Why did firms practice segregation? Evidence from movie theaters during Jim Crow*

Ricard Gil†
Justin Marion‡

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Abstract

Racial segregation by businesses during Jim Crow was often voluntary and practiced without a legal mandate. We study movie theaters in the 1950s, taking two approaches to assess the importance of customer and firm discrimination in driving the exclusion of customers based on race. First, we examine the 1953 desegregation of Washington, DC businesses, which occurred rapidly as a result of a court ruling. Using weekly data for a nationwide sample of theaters, we find that revenues of DC theaters fell relative to theaters in other cities, consistent with reduced demand from biased white customers. Second, we study films with black actors cast in prominent roles. Their box office performance was worse in cities with greater racial bias, yet, using tests derived from a model of the screening decision, we cannot reject that firms were unbiased. Together, our results point toward customer discrimination as a primary cause of public accommodation segregation.

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†Associate Professor, J. R. Smith School of Business, Queen's University. Email: ricard.gil@queensu.ca.

‡Associate Professor, Economics Department, University of California, Santa Cruz. Email: marion@ucsc.edu.

1 Introduction

Many cities and states were slow to dismantle the institutions of segregation. Prior to the Civil Rights Act of 1964, segregation in public accommodation persisted in many parts of the country, which significantly affected African-American access to public services and private businesses. While Southern cities frequently mandated the separation of races – often in specific settings such as in hospitals, restaurants, or public transportation – to a significant degree segregation was practiced by the choice of businesses even in the absence of any legal proscription. Indeed, businesses often excluded black patrons despite city ordinances banning segregation, as these laws 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 the owners, workers, and customers. Our goal in this paper is to understand how the racial biases of customers and firms influenced racial exclusion, with the aim of shedding light on the forces leading to the persistence of segregation. We study movie theaters in the early 1950s, when explicit segregation was still common in Southern states, and some theaters still refused to show movies with black actors playing prominent roles. Both of these types of racial exclusion policies were important in the historical institution of segregation and were both governed by the racial views of customers and firms. The decision to segregate is profit-enhancing if the firm is catering to the racial bias of its customer base. On the other hand, business owners may be willing to sacrifice profits to satisfy their own prejudice or that of their workers.

Using unique data on the weekly box office revenue from a nationwide sample of movie theaters, we take two distinct yet complementary approaches. (i) We estimate the effects of the 1953 desegregation of Washington, DC businesses on the revenues earned by the city's theaters. (ii) We examine films with black actors cast in prominent roles, and we estimate how their box office performance, as measured by revenues and run length, depended on the racial bias of the city in which the theater is located. As we discuss below, the results from these two empirical exercises together allow us to separately test for the influence of customer and firm discrimination.

The desegregation of Washington, DC businesses occurred rapidly in the summer of 1953. Until then, the movie theaters in DC barred attendance by African-Americans despite long-forgotten 19th century laws outlawing segregation in public accommodation in the city. A U.S. Supreme Court ruling in June 1953, which applied only within the District of Columbia, subsequently required those laws be enforced, leading to a rapid desegregation of the city's businesses. Using a difference-in-difference design, we find that revenues of theaters in DC fell by 10 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 ruling. While the opening of movie theaters to the African-American market could conceptually influence optimal theater pricing, we do not find strong evidence of a price response. We conclude that tickets sold to white customers fell after desegregation, at least in the short run.

The post-integration decline in profits strongly suggests customer discrimination, but from this result we cannot say whether or not firms were also prejudiced and whether that too may have contributed to racial segregation. We address this question in the second part of the paper. Using a theoretical model of the screening decision by movie theaters, we show how the revenue earned by films with black actors, in conjunction with their run length, can be used to specifically test for firm prejudice. The intuition of the test is 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 continuation probability in week $t+1$ of a movie with a black cast member will depend on firm discrimination but not customer discrimination. 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 produced by major studios and released during the years covered by our theater revenue data. To measure racial bias, we use the index constructed in Gil and Marion (2018), which we formed from respondent's 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 to what it would have earned in an area with less racial bias. The run length contributed to this difference, as there is a 0.12 difference between racially biased and unbiased areas in the number of weeks that black-actor movies are screened. However, we are unable to reject a null hypothesis that firms are unbiased. Conditional on the revenue earned in their 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 these two types of movies does not depend on racial bias. Both of these results are consistent with racially unbiased firms. Together, our results point toward customer discrimination as a key determinant of theater policy during this era.

We fail to find evidence of firm racial discrimination, and to the extent that firm owners or their workers are biased, the effects of these preferences are secondary.

The analysis in our paper closely relates to the seminal work of Becker (1957), which also guides the welfare implications of our results. Analogous to the discrimination faced by black workers in the Becker model, the prejudice experienced by black movie-goers is determined by the discrimination of the marginal theater, and it is conceivable that 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. However, customer discrimination creates profit rewards for racial exclusion that can survive entry, and, in contrast to firms in the Becker model, 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.

Public accommodation segregation has received little attention in the economics literature. This may be due in part to the practical complication that segregation laws were inconsistent and piecemeal, and their enforcement was uneven. Furthermore, segregation often resulted from informal local practice rather than formal laws. Segregation laws are therefore unlikely to be a useful source of variation in studying the effects of segregation. In contrast, the desegregation episode we examine is more well defined. Also, the theater-week data we employ is well suited to exploit the timing of the court case, which allows us to account for broader economic trends such as the adoption of television and improvements in black economic outcomes in the South. The data also allows us to control for detailed demand factors such as movie quality and weather.

Wright (2013) provides a history of desegregation efforts in public accommodation leading up to the passage of the Civil Rights Act of 1964, noting that the fear of alienating white customers and harming profits was an important motivation by firms engaging in voluntary segregation. While not specifically attempting to identify the effect of segregation on profits, Wright shows that aggregate retail sales grew in Southern areas during the 1960s at a rate meeting or exceeding other regions. Since this period coincided with widespread desegregation, Wright then argues that desegregation was a positive force for businesses, which stands in contrast to our findings in this paper. One way to reconcile the two results is to consider that racial attitudes were

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

changing for the better during this time, and the effect of desegregation on demand from white customers may have been declining. An alternative interpretation is that the Civil Rights Act had positive impacts on other aspects of the economy, such as Southern labor markets, which may instead be responsible for the increase in retail sales in the South.

