The first-stage effects of theater presence on BON screenings are shown in Table III under several specifications. Column 1 tests the bivariate relationship between having a theater in 1914 and receiving the film from 1915 to 1919 and yields a highly-significant and positive coefficient. Per Andrews and Stock (2005), I report Kleibergen-Paap F-statistics, which far exceed the 16.38 benchmark for maximal 10% bias in all cases.¹⁵

[Table III about here.]

Column 2 demonstrates that this relationship remains highly significant, if slightly attenuated in magnitude, when controlling for county population and demographics. Specifically, I include a quadratic of total population as well as controls for density, Black population,

¹⁵Kleibergen-Paap F-statistics are equivalent to Oleva-Pflueger (2013) in $k = 1$ situations.

Black population share, native-born population share, and the share of residents who would have been of draft-eligible age in World War I. All measures come from the 1910 Census.

In Column 3, I further control for measures of social capital by including urban population share and illiteracy rate in 1910, voter turnout in 1912, and the share of residents in religious organizations in 1906. I also control for average occupational income score and for quadratics of the railroad distance to the two nearest major cities (i.e., the 10 largest cities in 1910). In Column 4, I additionally include proxies of local demand for media – specifically, per capita newspaper circulation and the number of media markets in a county in 1912.¹⁶ The coefficient of interest shrinks with additional controls but remains large and highly significant.

Finally, in Column 5, I add controls for observable measures of racial prejudice and activism. These include the number of lynchings in a county from 1900 to 1905, Democratic vote share in 1912, and the presence of Confederate monuments and NAACP chapters in 1914. Notably, including these controls has no impact on the estimated relationship between theater presence and BON screenings. Both the coefficient of interest and the R-squared remain virtually unchanged. This is consistent with historical accounts of the widespread demand for the film. In line with DellaVigna and La Ferrara (2015)’s assertion that “consumer demand for [entertainment] media content is largely due to demand for entertainment” rather than “preference for particular economic outcomes,” I find little evidence that local racial animus influenced where *The Birth of a Nation* was shown.

As a visual representation of the first-stage relationship, Appendix Figure A.XV presents a binned scatterplot comparing actual treatment to predicted treatment from the fully-controlled specification shown in Column 5 of Table III. The predicted likelihood of receiving the film is highly correlated with the actual likelihood (slope=0.94, p-value<0.001).

C IV Estimates

Correspondence to Event Study Estimates

To validate the IV strategy against the event study model, I first estimate Equation 2 on lynchings and race riots, instrumenting for screenings using theater stock in 1914. These results are shown in Appendix Figure A.XVI. In support of exogeneity, I find insignificant, near-zero estimates for periods prior to the film’s release. However, consistent with the event study analysis, I find evidence of contemporaneous increases in lynchings coinciding with the film’s arrival in treatment counties (i.e., from 1915 to 1919). This effect continues through the period spanning 1920 to 1924 before converging to zero from 1925 to 1939, when country

¹⁶Railroad distances are derived from data by Attack (2017). Newspaper measures come from Gentzkow et al. (2011).

averaged fewer than 10 lynchings per year. I also find a large, if insignificant, spike in race riots from 1915 to 1919, the period spanning the movie’s initial run and the Red Summer. Despite employing a different identification strategy, these results corroborate *The Birth of a Nation*’s near-term impact on racial violence.¹⁷

Klavern Formation

I next investigate BON’s role in the rebirth of the Ku Klux Klan. While the Second KKK was founded shortly after the premiere of the film, it did not achieve prominence until several years later. Data from Kneebone and Torres (2015) include only five Klaverns prior to the end of 1919. Klan activity exploded shortly afterwards with 1,047 Klaverns by 1925. Thus, I examine whether a Klavern had ever existed in a county by the end of 1930, when national KKK membership had dwindled to 30,000.¹⁸

[Table IV about here.]

Results are presented in Table IV, instrumenting for BON screenings using the presence of a theater in 1914. Column 1 examines all counties in the continental U.S. and finds that areas that screened *The Birth of a Nation* were significantly more likely to have a Klavern by 1930. Columns 2 through 6 present results from restricted samples. In Column 2, I limit the control group to counties adjacent to treatment counties in order to increase comparability. In Column 3, I instead exclude neighboring control counties, as residents of those areas could have traveled to see the film in nearby areas. To ensure that effects are not driven by major cities during a period of increasing urbanization, Column 4 drops the 20 counties with the largest 1910 populations. To account for the possibility that areas with theaters differed on unobservables from those without theaters, Column 5 restricts the sample to counties that had at least one movie theater by 1930. Finally, because the location of BON screenings and Klavern foundings were both partly drawn from historical newspapers, selection due to incomplete newspaper coverage may bias the estimates upwards. To account for this, Column 6 restricts the sample to those counties where digitized pages exist for at least one local newspaper in 1915.

In all cases, I find large, significant effects on Klavern formation. The 2SLS estimate for the main sample of 68.5 percentage points is highly significant ($p < 0.001$) and roughly twice as large as the sample mean (0.31) and the raw difference between treatment and control

¹⁷While not directly comparable, the magnitude of the effects is also similar to those estimated earlier. Extrapolating the per-week effects from the instrumented event study results shown in Figure A.XI suggests a 21 percentage point increase in the likelihood of a lynching over five years. This is quite similar to the IV estimate for lynchings from 1915-1919 of 0.23.

¹⁸Appendix Figure A.XVII shows similar effects on Klavern presence at other points in time.

groups (56% of treatment counties had a Klavern by 1930 versus 25% of control counties). This suggests that, at the margins, areas that screened BON were almost certain to contain a Klavern in the future.

Across models, the 2SLS estimates are appreciably larger than the OLS estimates. This is likely due to measurement error and selection. Regarding the former, the use of a binary endogenous variable with a binary instrument can magnify the differences between OLS and IV estimates when the rate of false negatives (i.e. counties classified as control that actually screened BON) exceeds the rate of false positives (i.e. counties classified as treatment that did not actually screen BON) (Pischke, 2007). This is likely the case here, as the rate of false positives in movie advertisements is plausibly zero. Nonetheless, Black et al. (2000) demonstrate that OLS and IV estimates may serve as bounds of the true coefficient. Consistent with this, the two estimates converge as I decrease the rate of false negatives (ex: by restricting the sample to areas with greater newspaper coverage) and diverge as I increase the rate of false negatives (ex: by adopting more stringent definitions of treatment). Columns 2 and 6 of Table IV provide initial corroboration of this, while additional evidence is discussed in the Robustness subsection. In all cases, both the OLS and IV estimates remain positive and significant, suggesting that the true coefficient is also above zero.

Differences in OLS and IV estimates could also be driven by divergent selection into Klaverns and BON screenings. While Klan strength and activities varied considerably across region, evidence suggests that rank-and-file membership was relatively homogeneous and defined by a few common characteristics: lower-middle-class, native-born, Protestant (primarily Baptist or Methodist) white men hailing from medium-sized towns and cities (Jackson, 1992).¹⁹ By contrast, *The Birth of a Nation*’s distributors targeted affluent, urban areas with “the highest quality theaters” and “the finest looking people”: “social and literary elite”, “traditional theatergoers”, and middle- and upper-class residents capable of affording the \$2 price of admission (Stokes, 2007). Thus, area-based confounders would likely bias OLS estimates downwards. As corroborating evidence, Appendix Figure A.XVIII shows that the defining features of Klan membership are all inversely correlated with BON screenings. Notably, these same features are uncorrelated with historical theater presence, reinforcing the validity of the instrumental variables approach.

To better understand the areas where the film catalyzed Klan support, Figure VI exam-

¹⁹Examining chapter records and membership lists from Klaverns across the country, Jackson (1992) concludes “the secret order was a lower middle class movement. Few men of wealth, education or professional position affiliated with the Invisible Empire. The greatest source of Klan support came from non-union blue collar employees of large businesses and factories” whose “religious loyalty was to conservative, non-ritualistic Protestant denominations such as Baptist, Methodist or Christian Churches.” As such, “the Invisible Empire was close to impotence...in rural areas of high non-white concentration, such as the Mississippi and Alabama black belts,” and thrived instead “in the border regions and in industrial communities.”

ines heterogeneous effects across various county sub-samples. Strikingly, I find significant positive effects on historical Klavern formation regardless of demographic, geographic or cultural conditions. IV estimates are similar for counties with high and low levels of population, density, Black shares, Democratic support, and illiteracy rates. They are also quite similar when comparing former Confederate and non-Confederate areas as well as counties with and without historical episodes of racial violence or Confederate monuments.

[Figure VI about here.]

However, one dimension where evidence of differential effects emerges is in religious participation. Point estimates for counties with below median rates of religious membership are more than twice as large as for counties with above median rates (1.02 vs. 0.39; $p(\text{diff}) < 0.001$). In light of work by Satyanath et al. (2017) showing that social organizations helped facilitate the rise of the Nazi Party, these results suggest that the *The Birth of a Nation* may have had the largest catalyzing effect in areas where existing social capital was weakest. That is, the movie may have served as a substitute for social and religious networks in the organization of new Klaverns. While this last point is only suggestive, the heterogeneity analysis makes clear that *The Birth of a Nation* inspired Klan support across a wide range of areas, not just those where racial prejudice was highest. This pattern of effects is consistent with the overwhelming praise the film received across the country as well as the broad historical appeal of the Second KKK.

Klan Intensity

I next examine effects on the intensive margin of Klan support. While comprehensive county-level Klan membership data does not exist, Jenkins (1986) states “the strength of the Klan...can be generally deduced from the number of individual ‘Klaverns’ in a region”. Thus, Appendix Table A.II examines the total number of Second KKK Klaverns in a county as a share of Klan-eligible population (i.e., per 10,000 native-born white males in 1930). The IV estimates suggest that film screenings significantly increased the prevalence of Klan activity in an area ($\beta_{IV}=0.969$, $p=0.027$), not simply whether a Klavern existed. To put these results in context, scaling by national estimates of the number of Klansmen per Klavern suggests that the film led to the induction of roughly one million members.

As further corroboration, I leverage estimates of Second KKK membership by state from Jackson (1992). Appendix Figure A.XIX presents results from a state-level IV regressing Klansmen per eligible population (i.e. white native-born men) on the share of eligible population living in treatment counties, as instrumented by the share of eligible population living in counties with theaters. As shown, there is a strong positive relationship between film

screenings in a state and Klan intensity ($\beta_{IV}=0.13$, $p = 0.107$). While only suggestive, this implies a persuasion rate of 18% or roughly one additional Klan member for every six white native male moviegoers (DellaVigna and Gentzkow, 2010).²⁰ In line with the Klavern per capita results, this translates to over 1.5 million additional members. While such a persuasion rate is high relative to other media studies, historians have long claimed *The Birth of a Nation* “was a powerful force in the revival of the Klan” and it is plausible that the Second KKK would never have existed without the film (Cook, 1962).²¹ While this counterfactual is impossible to prove, the empirical analysis corroborates the film’s role in not only inspiring the formation of Klaverns but also fueling the intensity of Klan support.

D Robustness

Measurement Error

Because BON screenings were determined predominantly from newspaper advertisements, one may be concerned that counties with screenings that were not advertised may be incorrectly included in the control group. Supplemental information from theater and roadshow dispatches published in *Moving Picture World* should alleviate some of this concern. To further examine robustness to potential measurement error, I make use of alternative data on BON screenings from Esposito, Rotesi, Saia, and Thoenig (ERST). The ERST data contain 442 screening counties determined from *newspapers.com* article text. The authors visually inspected articles for mentions of BON showings that seemed to occur *within* the county of the paper’s headquarters and coded screenings according to the newspaper’s county and the month of the article’s publication. The two measures present different trade-offs, but are nonetheless highly congruent, agreeing for 83% of counties in both samples.²² Appendix Table A.III replicates the analysis using my primary treatment measure, the union of mine

²⁰The persuasion rate ($f = 100 \times \frac{y_t - y_c}{e_t - e_c} \frac{1}{1 - y_0}$) adjusts the treatment effect ($y_t - y_c$) for actual exposure to the film ($e_t - e_c$) and the share of potential compliers ($1 - y_0$). In this case, I adjust my IV estimate (0.13) by the estimated share of white native-born men living in counties with BON who saw the film (0.69). The latter figure assumes that two-thirds of the film’s estimated 15 million tickets went to white men. Further adjusting for the share of potential compliers based on rates of Klan membership in Kansas (i.e., $y_0 = 0.075$) has little impact on the persuasion rate.

²¹As reference, DellaVigna and Gentzkow (2010) examine five studies on the impact of media on voter turnout and ballot choice and find an average persuasion rate of 10.2% with a range of 4.3% to 20%.

²²On the one hand, my measure draws on a larger set of data sources and identifies more screenings, but may suffer from false negatives if screenings were not advertised in any paper in *newspapers.com*, *newspaperarchive.com*, and *loc.gov*, and were not mentioned in *Moving Picture World*. On the other hand, ERST’s data may correctly identify unadvertised screenings that were mentioned in *newspapers.com*, but may suffer from false negatives for screenings not mentioned in *newspapers.com* and for *mentioned* screenings that occurred outside the paper’s headquartered county. Dates of screenings may also differ from the month of articles mentioning them.

and ERST’s measures, the intersection of the two, and ERST’s alone. In all cases, I find that BON screenings are associated with large, significant increases in future Klavern support. However, note that the difference between OLS and IV estimates increases considerably when using the intersection of the two measures or ERST’s measure alone. Consistent with the non-classical measurement error discussed by Pischke (2007), this is because the rate of false negatives increases as treatment is defined more restrictively.

Incomplete newspaper coverage could also introduce measurement error to the screening variable. Some counties did not have a local newspaper in 1915 (Gentzkow et al., 2011). Even among counties with historical papers, selection into the *newspapers.com*, *newspaper-archive.com* and *loc.gov* digital archives may be non-random. While the stability of my estimates when restricting to counties with a digitized paper in 1915 (Column 6 of Table IV) suggests this is unlikely to induce severe bias, Table A.IV provides additional reassurance using alternative methods of accounting for newspaper coverage. First, I restrict to counties with at least one newspaper (digitized or not) in 1915 (Column 1) and directly control for the number of digitized historical newspapers in each county (Column 2). Next, I further limit the sample to counties with at least one (Column 3) or two (Column 4) digitized newspapers during *each* year of the film’s road show. Finally, I restrict to the set of counties where the 1918 film *Mickey* was shown (Column 5). Given that Mickey screenings were identified from advertisements in the same three digital archives, any remaining differences in the nature or availability of historical newspaper content between treatment and control counties are likely limited. Across all specifications, OLS and IV estimates indicate significant effects on future Klan support. Consistent with measurement error explaining some of the wedge between those estimates, the differential between the two converges as the sample narrows.

Alternative Instruments

Table A.V demonstrates robustness to alternative instruments. Column 1 leverages variation in the intensive margin using the number of theaters per 1,000 white residents. Because the film’s distributors targeted larger theaters in order to maximize attendance and revenue, Column 2 uses the seating capacity of a county’s largest theater in 1914. Column 3 uses this same seating capacity instrument but restricts the analysis to counties with at least one theater. Column 4 exploits variation in the timing of theater construction based on the year a county’s first theater was opened. Column 5 uses the same instrument but restricts to counties with at least one theater by 1930. Finally, in Column 6, I instrument for BON screenings using screenings of *Mickey* from 1918 to 1922. Across specifications, I find evidence of large, positive effects on Klavern formation.

Spatial Correlation

Racial attitudes may be spatially correlated, which could present concerns when examining place-based predictors of future outcomes. Thus, I perform a series of checks to interrogate the role of spatial correlation. As a first step, I estimate the degree of spatial autocorrelation in my primary regression residuals using Moran’s I statistic, which Kelly (2019) shows to be insignificant in studies where results are robust to potential misspecification. Results are shown in Appendix Table A.VI and are insignificant across a variety of distance thresholds. I next directly test robustness to county-clustered standard errors, Conley (1999) adjustments allowing for spatial correlation at different bandwidths, and the principal component method proposed by Müller and Watson (2021) to construct confidence intervals under “worst-case” spatial correlation models. Finally, I repeat the above tests while directly controlling for quadratics of longitude and latitude of county centroids. Results remain statistically significant, or borderline so, in all cases.

Other Robustness

The Appendix contains additional robustness tests. Table A.VII compares the baseline OLS estimates to average treatment effects derived from propensity score matching. Propensity scores are estimated from the full set of control variables with state as a blocking variable (i.e. restricting potential matches to counties in the same state). This exercise is repeated for three samples: all control counties, control counties with movie theaters, and control counties without movie theaters. For each sample, I present results with and without further adjusting for the predicting covariates. In all cases, estimates indicate BON screenings led to significant increases in the likelihood of having a future Klan chapter.

To provide a sense of potential selection on unobservables, I also report Oster’s δ , which measures how large bias from unobservables would have to be for the true treatment effect to be zero (Oster, 2019). As shown, δ is near 1 in the unmatched sample and ranges between 2.1 and 5.5 in the matched samples. These results provide strong evidence that selection is unlikely to explain my effects, as selection on unobservables would need to be several times as large as selection on observables to drive the treatment estimates to zero.

Finally, I perform placebo tests using screenings of *Mickey*, the most widely-seen movie of the era. Appendix Table A.VIII shows that while areas that screened *Mickey* were more likely to have a Klavern in the future, this is due to the large overlap in distribution areas of the two films. Roughly 50% of counties that screened *Mickey* also screened BON. Controlling for BON screenings, the effect of Mickey showings is near zero and insignificant. Similarly, Mickey showings are not associated with increased Klan presence in Kansas (where BON

was banned) or in other counties where BON was not shown. This is true for both OLS and IV models.²³ These results provide further confidence that differences in Klan support are not driven by unobserved factors correlated with theater locations or the arrival of popular movies more generally, but rather by the *The Birth of a Nation*’s specific racist influence.

VI Mechanisms

The observed effects on racial violence and Klavern formation could operate through a number of different channels. Given the increase in group-based acts of racial hate, one likely mechanism may be the film’s facilitation of group coordination. For example, the popularity of local screenings may have revealed latent racism, leading to enhanced coordination among individuals predisposed to racial hate or to the unraveling of social norms that otherwise might have prevented individuals from acting on their prejudices (Bursztyn et al., 2020, 2019). Mass propagation of Klan imagery could also have provided a common reference for those seeking kinship with other racists.²⁴ Such mechanisms would be consistent with the heterogeneous effects by religious share, which suggest that BON’s largest catalyzing effects were in areas where existing coordination was weakest.

Media imitation is another likely mechanism. The KKK’s continued use of white robes and cross-burning points to real-world emulation of the film’s fictionalized Klan. Further evidence of imitation can be found in naming conventions. Examining Census data, Appendix Figure A.XXIII finds a significant increase in the share of white boys named “Benjamin” (the name of BON’s protagonist) in the years after the film’s release. Consistent with studies documenting the popularity of names first seen in media (La Ferrara et al., 2012) and the use of names to reflect cultural norms and beliefs (Bazzi et al., 2017), these findings corroborate the role of media imitation in explaining the film’s cultural impact.

A third set of mechanisms revolves around persuasion. Through this lens, the film may have directly affected the racial preferences and beliefs of its viewers.²⁵ This is consistent with

²³With two endogenous regressors, I can no longer employ a single instrument. Instead, I instrument for BON and Mickey screenings using a set of indicator variables based on the year a county’s first theater was founded.

²⁴While the Second KKK’s direct involvement in lynchings has been disputed (Fryer and Levitt, 2012), Jackson (1992) notes that from 1920 to 1925 “no fewer than six murders were directly attributable to the secret order” in the South as well as “scores of floggings, tar and featherings and other forms of physical abuse.” That the effects on lynchings (Figure A.XVI) and non-white homicide rates (Figure A.VI) both dissipate after 1925 when Klan membership began to drop precipitously also suggests the KKK’s role in the historical racial violence effects.

