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Why Did Firms Practice Segregation? Evidence from Movie Theaters during Jim Crow

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Abstract

Racial segregation by businesses during Jim Crow was often voluntary and practiced without a legal mandate. Voluntary segregation can be driven by profit-motivated business owners catering to racist white customers or discrimination by business owners. We assess the relative importance of customers' and firms' discrimination by examining the 1953 desegregation of Washington, DC, movie theaters, which occurred rapidly because of a Supreme Court ruling affecting only businesses in Washington. Using weekly data for a nationwide sample of theaters, we find that revenues of Washington theaters fell relative to other theaters, consistent with reduced demand from biased white customers. We use a test for firms' discrimination based on a model of the screening decision for films with black actors cast in prominent roles. We cannot reject that the run length of these films was profit motivated. Together, our results point toward customer discrimination as a primary cause of public accommodation segregation.

1. Introduction

Many cities and states were slow to dismantle the institutions of segregation. Prior to the Civil Rights Act of 1964 (Pub. L. No. 88-352, 78 Stat. 241), segregation in public accommodations significantly affected African Americans' access to public services and private businesses. While southern cities frequently mandated the separation of races—often in specific settings such as hospitals, restaurants, and public transportation—segregation was practiced to a significant

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degree by businesses even in the absence of any legal proscription. Indeed, businesses often excluded black patrons despite city ordinances banning segregation, which were regularly ignored and unenforced.

In the absence of mandated segregation, a firm's decision to exclude minority customers reflects the racial preferences of its stakeholders, including owners, workers, and customers. Our goal is to understand how the racial biases of customers and firms influenced the practice of racial exclusion. We study movie theaters in the early 1950s, when explicit segregation was still common in southern states. The effect of segregation on firms' profitability can be used to infer the relative importance of the racial preferences of the firm's stakeholders. Racial exclusion can be profitable when responding to the biases of white customers. Conversely, business owners may be willing to sacrifice profits to satisfy their own prejudices.

In the main part of the paper, we use unique data on the weekly box office revenues from a nationwide sample of movie theaters to estimate the effects of the 1953 desegregation of Washington, DC, businesses on the revenues earned by the city's theaters. We then supplement these results by examining films with black actors cast in prominent roles and how their box office performance, as measured by revenues and run lengths, depended on the racial bias of the city in which the theater was located. As we discuss below, the results from these two empirical exercises together allow us to separately test for the influence of customers' and firms' discrimination.

The desegregation of Washington, DC, businesses occurred rapidly in the summer of 1953. Until then, the movie theaters in Washington barred attendance by African Americans despite long-forgotten 19th-century laws outlawing segregation in public accommodations in the city. A US Supreme Court ruling in June 1953, which applied only in the District of Columbia, subsequently required that those laws be enforced, which led to a rapid desegregation of the city's businesses.

Using a difference-in-difference design, we find that revenues of theaters in Washington fell by 11 percent after desegregation relative to theaters in other cities showing similar movies, and the timing of the revenue response matches the date of the Supreme Court's ruling. While the opening of movie theaters to the African American market could conceptually influence optimal theater pricing, there is no strong evidence of a price response. We conclude that ticket sales to white customers fell after desegregation, at least in the short run. Finally, we find suggestive evidence that desegregation altered the composition of movies selected to be screened in favor of those more popular with African American audiences. These results are consistent with the hypothesis that customers' discrimination played a role in perpetuating segregation.

The postintegration decline in profits strongly suggests discrimination by customers, but from this result we cannot say whether firms were also prejudiced and whether that may have contributed to racial segregation. We address this question in the second part of the paper. Using a theoretical model of screening decisions by movie theaters, we show how the revenue earned by films with black actors, in conjunction with their run lengths, can be used to test for firms'

prejudice. The intuition of the test is based on the fact that a movie's run length is decided by the theater (or its agreement with the production studio) and not by the customer. Conditional on revenues earned by a movie through week t of its run, the probability of continuation (hereafter, continuation probability) in week $t + 1$ of a movie with a black cast member depends on discrimination by firms but not customers. The greater is the racial bias of the firm, the lower will be the conditional continuation probability of the black-cast movie. In other words, by ending the run of still-profitable movies, a racially biased theater owner makes a fiscal sacrifice to satisfy his or her racial bias.

We compile data on all movies with black actors that were produced by major studios and released during the years covered by our theater revenue data. To measure racial bias, we use the index we constructed in Gil and Marion (2018), which was formed from respondents' views on race and segregation in public opinion polls from the late 1940s and early 1950s. We find that a movie with black actors screened in an area with greater racial bias earned less revenue—around 11 percent less—compared with what it would have earned in an area with less racial bias. The run length contributes to this difference, as there is a .12 difference between racially biased and unbiased areas in the number of weeks that black-actor movies were screened. However, we are unable to reject a null hypothesis that firms are unbiased. Conditional on the revenue earned in the first week, the difference in the continuation probability of black-cast and white-only movies was not influenced (statistically) by the racial bias in the city. Similarly, conditional on run length, the revenue difference between the two types of movies does not depend on racial bias. Together, our results point toward customers' discrimination as a key determinant of theater policy during this era. We fail to find evidence of racial discrimination by firms, and to the extent that firms' owners or their workers are biased, the effects of these preferences appear to be secondary.

Analogous to the discrimination faced by black workers discussed in Becker (1957), the prejudice experienced by black moviegoers is determined by the discrimination of the marginal theater, and racial exclusion could remain the policy at many theaters without affecting the consumption opportunities of black audiences.¹ Indeed, at the time there were many theaters specifically targeting African American customers (Gil and Marion 2018). However, customers' discrimination creates profit rewards for racial exclusion that can survive entry, and movie theaters (along with many other public accommodations) are characterized by increasing returns. Entry of firms specifically serving a minority market would be disadvantaged by operating at a smaller scale, and African American theaters of the era did in fact have less capacity, smaller screens, and fewer amenities. Therefore, black welfare can suffer as a result of racial exclusion even with the entry of unbiased firms.

Segregation of public accommodations has received little attention in the economics literature. This may be due in part to the practical complication that

¹ By focusing on movie consumption, we do not wish to downplay the broader negative repercussions of a society widely excluding individuals on the basis of race.

segregation laws were inconsistent and piecemeal, and their enforcement was uneven. Furthermore, segregation often resulted from informal local practices rather than formal laws. Cook et al. (2020) use data from volumes of *The Negro Motorist Green Book*, an annual directory published from 1938 to 1966 listing businesses serving African American customers, to document several facts about the geographic patterns of response to discrimination in public accommodations. Wright (2013) provides a history of desegregation in public accommodations preceding the passage of the Civil Rights Act of 1964, noting that the fear of alienating white customers motivated segregating firms. While not specifically attempting to identify the effect of segregation on profits, Wright shows that retail sales grew in southern areas during the 1960s at a rate meeting or exceeding that in other regions. Since that period coincided with widespread desegregation, Wright argues that desegregation was a positive force for businesses, in contrast to our findings.² One way to reconcile the two results is to consider that racial attitudes improved during that time, and the effect of desegregation on demand by white customers may have been declining. An alternative interpretation is that the Civil Rights Act positively affected the economy, including southern labor markets, which could instead be responsible for the increase in retail sales in the South.

Our study also fits with a recent literature studying historical institutions of racial bias and residential segregation. Troesken and Walsh (2017) examine how residential segregation ordinances arose in the early 1900s, finding that where whites more easily organized to enforce informal residential segregation norms, segregation laws were less likely to be implemented. Boustan (2010) examines the role that postwar black migration played in the suburbanization and resulting residential segregation of northern cities. Cook, Logan, and Parman (2018), using the detailed measures of residential segregation developed by Logan and Parman (2017), find that segregation increased racial violence in the form of lynchings of African Americans. This suggests a causal channel running from segregation to racial preferences and discrimination. Importantly, historical lynchings have lingering effects and are related to modern rates of racial violence (see Messner, Baller, and Zevenbergen 2005; King, Messner, and Baller 2009).