Our study fits with a recent literature conducting historical studies of racial bias and residential segregation. Troesken and Walsh (2017) examine how residential segregation ordinances arose in the early 1900s as a result of the breakdown of informal institutions that acted to discourage property transfer to other races. They find that informal vigilante activity by whites and explicit statist segregation policy act as substitutes. Where whites more easily organized to enforce informal residential segregation norms, segregation laws were less likely to be implemented.

Other recent historical studies of residential segregation and racial bias include Boustan (2010), who examines the role that post-war black migration played in the suburbanization and resulting residential segregation of northern cities. Cook et al. (2018) examine the hypothesis that residential segregation may contribute to ethnic fractionalization, and thereby violent crimes, by studying the historical pattern of residential segregation and how it correlates with racial violence. Using the detailed measures of residential segregation developed by Logan and Parman (2017), they find that segregation increases 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 and other measures of racial intolerance such as compliance with hate laws. (see King et al., 2009; Messner et al., 2005)

Our results relate closely to the established literature on customer and worker discrimination. An important antecedent to our study is Heckman and Payner (1989). They examine the impact of federal antidiscrimination legislation on the employment outcomes of black workers. In a clear analogue to our setting, firm owners had a profit motive to hire black workers, yet also felt pressure from customers, white workers, or other stakeholders to exclude black employees. In a paper closely related to ours, Kuppuswamy and Younkin (2016) estimates how the box office performance of films from 2011-2015 varies based on cast diversity, finding that films with more diverse casts are associated with greater box office revenue. As with our study, Leonard et al. (2010) use sales to uncover customer discrimination, finding that a mismatch between the demographics of the employees at a retail outlet and the residents in the surrounding neighborhood has a negative though small impact on sales. Similarly, Holzer and Ihlanfeldt

(1998) also find evidence of customer discrimination in US retail, and that this has implications for wage rates of black workers. This is apparently consistent with the theoretical prediction of Kahn (1991), who suggests that customer discrimination could contribute to long-run racial wage gaps. Bar and Zussman (2017) examine a similar question, providing evidence that Jewish customers in Israel prefer to be served by Jewish rather than Arab workers, and that this preference influences the hiring decisions of employers. Waldfogel and Vaaler (2017) consider how firms are willing to forego profits in order to appease the ethnic biases of customers, specifically by omitting Israel from online route maps if the airline serves customers from countries with stronger anti-Semite views. Nardinelli and Simon (1990) finds that baseball cards depicting black and Hispanic players sell for less than otherwise comparable white players. The impact of customer discrimination could be felt more broadly in labor markets. Longley (2003) also attempts to distinguish customer from firm discrimination.

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 our empirical results related to the impact of desegregation on firm profits, while in section 6 we describe our 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 will formalize the intuition of how we will empirically distinguish the role of customer and firm discrimination in influencing the racial policies of firms. The profit response to desegregation can help distinguish the relative importance of consumer from firm discrimination, and we can specifically test for firm discrimination by examining the screening decisions of theater owners.

The theater owner receives utility from the profits earned by screening movies. If she or her employees are racially biased, the firm receives disutility δ^f if black customers attend, or if the movie being screened has a black actor.[2] 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]While 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, we do so for convenience. While likely correlated, there is no reason to think they are of the same magnitude.

2.1 Desegregation and theater revenue

Under integration, the theater sells 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 respectively with and without segregation, 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, 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:

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

What can the sign of $\Delta\Pi$ tell us about discrimination? We know from equation (1) that δ^f is greater than the right-hand side of (2). Therefore, if profits rise, it must be the case that $\delta^f > 0$ and firms are biased. If profits fall, then customer bias must exist, and it must be large enough to outweigh any gains from serving black customers.

2.2 Screening choice and racial bias

Based on the sign of the profit response to desegregation, we can say whether one or the other side of the market is biased, but we cannot draw conclusions about both sides of the market. For instance, suppose profits fall after desegregation, which is what we will in fact find in the empirical section. In that case, we know that customers are biased, but we cannot rule out that firms are also racially biased. Hence, a test that specifically evaluates firm bias will be particularly valuable.

We now provide the theoretical foundations for such a test. We enrich the specification of movie theater profits to describe their dependence on the movie being screened and the length of run of the movie. From this specification of profits, we can model the endogenous screening choice of theater owners, from which we can derive tests for owner 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 member depends on the overall popularity of the movie, the racial bias of the local population, and how long the run of the film has been at the theater:

$$\pi(t) = (\rho - \delta^c I(B))e^{-t} \tag{3}$$

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 at the theater 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 that 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.

The movie is screened until its revenue falls below the outside option. The total length of run of the film at the theater is therefore found by equating $\pi(t) = r_0 + \delta^f I(B)$, which yields

$$t^* = ln((\rho - \delta^c)/(r_0 + \delta^f)) \tag{4}$$

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

$$\Pi = \int_0^{t^*} \pi(t)dt = \rho - r_0 - (\delta^c + \delta^f)I(B) \tag{5}$$

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

$$Pr(screened) = F(\rho - (\delta^c + \delta^f)I(B)). \tag{6}$$

Inspecting equations (4)-(6), we see the difficulty in distinguishing customer from firm 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 (3) contains only the customer discrimination term. However, we must account for selection, and revenue is only observed 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

$$E[\pi(0)|screened] = \int_{\rho-\delta^c-\delta^f}^{\infty} (\rho - \delta^c) f(r_0) dr_0 = (1 - F(\rho - \delta^c - \delta^f))(\rho - \delta^c) \tag{7}$$

where yet again consumer and firm discrimination are inseparable.

To overcome this challenge, we propose a test that will specifically distinguish firm discrimination. Intuitively, if one were to condition on the revenues in the first week of a movie's run, firm 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 where $\rho_B - \delta^c = \rho_W$. The conditional continuation probability of a black movie is therefore $Pr(B \text{ screened in } t = 1) = F(\rho^B - (\delta^c + \delta^f)I(B)) = F(\rho^W - \delta^f)$, 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 (4), 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 firm discrimination. Combining this expression with (5), then $\Pi_B = \rho - r_0 + \delta^f(\frac{\rho}{r_0} - 1)$. This expression 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, then it will earn more revenues over the course of its run.