²⁵The short-run spikes in racial violence could also be explained by emotional responses to the film (Card and Dahl, 2011), as audiences often “became sufficiently emotionally engaged with the story as to lose their natural inhibitions” (Stokes, 2007).

the 1931 Payne Fund study – one of the earliest social science experiments on media – which found that white schoolchildren expressed persistently higher rates of anti-Black prejudice after seeing *The Birth of a Nation*.²⁶ Relatedly, Esposito et al. (2021) document significant increases in newspaper rhetoric espousing North-South reconciliation and in article mentions of the phrase “White Americans”, in line with the film’s title card exhorting “the former enemies...[to] unite again in common defence of their Aryan birthright.”

Unfortunately, data constraints make it difficult to directly corroborate the film’s contemporaneous impact on racial preferences. Detailed surveys of racial attitudes from around the time of the film’s release do not exist. The nearest proxies available at the local-level are political outcomes like Democratic vote share and DW-NOMINATE Congressional ideology scores. Appendix Figures A.XX, A.XXI and A.XXII examine these margins and find null effects. This is consistent with Fryer and Levitt (2012), who find little relationship between Klan presence and Republican vote share in an area. However, it is important to note that the available measures are quite coarse and represent the views of only a small share of individuals.²⁷ As a result, they may fail to capture important infra-marginal effects — for example, if the film inflamed racial hate among existing racists.

To explore these dynamics, I turn to survey data from later decades. First, I examine responses from a 1946 Gallup survey on knowledge and views of the KKK. As the data is geographically identified by state, I regress individual responses on the share of white native males in a state living in BON counties, as instrumented by the share of those individuals living in counties with theaters. Results are shown in Appendix Table A.IX and indicate that BON screenings are associated with increased local knowledge of the KKK as well as a reduced desire to ban the organization, particularly among white respondents.

Next, I explore American National Election Studies data from the 1970s, when “feeling thermometers” measuring attitudes towards various Black groups and individuals were first introduced.²⁸ As the data include each respondent’s county of residence, I estimate an individual-level analogue of Equation 2. Results are shown in Figure A.XXIV and indicate that BON screenings are associated with markedly less favorable opinions of “Blacks”, “Civil Rights Leaders”, and “Black Militants.” Notably, these effects are driven by a large increase in the share of individuals holding extremely negative attitudes and a corresponding reduction in moderately negative to neutral views. I find little change in the share of individuals

²⁶Students were asked to agree or disagree with a series of 24 statements like “Under no circumstances should Negro children be allowed to attend the same schools as white children” and “The Negro should have freedom but should never be treated as the equal of the white man.”

²⁷For example, 15 million people out of a population of 95 million voted in the 1912 presidential election.

²⁸Questions about “Civil Rights Leaders” and “Black Militants” were introduced in 1970. Prior to that, only views of “Blacks” more generally were solicited. Results are robust to including those earlier responses.

expressing positive views. Though it is difficult to disentangle the extent to which these changes mediate the effect on historical Klan support – as opposed to being a consequence of it – they nonetheless suggest that *The Birth of a Nation* ultimately served to further radicalize racists.

VII Long-Run Effects on Racial Hate

A Modern Hate Groups and Crimes

Turning to long-run ramifications, I investigate whether effects on racial hate persisted beyond the collapse of the Second KKK in the 1940s. To do so, I leverage my IV model to explore more recent data on hate groups and hate crimes. Results are shown in Table V

[Table V about here.]

I first examine the presence of Klaverns of the Third KKK during the 1960s. In contrast to the centrally-organized nature of the Second KKK, the Third Klan is comprised of smaller, isolated branches with estimates suggesting a peak of 10,000 members during the early 1980s (Baudouin, 2011). Despite this, historical BON screenings continue to predict Klan activity in the 1960s (Column 1). Returning briefly to the heterogeneity analysis presented in Figure VI, I find that these effects are driven by counties with high levels of historical Democratic support, racial violence, and religious participation. This differs meaningfully from the near-universal impacts on the Second KKK. Thus, while the film may have catalyzed historical Klaverns across a wide variety of areas, the long-run persistence of those institutions likely depended on pre-existing levels of racial prejudice and social capital.²⁹

I next estimate the film’s impact on the prevalence of hate groups in the 21st century. As shown in Column 2 of Table V, areas that screened BON are significantly more likely to house an institution of hate in recent years. The estimated IV effect of 34.5 percentage points represents a 90% increase over the sample mean and exceeds the raw difference between treatment and control groups ($\mu_{treat}=0.60$; $\mu_{control}=0.32$). Columns 3 through 5 disaggregate this effect by type of hate group. Notably, while some of the effect is attributable to the continued presence of Klan chapters ($\beta_{IV}=0.126$), most of it is due to increases in *other* white supremacist organizations ($\beta_{IV}=0.297$). This effect is large and highly significant. At the same time, I find no effect for other types of hate groups, such as radical Catholics or male supremacists ($\beta_{IV}=0.018$).

²⁹These findings provide further evidence of the role of social capital in the long-run propagation of racial preferences (Satyanath et al., 2017; Jha, 2013) and are consistent with recent work by Bursztyn et al. (2019), who find that social media penetration only incites hate crimes in already-xenophobic areas.

Finally, I examine effects on present-day hate crimes. As shown in Column 6 of Table V, historical BON screenings predict significant increases in hate crimes during the 21st century. The IV estimate of 1.1 hate crimes per 100,000 residents represents a 88% increase over the average annual rate from 2000 to 2018. Columns 7 through 9 disaggregate these effects by victim race. Consistent with the historical effects on lynchings, I document significant increases in modern anti-Black hate crimes (0.47 crimes per 100k, $p = 0.009$). Notably, I find equally large, if slightly less precise, increases in hate crimes directed towards other racial, religious and sexual minorities (0.50 crimes per 100k, $p = 0.054$). In contrast, estimates for anti-white hate crimes are near-zero and insignificant.

Additional validation of these findings is provided in the Appendix. Table A.X replicates the analysis under alternative samples. Table A.XI examines robustness to alternative instruments based on historical theater presence and screenings of *Mickey*. Table A.XII uses alternative screening data from Esposito et al. (2021). Table A.XIII demonstrates robustness to spatial correlation. Table A.XIV leverages Mickey screenings to estimate long-run placebo tests. Finally, Table A.XV replicates the two exogeneity tests used for the historical analyses. First, regressing long-run outcomes on a set of indicators based on the timing of theater openings. Second, estimating reduced form relationships between historical theater presence and long-run outcomes for counties in Kansas, where BON was banned. Together, the results document the *The Birth of a Nation*'s far-reaching racial legacy, extending well beyond the Second KKK to ever newer manifestations of hate.

B Understanding Long-Run Persistence

Several pieces of evidence suggest that the long-run effects on racial hate were mediated by the film's historical seeding of the Second KKK. First, although the Second KKK was officially disbanded in 1944 after being sued by the IRS for \$685,000, its members, practices, and chapters persisted well beyond its dissolution. Many former Klaverns continued to operate independently and were joined over time by new groups that were inspired to adopt the Klan's regalia and rituals (Baudouin, 2011). Though smaller in scale than its predecessor, the Third KKK would continue to shape how racial hate is organized and propagated in the modern era.³⁰

In recent years, Klan members have assisted in the recruitment efforts of other hate groups, appearing at rallies for the neo-Nazi National Socialist Movement and forming al-

³⁰The Klan was also a prominent political actor, at least historically. Five governors and at least four U.S. senators were Klansmen in the Second KKK (Fryer and Levitt, 2012). One of those senators, Hugo Black, would go on to serve on the U.S. Supreme Court until the early 1970s. While this raises the possibility of enduring impacts on politics and governance, the null effects on party vote share found in Figures A.XX and A.XXI and in Fryer and Levitt (2012) suggest limited influence on ballot choice.

liances with racist skinhead organizations. Klan members have also been directly involved in the mass dissemination of hate speech. In 1996, Don Black, a former “Grand Wizard” of the Third KKK, founded the internet’s first major hate website, *Stormfront*. At its peak, the neo-Nazi forum hosted over 300,000 registered users. In addition to propagating a raft of white supremacist and neo-Nazi content and conspiracy theories, *Stormfront* members were responsible for the murders of nearly a hundred people from 2009 to 2014 (Beirich, 2014). Those killings are part of a long history of violent extremism that can be traced back to the Klan. As early as the 1930s, members of the Black Legion, a splinter group of the Second KKK, were convicted of murdering a federal organizer and suspected in the deaths of 56 other Michigan residents (Waldrep, 2000).

Second, while the long-run increases in non-Black hate crimes do not directly accord with the anti-Black antagonism espoused by the First KKK and *The Birth of a Nation*, they are entirely consistent with the practices of future iterations of the Klan. In response to the changing composition of European immigrants, Klan members came to fear that “Catholics, Jews, and foreigners could only undermine the national heritage” and began targeting those groups as means to “maintain a neighborhood status quo” (Jackson, 1992). This again suggests that the causal chain by which the *The Birth of a Nation* impacts modern racial animus runs through the Second KKK.

Finally, as empirical corroboration, I employ the framework proposed by Dippel et al. (2020) for causal mediation analysis in instrumental variables settings. While this method requires strong identifying assumptions – particularly, that BON screenings and long-run outcomes are exogenous, conditional on historical Klaverns – it allows me to unpack the film’s “direct” and “indirect” effects using only a single instrument.³¹ The findings are presented in Appendix Table A.XVI and suggest that nearly all of the long-run effects on racial hate are mediated by the historical formation of Second KKK chapters. Together, the historical record and empirical evidence highlight the critical role that the Klan played in propagating *The Birth of a Nation*’s hateful messages through time.

VIII Conclusion

The Birth of a Nation remains one of the most influential films in cinema history. Decades after its release, scholars would claim that it “propelled film into a new artistic level” from

³¹In effect, I regress long-run outcomes on the mediating variable (historical Klaverns) as instrumented by historical theater presence, controlling for the full set of covariates and my treatment variable (BON screenings). This yields the long-run effect of Klaverns, conditional on BON screenings, which I scale by the effect of BON screenings on Klaverns (from my base IV regression). The resulting “indirect” effect of Klaverns on long-run hate is then compared to the “total” effect of BON screenings on long-run hate.

a “cheap show for cheap people” to a medium “regarded seriously by many intellectuals and sophisticated stage patrons” (Stokes, 2007). Simultaneously, the film was an incredibly virulent work of fiction that glorified white supremacy and perpetuated heinous racial stereotypes.

These same hallmarks provide for a unique case study of the far-reaching consequences of racially-charged entertainment media. In addition to immediate increases in violence, my findings corroborate the film’s prominent role in the rebirth of the Klan under William Simmons. Areas that screened *The Birth of a Nation* became fertile ground for the Second KKK in the following decade. Extending beyond the Jim Crow era, historical showings continue to inform the geography of white supremacist ideology in the 21st century.

These results highlight critical areas for future research. While much of the recent concern around “filter bubbles” and “echo chambers” is centered around selective exposure to partisan media, my results point to the devastating and unintentional impacts of fictional entertainment. Viewed from a modern perspective, the racist imagery permeating *The Birth of a Nation* may be so obvious as to border on harmless caricature. Yet, many of the film’s underlying themes likely persist, if in subtler form, throughout movies and television today. As Alan Moore, creator of *Watchmen*, observed, “a good argument can be made for D.W. Griffith’s *The Birth of a Nation* as the first American superhero movie, and the point of origin for all those capes and masks,” which “are still very much white supremacist dreams of a master race” (Sasaki, 2019). In this light, my findings emphasize the continued importance of interrogating the consequences of even seemingly-innocuous forms of popular entertainment.

More pressingly, this paper demonstrates the power of media to propagate extreme movements and ideologies. The Reconstruction-era KKK was a fringe organization, one that operated largely in secrecy in a handful of Southern states. Yet through *The Birth of a Nation*, a fictionalized version of the Klan was introduced to millions of Americans across the country, leading to the rebirth of the Ku Klux Klan on a scale far larger than had ever existed before. While the media landscape has changed considerably over the past hundred years, this same arc is reflected in the recent spread of extremist ideologies from obscure online forums to mainstream news outlets and ultimately the highest levels of government. Better understanding the role of media in fueling this rise may be critical to mitigating its impact on racial violence and animus.

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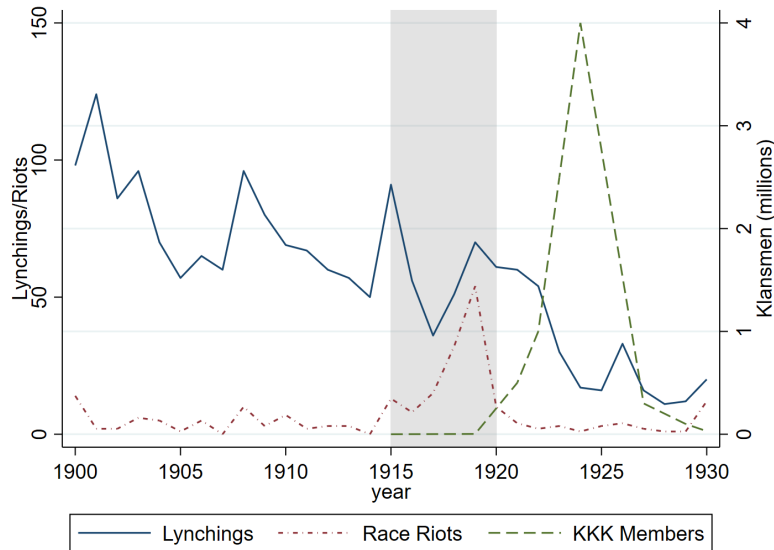
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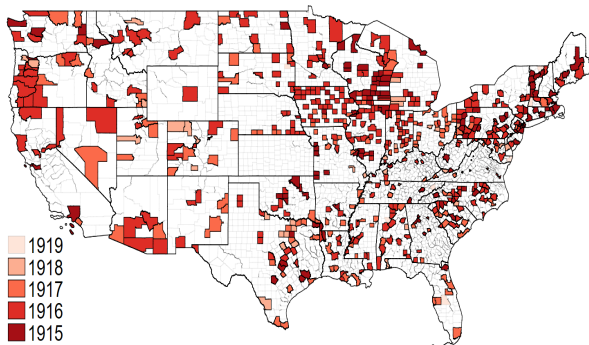
Figure I: Racial Hate over Time



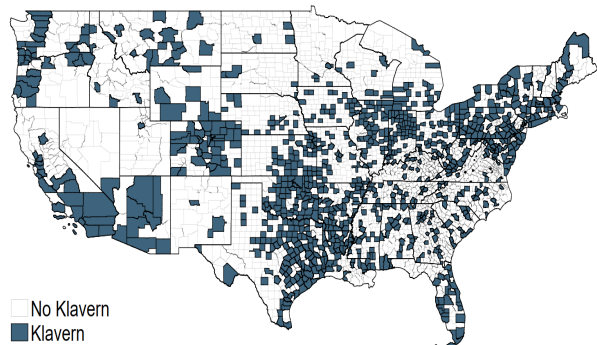
Notes: Figure plots annual lynchings, race riots and Ku Klux Klan members over time. Shaded area represents *The Birth of a Nation*'s roadshow, which began in early 1915 and concluded in late 1919. The KKK had dissolved prior to 1900 and was revived in November 1915, six months after the film's premiere. Lynchings data come from Project HAL and Seguin and Rigby (2019). Race riots data come from Gilje (1996) and the Red Summer Archive. Estimates of Klan membership come from Fryer and Levitt (2012).

Figure II: Mapping *The Birth of a Nation* and Second KKK

Panel A: BON Screenings (1915-1919)



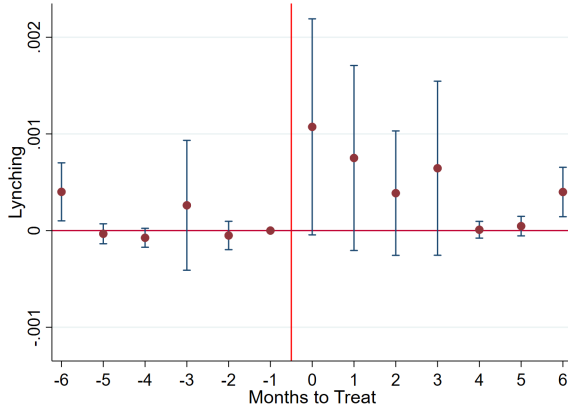
Panel B: Klaverns (1930)



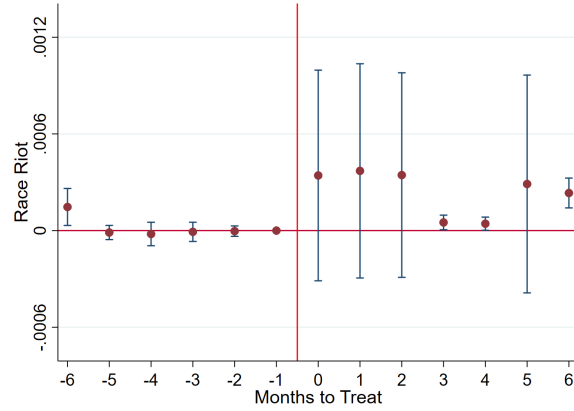
Notes: Panel A displays a map of the *The Birth of a Nation*'s roadshow, color-coded by the year of its first arrival in a county. From 1915 to 1919, the film was shown in 621 counties across every state in the continental U.S., except Kansas, where it was banned statewide by Governor Arthur Capper. Panel B displays a map of counties with Second KKK chapters (i.e., Klaverns) founded by 1930. This includes 972 counties across every state.