Our results also relate closely to the established literature on discrimination against customers and workers. An important antecedent to our study is Heckman and Payner (1989), which examines the impact of federal antidiscrimination legislation on the employment outcomes of black workers. In a clear analogue to our setting, firms' owners had a profit motive to hire black workers yet also felt pressure from customers, white workers, and other stakeholders to exclude black employees. In a paper considering the modern film industry, Kuppusswamy and Younkin (2020) find that films in 2011–15 with more diverse casts are associated with higher box office revenue. As in our study, Leonard, Levine, and Giuliano (2010) use sales to uncover discrimination by customers, finding that a mismatch between the demographics of the employees at a retail outlet and the residents in

² Epstein (1995) also suggests that private businesses may have welcomed the desegregation of public accommodations resulting from the Civil Rights Act.

the surrounding neighborhood has a small negative impact on sales. Similarly, Holzer and Ihlanfeldt (1998) find evidence of customers discriminating in US retail. Bar and Zussman (2017) examine a similar question, showing that Jewish customers in Israel prefer to be served by Jewish rather than Arab workers, which in turn influences the hiring decisions of employers. Waldfogel and Vaaler (2017) consider how firms are willing to forgo profits to appease the ethnic biases of customers: airlines omit Israel from online route maps if they serve customers from countries with stronger anti-Semitic views.

The paper is organized as follows. Section 2 contains the model. In Section 3, we provide a background description of relevant institutional details, and in Section 4 we describe the data. Section 5 shows the empirical results related to the impact of desegregation on firms' profits, while in Section 6 we describe the results related to the box office performance of films with black actors. Section 7 concludes.

2. Model

We begin by specifying a simple economic framework that formalizes the intuition of how we empirically distinguish the role of customers' and firms' discrimination in influencing the racial policies of firms. The profit response to desegregation can help distinguish the relative importance of consumers' discrimination from firms' discrimination, and we can test for discrimination by firms by examining the screening decisions of theater owners.

The theater owner receives utility from the profits earned by screening movies. If the owner is racially biased, the firm receives disutility δ^f if black customers attend or if the movie being screened has a black actor.³ White customers may also be racially biased, and movie demand from white customers is lower if the theater is integrated or if the movie being shown has a black cast member.

2.1. Desegregation and Theater Revenue

Under integration, the theater sells tickets to both whites and blacks, but the racially biased owner incurs disutility from serving blacks. Under segregation, the theater sells only to the white customer base but does not incur a utility loss from racial bias. The theater owner's utility therefore is given by $U^S = \pi_w^S$ under segregation and $U^{DS} = \pi_w^{DS} + \pi_b^{DS} - \delta^f$ under desegregation, where π_w^S and π_w^{DS} are the profits from white customers with and without segregation, respectively, and π_b^{DS} is the profit earned from black customers under desegregation. Without a legal mandate, the firm chooses to exclude black customers if $U^S > U^{DS}$:

$$\pi_w^S - \pi_w^{DS} + \delta^f \geq \pi_b^{DS}. \tag{1}$$

Segregation increases utility by increasing the profits from white customers,

³ For convenience, we use the parameter δ^f when referencing the racial preferences of firms both with respect to serving customers and screening movies with black cast members. While likely correlated, there is no reason to think they are of the same magnitude.

given by $\pi_w^S - \pi_w^{DS}$, and from satisfying the firm's racial bias but sacrifices profits from black customers.

Suppose the firm is initially segregated and is exogenously induced to integrate. In this case, the profit response can be expressed by

$$\Delta\Pi = -(\pi_w^S - \pi_w^{DS}) + \pi_b^{DS}. \quad (2)$$

According to equation (1), δ^f is greater than the right-hand side of equation (2). Therefore, if profits rise, it must be the case that $\delta^f > 0$, and firms are biased. If profits fall, then customers' bias must exist, and it must be large enough to outweigh any gains from serving black customers.⁴ A drawback is that this prediction is one-sided—its sign points toward discrimination of one side of the market but cannot at the same time test for the other side of the market.

To examine the empirical implications explicitly, consider the potential outcome model for profits:

$$\Pi_{ijt} = \Pi_{ijt}^{DS} D_{it} + \Pi_{ijt}^S (1 - D_{it}), \quad (3)$$

where i , j , and t index theater, movie, and date, respectively, and D_{it} is a desegregation treatment indicator. Equation (3) can be rearranged as follows:

$$\Pi_{ijt} = \Pi_{ijt}^S + \Delta\Pi_{ijt}^{DS} D_{it}. \quad (4)$$

The term $\Delta\Pi^{DS}$ represents the change in profits from desegregation, $\Pi^{DS} - \Pi^S$, and as described in equation (2) is composed of lost profits from racially biased whites and the gain in profits from newly served blacks. It is the effect of treatment on the theaters receiving the desegregation treatment. In our empirical model, the impact of a change in policy from segregation to desegregation, given by Δ^{DS} , can be identified from a difference-in-difference specification in which the profit level after desegregation is compared with the profit level under segregation, and the difference is benchmarked against a set of control theaters that did not experience a change in segregation policy.

2.2. Screening Choice and Racial Bias

This section provides the theoretical foundations for a test that evaluates firms' bias. We enrich the specification of movie theaters' profits to describe their dependence on the movie being screened and the length of the movie's run. From this specification of profits, we can model the endogenous screening choice of theater owners, from which we can derive tests for owners' discrimination.

The revenue earned by a film at a particular theater is a combination of exogenous movie popularity and the endogenous decision of the theater owner regarding whether, and for how long, to screen a film. Consistent with movie theaters of this era, we assume that a theater screens only one film per week. Suppose that the weekly revenue that a theater earns by screening a film with a black cast mem-

⁴ We do not explicitly model employees' discrimination. Racist workers may require higher wages after desegregation, which could contribute to a decline in profits. Our estimations examine revenues rather than profits, and so any such wage response would be absent from our estimates.

ber depends on the overall popularity of the movie, the racial bias of the local population, and how long the film has been at the theater:

$$\pi(t) = [\rho - \delta^c I(B)]e^{-t}, \tag{5}$$

where ρ is the film’s overall popularity and $I(B)$ is an indicator for a black-actor movie. The degree of racial bias of customers in the city is given by the parameter δ^c . The variable t is the amount of time the film has been shown at the theater, with opening weekend set to $t = 0$.

There is an outside option for the theater, $r_0 \sim F(r_0)$, which it earns should it choose not to screen the movie. The firm may also be racially biased, which is captured by the parameter δ^f . The revenues of a film with a black cast member must be above $r_0 + \delta^f$, so such a movie is screened if $\pi(t) \geq r_0 + \delta^f I(B)$. If $\rho - \delta^c < r_0 + \delta^f I(B)$, then revenue is initially (for $t = 0$) less than the reservation value, and the film is never screened by the theater.⁵

The movie is screened until its revenue falls below the outside option. The total length of the film’s run at the theater is therefore found by computing $\pi(t) = r_0 + \delta^f I(B)$, which yields

$$t^* = \ln[(\rho - \delta^c)/(r_0 + \delta^f)] \tag{6}$$

for movies with black actors. The total box office take, Π , is then found by integrating $\pi(t)$ over the length of the run:

$$\Pi = \int_0^{t^*} \pi(t)dt = \rho - r_0 - (\delta^c + \delta^f)I(B). \tag{7}$$

The probability that the movie is screened at a theater is equal to the likelihood that initial revenues exceed the outside option: $\Pr(\text{Screened}) = \Pr[\rho \geq r_0 + (\delta^c + \delta^f)I(B)]$. From the distribution of r_0 , this becomes

$$\Pr(\text{Screened}) = F(\rho - (\delta^c + \delta^f)I(B)). \tag{8}$$

Inspecting equations (6)–(8), we see the difficulty in distinguishing customers’ from firms’ discrimination. For each of the three outcomes, the effect of the two channels of bias cannot be separately identified. One might think that using weekly revenue would overcome this problem, since equation (5) contains only the term for customers’ discrimination. However, we must account for selection, and revenue is observed only for films for which revenue exceeds the reservation value. The expected revenue of a black-actor film in the initial week of release is given by

$$E[\pi(0) | \text{Screened}] = \int_{\rho - \delta^c - \delta^f}^{\infty} (\rho - \delta^c)f(r_0)dr_0 = [1 - F(\rho - \delta^c - \delta^f)](\rho - \delta^c), \tag{9}$$

where yet again consumers’ and firms’ discrimination are inseparable.