To implement these tests in practice, we will employ variation in racial bias across cities. As racial bias rises, neither the conditional continuation probability nor conditional total revenue of black-actor movies will respond under the null hypothesis of unbiased firms. One key assumption underlying both versions of the test is that the time path in revenues will be otherwise similar for movies with a black cast member as those without. In the empirical section, we will verify that this assumption holds in the data.[3]

[3]Testing this assumption will also address one further concern. The discussion so far has treated movie popularity as an exogenous variable, but the firm can influence demand for a movie via advertising. The parameter ρ is therefore not necessarily exogenous to firm's racial bias. However, so long as the rate of decay of the film's revenue is not

3 Background

3.1 Segregation and Jim Crow laws

Segregation in public accommodation was an important feature of African-American life for much of the 19th and 20th century. The separation of races in business, 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.[4] In many localities, or in many types of businesses within those localities, there was no law mandating the separation of races. Rather, segregation was implemented by choice of businesses for fear of alienating their white customer base. In the North, segregation was not institutionalized, and many Northern states passed laws at various points in time banning segregation in public accommodation. However, these laws were often ignored, imperfectly enforced, or interpreted in such a way that allowed for the continued exclusion of blacks.

Morris (1986) dates the "modern civil rights movement" as beginning in a 1953 boycott of the segregated Baton Rouge, LA buses, which may have inspired similar demonstrations in Montgomery, AL in 1955. It is clear that the practice of segregation was entrenched in the South well into the 1960s. Wright (2013) documents the progress of desegregation of public accommodation through the passage of the 1964 Civil Rights Act. Desegregation up until the Act was largely voluntary, and progress was uneven across types of private businesses. The Kennedy Administration tallied the progress of desegregation of public accommodation in Southern cities, which is summarized by Wright (2013). Of the 566 cities monitored, only a little more than 100 had desegregated theaters by early 1963, which lagged behind the number of cities with desegregated eating establishments or hotels. The civil rights movement, and the threat of federal legislation, led to a wave of desegregation, and the number of Southern cities with desegregated theaters and restaurants more than doubled between 1963 and mid-1964. Progress subsequently stalled. One factor slowing integration was that Southern businesses often did not wish to unilaterally desegregate for fear of losing white customers to still-segregated competitors. Coordinated action was likely required, though substantial uncertainty surrounded the likelihood of including public accommodation desegregation in federal legislation. Despite the doubts at the

influenced, the tests will remain valid.

[4] For instance, one local statute may enact segregation in public transportation, while another may be later enacted that covers hospitals. An example cited by Woodward (1974) was a Birmingham law making illegal mixed race games of dominoes or checkers. The first statewide segregation laws were applied to seating in railroad cars (Wright, 2013).

time, Title II of the Civil Rights Act banned public accommodation segregation nationwide. The practice of segregation died out over the following years, owing in large part to the monitoring efforts of civil rights groups and enforcement by the Department of Justice.

3.2 Movie theaters

Until the Civil Rights Act of 1964, segregation was a pervasive feature of movie theaters. 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. While segregation was often met with resistance from civil rights groups such as the NAACP in some cities, the practice was still common, and often extended to barring entertainment with black performers.

A substantial number of theaters serving black customers entered the market in response to the limited access to the mainstream movie theaters. Gil and Marion (2018) documents the pattern of entry by the so-called "Negro theaters." Cities and counties with greater black population shares experienced more theater entry, particularly those areas with a greater degree of residential segregation. This entry could be substantial. According to the 1953 Film Daily Yearbook, there were 20 "Negro theaters" in Washington DC in 1952, or one for every 14 thousand black residents. By comparison, there were 12 thousand residents per theater overall, counting all theaters and residents of all races. 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 often showed more 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.

3.3 Washington DC desegregation

Prior to 1953, segregation in Washington DC was widely practiced. Most movie theaters excluded customers of color, as did restaurants and other private businesses. To the best of our knowledge, no Jim Crow laws mandated segregation in the District, and in fact anti-segregation laws were enacted by the DC city government in 1872 and 1873, at a time when Washington was an independent municipality. These laws 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 restaurant. It was this lack of enforcement that was challenged in the courts. The case eventually reached the US Supreme Court, who ruled on

June 8, 1953 that the laws must be upheld. The ruling applied only to the enforcement of these historical anti-segregation laws in DC specifically and consequently did not relate to the legality of segregation in other parts of 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.

Newspaper articles at the time focused on the desegregation of restaurants, as this was the impetus for the legal case, and because the 1873 anti-segregation 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 DC theaters desegregated in 1953 at some point after the Supreme Court ruling. Pinning down the precise date is elusive, and we unfortunately do not have direct evidence regarding the exact timing of when admission to DC theaters was opened to black customers. Several major theaters in late September 1953 issued statements that they had been admitting black patrons "for several months." (Headley, 1999) There is also other indirect evidence 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, had signaled her intent to also bring suit against Washington DC movie and stage theaters should they not change their admission policies.[5] 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," which combined with the Supreme Court ruling would strongly indicate that public accommodation segregation was at an end.

In the empirical work that follows, we will treat June 1953 as the date of theater desegregation. As a robustness check, we will run a series of placebo tests on other possible treatment dates, and we will provide evidence regarding the timing of the response of theater revenue.

4 Data

The primary data used in the paper comes from weekly Variety magazine issues published between January 3, 1945, and December 28, 1955. When estimating the effect of desegregation, we will restrict attention to the time periods immediately surrounding the desegregation event, usually 1951-1955. This unusually detailed publication contains weekly information for an un-

[5] "Ready to Fight Jim Crow Theaters Next, Mrs Terrell Says in Chicago," *Afro-American*, June 20, 1953.

balanced panel of 393 theaters in 26 different cities. Each week, the publication listed the revenue earned and the high and low prices charged by theater, as well as a listing of movies screened by the theater that week.[6] The data also contains a limited amount of theater-level information including capacity and ownership. The panel of theaters is imbalanced, as not all theaters report revenue data in all weeks. In the empirical section, we will limit our attention to those theaters with at least two full years worth of movie observations.

We calculate within the data the film age and the number of weeks a film has been at a particular theater, which are likely to be important for weekly demand. Film age is calculated based on the time elapsed since the date at which the film first appeared in the data. Weeks at theater is similarly calculated as the time elapsed since the film was first screened at a particular theater.

4.1 African-American actors

A secondary focus of the paper is on the revenue earned by films with black actors in significant roles. Few movies in this era had black cast members at all. This fact owes itself to several forces. First, as we will argue, white customers were not receptive to black cast members. A second reason related to the economics of movie production, and the ability of studios to produce films specifically 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 film industry.

We obtain information on the racial makeup of a film's cast by compiling information provided in Klotman (1997). This source provides, by film, a listing of any black actor, writer, director, or producer involved in the film. We suspect that black actors in very small or non-speaking roles will 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 on an actor's screen time or the number of his or her 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.[7] Because foreign and independent films are likely to have niche audiences that differ from the average movie-goer, our focus is on movies produced

[6] The publication does not indicate what the prices represent. They can potentially represent differential pricing for matinees versus prime time showings, or differential pricing by the desirability of the seating within the theater.