Figure III: Contemporaneous Effects on Racial Violence

Panel A: Lynchings

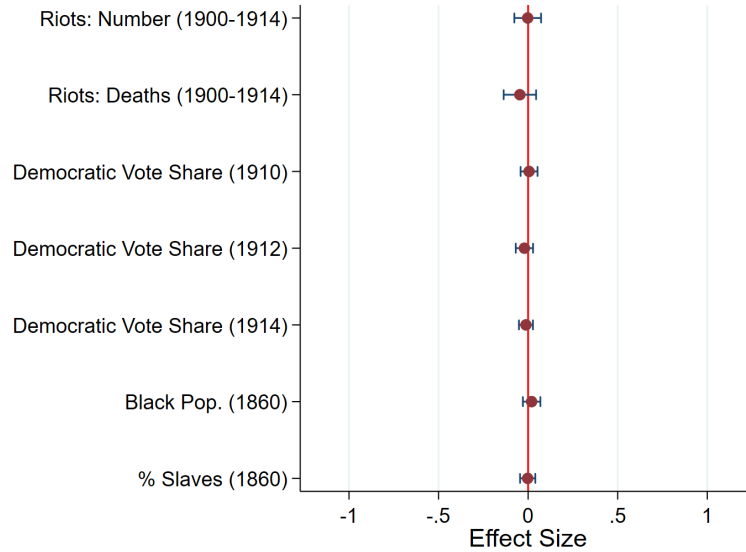


Panel B: Race Riots



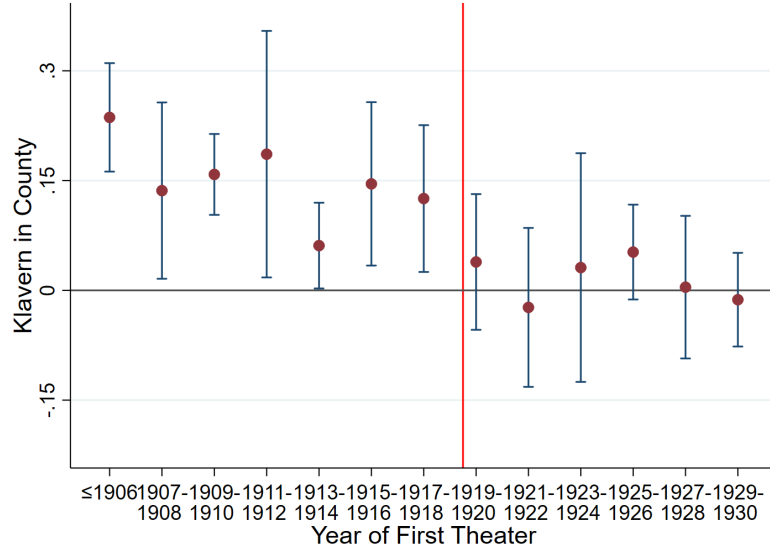
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Sample spans 1913 to 1922. Unit of observation is county-week. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure IV: Exogeneity Tests: Theaters and Historical Racism



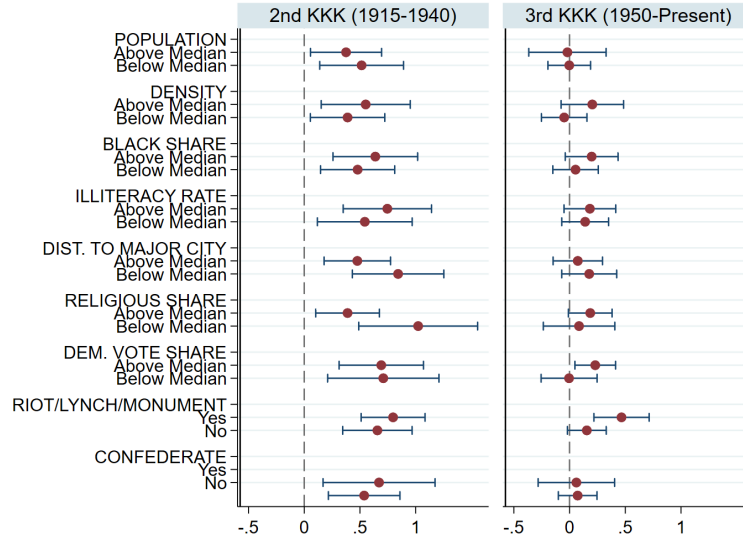
Notes: Figure shows results from series of exogeneity tests regressing historical demographic and racism measures on my primary instrument: whether a county had a theater in 1914. Outcomes include: the number of race riots in a county from 1900 to 1914, the number of deaths from race riots from 1900 to 1914, the share of votes cast for Democratic candidates in Congressional elections in 1910, 1912 and 1914, and total Black population and share of Blacks who were enslaved in 1860. All outcomes are standardized to mean = 0 and standard deviation = 1. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state.

Figure V: Exogeneity Tests: Theater Timing and the Second KKK



Notes: Figure displays estimates and 95% confidence intervals from a regression of Klavern formation on a set of mutually-exclusive indicator variables corresponding to the year of a county's first theater was opened. The outcome of interest is a dummy for whether a county had a Klavern by 1930. The omitted group are counties that had not opened a theater by 1930. Regressions include all demographic, social capital, media and racism controls described in Table III. Red vertical line corresponds to the end of *The Birth of Nation's* roadshow. Standard errors clustered by state.

Figure VI: Heterogeneous Effects on Klavern Formation



Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BON screenings on Klavern formation. Each dot represents a separate regression on a different county sub-sample. Outcomes of interest are: whether a county had a Klavern of the Second KKK by 1930 (left panel) and whether a county had an *active* Klavern of the Third KKK in the 1960s or 2000s (right panel). Riot/Lynch/Monument are areas that experienced a lynching or race riot from 1900-1905 or had a Confederate monument by 1914. Confederate refers to counties in former Confederate states (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX and VA). All other sub-samples are based on area characteristics in 1910. Regressions include all controls described in Table III except the dimension of heterogeneity. Standard errors clustered by state.

Table I: Summary Statistics

	Treatment Counties		Control Counties				(Treat-Ctrl Thtr)
			All		Theater		t-stat
<i>Demographics</i>							
Population (000s)	75.51	(240.93)	18.13	(21.29)	25.33	(27.84)	5.19
Density (mi)	389.24	(1826.72)	51.97	(361.68)	83.21	(521.71)	4.09
% Black	11.88	(19.79)	11.93	(20.39)	7.94	(15.80)	4.00
Pop Black (000s)	6.21	(13.00)	2.38	(4.72)	2.03	(4.45)	7.63
% U.S. born	84.20	(19.24)	80.91	(29.65)	86.35	(19.02)	-2.35
% Draft Eligible	9.84	(1.78)	9.67	(2.69)	9.94	(1.84)	-1.14
<i>Social Capital</i>							
Dist. to Major City (mi)	515.26	(535.16)	518.96	(481.60)	524.33	(535.78)	-0.25
% Urban	15.08	(29.19)	1.13	(8.43)	2.38	(12.11)	10.47
% Illiterate	5.59	(6.26)	6.40	(6.79)	4.79	(5.54)	2.39
% Religious	33.89	(13.72)	28.51	(15.79)	30.84	(13.76)	4.73
Occupation Score	6.83	(1.70)	5.54	(1.92)	6.32	(1.61)	6.57
% Turnout (1912)	16.76	(9.64)	15.51	(9.75)	18.49	(8.45)	-3.47
<i>Newspapers</i>							
Circulation per Cap. (1912)	0.16	(0.22)	0.03	(0.08)	0.05	(0.11)	12.05
Markets (1912)	1.30	(1.71)	0.47	(0.83)	0.81	(1.02)	6.72
<i>Theaters</i>							
Any (1914)	0.98	(0.15)	0.48	(0.50)	1.00	(0.00)	-
Per 1k Pop (1914)	0.14	(0.10)	0.07	(0.12)	0.15	(0.13)	-1.35
Max Seats (1914)	786.94	(808.39)	176.29	(361.20)	370.95	(449.87)	12.29
<i>Racism</i>							
% Democrat (1912)	47.95	(20.96)	48.95	(23.03)	46.17	(17.99)	1.47
Confed. Monuments (1914)	0.29	(0.78)	0.15	(0.42)	0.13	(0.44)	4.46
Lynchings (1900-1905)	0.19	(0.78)	0.14	(0.56)	0.13	(0.56)	1.64
% NAACP (1914)	2.90	(16.79)	0.20	(4.48)	0.34	(5.81)	3.70
% Confederate	27.70	(44.79)	39.11	(48.81)	22.29	(41.64)	2.10
Counties	621		2,483		1,181		1,787

Table presents summary statistics for counties that screened *Birth of a Nation* (treatment), for counties that did not (control) and for the subset of control counties with a theater in 1914. For each group, means and standard deviations (in parentheses) are reported. T-statistics comparing means across treatment and control counties with theaters are shown in the rightmost column. Demographic and social capital variables come from 1910 Census, with the exceptions of % Religious which is drawn from the 1906 Census of Religious Bodies and railroad distance to nearest major city (i.e., 10 largest cities in 1910) which is derived from Attack (2017). Newspaper variables come from Gentzkow et al. (2011). Theaters data refers to county stock of theaters in 1914. *Any* refers to a county that had at least one theater, *Per 1k Pop* is the number of theaters per 1,000 white residents and *Max Seats* refers to the seating capacity of the largest theater. *Democrat* is the share of votes cast for Woodrow Wilson in the 1912 presidential election. *NAACP* are counties that had an NAACP chapter in 1914. *Confederate* are counties in former Confederate states (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX and VA).

Table II: Exogeneity Tests: Theaters and the Second KKK

Indep. Var.	DV = Klavern by 1930				
	Thtrs > 0 (1)	Thtrs per 1k White (2)	Max Thtr Capacity (3)	First Thtr Year (4)	First Thtr Year (5)
Panel A: Counties in Kansas (BON Banned)					
β_{RF}	-0.134 (0.111)	-0.172 (0.379)	-0.205 (0.151)	0.007 (0.005)	0.009 (0.011)
Mean	0.352	0.352	0.352	0.352	0.457
Obs.	105	105	105	105	70
Panel B: Counties outside Kansas (BON Allowed)					
β_{RF}	0.146*** (0.025)	0.231*** (0.072)	0.130*** (0.030)	-0.008*** (0.001)	-0.009*** (0.002)
Mean	0.312	0.312	0.312	0.312	0.384
Obs.	2,998	2,998	2,998	2,998	2,179
Counties	All	All	All	All	Thtr by 1930
Controls	✓	✓	✓	✓	✓

Notes: Table shows reduced form relationships between measures of historical theater stock and Klavern formation in 1930. Panel A examines counties in Kansas, where BON was banned statewide until the mid-1920s. Panel B examines counties in all other states, where BON was not banned. Coefficients generated from reduced form estimation of Equation 2 including all demographic, social capital, media and racism controls described in Table III. Column 1 examines my preferred instrument: an indicator for whether a county had a theater in 1914. Column 2 examines the number of theaters in 1914 per 1,000 white residents. Column 3 examines the number of seats available in a county's largest theater in 1914, imputting the median non-missing value (700) for counties with theaters that are missing seat information. Column 4 examines the year a county's first theater was opened. Column 5 examines the same variable and restricts to counties where a theater was opened by 1930. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table III: First-Stage Effects on *The Birth of a Nation* Screenings

	DV = Screened in County				
	(1)	(2)	(3)	(4)	(5)
Theater	0.324*** (0.024)	0.276*** (0.025)	0.239*** (0.024)	0.213*** (0.023)	0.211*** (0.023)
Mean	0.200	0.200	0.200	0.200	0.200
Demographics		✓	✓	✓	✓
Social Capital			✓	✓	✓
Media				✓	✓
Racism					✓
F-Stat	185.34	125.63	102.75	83.90	81.97
Obs.	3,103	3,103	3,103	3,103	3,103
R-sq (adj.)	0.201	0.266	0.296	0.319	0.319

Table shows first-stage effects of theater stock in 1914 on *Birth of a Nation* screenings. The outcome of interest is a dummy for whether BON was screened in a county from 1915 to 1919. *Theater* is a dummy for whether a county had a theater in 1914. Demographic controls include a quadratic of total population, population density, Black population, Black population share, U.S. born population share and the share of individuals who would have been of draft-eligible age during World War I. All measures come from the 1910 Census. Social capital controls include urban population share, illiteracy rate, and average occupational income score in 1910, voter turnout in 1912, as well as quadratics of the railroad distance to the two nearest major cities (i.e., the 10 largest cities in 1910) and the share of residents in religious organizations (from the 1906 Census of Religious Bodies). Media controls include per capita newspaper circulation and the number of media markets in a county in 1912 from Gentzkow et al. (2011). Racism controls include the number of historical lynchings in a county from 1900 to 1905, Democratic vote share in 1912, the number of Confederate monuments in 1914 and the presence of an NAACP chapter in 1914. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table IV: Historical Effects on the Second KKK

	DV = Klavern by 1930					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Ordinary Least Squares</i>						
Screened	0.099*** (0.020)	0.108*** (0.023)	0.134*** (0.032)	0.053** (0.021)	0.088*** (0.019)	0.116*** (0.026)
<i>Reduced Form</i>						
Theater	0.144*** (0.024)	0.143*** (0.029)	0.191*** (0.036)	0.102*** (0.021)	0.122*** (0.026)	0.182*** (0.037)
<i>Two-Stage Least Squares</i>						
$\widehat{\text{Screened}}$	0.685*** (0.144)	0.479*** (0.104)	0.537*** (0.112)	0.563*** (0.139)	0.593*** (0.156)	0.523*** (0.120)
Mean	0.313	0.353	0.356	0.308	0.386	0.423
Counties	All	Neighbors Only	Drop Neighbors	Drop 20 Largest	Thtr by 1930	Paper in 1915
Controls	✓	✓	✓	✓	✓	✓
F-Stat	81.97	191.20	46.01	57.60	76.67	35.97
Obs	3,103	2,161	1,560	3,083	2,249	1,392

Table shows estimates from Equation 2 of the effect of BON screenings on historical Klavern formation. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. Screened is an indicator for whether BON was screened in a county from 1915 to 1919. Theater is an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Column 1 includes all counties in the continental United States. Column 2 restricts the control group to counties adjacent to treatment counties. Column 3 excludes control counties adjacent to treatment counties. Column 4 drops the 20 largest counties by 1910 population. Column 5 restricts to counties that had at least one theater by 1930. Column 6 restricts to counties with at least one digitized local newspaper from 1915. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table V: Long-Run Effects on Racial Hate

Dep. Var.	KKK (1960s)	Hate Group (2000-2019)				Hate Crimes per 100k (2000-2018)			
	(1)	Any	KKK	White Supr	Other	All	Anti-Black	Anti-Other	Anti-White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Ordinary Least Squares</i>									
Screened	0.021 (0.015)	0.112*** (0.024)	0.055** (0.024)	0.100*** (0.023)	0.021 (0.015)	0.154* (0.076)	0.028 (0.032)	0.098** (0.046)	0.027 (0.020)
<i>Reduced Form</i>									
Theater	0.031* (0.016)	0.073*** (0.023)	0.026* (0.015)	0.063*** (0.019)	0.004 (0.010)	0.225** (0.097)	0.098*** (0.033)	0.106** (0.053)	0.021 (0.044)
<i>Two-Stage Least Squares</i>									
$\widehat{\text{Screened}}$	0.147** (0.070)	0.345*** (0.100)	0.126* (0.066)	0.297*** (0.085)	0.018 (0.048)	1.068** (0.488)	0.465*** (0.171)	0.503* (0.254)	0.100 (0.211)
Mean	0.106	0.374	0.186	0.279	0.059	1.372	0.465	0.678	0.23
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
F-Stat	81.97	81.97	81.97	81.97	81.97	81.97	81.97	81.97	81.97
Obs	3,103	3,103	3,103	3,103	3,103	3,103	3,103	3,103	3,103

Table shows estimates from Equation 2 of the effect of BON screenings on long-run racial hate. For Column 1, the outcome is an indicator for whether a Klavern of the Third KKK was *active* in a county during the 1960s, based on contemporaneous reports from the House Un-American Activities Committee compiled by (Mazumder, 2018). For Columns 2 through 5, outcomes are indicators for whether a hate group was active in a county any time from 2000 to 2018, based on reporting from the Southern Poverty Law Center. *KKK* refers to Klaverns of the Third KKK, *WhiteSupr* refers to other (non-KKK) white supremacist groups (i.e., white nationalists, racist skinheads, neo-Volkischs, neo-Nazis, neo-Confederates, Holocaust deniers, Christian identity, Anti-Muslim and Anti-LGBTQ), *Other* refers to non-white supremacist hate groups (i.e., “radical Catholicism”, “male supremacy”, “hate music” and general hate). For Columns 6 through 9, the outcome is the average annual number of hate crimes in a county per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports. *Anti – Black* refers to hate crimes against Blacks, *Anti – White* are hate crimes against whites and *Anti – Other* are hate crimes against other racial, sexual or religious minority groups. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors are clustered by state. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

The Birth of a Nation: Media and Racial Hate

Desmond Ang

Online Appendix

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Supplementary Figures

Figure A.I: Advertisement for *The Birth of a Nation*

Temple Theatre
ALTON

Same Company and Orchestra Now
Playing St. Louis.

Tuesday, Nov. 30--Wednesday, Dec. 1
Matinee Daily 2:15 P. M. Nights 8:15 Sharp

THE GREAT AMERICAN PLAY AT LAST
D. W. Griffith's EIGHTH WONDER OF THE WORLD

18,000 People
3,000 Horses
Cost \$500,000
Took 8 Months to Produce

THE BIRTH OF A NATION

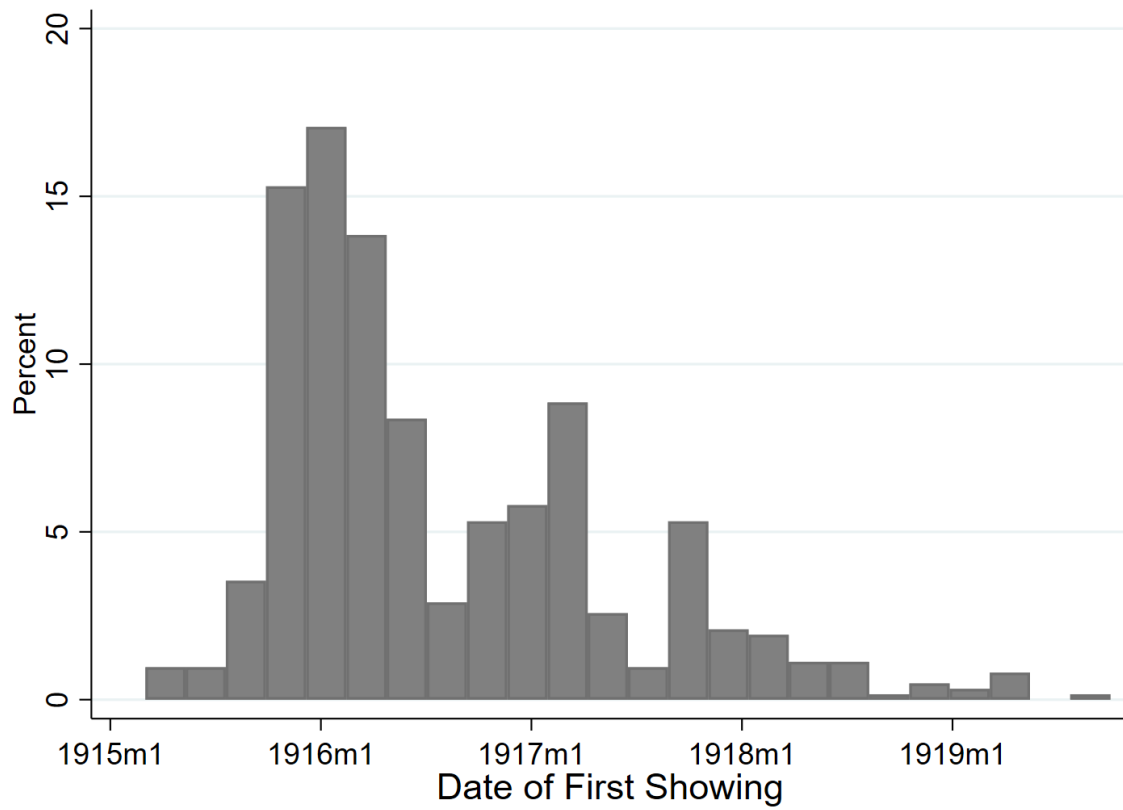
A Red Blooded Tale of True American Spirit
Love and Romance Midst Historic Scenes
Gripping Heart Interest and Soul Stirring Emotions

30--SYMPHONY ORCHESTRA OF--30

Prices Night - - - - - 25c to \$2.00
Matinee - - - - - 25c to \$1.00 **Seats Thursday**

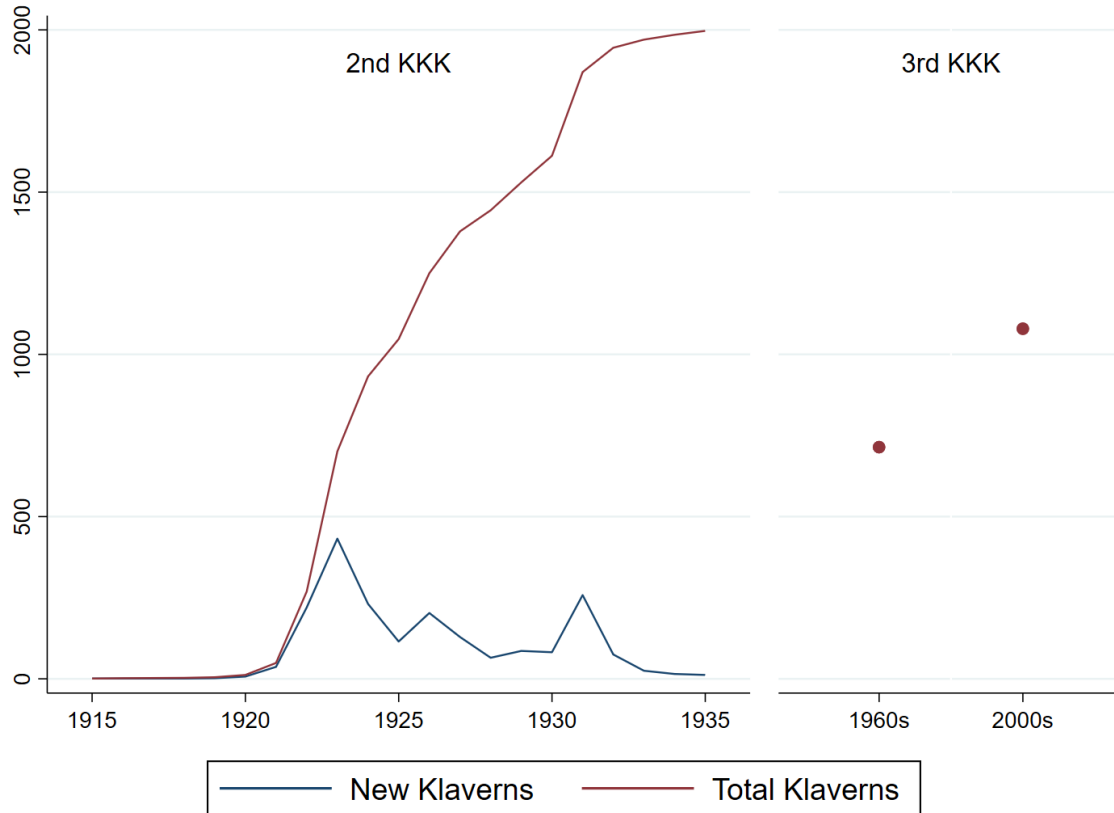
Notes: Figure shows an example advertisement for a screening of *The Birth of a Nation*.