⁵ Employees’ discrimination in this model will have an effect similar to owners’ discrimination. If workers are racially biased, then a theater owner willing to screen black-cast movies may have to pay workers a higher wage. This will increase the revenue threshold that the black-cast movie would need to clear to be screened.

To overcome this challenge, we propose a test that will distinguish firms' discrimination. Intuitively, if one were to condition on the revenues in the first week of a movie's run, firms' discrimination can be singled out by examining the continuation probability—the likelihood that the film will continue to be screened. The biased owner will be willing to stop the run of a still-profitable film. An analogous test conditions on run length and examines the total revenue earned during the run.

Consider two movies, one with a black cast member and the other with an all-white cast. Refer to these movies as B and W. By conditioning on initial revenues, we are comparing movies for which $\rho_B - \delta^c = \rho_W$. The conditional continuation probability of a black movie is therefore $\Pr(\text{B Screened in } t = 1) = F(\rho_B - (\delta^c + \delta^f)I(B)) = F(\rho_W - \delta^f)$, which depends only on δ^f .

A similar observation can be derived by examining the total revenues conditional on run length. Consider again the hypothetical movies B and W. If we condition on run length, then we are comparing movies for which $t_W^* = t_B^*$. From equation (6), this implies $\rho_W/r_0 = (\rho_B - \delta^c)/(r_0 + \delta^f)$. Notice that this implies that the black-actor movie is more popular with customers under firms' discrimination. Combining this expression with equation (7), we see that $\Pi_B = \rho - r_0 + \delta^f[(\rho/r_0) - 1]$. This equation does not depend on δ^c but depends positively on δ^f since $\rho > r_0$ for screened movies. Since the racially biased owner stops screening the more popular black-actor movie while it is still profitable, it will earn more revenues over the course of its run.

3. Background

3.1. Segregation, Jim Crow Laws, and Movie Theaters

Segregation in public accommodations was an important feature of African American life for much of the 19th and 20th centuries. The separation of races in businesses, schools, and social services was the norm in many parts of the country, even prior to being codified into law as was eventually the case in much of the South (Wright 2013). There was a substantial degree of variation in segregation-related laws over time and across jurisdictions, yet laws mandating segregation tended to be piecemeal and local. As an example, the first statewide segregation laws were applied to seating in railroad cars (Wright 2013). When segregation was implemented by businesses, it was often voluntary rather than mandated by law. In the North, segregation was not institutionalized, and many northern states passed laws at various points in time banning segregation in public accommodations. However, these laws were often ignored, imperfectly enforced, or interpreted in such a way that allowed for the continued exclusion of blacks.

Until the Civil Rights Act of 1964, explicit segregation was a pervasive feature of movie theaters in the South. Movie theaters for white audiences often either completely barred admission to black customers or would offer worse seating to only a portion of the screenings. A substantial number of theaters serving black customers entered the market in response to the limited access to mainstream

movie theaters. Elsewhere (Gil and Marion 2018), we document the pattern of entry by theaters primarily serving African American audiences. Cities and counties with greater black population shares experienced more theater entry, particularly areas with a greater degree of residential segregation. This entry could be substantial. While African American theater entry filled the void created by theater segregation practices, movie consumption remained unequal between races. African American theaters were not perfect substitutes for mainstream theaters, as they more often showed second-run movies and were less likely to offer amenities. It was rarer, for instance, for an African American theater to be air conditioned, and in the early years of cinema it was less likely for an African American theater to have sound.⁶

3.2. Desegregation in Washington, DC

Prior to 1953, segregation in Washington, DC, was widely practiced. Racial exclusion was not required by law, and some businesses admitted customers of any race. However, segregation in restaurants, hotels, theaters, educational institutions, hospitals, and other places of public accommodation was the norm (Gomery 1998; Pritchett 2005).^{7,8} According to historical accounts in Headley (1999), the exclusion of black patrons at white theaters was complete and did not allow for special sections or showtimes for African American audiences.⁹ It was also theater policy to exclude live productions with black cast members.¹⁰

Antisegregation laws were enacted by the Washington city government in 1872 and 1873, at a time when Washington was an independent municipality. These laws, later referred to as the “lost laws” because most people were unaware of their existence, were unenforced and largely forgotten until they became the center of a legal challenge brought by a small group of civil rights activists who were denied service at Thompson Restaurant, a local business. The case eventually reached the US Supreme Court, which ruled on June 8, 1953, that the laws must be upheld (*District of Columbia v. Thompson*, 346 U.S. 100). The ruling applied only to the enforcement of these historical antisegregation laws in Washington and consequently did not relate to the legality of segregation in other parts of

⁶ Unfortunately, our primary data set does not contain the revenues of African American theaters, and we are unable to study how they may have been impacted by desegregation.

⁷ As Gomery (1998, p. 14) states, “A handful of hotels and restaurants opened their doors to all races, but through the early 1950s, hospitals, hotels, restaurants, public schools and movie theaters remained ‘Jim Crow.’”

⁸ One unique feature of Washington, DC, is the substantial presence of the federal government as an employer. Federal office buildings may have experienced less segregation than private workplaces, though the federal government does have a history of discriminatory practices whose legacies may have continued into the 1950s. The Woodrow Wilson administration explicitly segregated the federal workforce and demoted black employees, as documented by Aneja and Xu (2020). As described in King (2007), applicants to federal jobs from 1914 to 1940 were required to submit a photo, which likely reinforced segregation practices.

⁹ The Dupont Theater was desegregated from the date of its opening in 1948. It was a small theater, and the vast majority of films it screened were independently produced.

¹⁰ Headley (1999) recounts that in 1946, the dramatists guild refused to sign contracts with Washington, DC, theaters over this policy. Theaters claimed that the policy was driven by local sentiment.

the country. The ruling was widely reported after its announcement. Much of the front page of the June 9, 1953, issue of the *Washington Afro-American*, a newspaper with an African American readership, was devoted to coverage of the ruling. That day's edition of the *Washington Post* also included an article about the ruling, stating, "The court voted 8–0 to declare still valid the 'lost law' of 1873 which made it criminal for operators of restaurants and similar establishments to refuse equal service" (*Washington Post* 1953, p. 1).

Newspaper articles at the time focused on the desegregation of restaurants, as this was the impetus for the legal case and because the 1873 antisegregation law specifically applied to eating establishments, while the 1872 law was somewhat broader. Although there was initially some uncertainty whether the ruling would apply narrowly to restaurants or more broadly to other places of public accommodation, the historical accounts clearly indicate that Washington theaters desegregated in 1953 at some point after the Supreme Court ruling.¹¹ We do not have direct evidence regarding the exact timing of when admission to Washington theaters was opened to black customers, but the article in the *Washington Post* indicated that the ruling would take effect almost immediately, stating that "operators will be given 'a day or so' to study the court's ruling. Thereafter, any failing to comply will be prosecuted" (*Washington Post* 1953, p. 1). Several major theaters in late September 1953 issued statements that they had been admitting black patrons "for several months" (Headley 1999, p. 180). Indirect evidence suggests that theaters would have been prompted to desegregate in response to the ruling. The civil rights activist Mary Church Terrell, one of the restaurant patrons who brought the original suit against Thompson Restaurant, signaled her intent on June 20, 1953, to also bring suit against Washington movie and stage theaters should they not change their admission policies (*Afro-American* [Washington, DC] 1953). Furthermore, President Dwight Eisenhower said in his State of the Union address earlier in 1953 that he would "use whatever authority exists in the office of the President to end segregation in the District of Columbia" (H.R. Doc. 53-75, 99 Cong. Rec. 752), which combined with the Supreme Court ruling would strongly indicate that segregation of public accommodations was at an end.