[7] Standard practice in movie billing lists actors in order of appearance. While not necessarily the case, it is often true that important characters are introduced early in a film.

in the United States, and on those films produced by one of the major production studios. We also rule out those movies where a musician is playing themselves on-stage (such as Count Basie playing music), sports movies, and movies portraying African-Americans in a negative or stereotypical light. Table A1 provides a listing of the movies that meet these criteria.

In total, 176 movies were produced between 1945-55 with any black actors. The actor's role was small in a majority of the films. Normally, when a black was a lead, or had a significant part, the production company was an independent studio. From 1945-55, 92 movies were produced by independents with black actors. In more than half (51 percent) of these films, the black actor was a lead, which we define as being in the first two in the IMDB cast list. In contrast, only 3 of the 84 major studio movies with black actors featured a black lead. In Figure 1, we depict the number of films with significant black actors produced by year from 1945-1955, separately for major studios and independents. Independent studios produced a number of films with black actors in significant roles in the late-1940s. From 1946-1948, 41 such films were produced by independent studios compared with just two films produced by major studios. It is worth noting that even during the late 1940s, the number of "race films" were a small share of total film production. According to data from the American Film Institute reported in Gil (2010), a total of 1218 films were produced from 1946-1948. Consistent with anecdotal evidence, the following years experienced a rapid decline in "race film" production. From 1949-1955, less 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-fifties, 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 that will follow at the end of this paper, we will focus on films produced by one of the 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.

4.2 Racial bias

In Gil and Marion (2018), we form a measure of racial bias that varies by state. We use micro data from public opinion polls from the late 1940s and 1950s, usually conducted by Gallup. The micro data contains a person's race, state of residence, and the response to questions related to racial attitudes.[8] We form an index of racial bias by following a similar approach as that taken in Charles and Guryan (2008).[9] First, we order the possible responses to each question from what we judge to be most racially tolerant (which we assign a low score) to the least racially tolerant (which receives 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

In Table 1, we present the summary statistics of the estimation sample. The average theater earned \$13,486 in a week, with the figure slightly lower in DC than in other cities (\$10,973 vs. 13,605). This may be in part due to differences in average theater capacity. The average capacity across theater-weeks in the data is 1521 for DC theaters versus 2065 for theaters outside of Washington DC. The high price charged for movies is just over \$1 for both DC-area theaters and theaters outside of DC. The average low price in the data is 68 cents and is very similar for DC and non-DC theaters. The average age of the movie screened is 5.6 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 1 month old. Consistent with a median film age of one month, the average film shown has been in the theater for 3.4 weeks. For DC theaters, the average length of a movie's run at a given time is longer (5.1 weeks versus 3.3 for the rest of the US). This is largely driven by a few movies experiencing very long runs.[10]

It is conceivable that optimal pricing could respond to desegregation, and we will examine price as an outcome in the empirical work. That said, it is worth discussing the price variation in further detail, and why it is unlikely we will detect price effects in our setting. Prices are fairly stable within theater. As we will discuss, our empirical specification includes year*city effects, so that the effect of desegregation is identified from variation in revenue and price within 1953, the year of the Supreme Court decision. In 1953, the average theater in the data changes

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

[9] 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 election years.

[10] If one were to exclude the longest running film during this time, *Cinerama Holiday*, the average weeks at theater would drop to 2.80 for DC and 2.90 for the rest of the US.

its high price 3.4 times. For theaters in DC, this figure is 3.1. Many of these price changes appear to reflect temporary increases or decreases in price. In DC, the average price change for the year is 25.6 cents in absolute value, while the cumulative sum of the absolute value of weekly price changes is on average 93.3 cents. This indicates that 73 percent of the 1953 price changes were subsequently undone by offsetting price changes of the opposite sign within 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 DC, theaters virtually always screen only one movie in a given week. This is true for 97.5 percent of the theater-week observations. Outside of DC, it was more common for multiple films to be screened in the same week. 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 0.24 percent of theater-weeks.

5 Estimated effect of desegregation

In the first half of the empirical portion of the paper, we will estimate a specification comparing the weekly box office revenues earned by theaters in DC with revenues earned by theaters in other cities, before versus after the desegregation of DC theaters. This is a quasi-experimental design, comparing the revenues of treated theaters – those experiencing desegregation – with a set of control theaters who did not experience a change in segregation practices.

The unit of analysis is a theater-week, and we estimate a difference-in-difference specification

$$y_{ijt} = \beta_0 + \alpha Post_t * D_j + BX_{it} + \rho_i + \gamma_{jt} + \zeta_t + \epsilon_{ijt} \tag{8}$$

where i, j, and t index theater, city, and time, respectively. We consider two outcomes of interest, log revenue and the log high price. To reduce the influence of outliers, we windsorize top and bottom 2 percent of the dependent variable. The parameters γ_{jt} and ζ_t represent city-year and year-month effects, respectively. In some specifications, we will estimate theater effects rather than city effects. Also, we can allow for richer specifications of time effects, such as city-specific seasons.

Our empirical specifications will include year*city effects, so that identification will be based only on the months immediately surrounding the desegregation event, while other years will primarily aid in providing more precise estimates of other variables in the empirical model. Without a tight date restriction, the change in theater revenues due to desegregation will be difficult to disentangle from other demand-related trends, such as the rapid adoption of television.

The vector of controls X include a full set of film indicators (3,332 unique movies were shown in the estimation sample), so that we are able to compare the revenue earned by the theater with that earned by other theaters showing the same set of movies. The vector X also includes weather experienced in the city that week, the average age of the films being screened, the average length of run at the theater of the films screened, and the seating capacity of the theater (in those specifications not including theater effects). The latter variable is observed in the data directly. We calculate the age of the movie within the data as the months elapsed since the first screening of the film across any movie theater. Similarly, the length of run within the theater is the number of weeks elapsed since the date the movie was first screened at that theater. Since there were sometimes more than one film screened by a theater in a given week, in those cases we take the average age and length of run of the films shown by the theater in a given week.

We obtain daily weather 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.