Figure A.II: Distribution of Roadshow Arrival Dates



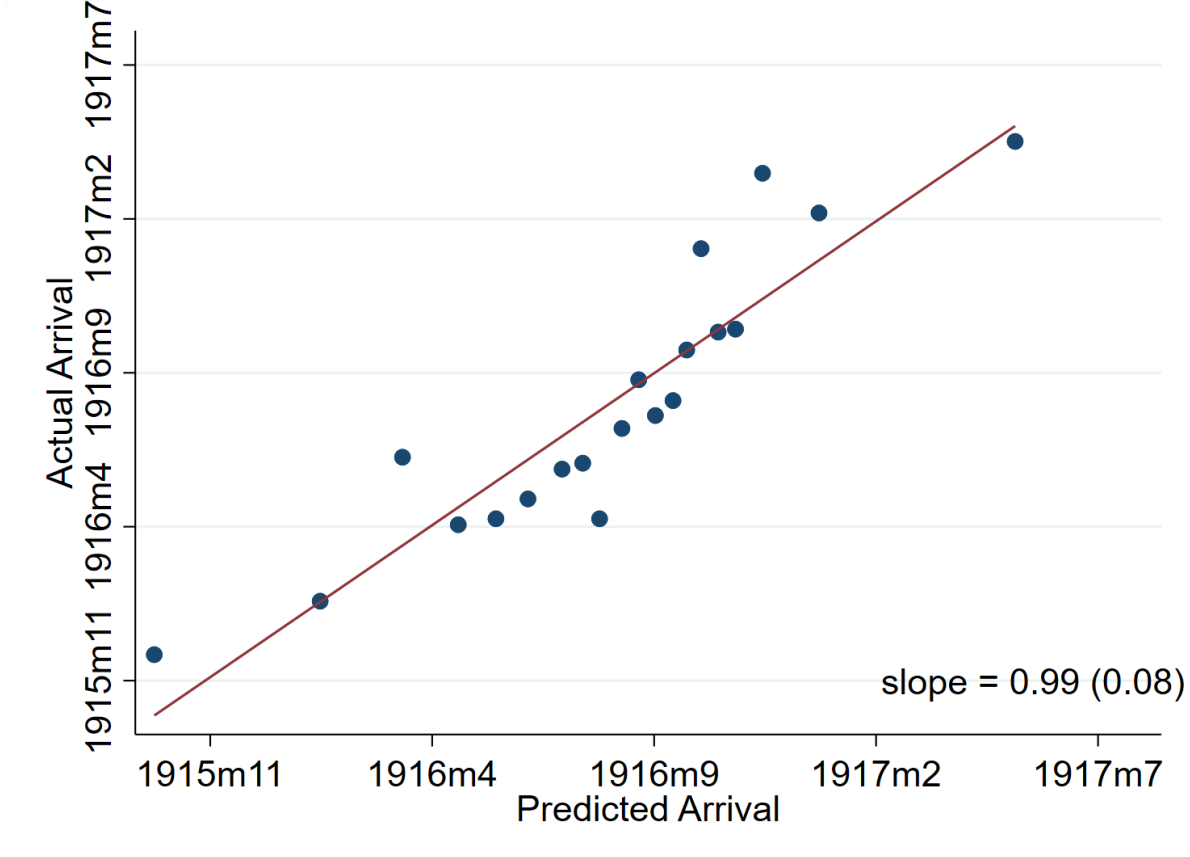
Notes: Figure plots a histogram of *The Birth of a Nation*'s county premiere dates during its roadshow from 1915 to 1919.

Figure A.III: Klaverns over Time



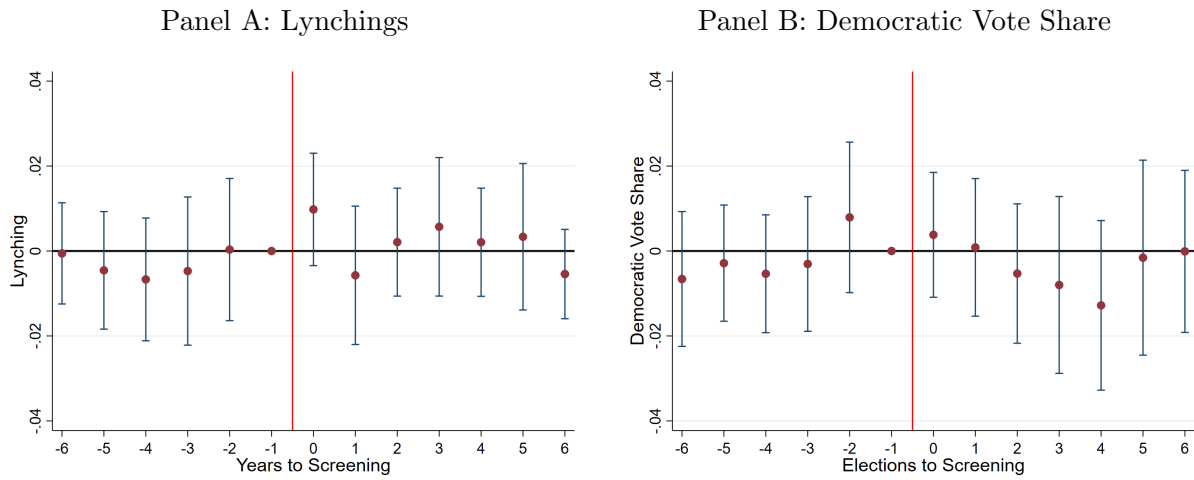
Notes: Figure shows number of Ku Klux Klan chapters (“Klaverns”) over time. Red line represents cumulative number of Klaverns of the Second KKK founded by a given year. Blue line represents number of new Klaverns of the Second KKK founded in a given year. Red dots represent total number of active Klaverns of the Third KKK in the 1960s and 2000s, respectively. Data on the Second KKK come from Kneebone and Torres (2015). Data on the Third KKK in the 1960s comes from Mazumder (2018). Data on the Third KKK in the 2000s comes from the Southern Poverty Law Center.

Figure A.IV: Relationship between Predicted and Actual Arrival Dates



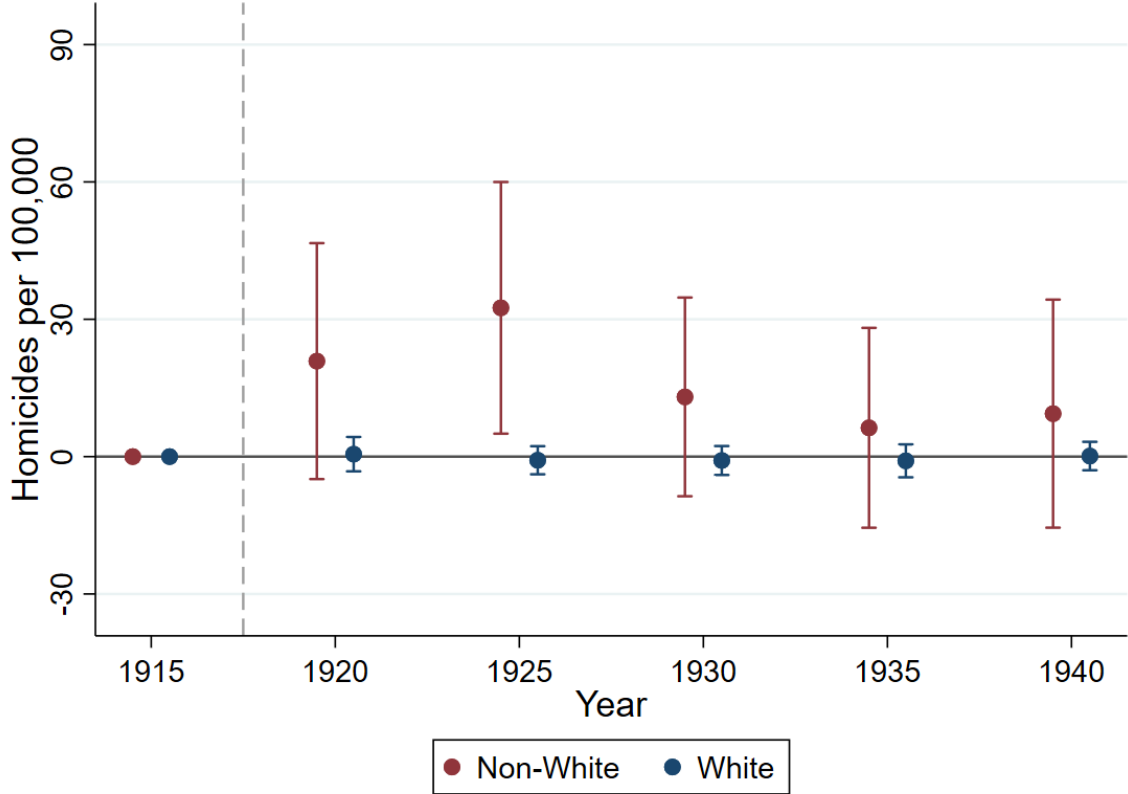
Notes: Figure displays binscatter of predicted BON screening dates against actual BON screening dates among treatment counties. Predicted screening dates come from regressing actual screening dates on all demographic, social capital, media and racism controls described in Table III, region fixed effects as well as the screening dates in the county's two nearest large cities (i.e. those in the top 10 in 1910 population) and their interaction with railroad distance. Each dot represents 5% of the sample. Red line represents slope coefficient from bivariate regression of $\widehat{ScreenedDate}$ on $ScreenedDate$. Standard errors are clustered by state and shown in parentheses.

Figure A.V: Event Study on Lynchings and Democratic Vote Share: Extended Panel



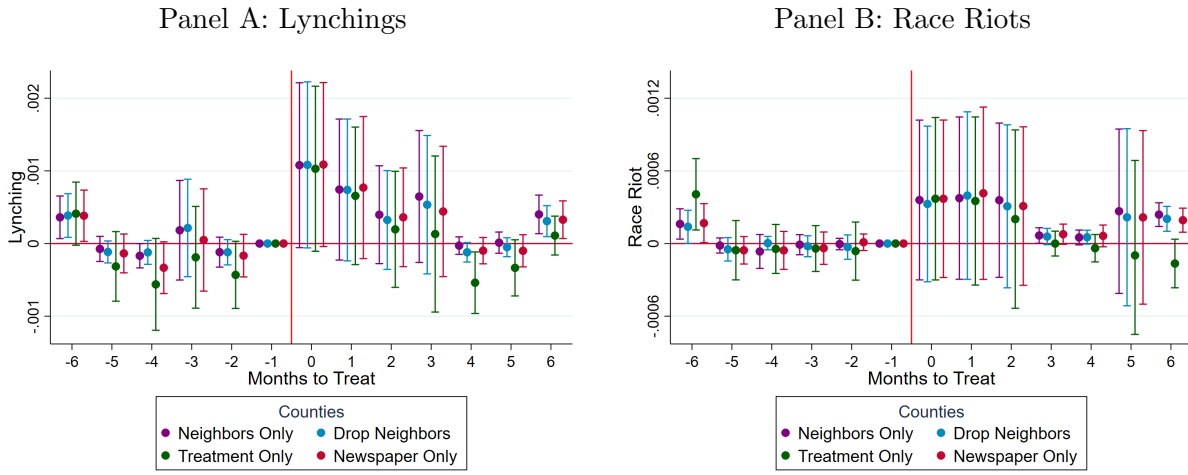
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on lynchings and Democratic vote share using an extended panel. For Panel A, the unit of observation is the county-year and the outcome is whether a county had a lynching in a year. For Panel B, the unit of observation is the election-year and the outcome is the share of major party Congressional votes cast for Democratic candidates. Sample spans 1900 to 1930. Standard errors clustered by state.

Figure A.VI: Event Study on State Homicide Rates



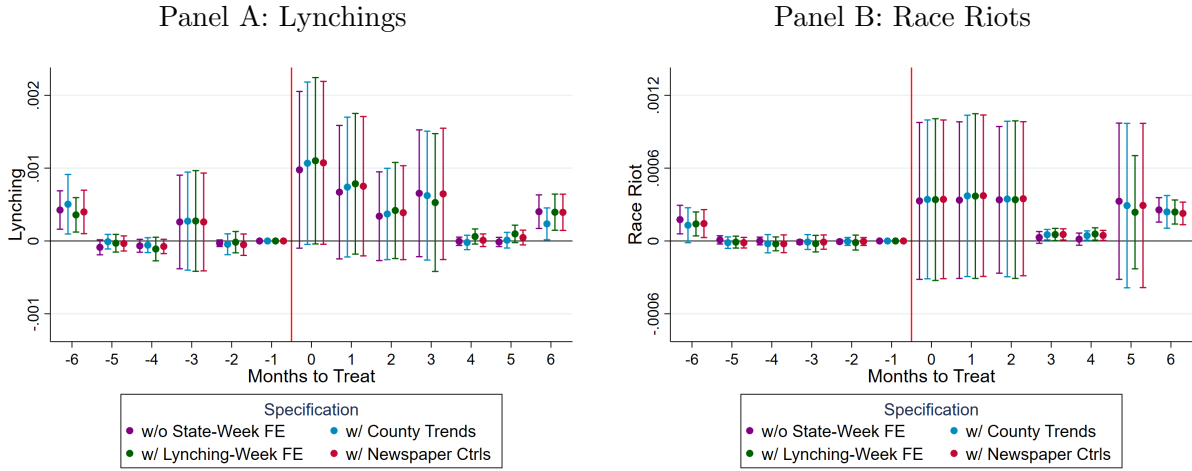
Notes: Figure shows event study estimates and 95% confidence intervals of the effect of BON screenings on state homicide rates. Specifically, I estimate $y_{s,t} = \delta_s + \lambda_{r,t} + \sum_{\tau \neq 1915}^{1940} \beta_{\tau} \text{ShareScreened}_s \times I_{t,\tau} + \epsilon_{s,t}$, where $y_{s,t}$ is the homicide rate for a given race group in state s at year t and $\lambda_{r,t}$ are Census region-year fixed effects. Coefficients of interest are β_{τ} on the interaction between ShareScreened_s , the share of native white adult males in state s living in screened counties in 1920, and year indicators. Red dots examine non-white homicide rates. Blue dots examine white homicide rates. Data is reported every five years and come from *Vital Statistics Rates in the United States 1900-1940*. The sample includes the 25 registration states with race-specific homicide rates available from 1915 or earlier. Heteroskedasticity-robust standard errors are reported.

Figure A.VII: Contemporaneous Effects on Racial Violence: Alternative Samples



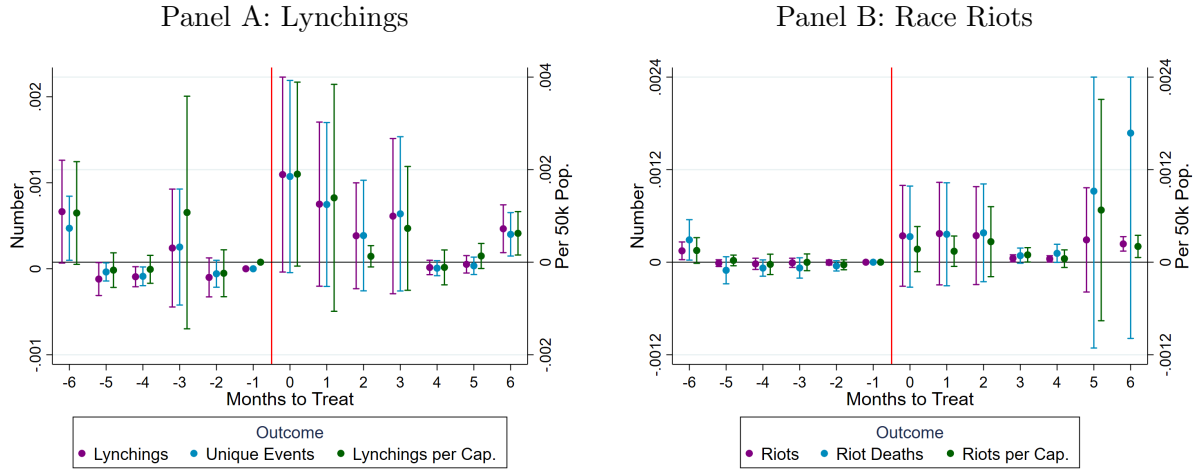
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence using alternative samples. Purple dots limit the control group to counties that did not receive the film but that are adjacent to counties that did. Blue dots instead exclude neighboring control counties from the sample. Green dots limit the sample to treatment counties only (i.e., those that screened BON during its roadshow from 1915 to 1919). Red dots restrict the sample to counties with at least one digitized newspaper in 1915. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Unit of observation is county-week. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure A.VIII: Contemporaneous Effects on Racial Violence: Alternative Specifications