The date of the Supreme Court ruling in June 1953 is our treatment date in the empirical work that follows. As a robustness check, we run a series of placebo tests on other possible treatment dates, and we provide evidence regarding the timing of the response on theater revenue.

4. Data

Our primary data come from weekly issues of *Variety* published between January 3, 1945, and December 28, 1955. These data consist of an unbalanced panel of 393 theaters in 26 cities. When estimating the effect of desegregation, we restrict attention to the time periods immediately surrounding the Supreme Court's desegregation decision, usually 1951–55. Each week, *Variety* listed the revenue

¹¹ Segregation in education was not affected by the ruling and continued for another year until the US Supreme Court ruling in *Bolling v. Sharpe* (347 U.S. 497 [1954]).

earned by a theater, the highest and lowest ticket prices it charged, and the movies it screened.^{12,13} Two revenue measures are presented—a preliminary estimate of the revenue earned in the current week and a revised measure of the revenue earned in the prior week. These measures regularly differ, and we take the revised measure as the more accurate one. The data also contain a limited amount of theater-level information, including capacity and ownership. Not all theaters report revenue data in all weeks, and in the empirical section we limit our attention to theaters with at least 2 full years of movie observations. One might suspect that the effects of desegregation may spill over to nearby theaters, as customers substitute from Washington theaters toward those located outside the city limits in southern Maryland or northern Virginia. The cities nearest to Washington in our data set are Baltimore and Philadelphia. In our view, these cities are too distant for this type of substitution to be detectable.¹⁴

We calculate two demand covariates in the data—age of the film and number of weeks a film has been at a particular theater. Age is calculated on the basis of the time elapsed since the date the film first appeared in the data. Weeks at a theater is similarly calculated as the time elapsed since the film was first screened at a particular theater. We also obtain daily weather data for each city from the National Oceanic and Atmospheric Administration online historical climate data, typically using observations from the weather station located at the city's airport, from which average daily high and low temperatures and precipitation are calculated for the relevant week.

4.1. African American Actors

A secondary focus of the paper is the revenue earned by films with black actors in significant roles. Few movies in this era had black cast members at all. This fact is the result of several forces. First, as we argue, white customers were not receptive to black cast members. A second reason is related to the economics of movie production and the ability of studios to produce films intended to appeal to minority groups. Silent films were much cheaper to produce than movies with sound, and consequently the size of the target audience required to make movie production profitable could be much smaller. Silent films were made obsolete by the advent of films with sound, which contributed to the decline of a relatively thriving African American silent film industry.

¹² *Variety* does not indicate what the prices represent. They may represent differential pricing for matinees versus prime-time showings or differential pricing by the desirability of the seating in the theater.

¹³ The revenue sometimes, but not always, exhibits rounding in \$500 increments. Since revenue is the dependent variable, this will not lead to bias in the regression estimates (Schneeweiss, Komlos, and Ahmad 2010).

¹⁴ In general, substitution effects will be difficult to detect. Even if all the customers who stopped attending Washington theaters shifted to movie attendance in the Washington suburbs, attendance would be spread across theaters located in a donut surrounding the District of Columbia, perhaps inclusive of as many or more theaters as are located in the city. More generally, substitution away from movies in Washington would be toward movies outside Washington, baseball games, TV or radio at home, or perhaps nothing at all. Detecting substitution patterns in any one of these alternatives is unlikely.

We obtain information about the racial makeup of a film's cast by compiling information provided in Klotman (1997). This source provides a listing of any black actor, writer, director, or producer involved in a film. We suspect that black actors in very small or nonspeaking roles have little impact on a movie's prospects, and so we collect further information on the importance of each actor's role. We do not have information about an actor's screen time or the number of lines. To assess whether a role is significant, we use the movie cast list in the Internet Movie Database (IMDb). We consider the role significant if the actor is among the first five cast members listed.¹⁵ Because foreign and independent films are likely to have niche audiences that differ from the average moviegoer, our focus is on movies produced in the United States by one of the major production studios. We also rule out movies in which musicians are playing themselves (such as Count Basie playing music), sports movies, and movies portraying African Americans in a negative or stereotypical light.

In total, 176 movies were produced between 1945 and 1955 with any black actors. Actors' roles were small in a majority of the films. Normally, when a black cast member was a lead or had a significant part, the production company was an independent studio. From 1945 to 1955, 92 movies with black actors were produced by independent studios. In more than half (51 percent) of those films, the black actor was a lead, which we define as being in the first two in the IMDb cast list. In contrast, only three of the 84 major studio movies with black actors featured a black lead. Figure 1 shows the films with significant black roles produced by year. Independent studios produced a number of films with black actors in significant roles in the late 1940s. From 1946 to 1948, 41 such films were produced by independent studios compared with just two produced by major studios. It is worth noting that even during the late 1940s, "race films" were a small share of total film production. According to data from the American Film Institute, 1,218 were produced from 1946 to 1948 (Gil 2010). Consistent with anecdotal evidence, the following years experienced a rapid decline in production. From 1949 to 1955, fewer than two movies with black actors in significant roles were produced per year by independent studios on average. This coincided with a modest increase in the production of such movies by major studios. While still relatively rare, by the early 1950s movies with black casts had become more commonplace, particularly as evidenced by the 1953 release of *Bright Road* by MGM and the 1954 release of *Carmen Jones* by 20th Century Fox. The cast members of both films were primarily African American.

In the empirical analysis of the revenues earned by films with black actors, we focus on films produced by major studios. The theaters in the revenue data were larger than a typical theater, and major studio films were more important for these mainstream movie theaters. This is most important for the independently produced race films, which were likely to be screened at African American theaters. Table A1 provides a listing of the movies that meet these criteria.

¹⁵The Internet Movie Database typically reports the cast list from the end credits, which is commonly in order of appearance. While not necessarily the case, it is often true that important characters are introduced early in a film.

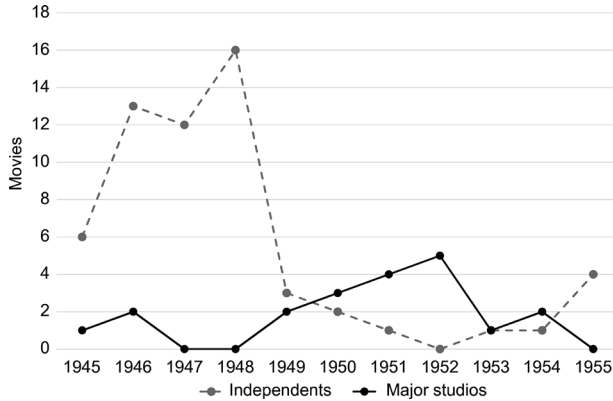


Figure 1. Movies with significant black characters

4.2. Racial Bias

In Gil and Marion (2018), we form a measure of racial bias that varies by state. We use microdata from public opinion polls from the late 1940s and 1950s, most of them conducted by Gallup. The microdata contain a person’s race, state of residence, and responses to questions related to racial attitudes.¹⁶ We form an index of racial bias by following an approach similar to that taken in Charles and Guryan (2008).¹⁷ First, we order the possible responses to each question from what we judge to be most racially tolerant (assigned a low score) to the least racially tolerant (assigned a high score). The rescaled scores are then standardized using the within-survey mean and standard deviation for a question. Since the number of respondents varies across surveys, we average the standardized responses by survey and state, so that no one survey receives undue weight in the racial bias measure. We then average across surveys by state to obtain the index of racial bias.

4.3. Summary Statistics

Table 1 presents summary statistics at the theater level for the estimation sample. The average theater earned \$13,464 in a week, with the figure slightly lower in Washington than in other cities. This may be in part due to differences in average theater capacity. The average capacity across theater-weeks in the data is 1,520 for Washington theaters versus 2,064 for theaters outside the city. The high price charged for movies is just over \$1 for both Washington-area theaters and theaters outside of Washington. The average low price in the data is 68 cents and is very similar for Washington and non-Washington theaters. The average

¹⁶ A list of the polls we use, and the relevant race-related questions from those polls, is provided in Gil and Marion (2018).