Since only DC 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 over-rejection of the null hypothesis. To address this issue, we obtain p-values for the coefficient α in equation (8) using the randomization inference solution based on placebo t-values suggested by MacKinnon and Webb (2016) (MW hereafter).[11] The intuition behind this approach with only one treated cluster is straightforward. We first estimate equation (8) and obtain the t-statistic for coefficient α correcting for clustering at the city level. We then form a distribution for this test statistic by repeating the estimation of (8) $J-1$ times, where J is the number of cities in the sample. For each iteration, we assign a different placebo city the desegregation treatment. We then compare the baseline t-statistic with the placebo distribution to form a p-value. Because the sample includes a finite number of cities, the distribution of p-values is discrete. The p-value we report is the midpoint of the lowest and highest possible p-values.[12]

[11] Other approaches suggested by the literature include Conley and Taber (2011) and Cameron et al. (2008), though according to MacKinnon and Webb (2017) the latter, while appropriate for settings with finite clusters, fails in settings such as ours where the number of treated clusters is small.

[12] For instance, if our baseline t-statistic is higher than that obtained from the 22 placebo t-statistics, we assign a

According to MW, both their randomization inference approach, as well as 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, cluster size variation is not substantial, as the number of observations does not vary dramatically across cities – the average number theater-week observations being 2020 and the standard deviation 1179. This stands in contrast to, say, household-level data across states, where the variation in cluster size would be a more significant issue. Moreover, the treated city Washington DC has a similar number of observations to the median city (2030 in DC versus a median of 1775 in other cities). As MW 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. The intuition is that when the number of observations in the treated group is small, then the treatment effect will be less precisely estimated in the placebo groups, leading randomization inference based on the t-statistic to over-reject. The reverse is true when the number of observations in the treated group is large – the treatment effect will be more precisely estimated in the treated group than in the average placebo group, leading to under-rejection.

5.1 Revenue Results

The results of estimating (8) for log revenue are shown in Table 2. In column (1), the displayed specification contains year*month effects, which allow for common shocks to affect the demand for all movies shown for a particular month. Such shocks may include macroeconomic effects, as well as any other common demand factors such as the popularity of national TV programs aired at the same time. In this specification, we estimate that theater revenues in Washington DC declined by 11 percent after desegregation ($p=0.068$). Examining the other covariates in the model of film revenues, we see that log theater capacity is associated with significantly higher revenues. The elasticity of capacity with respect to revenue is 0.45. As films age, revenues decline. The coefficient estimate on the log(film age) variable is -0.12, where film age is equal to one if a film is in its first month of release. This estimate suggest that a film that is in its second month of release will earn 3.6 percent less revenues than a film that is only one month old. The revenue earned also declines as the length of the film's run at a theater increases. For each ten percent increase in the number of weeks the film has been shown at a theater, the revenue earned drops by 3.4 percent. Among the weather variables, as the average high temperature rises or the low temperature falls, movie revenues increase. Revenues also respond positively to

p-value halfway between zero and 1/22.

precipitation falling as snow, though decline as ground snow depth rises.

In columns (2)-(4) of Table 2, we add sequentially more detailed fixed effects, and we find that the main coefficient of interest varies little across these specifications. In column (2), we control for year*week effects. In column (3), we include city-specific seasonal patterns with city*month effects. In column (4) we include theater fixed effects. In each case, the impact on the estimated coefficient of interest, the difference-in-difference term Post*DC, changes little. With the full set of controls, we can account for 84 percent of the variation in movie theater revenues.

Finally, in column (5), we include controls for the high and low prices charged for the movie by the theater. Including these controls only attenuates slightly the difference-in-difference estimates. Conditional on price, revenues fell by 8.7 percent in Washington DC after desegregation. This suggests that our main findings cannot be explained by any concurrent changes in price. The estimated coefficient on the high-price variable is 0.25. It is important to note that this coefficient is not estimated using exogenous variation in price. To the extent that the variation in price reflects unobserved demand shifts, then the estimated coefficient will be biased. With that caveat, the magnitude of this coefficient suggests that theater owners are pricing movies on the inelastic portion of the demand curve if marginal cost is close to zero. Rather than pointing toward suboptimal pricing, it may reflect the profits unrelated to ticket sales, such as those from concession sales.

We next establish that the timing of the revenue response corresponded with the date of the desegregation event. To do so, we estimate a version of equation (8) that is identical to the specification shown in column (3) of Table 2 except that in place of the Post$*$DC interaction, we include interactions between the DC dummy variable and a series of indicators for the months surrounding the date of Supreme court decision that led to desegregation. In Figure 2, we plot these estimated coefficients, which represent the mean log revenue earned in DC relative to the rest of the US in each month, conditional on covariates. The bars represent the 95% confidence intervals based on the asymptotic clustered standard errors.

The figure establishes parallel trends of revenues prior to desegregation. In the month of desegregation, DC theaters experienced a statistically significant decline in revenues relative to the rest of the US. While some of the revenue effect dissipates one month after desegregation, the revenues in DC are consistently lower throughout the five months after desegregation.

Finally, we conduct a placebo exercise, where we estimate the difference-in-difference specification for each possible counterfactual treatment week in DC from January 1952 until June

1955. In our main regression specifications described above, we include year*city effects, meaning that the desegregation impact is identified off of variation within 1953. We make the placebo exercise consistent with this by defining the "year" to begin five months prior to the placebo treatment date and end seven months after. This way, the coefficient of interest is identified of the same number of pre- and post-treatment weeks.

We plot the results of the placebo exercise in Figure 3. In panel (a), we plot the histogram of treatment effects, from which we can conclude that the "true" treatment effect was well outside the range of most of the placebo effects. The percentile of the true estimated treatment effect is 1.1, meaning that nearly 99 percent of the placebo effects were higher than the true estimate. The 10-90 percentile range is from -0.063 to 0.064. In panel (b), we plot the estimated placebo effects by week in 1953, and the 95 percent confidence interval based on the asymptotic standard errors clustered by city. This plot also includes the true date of treatment. 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, 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 Price Results

We next explore how prices may have changed in response to desegregation. Even though controlling for price had little impact on the estimated revenue effect of desegregation, this is not necessarily indicative of the absence of a pricing response. The optimal pricing structure that a theater owner sets will change in response to the change in the customer base resulting from desegregation. Such a change in pricing may offset some of the adverse effects of desegregation.

In Table 3, we present the results of estimating equation (8) using the log high price charged by the theater. The specifications shown in this table are otherwise identical to those displayed in columns (1)-(4) Table 2. The high price charged for movies is estimated to decline by between 2.2 percent and 2.8 percent depending on the precise set of fixed effects included in the estimation. These coefficient estimates, aside from that shown in column (4), are statistically insignificant both when inference is based on the asymptotic standard errors corrected for clustering at the city level and when based on the p-values obtained from randomization inference. (p=0.25 in the richest specification) As was discussed in Section 4, price changes are infrequent, and much of the intertemporal variation in prices result from temporary price changes by theaters. It is therefore not surprising that we are unable to detect a statistically significant price effect.