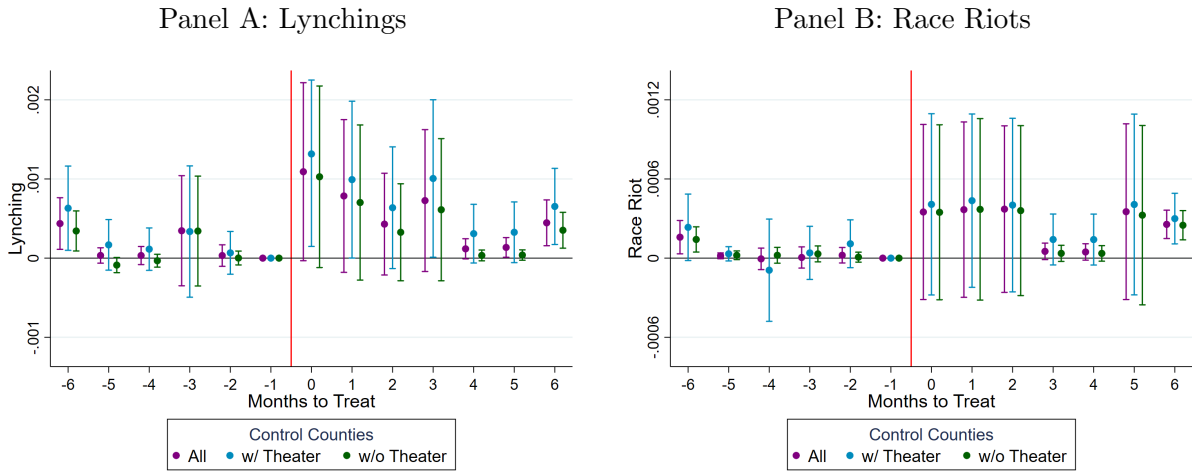
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence under alternative specifications. Purple dots replace state-week fixed effects with week fixed effects. Blue dots add county-specific linear time trends. Green dots add interactions between week fixed effects and the number of lynchings in a county from 1900 to 1905. Red dots include time-varying controls for the number of digitized newspapers in a county-year. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Unit of observation is county-week. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure A.IX: Contemporaneous Effects on Racial Violence: Alternative Outcomes



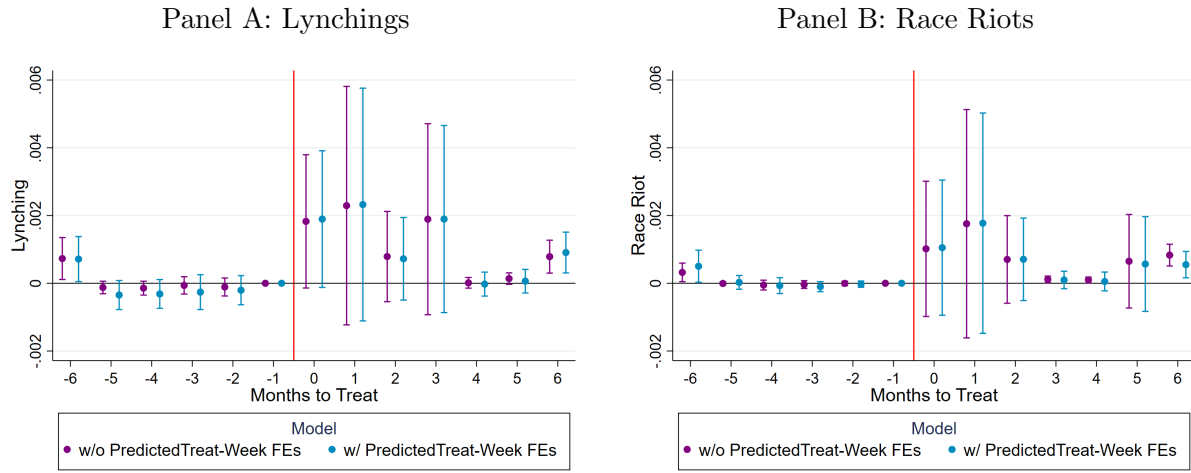
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence using alternative outcome measures. In Panel A, purple dots examine the number of lynchings, blue dots examine number of unique lynching events (i.e. number of days in a county-week with at least one lynching), and green dots examine lynchings per 50,000 residents. In Panel B, purple dots examine number of race riots, blue dots examine number of deaths from race riots, and green dots examine riots per 50,000 residents. Per capita effects are plotted against the right axis, all other effects are plotted against the left axis. Unit of observation is the county-week. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure A.X: Contemporaneous Effects on Racial Violence: Matched Samples



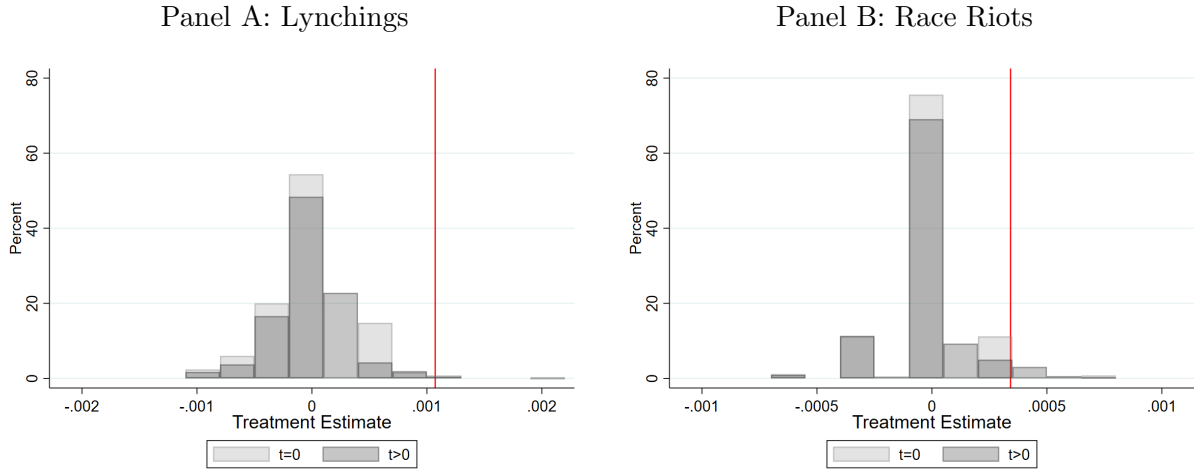
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence using matched control samples from propensity score matching. Treatment counties are matched to control counties in the same state using Epanechnikov kernel functions and the full set of covariates described in Table III. Purple dots match treatment counties to the full set of control counties. Blue dots match only to control counties with theaters in 1914. Green dots match only to control counties without theaters in 1914. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Unit of observation is the county-week. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure A.XI: Contemporaneous Effects on Racial Violence: Predicted Treatment



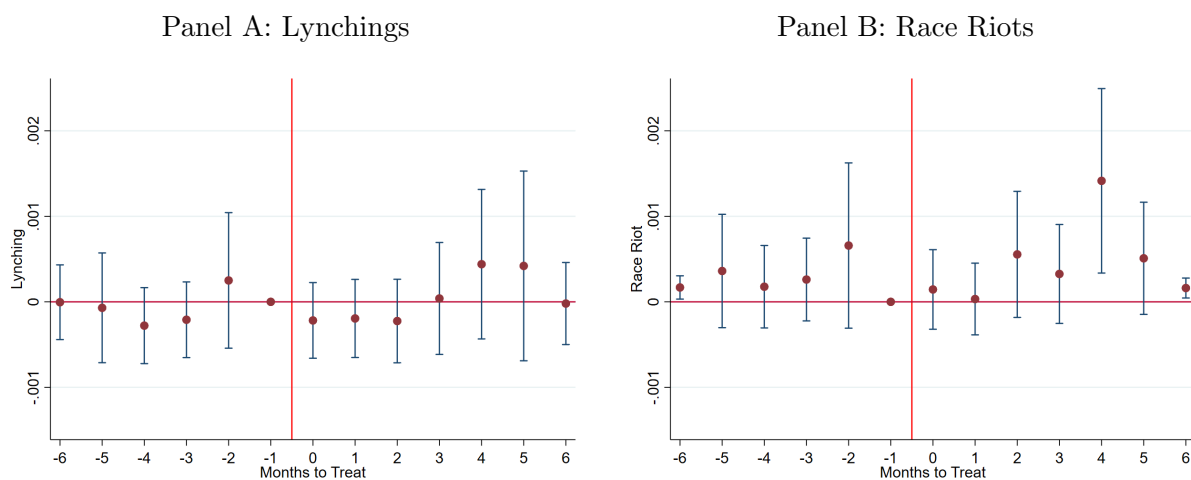
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of BON screenings on racial violence using predicted instead of actual screenings. BON screenings are predicted from first-stage regression presented in Equation 3 using theater presence in 1914 and all demographic, social capital, media and racism controls. Blue dots estimates my primary model substituting predicted for actual treatment. Purple dots further control for interactions between predicted treatment and week fixed effects. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Unit of observation is the county-week. Sample spans 1913 to 1922. Months to treat represent five-week intervals centered on the week of BON's arrival in a county. Standard errors clustered by state.

Figure A.XII: Contemporaneous Effects on Racial Violence: Randomization Inference



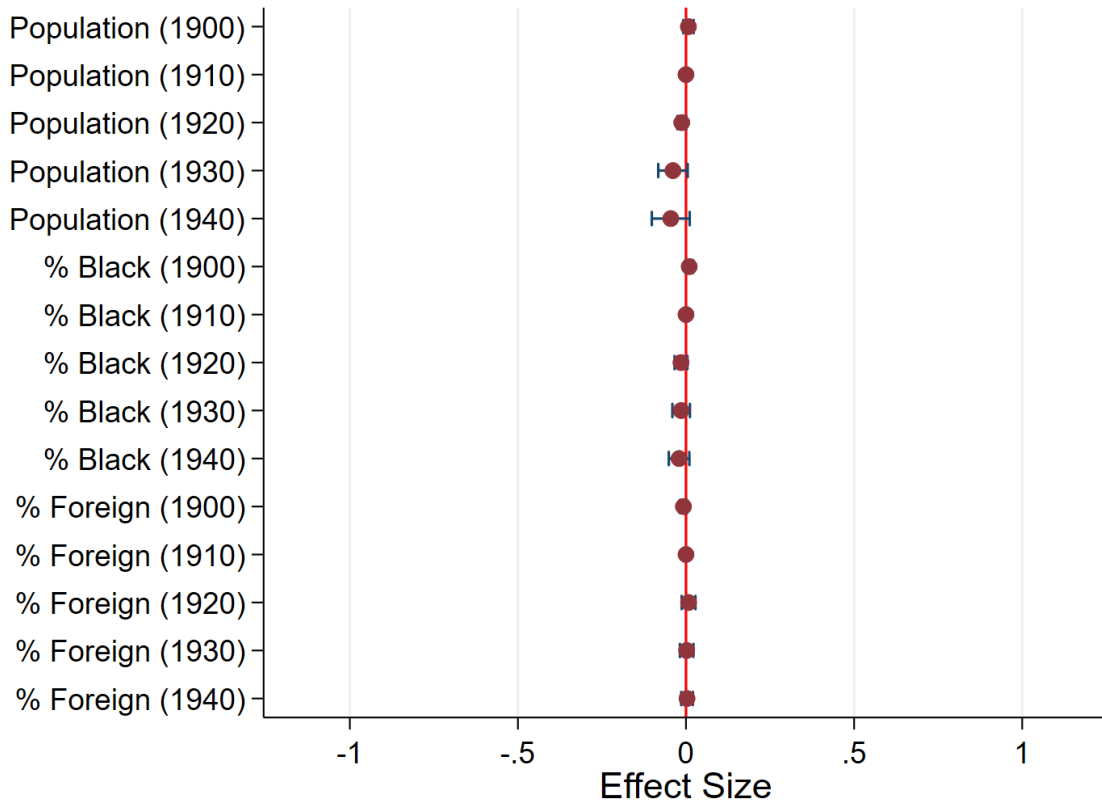
Notes: Figures show histogram of event study estimates from a series of 500 placebo regressions on lynchings (Panel A) and race riots (Panel B). In each regression, I randomly assign movie arrival dates in a state to counties in the same state (drawing on the actual distribution of arrival dates in a state without replacement) and estimate Equation 1 using the placebo treatment dates. Light gray bars indicate distribution of placebo treatment estimates for $\tau = 0$ (i.e., β_0), dark gray bars indicate distribution of placebo treatment estimates for $\tau > 0$ (i.e., β_1 to β_6). Red vertical line represents the true treatment estimates for $\tau = 0$, as shown in Figure III (i.e., 0.001 for lynchings and 0.0003 for race riots). Unit of observation is the county-week. Months to treat represent five-week intervals.

Figure A.XIII: Contemporaneous Effects on Racial Violence: *Mickey* Placebo



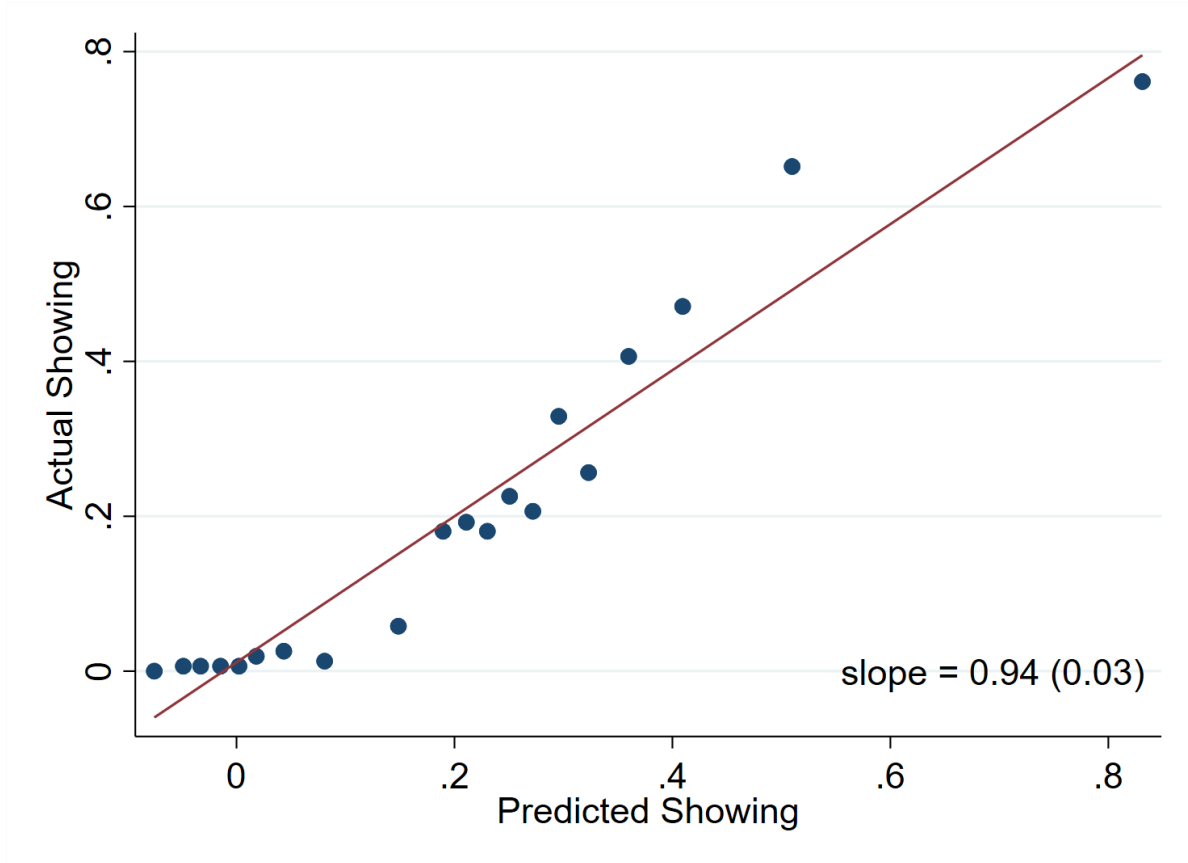
Notes: Figure shows event study estimates and 95% confidence intervals from Equation 1 of the effect of the the 1918 film *Mickey* on racial violence. Outcomes of interest are indicators for whether a county experienced a lynching (mean = 0.0003) or race riot (mean = 0.00009) in a week. Unit of observation is the county-week. Sample spans 1915 to 1923. Months to treat represent five-week intervals centered on the week of *Mickey*'s arrival in a county. Standard errors clustered by state.

Figure A.XIV: Exogeneity Tests: County Demographics



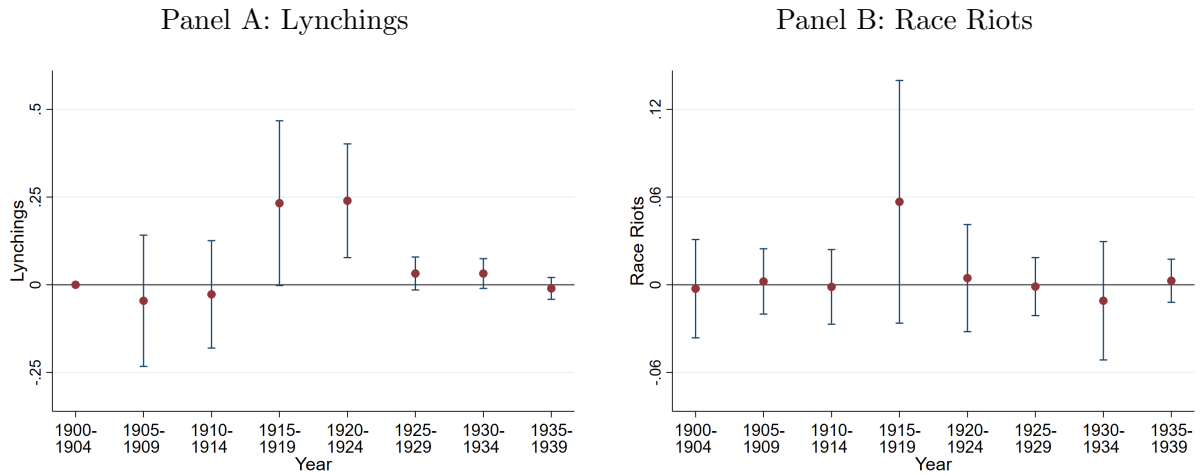
Notes: Figure shows results from series of exogeneity tests regressing alternative outcomes on my instrument: whether a county had a theater in 1914. Outcomes include: county population, Black population share and foreign population share from 1900 to 1940. All outcomes are standardized to mean = 0 and standard deviation = 1. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state.

Figure A.XV: Relationship between Predicted and Actual Showings



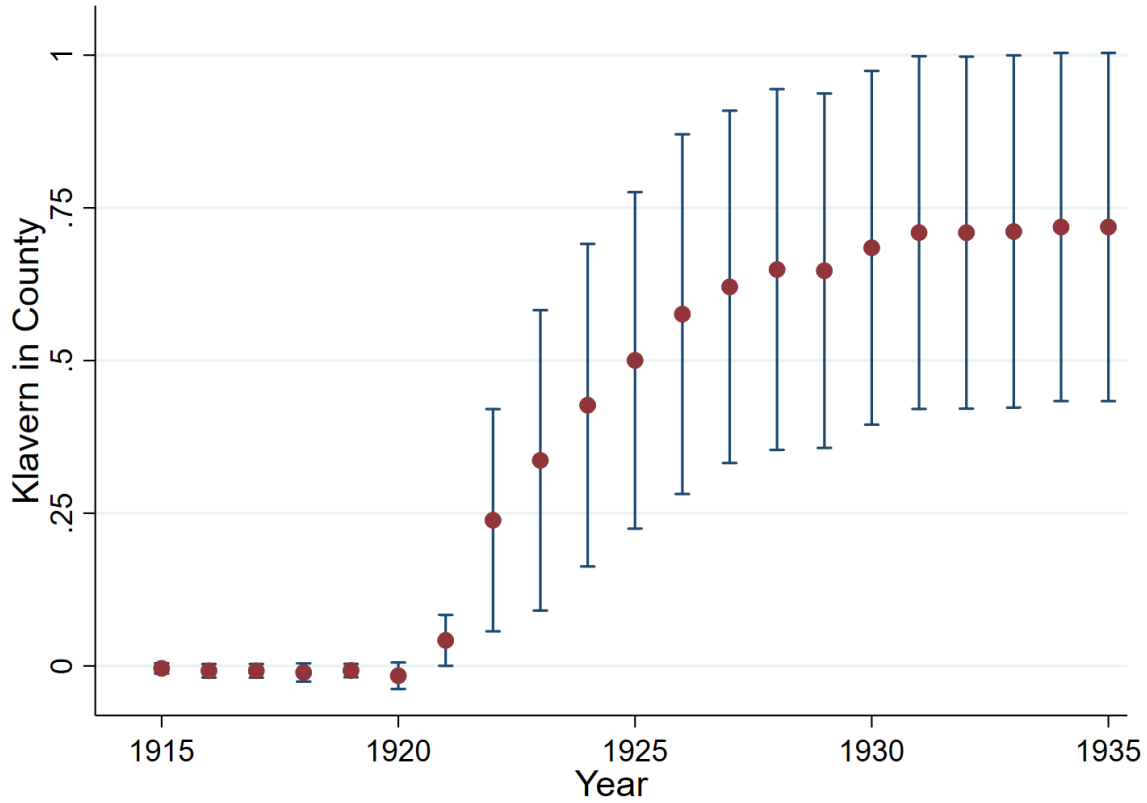
Notes: Figure displays binscatter of predicted likelihood of screening *The Birth of a Nation* against actual screenings of the film. Predicted likelihood comes from first-stage regression (Equation 3) including all demographic, social capital, media and racism controls described in Table III (i.e., predicted values from Column 5 of Table III). Each dot represents 5% of the sample. Red line represents slope coefficient from bivariate regression of *Screened* on $\widehat{Screened}$. Standard errors are clustered by state and shown in parentheses.