¹⁷ An example of a different approach taken in the literature to measure racial bias is Stephens-Davidowitz (2014), who forms a state-level measure of racial bias using Google searches to understand how racial animus affected voting in the 2008 and 2012 elections.

Table 1
Summary Statistics

	Washington	Other Cities	Average
Revenue earned in previous week	10,962.30 (7,498.70)	13,582.90 (15,327.60)	13,464.00 (15,070.60)
High price	1.039 (.395)	1.124 (.461)	1.120 (.459)
Low price	.644 (.196)	.684 (.237)	.682 (.236)
Film age (months)	7.177 (18.77)	7.270 (17.97)	7.265 (18.00)
Weeks film at theater	5.142 (12.43)	3.396 (8.370)	3.475 (8.603)
Theater capacity	1,520.4 (964.4)	2,064.4 (1,151.6)	2,039.7 (1,149.3)
Films screened	1.025 (.163)	1.350 (.488)	1.335 (.483)
N	2,125	44,713	46,838

age of the movie screened is 7.3 months, though the distribution of movie age is highly skewed. One-quarter of screenings are of movies released in the same month, and the median movie screened is 2 months old. Consistent with a median film age of 2 months, the average film shown has been in the theater for 3.48 weeks. For Washington theaters, the average length of a movie's run is longer (5.1 weeks versus 3.4 for the rest of the United States). This is largely driven by a few movies experiencing very long runs.¹⁸

In the empirical work to follow, we examine price as an outcome. However, we note here that price changes are infrequent and often temporary. In 1953, the average theater changed its high price 3.4 times, with Washington theaters changing them only 3.1 times, and many changes were quickly reversed. Furthermore, 73 percent of the 1953 price changes in Washington were subsequently undone by offsetting price changes of the opposite sign in the same calendar year.

Finally, it is possible for more than one movie to be screened at a theater in a given week, even though the multiplex had yet to become a feature of the theater industry. In Washington, theaters virtually always screened only one movie in a week. This is true for 97.5 percent of the theater-week observations. Outside Washington, it was more common for multiple films to be screened in the same week, and the average theater screened 1.35 films. In 65 percent of the theater-weeks, one film was screened, while two films were screened 34 percent of the time. More than two films were screened in only .24 percent of theater-weeks.

5. Estimated Effect of Desegregation

We estimate a difference-in-difference specification comparing the weekly box office revenues earned by films shown at theaters in Washington with revenues

¹⁸ If one were to exclude two movies, *Cinerama Holiday* and *This Is Cinerama*, the average theater run would drop to 2.8 weeks for Washington and 2.4 weeks for the rest of the United States.

earned by theaters in other cities before versus after the desegregation of Washington theaters. This specification follows naturally from the potential outcomes framework described by equation (4). The unit of analysis is a theater-film-week, and we estimate the following difference-in-difference specification:

$$y_{ikt} = \beta_0 + \alpha \text{Post}_t \times D_j + B\mathbf{X}_{ikt} + \rho_i + \gamma_{jt} + \varsigma_t + \varepsilon_{ijt}. \quad (10)$$

We consider two outcomes of interest, log revenue and the log high price. To reduce the influence of outliers, we winsorize the top and bottom 2 percent of the dependent variable.¹⁹

Each specification includes city-year effects γ_{jt} , except for specifications that include more detailed theater-year effects. In either case, identification is therefore based only on the months immediately surrounding the desegregation event, while other years primarily aid in increasing precision. The inclusion of city-year or theater-year effects accounts for city-specific trends that may have differentially affected demand for movies in the District of Columbia. The adoption of television is one such potential confounder. Television is a substitute for movies, and its adoption was rapid during the 1950s. The District of Columbia, with its educated populace, may have been quicker to adopt television than other cities.²⁰

Each specification also controls for year-month effects ζ_p , which allow for common shocks to affect the demand for all movies shown for a particular month. Such shocks may include macroeconomic effects and other common demand factors such as the popularity of national TV programs aired at the same time. We also include city-specific month effects, which allows seasonal patterns to differ by city. Some specifications include theater effects rather than city effects.

The vector of controls \mathbf{X} includes film fixed effects (3,332 movies were shown in the estimation sample), so we are able to compare the revenue earned by a theater with that earned by other theaters showing the same set of movies. The vector \mathbf{X} also includes weather experienced in the city that week, the age of the film being screened, the length of the film's run at the theater, and the seating capacity of the theater (in specifications not including theater effects).

Since only Washington-area theaters experienced desegregation, we have only one treated cluster. As is now widely recognized, with few treated clusters standard asymptotic standard errors corrected for clustering at the city level will likely lead to overrejection of the null hypothesis. To address this issue, we obtain p -values for the coefficient α in equation (10) using the randomization inference

¹⁹ The unit of observation in the regression is at the film-theater-week level since there are multiple films shown per week, though revenue is observed at the theater-week. We also examine the robustness to excluding theater-weeks with multiple films screened.

²⁰ Two Washington theaters in our data were renovated in 1954, which could affect revenue after desegregation. However, this will not impact our estimates because the effect will be absorbed by theater-year effects. Another change that affected movie demand was a decrease in the federal excise tax on movie tickets in 1954. From 1944 until 1954, the tax was 20 percent of the ticket price. In 1954, the rate was reduced to 10 percent, and tickets priced 50 cents or less were exempt. Since this change occurred in 1954 and affected theaters nationwide, we have no reason to think our estimates will be affected. Furthermore, we also include estimates that control for ticket prices, which should capture any relevant impacts from the change in tax.

solution based on placebo t -values suggested by MacKinnon and Webb (2019).²¹ It is still possible that an unobserved shock to theater demand in Washington, DC, coincided with desegregation by happenstance, but the randomization inference indicates the likelihood of this occurrence using the distribution of revenue changes over the same time frame in other cities.

The intuition behind this approach with only one treated cluster is straightforward. We first estimate equation (10) and obtain the t -statistic for coefficient α corrected for clustering at the city level. We then form a distribution for the t -statistic by repeating the estimation of equation (10) $J - 1$ times, where J is the number of cities in the sample. For each iteration, we assign a different placebo city to the desegregation treatment. We then compare the baseline t -statistic with the placebo distribution, and when reporting the results we show the rank in the placebo distribution.

According to MacKinnon and Webb (2019), both their randomization inference approach and a leading alternative in Conley and Taber (2011) may fail when there is only one treated cluster and there is sufficient variation in cluster size. This seems like a minor concern in our setting. First, variation in cluster size is not substantial, as the number of observations does not vary dramatically across cities. Moreover, observations for the treated city are similar to those for the median city (2,030 in Washington versus a median of 1,775 in other cities). As MacKinnon and Webb (2019) show, when the cluster size of the treated group is close to the median and cluster size does not vary too much, then randomization inference yields an appropriately sized test statistic.

5.1. Results

The results of estimating equation (10) for log revenue and log price are shown in Table 2.^{22,23} The first three revenue specifications display the robustness of the results to the inclusion of different levels of fixed effects, while column 4 includes price covariates to evaluate how any price response on the part of theaters impacts the estimated effect of desegregation on theater revenues. The point estimates of the key difference-in-difference interaction term and their precision are quite stable across the specifications. Our preferred specification, shown in column 2, includes theater fixed effects, which are important in explaining revenues.

²¹ Other approaches suggested in the literature include those in Conley and Taber (2011) and Cameron, Gelbach, and Miller (2008), though according to MacKinnon and Webb (2017) the latter, while appropriate for settings with finite clusters, fails in settings such as ours in which the number of treated clusters is small.

²² As described above, the unit of observation is at the theater-film-week, and a theater may screen multiple films in a given week. Table A2 explores the robustness of the main results to excluding theater-weeks with multiple films screened. As expected, because Washington theaters rarely screen multiple films in a week, the estimates are virtually identical to those for the full sample.

²³ The demographic characteristics of Washington are unique, and so we also consider limiting the estimation sample to theaters in cities similar to Washington. Table A3 presents the results of estimating the difference-in-difference specification of movie revenues for three subsamples of the data—cities with high black population shares, cities with a highly educated population, and East Coast cities. The estimated coefficients are similar to those for the complete sample.