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 firm discrimination. We estimate a specification of the form

$$y_{ijc} = \beta_0 + \beta Z_c * D_i + BX_{ij} + \gamma_j + \epsilon_{ijc} \quad (9)$$

where as before i and j index movie and theater, respectively. 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. This is a difference-in-difference specification, where we are comparing the outcome for black-cast movies in racially biased cities versus low-bias cities with the similar difference for white-only movies. The vector of controls X_{ij} includes theater capacity, the black population share in the city, an interaction between 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 will be needed if black-actor films are disproportionately screened at theaters with lower reservation values. In each case, we will correct standard errors for two-way clustering at the city and film level.

The tests for firm discrimination rest heavily on the assumption that racial bias by customers affects only the level of revenues, and not its time path over the course of a film's run. We test this assumption directly in the data. To do so, we restrict attention to movies screened for three weeks or longer, and then examine the decay of revenue between weeks one and two. This sample restriction reduces the selection issue that some films with black actors that are only screened for one week would have been screened for a second week if not for racial bias. Selection bias could remain when restricting to films with a minimum of a three week run, since some films screened one week may have been screened for three if not for racial bias, though the issue is greatly reduced. We estimate equation (9), with the dependent variable being the change in revenues from weeks one to two. The coefficient on the black actor*racial bias interaction is -0.008. This is statistically insignificant and is small compared to the median revenue decline of 28.8 percent.

In Table 4, we present three sets of results, each for a different dependent variable. In the first two columns, the dependent variable is the number of weeks film i is screened at theater j.

The specification shown in column (1) controls for city-level fixed effects, while the specification in column (2) controls for theater fixed effects. Films with black actors are screened for 0.28 fewer weeks in racially biased areas. Some of this is due to the type of theater at which the film is shown within the city. Once we control for theater fixed effects, as shown in column (2), this estimate attenuates to 0.12, and remains statistically significant. An intuitive result is that films with black actors are screened for longer when shown in cities with a larger black population share. In columns (3) and (4), we show similar specifications where the dependent variable is the log of the total revenue earned by the theater over the film's run. Mirroring the results for weeks screened, the estimates shown in column (3) indicate that film revenue is lower by 0.23 log points when a black-actor movie is screened in a city with greater racial bias. This coefficient estimate again attenuates to -0.11 when considering a specification including theater fixed effects. The results for run length and revenues both point toward racial bias influencing the box office success of films with black actors, however as we have discussed, we are not able to distinguish customer from firm discrimination from these estimates.

We next implement the test for firm discrimination suggested by the theoretical model. In column (5) of Table 4, we include in the revenue specification a control for the run length of the movie at that theater. As we showed in the model, the null hypothesis of no firm discrimination can be tested by estimating the effect of racial bias on total revenues conditional on film 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 racial bias*black actor interaction is just -0.030 (p=0.59), compared to -0.11 when not conditioning on run length.

In columns (6) and (7) of Table 4, we show the results of estimating specifications where the dependent variable is the likelihood that a film's run is greater than one week. As we demonstrated in our model, conditional on week-one revenue, how this continuation probability of black-actor films is affected by racial bias will detect firm discrimination. While our proposed test can be implemented for any week in the film's run, in our setting the continuation probability at week one has the greatest empirical relevance. To see why, consider the distribution of run length for films with a black actor, as shown in Figure 4. In racially biased areas, 72 percent of the run lengths are only one week, with films in less racially biased areas experiencing a one week run only somewhat less often (60 percent of theater-film observations). Relatively few films are run for three or more weeks.

In column (6) of Table 4, we present the results without conditioning on week one revenue.

The estimates here suffer from the same issue as those for total revenue and overall run length, in that they are affected by both customer and firm discrimination. As suggested by the unconditional means displayed in Figure 4, we find that films with black actors are less likely to have a run length greater than one week when screened in racially biased cities. In column (7), we include controls for the log revenue earned by the film in the first week of its run. The point estimate on the variable of interest in this specification is small and statistically insignificant. The continuation probability for black-actor films is 4.5 percentage points lower in racially biased areas (p-value=0.209), compared to a reduction of 7.3 percentage points when not conditioning on week one revenues.

While these tests fail to reject a null hypothesis of no firm discrimination, they do not definitively prove the absence of such bias. That said, when viewed in combination with the decline in profits after desegregation, it suggests that consumer discrimination may be the more relevant force determining the choices that movie theaters made.

7 Conclusion

In this paper, we examine the role of customer and firm discrimination in contributing to segregation and firm choices in public accommodation. We estimate the effects of desegregating movie theaters in the 1950s on theater revenues, which we use to evaluate the relative importance of racial bias of customers and firms. We find that theater revenues in DC fell after a plausibly exogenous court ruling desegregated movie theaters, making them available to theatergoers regardless of race. This result points toward customer discrimination as a driving force behind the practice of refusing service to black customers. We provide further evidence pointing toward customer discrimination, and away from firm 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 firm discrimination, and we are unable to reject a null hypothesis that firms are unbiased. These results may help to assess why *de facto* segregation by businesses persisted, despite the profit incentive of firms to expand their market by serving customers of all races.

The explicit segregation encountered by black movie customers prior to the 1964 Civil Rights Act is no longer legal. However, 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. The exercise of religious preferences provides another interesting modern example. The U.S. Supreme Court ruled in 2018 that business owners were permitted to refuse service to gay customers on religious grounds, which serves to highlight the continued importance of understanding firms' voluntary exclusion of customers.