Figure A.XVI: Historical Effects on Racial Violence: IV Correspondence



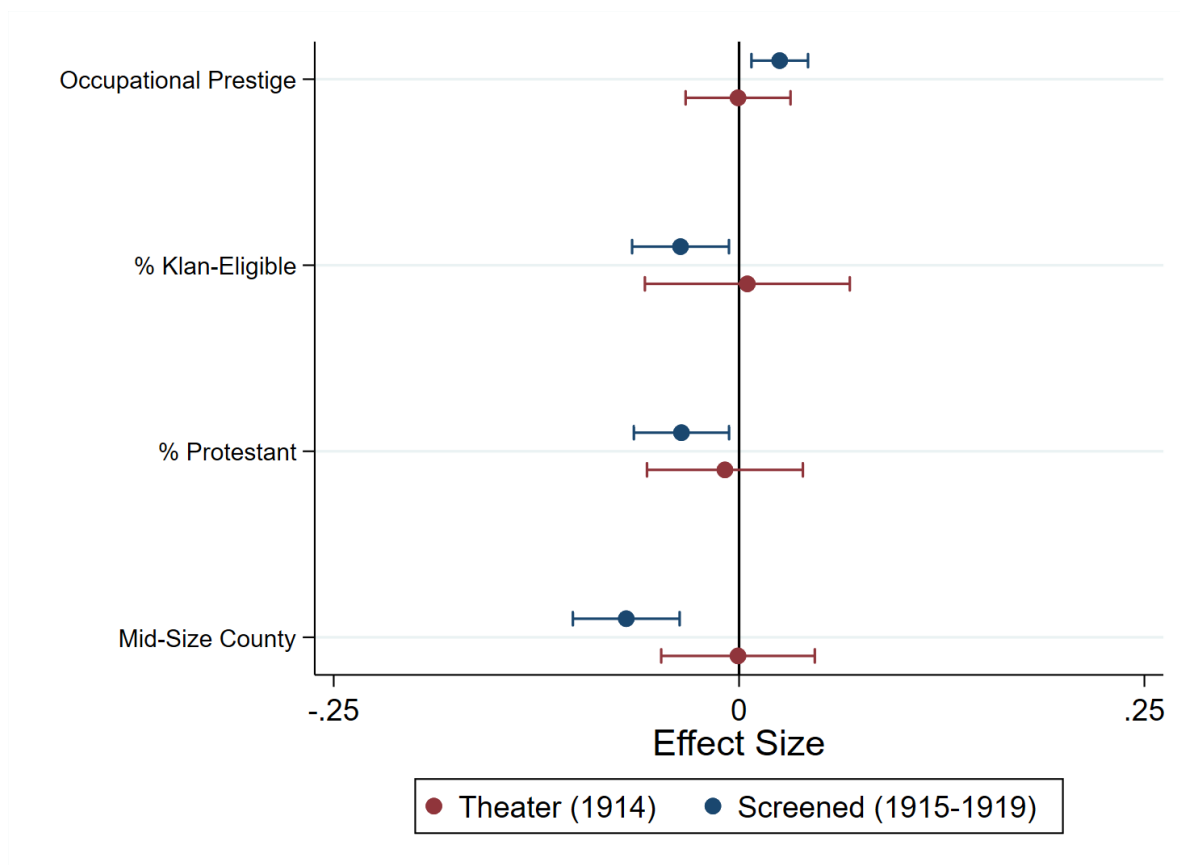
Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BONA screenings on racial violence. Each dot represents a separate regression. The outcomes of interest are the number of lynchings in a county over a five-year window (Panel A) and the number of race riots in a county over a five-year window (Panel B). Screenings are instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors are clustered by state.

Figure A.XVII: Historical Effects on the Second KKK by Year



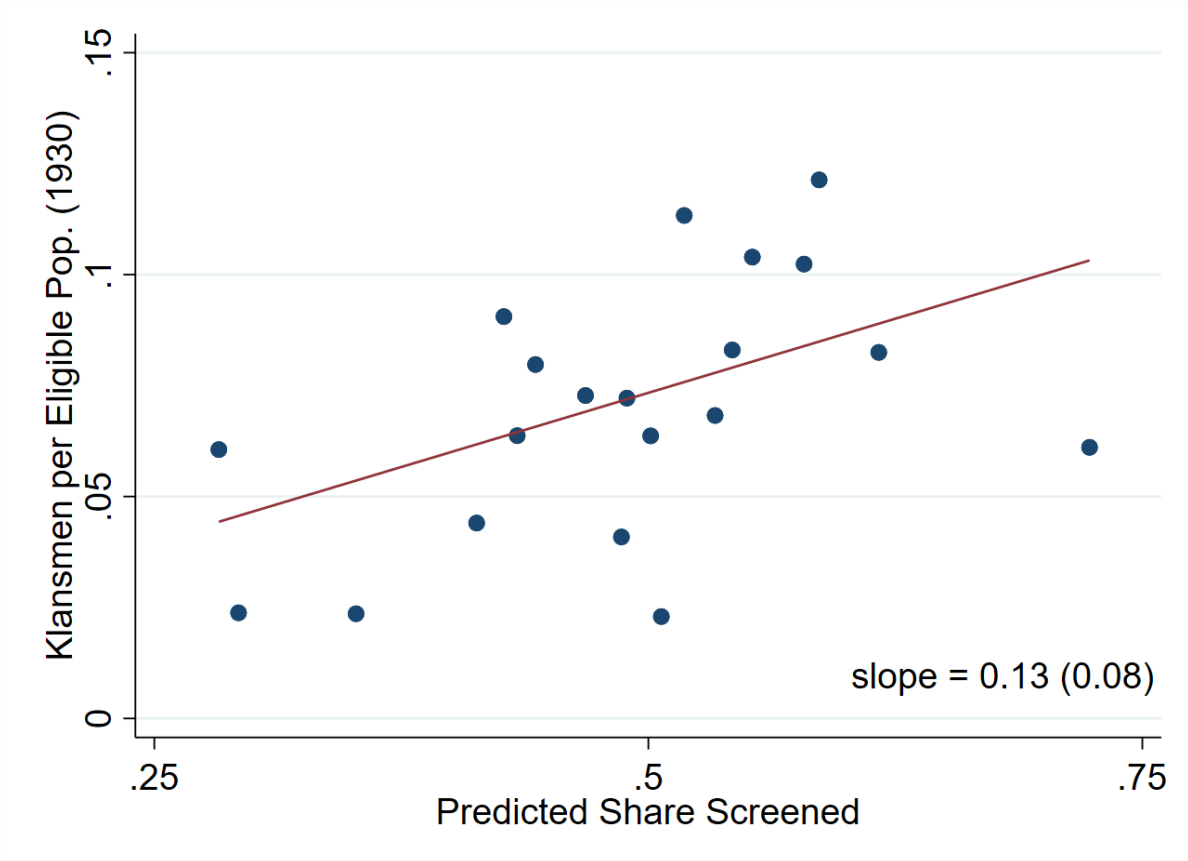
Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BON screenings on Klavern presence by year. Each dot represents a separate regression. The outcome of interest is whether a Klavern of the Second KKK had ever been founded in by a given year. Screenings are instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors are clustered by state.

Figure A.XVIII: Relationship between Klan Characteristics, Theaters, and Screenings



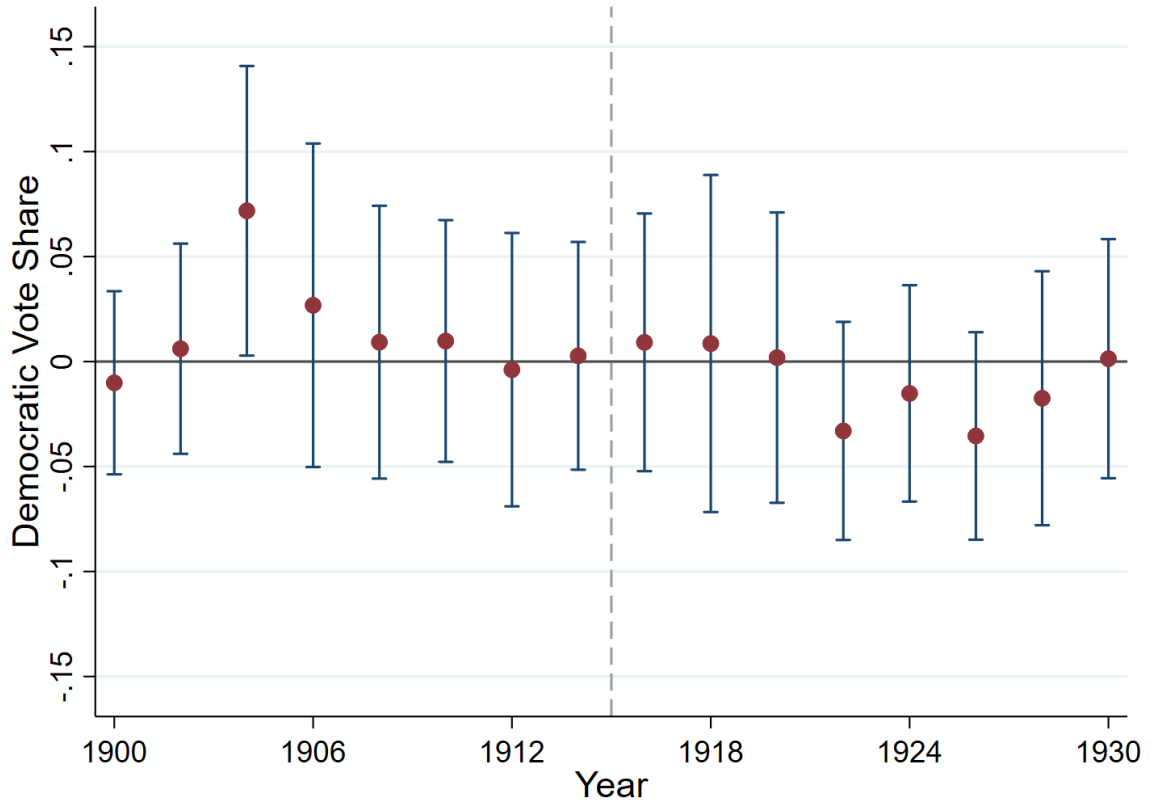
Notes: Figure shows OLS estimates and 95% confidence intervals from a series of univariate regressions of county outcomes on measures of common Klan characteristics per Jackson (1992) (i.e., white, lower-middle-class workers of native descent affiliated with the Baptist or Methodist Churches and living in mid-size communities) and state fixed effects. Each dot represents a separate regression. Outcomes are an indicator for theater presence in 1914 (red dots) and an indicator for BON screenings from 1915-1919 (blue dots). “Occupational Prestige” refers to Siegel prestige scores from the 1910 Census, measuring the average “social standing” of occupations in a county (where higher numbers correspond to esteemed white collar jobs like lawyers and physicians). “% Klan-Eligible” is the share of county population comprised of native-born white males over the age of 20 from the 1910 Census. “% Protestant” is the share of county population comprised of individuals affiliated with Baptist or Methodist churches from the 1906 Census of Religious Bodies. “Mid-Size County” is an indicator for counties between the 25th and 75th percentiles of 1910 population. Explanatory variables are standardized to mean 0 and standard deviation 1. Standard errors are clustered by state.

Figure A.XIX: Effects on Klansmen per Capita



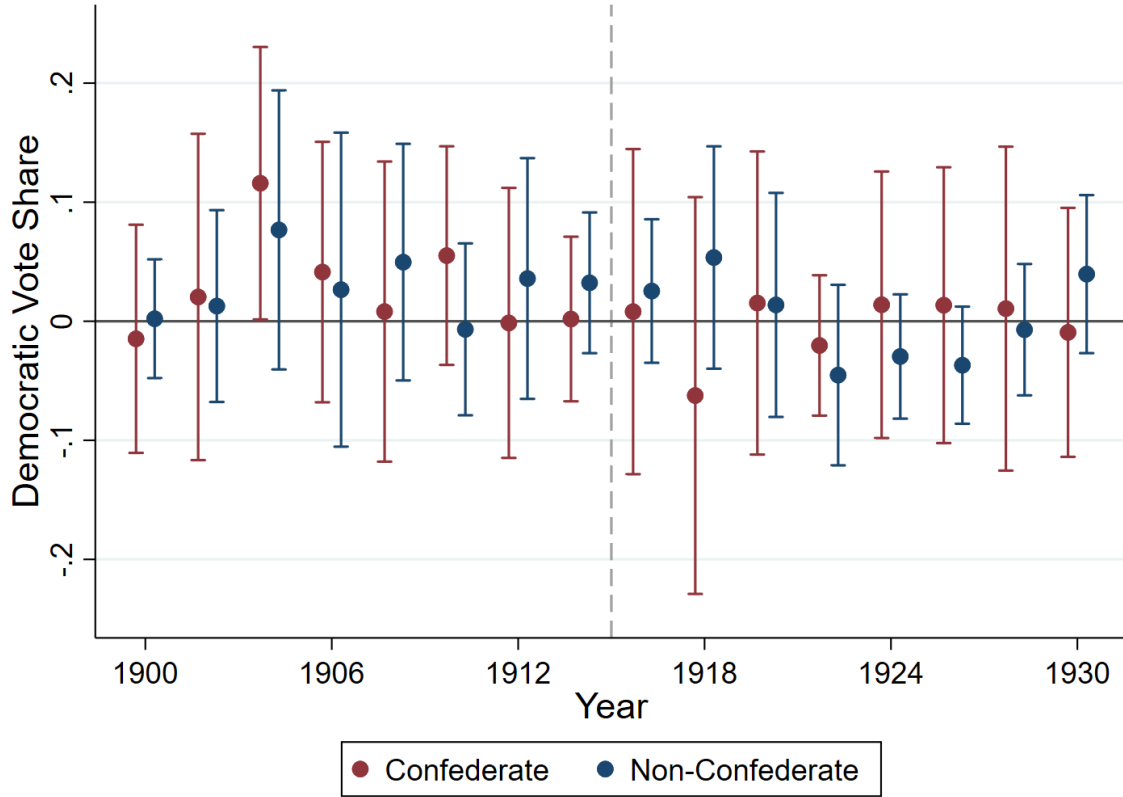
Notes: Figure displays binscatter of state Klan membership per eligible population (i.e., white native-born men in 1930) against the predicted share of eligible population in the state living in counties that screened BON (*ShareScreened*), which comes from a first-stage regression of *ShareScreened* on the share of eligible population living in counties with a theater in 1914 (*ShareTheater*) and region fixed effects. State-level Klan membership data were obtained from Jackson (1992). Each dot represents 5% of the sample. Red line represents IV estimate of effects of BON penetration on Klan membership, instrumenting for *ShareScreened* using *ShareTheater*. Heteroskedasticity-robust standard errors are shown in parentheses.

Figure A.XX: Effects on Democratic Vote Share



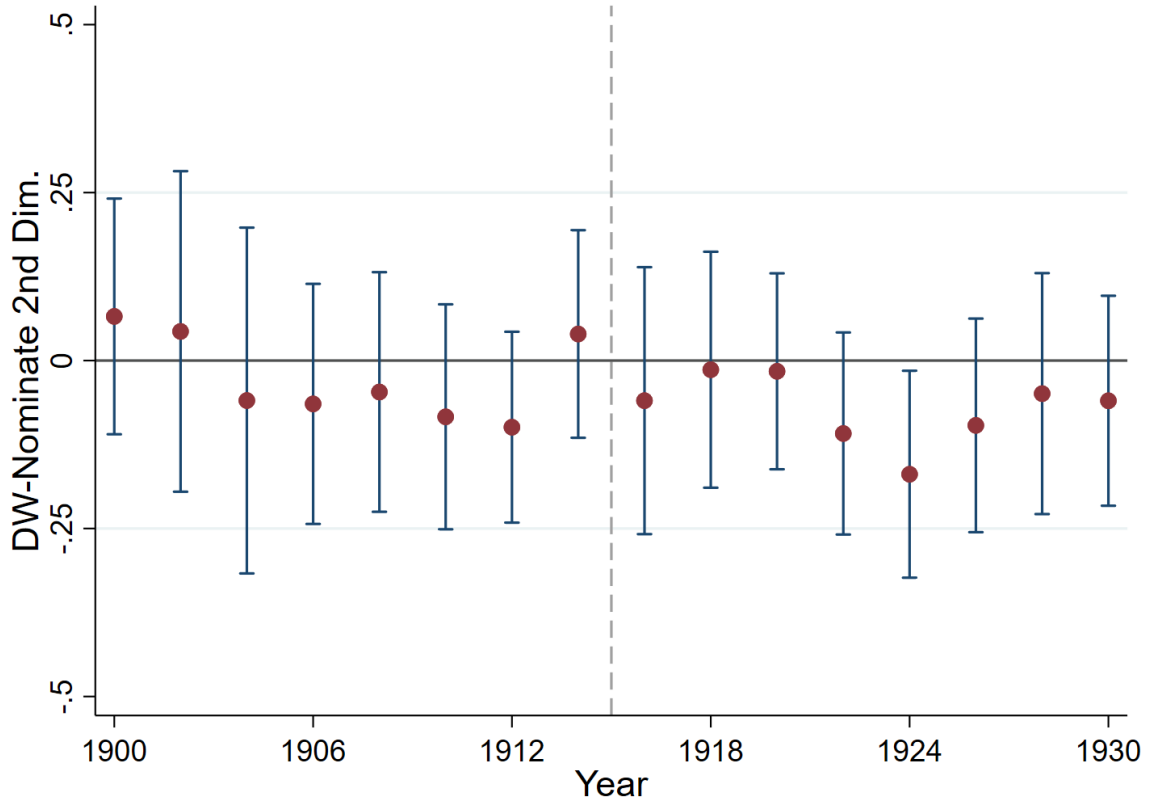
Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BONA screenings on Democratic vote share. Each dot represents a separate regression. The outcome of interest is the share of major party ballots in a county cast for Democratic candidates in Congressional elections. Screenings are instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors are clustered by state.