Table 2
Effect of Desegregation of Washington Theaters on Revenue and Prices

	Log Revenue				Log Price	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × DC	-.11** (.022) [2/23]	-.081** (.015) [2/23]	-.090** (.013) [2/23]	-.073** (.013) [3/23]	-.024+ (.012) [8/23]	-.027** (.0077) [6/23]
Log high price				.27** (.070)		
Log low price				.0020 (.035)		
Log theater capacity	.49** (.069)				.012 (.016)	
Log film age (months)	-.072** (.014)	-.067** (.012)	-.058** (.011)	-.066** (.012)	-.0093* (.0033)	-.0035 (.0024)
Log weeks film at theater	-.34** (.042)	-.33** (.029)	-.35** (.028)	-.33** (.030)	.014* (.0053)	.0024 (.0030)
City-year fixed effects	Yes	Yes	No	Yes	Yes	Yes
City-month fixed effects	Yes	No	Yes	No	Yes	No
Theater fixed effects	No	Yes	No	Yes	No	Yes
Theater-year fixed effects	No	No	Yes	No	No	No
<i>N</i>	61,508	61,507	61,500	61,507	61,508	61,507
<i>R</i> ²	.71	.82	.84	.82	.86	.90

Note. The unit of observation is a theater-film-week. All specifications include controls for high and low temperature, rain and snow precipitation, and film dummy variables. Asymptotic standard errors clustered by city are in parentheses; ranks of the randomization inference *t*-statistics are in square brackets. Significance refers to inference using the asymptotic standard error.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

Including more detailed theater-year effects, as shown in column 3, adds little explanatory power but requires the estimation of several hundred additional coefficients.

The specification in column 1 suggests that revenue fell in Washington, DC, theaters by 11 percent after desegregation compared with theaters in other states showing similar movies. The randomization inference procedure shows that the *t*-statistic from the clustered standard errors is ranked second among the 23 cities in the exercise. The theater fixed effects in column 2 account for changes in theater composition that arise because of missing values. This has little effect on the estimated effect of desegregation, as the point estimate is .081 with the same *p*-value. The point estimate for the specification in column 3 with theater-year effects is similar to those in columns 1 and 2. This rules out the results being driven by theater-specific shocks that affected only some theaters in the Washington area after mid-1953.

Column 4 explores whether a pricing response by firms can explain any of the estimated effect of desegregation on movie revenues. The elasticity of revenue with respect to the highest price charged by the theater is .27 and statistically sig-

nificant.²⁴ Conversely, the low price has little effect on revenue. The inclusion of controls for the log high and low prices has little effect on the estimated effect of desegregation. The decrease in revenue can almost entirely be attributed to a reduction in the quantity of tickets sold.

The last two specifications in Table 2 show that desegregation had at most a modest effect on the price charged. Column 5 indicates there was a 2.4 percent decline in the highest price charged by Washington theaters after desegregation compared with theaters in other cities. This point estimate is statistically insignificant whether inference is based on the asymptotic standard errors with city clustering or randomization inference. With theater fixed effects, the point estimate is little changed. This is statistically significant based on asymptotic standard errors, but randomization inference points toward a p -value above .25.

To establish that the timing of the revenue response corresponds with the date of the desegregation event, we estimate a version of equation (10) that includes interactions of the Washington dummy variable and a series of indicators for the months surrounding the date of the Supreme Court decision that led to desegregation. Figure 2 plots these estimated coefficients, which represent the mean log revenue earned in Washington relative to the rest of the United States in each month, conditional on covariates. The bars represent the 95 percent confidence intervals based on the asymptotic clustered standard errors. In the month of desegregation, there is a noticeable and sustained decline in the box office revenues of Washington theaters relative to the rest of the United States. The point estimates suggest a decline in revenues of 11.5 percent in the month following desegregation. While some of the revenue effect dissipates approximately 4 months after desegregation, the revenues in Washington are consistently lower for 7 months after desegregation. In the months prior to desegregation, the conditional mean revenues vary between 0 and .1. Three months prior to desegregation, there is a small spike in revenues that gives the visual impression of a pretreatment trend, but during the pretreatment period revenues generally are centered around a mean above 0. The decline in the month of desegregation is notably larger than nearly all of the variation in the pretreatment period, and revenue remains visibly lower after desegregation. For instance, the difference in coefficients for date $t = -2$ and date $t = -1$ is .042, and the difference between the coefficients for $t = -3$ and $t = -2$ is .06. The coefficients are small in comparison to the decline at $t = 0$.

Finally, we conduct a placebo exercise in which we estimate the difference-in-difference specification for each possible counterfactual treatment week in Washington from January 1952 until June 1955. In the main regression specifications the desegregation impact is identified from variation in 1953. The placebo exercise is consistent with this by defining the year as beginning 5 months prior to

²⁴ This implies an elasticity of demand for movie tickets of $-.73$. At this demand elasticity, theaters are pricing on the inelastic portion of the demand curve. If movie tickets were all that a theater sells, this would not be profit maximizing. However, theaters have other sources of profits, such as concessions. If screening a movie for an additional viewer has a marginal cost of 0, it is easy to show that the elasticity of demand for tickets will be equal to the share of marginal profits earned from ticket sales.

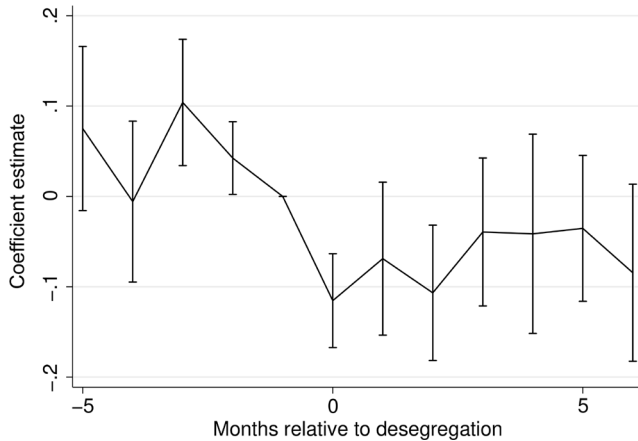


Figure 2. Log revenue in Washington

the placebo treatment date and ending 7 months after it. This way, the coefficient of interest is identified for the same number of pretreatment and posttreatment weeks.

The results of the placebo exercise are shown in Figure 3. From the histogram of treatment effects in Figure 3A, we can conclude that the treatment effect was well outside the range of most of the placebo effects. The percentile of the true estimated treatment effect is 1.1, which means that nearly 99 percent of the placebo effects were higher than the true estimate. The 10–90 percentile range is from $-.063$ to $.064$. Figure 3B plots the estimated placebo effects by week in 1953 and the 95 percent confidence intervals based on the asymptotic standard errors clustered by city, and the time pattern of placebo treatments follows what would be expected if the true treatment date were early June. They begin to decline toward the end of March, reach a trough around late May or early June, and then rise again until October before flattening out. The point estimates are statistically insignificant after October. Prior to April, they border on statistical significance, though it is worth noting that the confidence intervals are constructed from the asymptotic standard errors and not from randomization inference, and they would likely be insignificant with this correction.

5.2. Movie Composition

The final empirical exercise considers how movie theaters may have changed the composition of movies that were screened to appeal to an increasingly African American audience.²⁵ To do so, we form a proxy measure for the popularity

²⁵The responsiveness of movie composition may have some implications for our test of owners' discrimination. A racially biased theater owner may not reoptimize the movie selection after desegregation. As a consequence, not all potential gains in revenues to expanding the customer base to include African Americans would be realized. Therefore, the test would be biased in favor of finding discrimination by customers.