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Figure 1: Number of movies with significant black characters

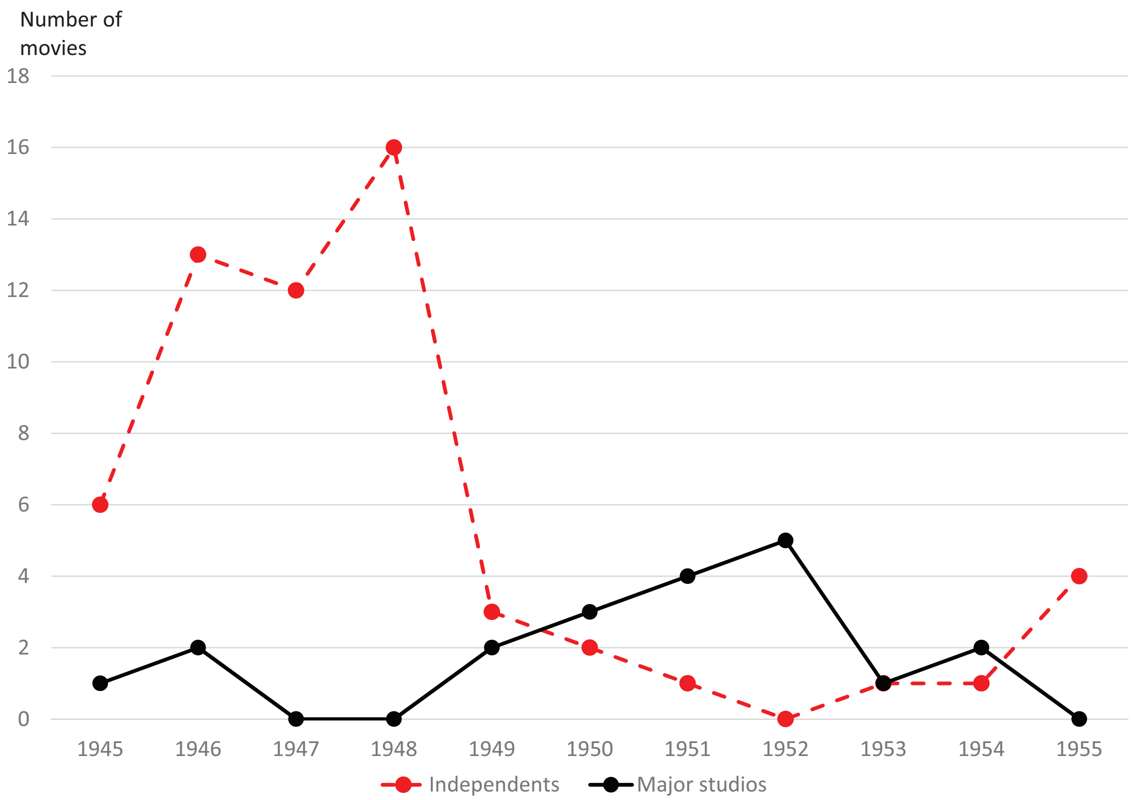


Figure 2: DC effect by month relative to desegregation, log revenue

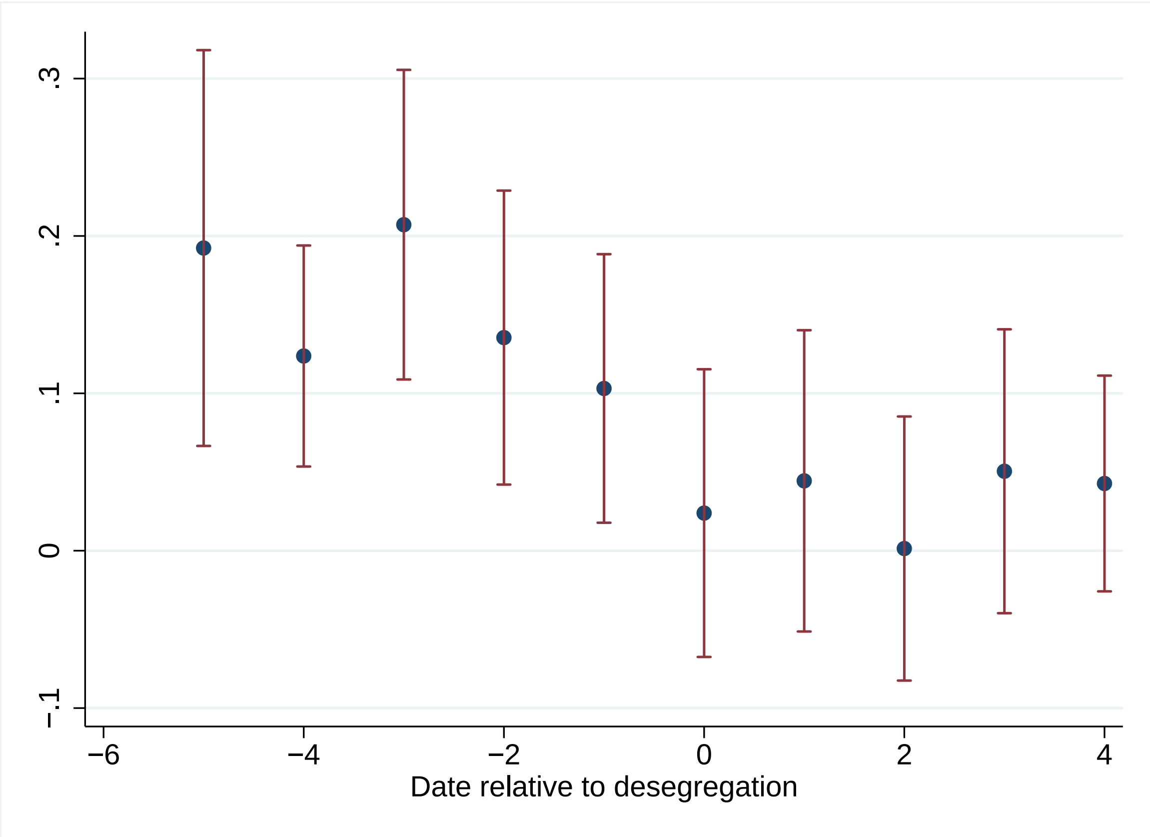
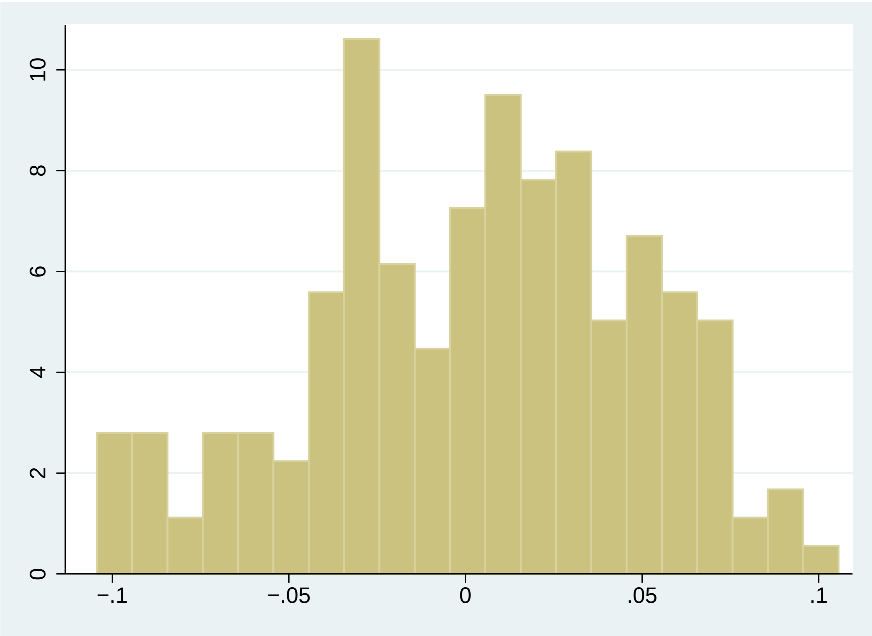