Figure A.XXI: Effects on Democratic Vote Share by Region



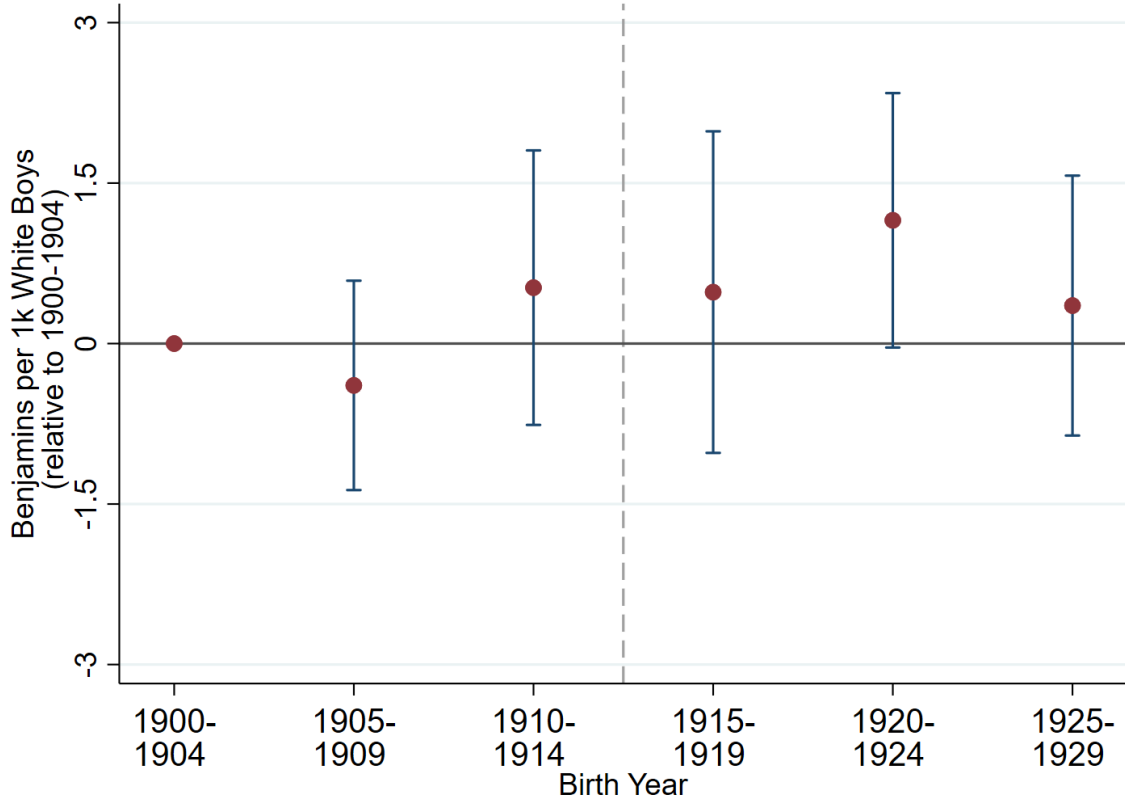
Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BONA screenings on Democratic vote share, separately for counties in former Confederate states and counties in other states. Confederate refers to counties in former Confederate states (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX and VA). Each dot represents a separate regression. The outcome of interest is the share of major party ballots in a county cast for Democratic candidates in Congressional elections. Screenings are instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Unit of observation is the county. Standard errors are clustered by state.

Figure A.XXII: Effects on Congressional Ideology



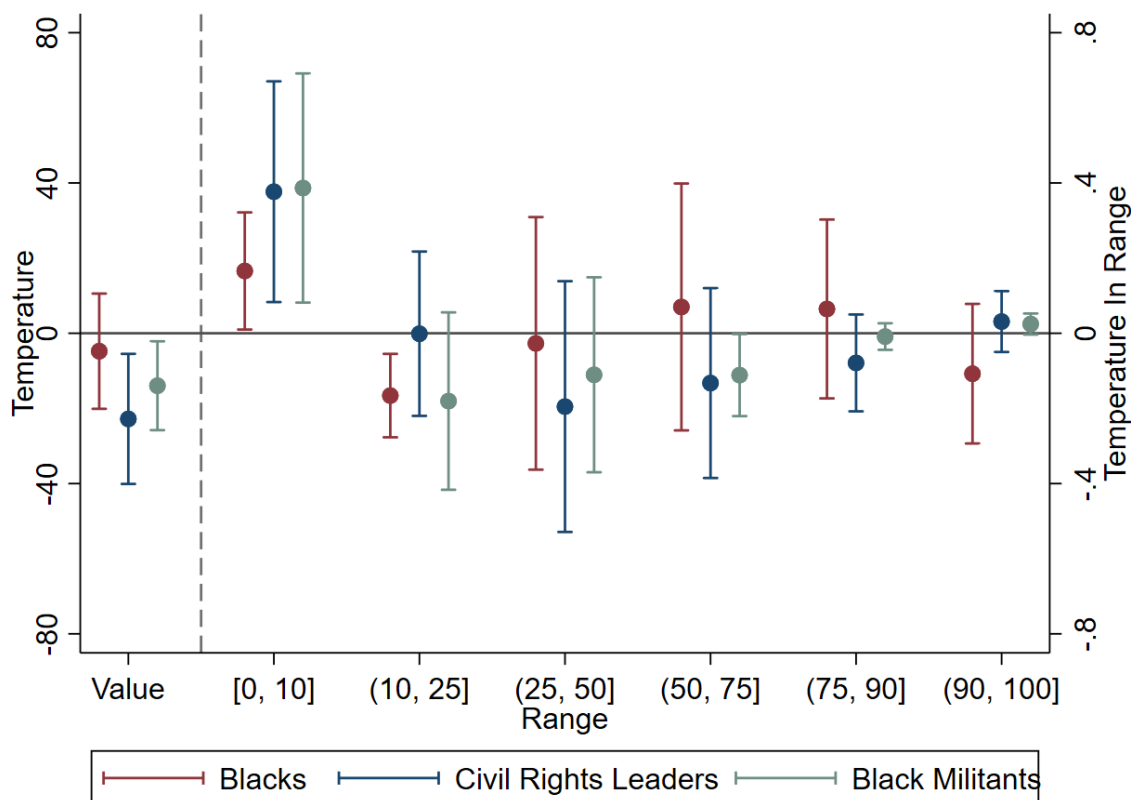
Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BON screenings on Congressional ideology. Each dot represents a separate regression. The outcome of interest is DW-Nominate 2nd Dimension score of a county's Congressional representative. Higher values correspond to more racially-conservative roll-call voting patterns. Year corresponds to the election year of a given Congress. Screenings are instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Sample drops counties that are part of multiple Congressional districts. Standard errors are clustered by state.

Figure A.XXIII: Effects on Naming Patterns



Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BONA screenings on naming patterns. Each dot represents a separate regression. The outcome of interest is the number of white males living in county c with birth year in period p named “Benjamin” (the name of BONA’s protagonist) per 1000 white males born in the same county-period. This number is then subtracted by the county’s “Benjamin” share from 1900 to 1904. Each dot represents a separate regression. Data come from the full-count historical Censuses. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors are clustered by state.

Figure A.XXIV: Effects on Anti-Black Prejudice



Notes: Figure shows IV estimates and 95% confidence intervals from Equation 2 of the effect of BON screenings on self-reported anti-Black prejudice. Individual-level data come from American National Election Studies “feeling thermometer” questions, in which respondents are asked to rate how favorably they feel towards a given group on a scale of 0 (very unfavorable) to 100 (very favorable). Sample includes 4,240 white respondents from 1974, 1972 and 1970, the first year with thermometer questions about “Civil Rights Leaders” and “Black Militants”. Results for feelings about “Blacks” are similar when including prior years. Each dot represents a separate regression. Point estimates for “Value” represent effects on raw temperature responses (from 0 to 100) and are plotted against left axis. All other estimates represent effects on a dummy variable equal to 1 if temperature is within the respective range and are plotted against right axis. Regressions include all county-level demographic, social capital, media and racism controls described in Table III as well as person-level controls for sex, age, education and survey year. Standard errors are clustered by county.

Supplementary Tables

Table A.I: Comparison of Treatment Counties with and without Racial Violence

	No Lynching/Riot within 1 year of BON		Lynching/Riot within 1 year of BON		t-stat
<i>Demographics</i>					
Population (000s)	76.09	(243.77)	51.87	(46.68)	-1.55
Density (mi)	397.02	(1848.53)	75.19	(55.53)	-4.21
% Black	11.19	(19.24)	39.60	(22.64)	4.82
% U.S. born	84.27	(18.97)	81.45	(29.00)	-0.37
% Draft Eligible	9.83	(1.80)	10.29	(0.70)	2.35
<i>Social Capital</i>					
Dist. to Major City (mi)	512.27	(535.98)	635.76	(502.95)	0.94
% Urban	14.95	(29.15)	20.34	(31.42)	0.66
% Illiterate	5.46	(6.20)	10.92	(6.65)	3.14
% Religious	33.93	(13.81)	32.21	(9.63)	-0.68
Occupation Score	6.83	(1.71)	6.69	(1.54)	-0.34
% Turnout (1912)	16.96	(9.62)	8.57	(6.48)	-4.89
<i>Newspapers</i>					
Circulation per Cap. (1912)	0.16	(0.22)	0.19	(0.22)	0.51
Markets (1912)	1.30	(1.72)	1.47	(1.30)	0.49
<i>Theaters</i>					
Any (1914)	0.98	(0.15)	1.00	(0.00)	3.78
Per 1k Pop (1914)	0.14	(0.10)	0.17	(0.10)	1.18
Max Seats (1914)	785.12	(810.02)	860.47	(761.78)	0.38
<i>Racism</i>					
% Democrat (1912)	47.41	(20.75)	69.71	(18.25)	4.66
Confed. Monuments (1914)	0.27	(0.78)	0.73	(0.80)	2.20
Lynchings (1900-1905)	0.17	(0.70)	1.40	(1.96)	2.44
% NAACP (1914)	2.81	(16.53)	6.67	(25.82)	0.58
% Confederate	26.40	(44.12)	80.00	(41.40)	4.94
Counties	606		15		

Notes: Table presents summary statistics for counties that screened *Birth of a Nation*, separately for treatment counties that did and did not experience a lynching or race riot within one year of the film's arrival. For full description of variables, see Table III. For each group, means and standard deviations (in parentheses) are reported. T-statistics comparing means are shown in the rightmost column.

Table A.II: Historical Effects on the Second KKK: Klan Intensity

DV = Klaverns per 10k Eligible Pop.						
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Ordinary Least Squares</i>						
Screened	0.110* (0.062)	0.113* (0.066)	0.191* (0.098)	0.109* (0.062)	0.073 (0.060)	0.128 (0.078)
<i>Reduced Form</i>						
Theater	0.204** (0.085)	0.141 (0.114)	0.319*** (0.102)	0.198** (0.087)	0.130 (0.107)	0.258 (0.161)
<i>Two-Stage Least Squares</i>						
$\widehat{Screened}$	0.969** (0.424)	0.470 (0.381)	0.894*** (0.291)	1.092** (0.501)	0.630 (0.529)	0.743* (0.437)
Mean	0.757	0.772	0.767	0.761	0.826	0.828
Counties	All	Neighbors Only	Drop Neighbors	Drop 20 Largest	Thtr by 1930	Paper in 1915
Controls	✓	✓	✓	✓	✓	✓
F-Stat	81.97	191.20	46.01	57.60	76.67	35.97
Obs	3,103	2,161	1,560	3,083	2,249	1,392

Notes: Table shows estimates from Equation 2 of the effect of BON screenings on Klan intensity. The outcome of interest is the number of Second KKK Klaverns founded in a county by 1930 as a share of Klan-eligible population (i.e., per 10,000 native-born white men in 1930). Column 1 includes all counties in the continental U.S. Column 2 restricts the control group to counties adjacent to treatment counties. Column 3 excludes control counties adjacent to treatment counties. Column 4 drops the 20 largest counties by 1910 population. Column 5 restricts to counties that had at least one theater by 1930. Column 6 restricts to counties with at least one digitized local newspaper from 1915. *Screened* is a dummy for whether BON was screened in a county during the film's roadshow. Theater is a dummy for whether a county had at least one theater by the end of 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.III: Historical Effects on the Second KKK: Alternative Treatment Measures

		DV = Klavern by 1930		
	(1)	(2)	(3)	(4)
<i>Ordinary Least Squares</i>				
Screened	0.099*** (0.020)	0.090*** (0.020)	0.066** (0.025)	0.052** (0.023)
<i>Reduced Form</i>				
Theater	0.144*** (0.024)	0.144*** (0.024)	0.144*** (0.024)	0.144*** (0.024)
<i>Two-Stage Least Squares</i>				
$\widehat{Screened}$	0.685*** (0.144)	0.616*** (0.125)	1.605*** (0.492)	1.275*** (0.355)
Mean	0.313	0.313	0.313	0.313
Screened	Ang	Union	Intersection	ERST
Controls	✓	✓	✓	✓
F-Stat	81.97	93.50	21.57	28.10
Obs	3,103	3,103	3,103	3,103

Notes: Table shows estimates from Equation 2 of the effect of BON screenings on historical Klavern formation using alternative measures of treatment. Column 1 uses this paper’s primary treatment measure, which is based on advertisements from *newspapers.com*, *newspaperarchive.com* and *loc.gov* located to theater locations and supplemented by information from *The Moving Picture World*. Column 2 uses the union of my measure and the measure employed by Esposito et al. (2021), which is drawn from newspaper mentions of BON screenings from *newspapers.com* and located to the county of the newspaper’s headquarters (i.e., *Screened* is set to 1 if a county is marked as treated under either measure). Column 3 uses the intersection of the two measures (i.e., *Screened* is set to 1 only if a county is marked as treated under both measures). Column 4 uses Esposito et al. (2021)’s measure alone. In all cases, treatment is instrumented by an indicator for whether a county had a theater in 1914. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.IV: Historical Effects on the Second KKK: Alternative Samples

		DV = Klavern by 1930				
		(1)	(2)	(3)	(4)	(5)
<i>Ordinary Least Squares</i>						
Screened		0.097*** (0.019)	0.092*** (0.022)	0.101*** (0.022)	0.118*** (0.035)	0.065* (0.034)
<i>Reduced Form</i>						
Theater		0.139*** (0.024)	0.136*** (0.024)	0.149*** (0.037)	0.129*** (0.045)	0.115** (0.046)
<i>Two-Stage Least Squares</i>						
$\widehat{Screened}$		0.667*** (0.143)	0.722*** (0.153)	0.486*** (0.115)	0.419*** (0.141)	0.299** (0.127)
Mean		0.327	0.327	0.419	0.444	0.479
Counties	≥ 1 Paper (1915)	≥ 1 Paper (1915)	≥ 1 Digit. Paper (1915-1919)	≥ 2 Digit. Paper (1915-1919)	Mickey	
Paper Ctrl		✓	✓	✓	✓	
F-Stat		73.41	86.84	42.74	19.54	52.72
Obs		2,901	2,901	1,343	910	895

Notes: Table shows estimates from Equation 2 of the effect on BON screenings on historical Klavern formation using alternative measures of treatment. Columns 1 and 2 restrict the sample to counties with at least one newspaper (digitized or not) operating in 1915. Columns 3 and 4 restrict the sample to counties with at least one or two digitized papers during each year of BON's roadshow, respectively. Column 5 restricts the sample to counties where the film *Mickey* was shown (as determined by digitized newspaper advertisements). Regressions include all demographic, social capital, media and racism controls described in Table III. Columns 2 through 5 include an additional control for the average number of digitized papers available from *newspapers.com*, *newspaperarchive.com*, and *loc.gov* from 1915 to 1919. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. *Screened* is a dummy for whether BON was screened in a county during the film's roadshow and is instrumented by an indicator for whether a county had a theater in 1914. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.V: Historical Effects on the Second KKK:: Alternative Instruments

	DV = Klavern by 1930					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Screened</i>	0.370* (0.187)	0.645*** (0.133)	0.473* (0.261)	0.507*** (0.105)	0.420*** (0.114)	0.193** (0.079)
Mean	0.313	0.313	0.440	0.313	0.386	0.313
Instrument	Thtrs per 1k white	Max thtr capacity	Max thtr capacity	First thtr year	First thtr year	Mickey
Counties	All	All	Thtr by 1914	All	Thtr by 1930	All
Controls	✓	✓	✓	✓	✓	✓
F-Stat	15.80	43.74	13.88	41.63	35.88	110.08
Obs.	3,103	3,103	1,786	3,103	2,249	3,103

Notes: Table shows IV estimates from Equation 2 of the effect on BON screenings on historical Klavern formation using alternative instruments. Column 1 uses a quadratic of the number of theaters in 1914 per 1,000 white residents. Column 2 uses a quadratic of the number of seats available in a county's largest theater in 1914, imputting the median non-missing value (700) for counties with theaters that are missing seat information. Column 3 uses the same instrument, restricted to the sample of counties for with at least one theater in 1914. Column 4 uses a vector of indicators based on the year a county's first theater was opened. Column 5 uses the same, restricting to counties where a theater was opened by 1930. Column 6 uses the location of screenings of the *Mickey* from 1918 to 1922. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. Screened is a dummy for whether BON was screened in a county during the film's roadshow. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.VI: Historical Effects on the Second KKK: Robustness to Spatial Correlation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		<u>Clustered SE</u>			<u>Conley SE</u>		<u>Muller-Watson SE</u>			<u>Moran's I</u>		
	Coef.	State	County	250mi	500mi	1000mi	$\rho=.03$	$\rho=.10$	$\rho=.20$	250mi	500mi	1000mi
Panel A: Base Model												
<i>Screened</i>	0.685	0.144	0.096	0.136	0.153	0.075	0.133	0.162	0.179	1.605	-0.129	-0.886
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.027)	(0.081)	(0.109)	(0.897)	(0.376)
Panel B: Longitude & Latitude Controls												
<i>Screened</i>	0.648	0.129	0.093	0.121	0.135	0.058	0.118	0.147	0.154	-1.016	-0.984	-0.819
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.023)	(0.063)	(0.310)	(0.325)	(0.413)

Notes: Table examines robustness to spatial correlation for the effects of BON screenings on historical Klavern formation. For reference, Column 1 shows IV estimate for *Screened* from Equation 2. Remaining columns show standard errors (Columns 2 through 9) or z-scores of Moran's I (Columns 10 through 12) with corresponding p-values in parentheses. Columns 2 and 3 cluster standard errors at the state and county-level, respectively. Columns 4 through 6 adjust for spatial correlation within various bandwidths per Conley (1999). Columns 7 through 9 construct confidence intervals and standard errors under "worst-case" spatial correlation models per Müller and Watson (2021). Columns 10 through 12 present z-scores for Moran's I statistic testing for spatial auto-correlation of residuals at various bandwidths. As reference, Kelly (2019) conduct a meta-analysis of 27 persistence studies in "Top-5" journals and find an average z-score of 9.24. Panel A includes all demographic, social capital, media and racism controls described in Table III. Panel B further include quadratics for the latitude and longitude of county centroids. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. *Screened* is a dummy for whether BON was screened in a county during the film's roadshow and is instrumented by an indicator for whether a county had a theater in 1914.