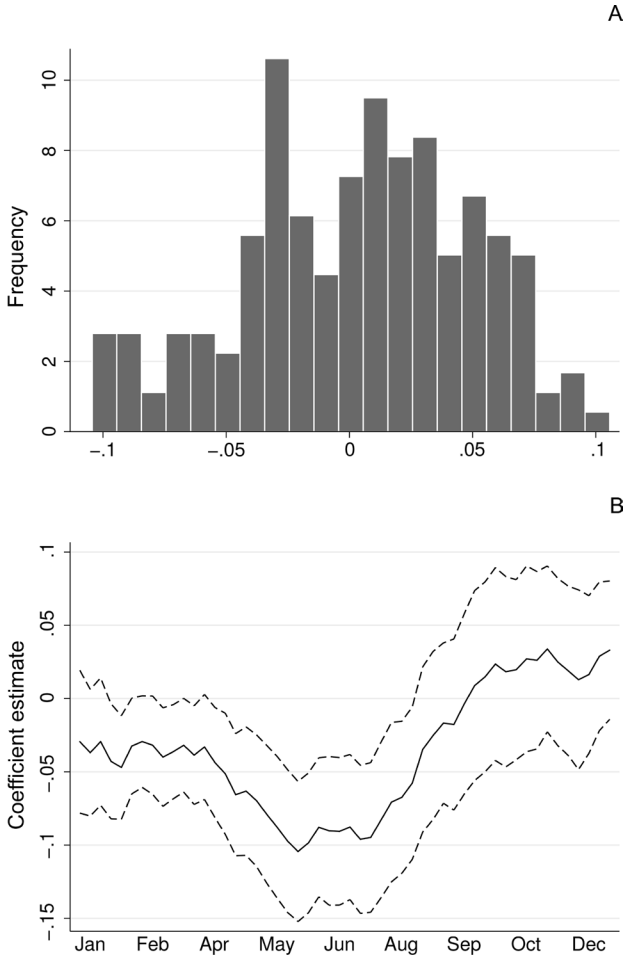


Figure 3. Placeto tests for alternate treatment weeks in Washington. *A*, Distribution of estimates; *B*, estimates by placebo weeks in 1953.

of a movie with black audiences. For each movie in our data, we take the average black population share across cities in which the movie is screened, weighting each city by the amount of revenue earned by the film there. A movie with a higher value for this average earned more of its revenue in cities with a higher black population share.²⁶ We exclude Washington theaters from the calculation to avoid introducing a mechanical correlation between measured popularity and $\text{Post} \times \text{DC}$.

²⁶ We check if this measure is sensible by examining how it correlates with whether a film has a black actor in a significant role. Indeed, movies with a black actor have a statistically significant higher value for our measure of black popularity.

Table 3
Movie Composition and Desegregation of Washington Theaters

	(1)	(2)	(3)	(4)
Post × DC	.00068* (.00029) [7/23]	.00076* (.00029) [5/23]	.00079* (.00033) [8/23]	.00069* (.00029) [6/23]
After desegregation	.00081 (.00078)		-.000039 (.00090)	-.000059 (.00050)
Log theater capacity	-.000062 (.00040)	-.000058 (.00040)	-.000063 (.00040)	
Year-week fixed effects	No	Yes	No	No
Year-month fixed effects	Yes	No	Yes	Yes
Theater fixed effects	No	No	No	Yes
City-specific seasons	No	No	Yes	No
R ²	.13	.14	.14	.17

Note. The unit of observation is a theater-film-week. All specifications include city-year fixed effects and controls for weather. Asymptotic standard errors clustered by city are in parentheses; rank of the randomization inference *t*-statistics are in square brackets. *N* = 62,151.

* Significant at the 5% level.

Table 3 presents estimates of the baseline difference-in-difference specification estimated at the theater-film-week level, so the coefficient of interest is an estimate of the change in the movie popularity measure in Washington after desegregation compared with the change for other similar cities. There is only suggestive evidence of a change in movie composition. The average movie shown after desegregation has higher measured popularity with black audiences, though the estimated coefficients are not statistically significant when considering the *p*-values from randomized inference.

6. Black Cast Members and Film Revenues

We now examine the revenue earned by movies starring black actors, and we implement the two tests derived in the theoretical model to test for firms' discrimination. We estimate a specification of the form

$$y_{ijc} = \beta_0 + \beta Z_c \times D_i + \mathbf{B}\mathbf{X}_{ij} + \gamma_j + \varepsilon_{ijc}. \quad (11)$$

The variable Z_c is an indicator for whether the racial bias index of the city is above average, and D_i is an indicator for movie i having a black actor in a significant role.²⁷ This specification compares the outcome for black-cast movies in racially biased cities versus in low-bias cities with the similar difference for white-only movies. Much of the racial bias is regional, and while there are clearly historical and institutional differences between northern and southern states during

²⁷ This specification makes use of all years in the data, 1945–55. When estimating the effects of desegregation in Washington, DC, it is most sensible to focus on the years immediately surrounding the change in desegregation. This consideration does not apply here, and utilizing more years of the data expands the set of black-cast movies we can consider.

this time, such confounding effects that may also affect regional movie demand are differenced out. The vector of controls X_{ij} includes theater capacity, the black population share in the city, an interaction of black population share and the black actor indicator, and an indicator for the year the film was released. We include indicators either for city or for theater. Theater indicators are needed if black-actor films are disproportionately screened at theaters with lower reservation values. In each case, we correct standard errors for two-way clustering at the city and film level.

Table 4 presents results for three dependent variables.²⁸ Films with black actors are screened for .28 of a week less in racially biased areas. Some of this is due to the type of theater that shows the film. Once we control for theater fixed effects, as shown in column 2, this estimate attenuates to .12 and remains statistically significant. An intuitive result is that films with black actors are screened longer when shown in cities with a larger black population share. Mirroring the results for weeks screened, the estimates for the log of total revenue earned over the film's run in column 3 indicate that revenue is lower by .23 log point when a black-actor movie is screened in a city with greater racial bias. This coefficient estimate again attenuates to $-.11$ when theater fixed effects are included. The results for run length and revenues point toward racial bias influencing the box office success of films with black actors; however, we are not able to distinguish between customers' and firms' discrimination in these estimates.

We next implement the test for firms' discrimination suggested by the theoretical model. Column 5 of Table 4 includes a control for the run length of the movie at the theater. As shown in the model, the null hypothesis of no discrimination by the firm can be tested by estimating the effect of racial bias on total revenues conditional on the film's run length. When run length in weeks is included as a control in the revenue regression, the coefficient of interest is small and statistically insignificant. The point estimate on the interaction of racial bias and a black actor is just $-.030$ ($p = .59$), compared with $-.11$ when not conditioning on run length.²⁹ This point estimate is small when compared with the variation in film revenues across theaters. To make this point clear, consider a regression of log film revenues on only theater and film fixed effects, which together explain 71 percent of the variation in film revenues. The residual from this regression is driven by local taste for the movie and any idiosyncratic demand factors that may coincide with the film's run at the theater. One standard deviation of this residual

²⁸ Table A4 considers additional outcomes that reflect the screening decisions of theaters. As discussed, the initial screening decision is influenced by both firms' and customers' discrimination, which are indistinguishable. The dependent variables in Table A4 are the theaters in a city screening a movie and whether the movie was screened in a city at all. The results mirror those in Table 4. Movies with black actors in the cast are less likely to be shown in more racially biased cities.

²⁹ An underlying assumption is that racial bias by customers affects the level of a film's revenues but not the rate of change during the film's run at the theater. We test this assumption by estimating equation (11), with the dependent variable being the change in revenues from weeks 1 to 2. To avoid selection bias, we restrict attention to films that were screened for at least 3 weeks. The coefficient on the interaction of a black actor and racial bias is $-.008$, which is small compared with the median revenue decline of 28.8 percent. This indicates virtually no difference in the time path of revenues.