Table A.VII: Historical Effects on the Second KKK: Propensity Score Matching

		DV = Klavern by 1930						
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		<i>OLS</i>	<i>Propensity Score Matching</i>					
Screened		0.099*** (0.020)	0.154*** (0.036)	0.110*** (0.031)	0.082*** (0.023)	0.050** (0.020)	0.266*** (0.069)	0.173*** (0.040)
Mean		0.313	0.313		0.439		0.275	
Control Sample	All		All		w/ Theater		w/o Theater	
Controls	✓	-	✓	-	✓	-	✓	
Oster δ	0.91	-	5.50	-	4.06	-	2.14	
Obs	3,103		2,947		1,699		1,732	

Notes: Table shows average treatment effects of BON screenings on historical Klavern formation under propensity score matching. For reference, Column 1 shows OLS estimates from Equation 2. Columns 2 through 7 display results from propensity score matching. Treatment counties are matched to control counties in the same state using Epanechnikov kernel functions and the full set of covariates described in Table III. Columns 2 and 3 match treatment counties to the full sample of control counties. Columns 4 and 5 match only to control counties with theaters in 1914. Columns 6 and 7 match only to control counties without theaters in 1914. Odd-numbered columns further control for all predicting covariates in the matched sample. Oster's δ measures how large bias from unobservables (relative to bias from observables) would have to be for the true treatment effect to be zero (Oster, 2019). The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. *Screened* is a dummy for whether BON was screened in a county during the film's roadshow. Standard errors clustered by state. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.VIII: Historical Effects on the Second KKK: *Mickey* Placebos

		DV = Klavern by 1930		
	(1)	(2)	(3)	(4)
	Ordinary Least Squares			
Screened(Mickey)	0.054** (0.022)	0.029 (0.023)	-0.122 (0.122)	0.003 (0.024)
Screened(BON)		0.088*** (0.022)		
	Instrumental Variables			
$\widehat{\text{Screened}}(\text{Mickey})$	0.595*** (0.095)	0.111 (0.196)	-0.268 (0.218)	0.237 (0.162)
$\widehat{\text{Screened}}(\text{BON})$		0.435*** (0.157)		
Mean	0.313	0.313	0.352	0.251
Counties	All	All	Kansas	Control
Obs	3,103	3,103	105	2,483

Notes: Table compares effects on historical Klavern formation of BON screenings to screenings of the 1918 film *Mickey*. Top panel presents OLS estimates from Equation 2 with separate dummies for Mickey screenings and BON screenings as the independent variables of interest. Column 1 and 2 examine the full sample. Column 3 restricts to counties in Kansas, where BON was banned. Column 4 restricts to counties where BON was not shown. Bottom panel presents corresponding IV estimates. Due to the presence of two endogenous variables, screenings are instrumented using a vector of indicators based on the period a county's first theater was opened. The outcome of interest is whether a Klavern of the Second KKK had formed in a county by 1930. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state, except when examining Kansas, when heteroskedasticity-robust SEs are used. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.IX: Effects on Attitudes towards the KKK

Dep. Var.	<i>All Respondents</i>			<i>White Respondents</i>			<i>Non-White Respondents</i>		
	Mean	(1)	(2)	Mean	(3)	(4)	Mean	(5)	(6)
Know KKK	0.696	0.587** (0.261)	0.344* (0.200)	0.701	0.570** (0.270)	0.318 (0.207)	0.606	-0.133 (0.445)	0.243 (0.528)
Ban KKK	0.677	-0.793** (0.311)	-0.686** (0.290)	0.665	-0.857** (0.327)	-0.747** (0.303)	0.899	0.240 (0.426)	0.098 (0.425)
State Ctrl		✓	✓		✓	✓		✓	✓
Person Ctrl		-	✓		-	✓		-	✓
Obs		3,071			2,916			155	

Notes: Table shows IV estimates from individual-level analogue of Equation 2 on knowledge and views of the Ku Klux Klan. Data come from a 1946 Gallup poll and are geographically identified to the state-level. Specifically, I estimate $y_i = \delta_r + \beta \text{ShareScreened}_s + X'_s \Lambda + R'_i \Gamma + \epsilon_i$, where ShareScreened_s is the share of native White male adults in state s in 1930 living in counties that screened *The Birth of a Nation*, as instrumented by the share of native white male adults in counties with a theater in 1914. δ_r are region fixed effects, X' is a vector of state-level demographic, social capital and racism controls, and R' is a vector of individual-level controls (i.e., race, gender, age and education). *Know KKK* is an indicator for whether a respondent could tell the interviewer “what the Ku Klux Klan is.” *Ban KKK* is an indicator for whether a respondent thought there should be a law “against joining the KKK” or forbidding the organization altogether (each respondent was only asked one of the two wordings). Columns 1 and 2 examine all respondents. Columns 3 and 4 examine white respondents only. Columns 5 and 6 examine non-white respondents only. Standard errors are clustered by state. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.X: Long-Run Effects on Racial Hate: Alternative Samples

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ku Klux Klan in County (1960s)						
$\widehat{Screened}$	0.147** (0.070)	0.115** (0.057)	0.142** (0.068)	0.116 (0.077)	0.190** (0.073)	0.152** (0.059)
Mean	0.106	0.120	0.102	0.106	0.097	0.102
Panel B: Hate Group in County (2000-2019)						
$\widehat{Screened}$	0.345*** (0.100)	0.280*** (0.082)	0.307*** (0.099)	0.160 (0.120)	0.320** (0.121)	0.128 (0.125)
Mean	0.374	0.414	0.407	0.370	0.409	0.407
Panel C: Hate Crimes per 100k (2000-2018)						
$\widehat{Screened}$	1.068** (0.488)	0.481 (0.321)	1.025** (0.417)	1.149* (0.606)	1.155* (0.634)	0.387 (0.259)
Mean	1.372	1.410	1.486	1.349	1.493	1.486
Counties	All	Neighbors Only	Drop Neighbors	Drop 20 Largest	Thtr by 1930	Paper in 1915
Controls	✓	✓	✓	✓	✓	✓
F-Stat	81.97	191.20	46.01	57.60	76.67	35.97
Obs	3,103	2,161	1,560	3,083	2,249	1,392

Table shows IV estimates from Equation 2 of the effect of BON screenings on long-run racial hate using alternative samples. Column 1 includes all counties in the continental United States. Column 2 restricts the control group to counties adjacent to treatment counties. Column 3 excludes control counties adjacent to treatment counties. Column 4 drops the 20 largest counties by 1910 population. Column 5 restricts to counties that had at least one theater by 1930. Column 6 restricts to counties with at least one digitized local newspaper from 1915. Outcomes of interest are: whether a county had an active chapter of the Third KKK in the 1960s (Panel A), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Panel B), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Panel C). *Screened* is a dummy for whether BON was screened in a county during the film's roadshow and is instrumented by an indicator for whether a county had a theater in 1914. Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.XI: Long-Run Effects on Racial Hate: Alternative Instruments

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ku Klux Klan in County (1960s)						
$\widehat{Screened}$	0.155 (0.136)	0.156*** (0.056)	0.148* (0.086)	0.079* (0.042)	0.077** (0.036)	0.073** (0.033)
Mean	0.106	0.106	0.094	0.106	0.097	0.106
Panel B: Hate Group in County (2000-2019)						
$\widehat{Screened}$	0.338* (0.197)	0.426*** (0.108)	0.666*** (0.244)	0.311*** (0.086)	0.297*** (0.097)	0.268*** (0.063)
Mean	0.374	0.374	0.440	0.374	0.409	0.374
Panel C: Hate Crimes per 100k (2000-2018)						
$\widehat{Screened}$	2.139** (0.934)	1.095** (0.428)	1.391* (0.721)	0.959** (0.408)	0.985** (0.433)	0.377 (0.375)
Mean	1.372	1.372	1.608	1.372	1.493	1.372
Instrument	Thtrs per 1k white	Max thtr capacity	Max thtr capacity	First thtr year	First thtr year	Mickey
Counties	All	All	Thtr by 1914	All	Thtr by 1930	All
Controls	✓	✓	✓	✓	✓	✓
F-Stat	15.80	43.74	13.88	41.63	35.88	110.08
Obs	3,103	3,103	1,786	3,103	2,249	3,103

Notes: Table shows IV estimates from Equation 2 of the effect of BON screenings on long-run racial hate using alternative instruments. Column 1 uses a quadratic of the number of theaters in 1914 per 1,000 white residents. Column 2 uses a quadratic of the number of seats available in a county's largest theater in 1914, imputting the median non-missing value (700) for counties with theaters that are missing seat information. Column 3 uses the same instrument, restricted to the sample of counties for with at least one theater in 1914. Column 4 uses a vector of indicators based on the year a county's first theater was opened. Column 5 uses the same, restricting to counties where a theater was opened by 1930. Column 6 uses the location of screenings of the *Mickey* from 1918 to 1922. Outcomes of interest are: whether a county had an active chapter of the Third KKK in the 1960s (Panel A), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Panel B), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Panel C). *Screened* is a dummy for whether BON was screened in a county during the film's roadshow. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Regressions include all demographic, social capital, media and racism controls described in Table III. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.XII: Long-Run Effects on Racial Hate: Alternative Treatment Measures

	(1)	(2)	(3)	(4)
Panel A: Ku Klux Klan in County (1960s)				
$\widehat{Screened}$	0.147** (0.070)	0.132** (0.065)	0.345** (0.154)	0.274** (0.132)
Mean	0.106	0.106	0.106	0.106
Panel B: Hate Group in County (2000-2019)				
$\widehat{Screened}$	0.345*** (0.100)	0.310*** (0.091)	0.808*** (0.279)	0.642*** (0.217)
Mean	0.374	0.374	0.374	0.374
Panel C: Hate Crimes per 100k (2000-2018)				
$\widehat{Screened}$	1.068** (0.488)	0.962** (0.437)	2.505** (1.222)	1.990** (0.948)
Mean	1.372	1.372	1.373	1.372
Screened	Ang	Union	Intersection	ERST
Controls	✓	✓	✓	✓
F-Stat	81.97	93.50	21.57	28.10
Obs	3,103	3,103	3,103	3,103

Notes: Table shows IV estimates from Equation 2 of the effect of BON screenings on long-run racial hate using alternative measures of treatment. Column 1 uses this paper’s primary treatment measure, which is based on advertisements from *newspapers.com*, *newspaperarchive.com* and *loc.gov* located to theater locations and supplemented by information from *The Moving Picture World*. Column 2 uses the union of my measure and the measure employed by Esposito et al. (2021), which is drawn from newspaper mentions of BON screenings from *newspapers.com* and located to the county of the newspaper’s headquarters (i.e., *Screened* is set to 1 if a county is marked as treated under either measure). Column 3 uses the intersection of the two measures (i.e., *Screened* is set to 1 only if a county is marked as treated under both measures). Column 4 uses Esposito et al. (2021)’s measure alone. In all cases, treatment is instrumented by an indicator for whether a county had a theater in 1914. Outcomes are: whether a county had an active chapter of the Third KKK in the 1960s (Panel A), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Panel B), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Panel C). Regressions include all demographic, social capital, media and racism controls described in Table III. Standard errors clustered by state. Kleibergen-Paap F-statistics are reported. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.XIII: Long-Run Effects on Racial Hate: Robustness to Spatial Correlation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		<u>Clustered SE</u>			<u>Conley SE</u>		<u>Muller-Watson SE</u>			<u>z(Moran's I)</u>		
	Coef.	State	County	250mi	500mi	1000mi	$\rho=.03$	$\rho=.10$	$\rho=.20$	250mi	500mi	1000mi
Panel A: Ku Klux Klan in County (1960s)												
<i>Screened</i>	0.147	0.070	0.053	0.073	0.070	0.056	0.077	0.082	0.092	-0.256	0.017	-0.284
		(0.040)	(0.005)	(0.045)	(0.035)	(0.009)	(0.148)	(0.267)	(0.404)	(0.798)	(0.986)	(0.776)
Panel B: Hate Group in County (2000-2019)												
<i>Screened</i>	0.345	0.100	0.096	0.081	0.076	0.102	0.061	0.081	0.096	2.913	-0.378	0.087
		(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)	(0.026)	(0.097)	(0.004)	(0.705)	(0.931)
Panel C: Hate Crimes per 100k (2000-2018)												
<i>Screened</i>	1.068	0.488	0.418	0.356	0.536	0.192	0.479	0.508	0.550	-2.537	-1.140	-0.631
		(0.034)	(0.011)	(0.003)	(0.046)	(0.000)	(0.099)	(0.199)	(0.319)	(0.011)	(0.254)	(0.528)

Notes: Table examines robustness to spatial correlation for the long-run effects on racial hate. For reference, Column 1 shows main IV coefficient from Equation 2. Remaining columns show standard errors (Columns 2 through 9) or z-scores of Moran's I (Columns 10 through 12) with corresponding p-values in parentheses. Columns 2 and 3 cluster standard errors at the state and county-level, respectively. Columns 4 through 6 adjust for spatial correlation within various bandwidths per Conley (1999). Columns 7 through 9 construct confidence intervals and standard errors under "worst-case" spatial correlation models per Müller and Watson (2021). Columns 10 through 12 present z-scores for Moran's I statistic testing for spatial auto-correlation of residuals at various bandwidths. As reference, Kelly (2019) conduct a meta-analysis of 27 persistence studies in "Top-5" journals and find an average z-score of 9.24. Outcomes are: whether a county had an active chapter of the Third KKK in the 1960s (Panel A), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Panel B), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Panel C). Regressions include all demographic, social capital, media and racism controls described in Table III.

Table A.XIV: Long-Run Effects on Racial Hate: *Mickey* Placebos

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ku Klux Klan in County (1960s)						
Screened(<i>Mickey</i>)	0.016** (0.008)	0.019 (0.063)	- -	- -	0.009 (0.008)	0.032 (0.113)
Screened(<i>BON</i>)	0.016 (0.015)	0.066 (0.060)				
Mean	0.106	0.106	0.000	0.000	0.097	0.097
Panel B: Hate Group in County (2000-2019)						
Screened(<i>Mickey</i>)	0.049** (0.020)	-0.148 (0.238)	0.031 (0.058)	0.056 (0.126)	-0.014 (0.020)	-0.037 (0.183)
Screened(<i>BON</i>)	0.095*** (0.028)	0.430** (0.200)				
Mean	0.374	0.374	0.105	0.105	0.318	0.318
Panel C: Hate Crimes per 100k (2000-2018)						
Screened(<i>Mickey</i>)	0.070 (0.118)	-0.327 (0.728)	0.464 (0.433)	-0.104 (0.850)	0.048 (0.142)	1.146 (0.781)
Screened(<i>BON</i>)	0.128 (0.096)	1.184* (0.604)				
Mean	1.372	1.372	1.800	1.800	1.268	1.268
Model	OLS	IV	OLS	IV	OLS	IV
Sample	All	All	Kansas	Kansas	Control	Control
Obs	3,103	3,103	105	105	2,483	2,483

Notes: Table presents placebo tests of the long-run effects on racial hate using screenings of the 1918 film *Mickey*. *Screened(Mickey)* is an indicator for historical showings of *Mickey*, *Screened(BON)* is an indicator for historical showings of *The Birth of a Nation*. Columns 1 and 2 examine the full sample. Columns 3 and 4 examine counties in Kansas, where *BON* was banned. Columns 5 and 6 examine counties where *BON* was not shown. Odd columns present OLS estimates, even columns present IV estimates. Due to the presence of two endogenous variables, screenings are instrumented using a vector of indicators based on the period a county's first theater was opened. Outcomes are: whether a county had an active chapter of the Third KKK in the 1960s (Panel A), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Panel B), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Panel C). Standard errors are clustered by state except for Kansas counties, when they are heteroskedasticity-robust. Regressions include all demographic, social capital, media and racism controls described in Table III. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.XV: Long-Run Effects on Racial Hate: Theater Timing and Censorship Placebos

Dep. Var.	KKK 1960s (1)	Hate Group 2000-2019 (2)	Hate Crimes 2000-2018 (3)
Panel A: Theater Timing Placebo			
First Theater in <1910	0.031* (0.017)	0.081*** (0.027)	0.242** (0.109)
1910-1918	0.011 (0.017)	0.038 (0.030)	0.088 (0.156)
1919-1930	0.002 (0.010)	0.041* (0.024)	-0.081 (0.138)
Mean	0.106	0.374	1.376
Obs	3,103	3,103	3,103
Panel B: Censorship Placebo			
<i>Counties in Kansas (BON Banned)</i>			
Theater by 1914	-	0.060 (0.062)	0.014 (0.457)
<i>Counties Outside Kansas (BON Allowed)</i>			
Theater by 1914	0.033*** (0.011)	0.076*** (0.021)	0.233*** (0.088)
Mean (KS)	0.000	0.105	1.800
Mean (Not KS)	0.110	0.383	1.361
Obs (KS)	105	105	105
Obs (Not KS)	2,998	2,998	2,998
Controls	✓	✓	✓

Notes: Table presents results from two placebo tests of the long-run effects on racial hate. Panel A shows results from regressing outcomes on a set of mutually-exclusive indicator variables corresponding to the period each county first opened a movie theater. The omitted group are counties that had not opened a theater by 1930. Panel B presents reduced form results from regressing outcomes on theater presence in 1914, separately for counties in and outside of Kansas, where BON was banned. Outcomes are: whether a county had an active chapter of the Third KKK in the 1960s (Column 1), whether a hate group was active in a county any time from 2000 to 2019, based on reporting from the Southern Poverty Law Center (Column 2), and the average annual number of hate crimes per 100,000 residents from 2000 to 2018 and comes from FBI Uniform Crime Reports (Column 3). Standard errors are clustered by state except for Kansas counties when they are heteroskedasticity-robust. Regressions include all demographic, social capital, media and racism controls described in Table III. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.

Table A.XVI: Long-Run Effects on Racial Hate: Mediation Analysis

Dep. Var.	KKK 1930	KKK 1960s		Hate Group 2000-2019		Hate Crimes 2000-2018	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Screened	0.685*** (0.144)	0.147** (0.070)	0.000 (0.017)	0.345*** (0.100)	0.073** (0.036)	1.068** (0.488)	-0.001 (0.108)
KKK1930			0.214 (0.132)		0.397** (0.177)		1.562* (0.868)
Endogenous Var.	Screened	Screened	KKK1930	Screened	KKK1930	Screened	KKK1930
Controls	✓	✓	✓	✓	✓	✓	✓
F-Stat	81.97	81.97	31.48	81.97	31.48	81.97	31.48
Obs.	3,103	3,103	3,103	3,103	3,103	3,103	3,103
Total Eff ($\beta_{\widehat{Screen}}^{KKK}$)		0.147		0.345		1.068	
Indirect Eff ($\beta_{\widehat{Screen}}^{KKK1930} \times \beta_{\widehat{KKK1930}}^{KKK}$)		0.147		0.272		1.070	
Indirect Effect/Total Effect		0.998		0.788		1.001	

Notes: Table examines the mediating role of historical Klavern formation on the long-run effects on racial hate per Dippel et al. (2020). Under assumptions of conditional exogeneity (i.e. that BON screenings and long-run outcomes are exogeneous, conditional on historical Klaverns), this method allows me to estimate the share of BON's long-run effect that can be attributed to its historical effect on Klavern formation using theater presence in 1914 to instrument for both BON screenings and historical Klavern formation. Column 1 presents IV estimates of the effect of BON screenings on the mediating variable of interest, Klavern formation in 1930 ($\beta_{\widehat{Screen}}^{KKK1930}$). Columns 2, 4, and 6 present IV estimates of the effect of BON screenings on various long-run outcomes ($\beta_{\widehat{Screen}}$). Columns 3, 5, and 7 then instrument for historical Klavern formation, while directly controlling for BON screenings, to recover the long-run impact of historical Klaverns ($\beta_{\widehat{KKK1930}}^{KKK}$). The bottom portion of the Table decomposes the share of the total long-run effects of BON screenings ($\beta_{\widehat{Screen}}$) that can be explained by the film's indirect effect on historical Klavern formation ($\beta_{\widehat{Screen}}^{KKK1930} \times \beta_{\widehat{KKK1930}}^{KKK}$). Standard errors are clustered by state. Regressions include all demographic, social capital, media and racism controls described in Table III. Stars represent significance at the 10% (*), 5% (**) and 1%-level (***), respectively.