Table 4
Racial Bias and Films with Black Actors

	Weeks at Theater		Log Total Revenue			Run >1 Week	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black Actor × Racially Biased	-.28** (.038)	-.12** (.029)	-.23** (.052)	-.11** (.042)	-.030 (.054)	-.073* (.030)	-.045 (.036)
Black Actor	-.014 (.067)	-.019 (.053)	.023 (.089)	-.068 (.081)	.055 (.058)	-.019 (.042)	-.0018 (.021)
Black Actor × Black Population Share	3.39** (.67)	1.10* (.49)	2.00** (.69)	.091 (.64)	-.65 (.59)	.57* (.24)	.66** (.16)
Run length (weeks)					.68** (.023)		
Log first-week revenue							.42** (.063)
City fixed effects	Yes	No	Yes	No	No	No	No
Theater fixed effects	No	Yes	No	Yes	Yes	Yes	Yes
N	92,074	92,074	92,070	92,070	92,070	92,074	92,036
R ²	.16	.29	.41	.56	.83	.24	.36

Note. The sample includes films with at least one significant character played by a black actor. The unit of observation is a theater-film. All specifications control for release year. The racially biased variable is an indicator for the racial bias index being above the average. Asymptotic standard errors clustered by city are in parentheses.

* Significant at the 5% level.

** Significant at the 1% level.

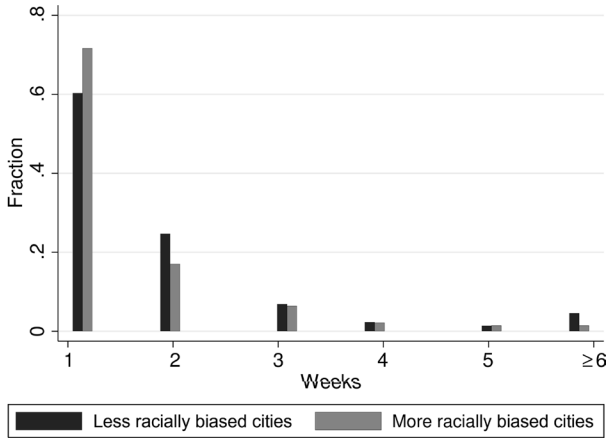


Figure 4. Distribution of run lengths for films with black actors

is .46 for movies with a black actor. Therefore, the estimate of our coefficient of interest of $-.030$ indicates that showing a film with a black actor in a racist city would lead to a decline in revenues of just 1/50th of a standard deviation in the unexplained portion of revenues.

Table 4 also shows the results of estimating specifications in which the dependent variable is the likelihood that a film's run is greater than 1 week. As demonstrated in our model, conditional on week 1 revenue, firms' discrimination can be detected by the effect of racial bias on the continuation probability of black-actor films. While this proposed test can be implemented for any week in the film's run, in our setting the continuation probability at week 1 has the greatest empirical relevance. To see why, consider the distribution of run lengths for films with a black actor, as shown in Figure 4. In racially biased cities, 72 percent of the runs are only 1 week, with films in less racially biased cities experiencing a 1-week run only somewhat less often (60 percent of theater-film observations). Relatively few films run for 3 or more weeks.

Column 6 presents the results without conditioning on week 1 revenue. The estimates suffer from the same issue as those for total revenue and overall run length in that they are affected by both customers' and firms' discrimination. As suggested by the unconditional means displayed in Figure 4, films with black actors are less likely to run longer than 1 week when screened in racially biased cities. Column 7 includes controls for the log revenue earned by the film in the first week of its run. The point estimate on the variable of interest is small and statistically insignificant. The continuation probability for black-actor films is 4.5 percentage points lower in racially biased areas ($p = .209$), compared with a reduction of 7.3 percentage points when not conditioning on week 1 revenues.

While these tests fail to reject a null hypothesis of no discrimination by firms, they do not definitively prove the absence of such bias. They test only for dis-

crimination by the average theater owner. That said, when viewed in combination with the decline in profits after desegregation, it suggests that consumers' discrimination may be the more relevant force determining the screening choices made by the average movie theater.

7. Conclusion

In this paper, we study how the profits of firms responded to racial segregation in public accommodations during Jim Crow, with the goal of understanding firms' motivations for voluntary racial exclusion practices. Using a plausibly exogenous court ruling that desegregated Washington, DC, movie theaters in 1953, we find that box office revenues fell in Washington compared with other cities in the United States. The results suggest that theaters may have excluded customers of color to cater to the racial biases of their white customers. We provide further evidence pointing toward customers' discrimination, and away from firms' racial bias, by examining the screening decisions made by theaters regarding movies with black actors in significant roles. These movies earned less revenue and were screened for fewer weeks in cities with greater racial bias. Using a theoretical model of the screening decision of theaters, we propose two related tests for firms' discrimination, and we are unable to reject a null hypothesis that firms are unbiased. These findings contribute to understanding the persistence of de facto segregation despite the profit incentive to expand a theater's market by serving customers of all races.

Explicit segregation is no longer legal, but the issues we examine in this paper remain relevant. There are direct parallels with the modern film industry, which continues to grapple with the level of representation of actors and directors of color. In a different context, recent court cases considered the question of whether business owners are permitted to refuse service to gay customers on religious grounds, which serves to highlight the continued importance of understanding firms' voluntary exclusion of customers and the forces behind the persistence of institutions favoring segregation and racial exclusion.

Appendix

Additional Tables

Table A1
Films by Major Studios with Black Actors in Significant Roles

Film	Actors	Release Year
<i>The Breaking Point</i>	1	1950
<i>Brewster's Millions</i>	1	1945
<i>Bright Road</i>	3	1953
<i>Bright Victory</i>	1	1951
<i>Carmen Jones</i>	5	1954
<i>Five</i>	1	1951
<i>Intruder in the Dust</i>	1	1949
<i>Lydia Bailey</i>	1	1952
<i>The Member of the Wedding</i>	1	1952
<i>No Way Out</i>	2	1950
<i>Pinky</i>	1	1949
<i>Red Ball Express</i>	1	1952
<i>Tall Target</i>	1	1951
<i>The Well</i>	3	1951
<i>Young Man with a Horn</i>	1	1950

Table A2
Revenue Estimates Excluding Theater-Weeks with More than One Film Screened

	(1)	(2)	(3)
Post × DC	-.12** (.031)	-.086** (.021)	-.078** (.019)
Log high price			.25** (.048)
Log low price			.023 (.040)
Log theater capacity	.45** (.064)		
Log film age (months)	-.17** (.016)	-.15** (.015)	-.15** (.015)
Log weeks film at theater	-.34** (.027)	-.33** (.020)	-.33** (.020)
City-month fixed effects	Yes	No	No
Theater fixed effects	No	Yes	Yes
N	31,023	31,021	31,021
R ²	.80	.87	.87

Note. All specifications include film dummy variables, city-year fixed effects, and year-month fixed effects. Asymptotic standard errors clustered by city are in parentheses; rank of the randomization inference *t*-statistic = 2/23 for all models.

** Significant at the 1% level.

Table A3
Sample Selection for Control Units

	(1)	(2)	(3)	(4)
High black population share:				
Post × DC	-.15* (.059)	-.090+ (.043)	-.12+ (.059)	-.086+ (.041)
N	14,065	14,065	14,064	14,065
R ²	.74	.82	.84	.82
Highly educated population:				
Post × DC	-.075+ (.031)	-.063* (.023)	-.086** (.020)	-.056* (.021)
N	25,391	25,390	25,386	25,390
R ²	.70	.80	.82	.80
East Coast:				
Post × DC	-.13+ (.058)	-.065 (.040)	-.099+ (.042)	-.063 (.036)
N	19,769	19,769	19,765	19,769
R ²	.76	.86	.88	.86

Note. The dependent variable is the log of revenue earned by a theater by week. Cities on the East Coast are Baltimore, Boston, Buffalo, New York, Philadelphia, and Providence. Asymptotic standard errors clustered by city are in parentheses.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

Table A4
Screening Decisions

	Number of Theaters	Number of Theaters Conditional on Being Screened	I(Screened)
Black Actor × Racially Biased	-.18* (.081)	.058 (.064)	-.14** (.020)
Black Actor × Black Population Share	.16 (.62)	-.79* (.33)	.75 (.56)
Black Actor	.50** (.10)	.051 (.043)	.34** (.057)
N	149,983	68,764	149,983
R ²	.12	.32	.036

Note. Results are at the city-film level. All specifications include release year dummy variables and city fixed effects. Standard errors corrected for two-way clustering by city and film are in parentheses.

* Significant at the 5% level.

** Significant at the 1% level.

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