Figure 3: Placebo tests for alternate treatment weeks in DC

(a) Distribution of estimates



(b) Estimates by placebo weeks in 1953

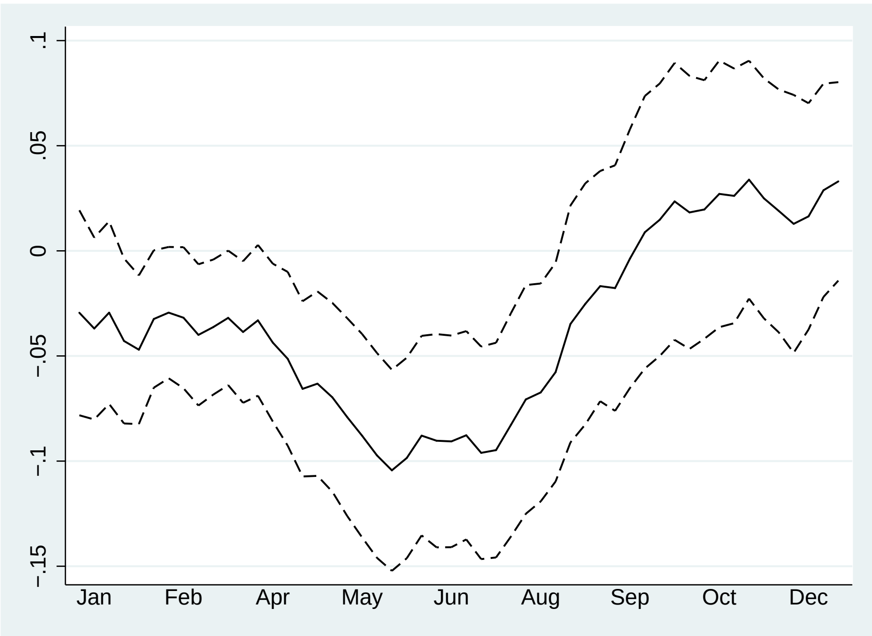


Figure 4: Run length distribution for films with black actors

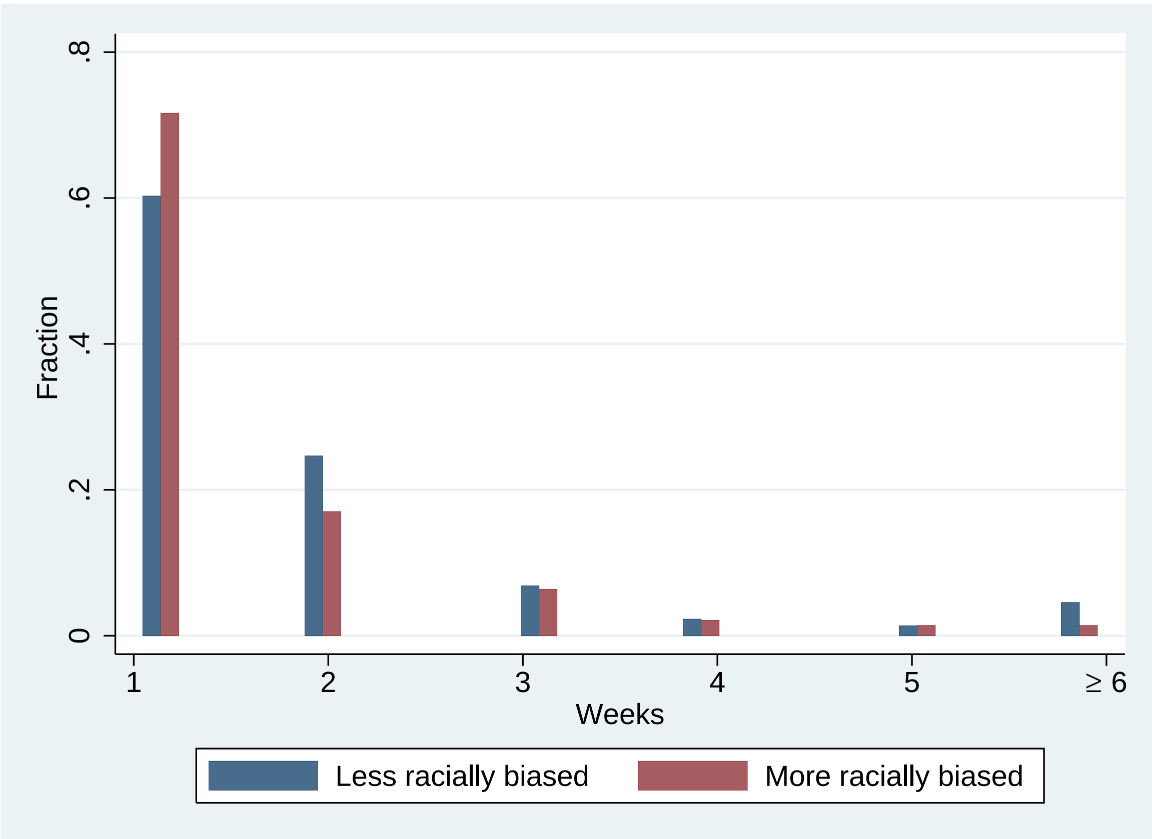


Table 1: Summary statistics

	DC	Other cities	Total
Revenue earned in previous week	10973.0	13605.5	13486.4
	(7508.1)	(15344.7)	(15088.4)
High price	1.039	1.123	1.119
	(0.395)	(0.461)	(0.458)
Low price	0.643	0.683	0.681
	(0.196)	(0.237)	(0.236)
Film age (months)	5.748	5.566	5.574
	(16.61)	(15.44)	(15.50)
Weeks film at theater	5.086	3.284	3.365
	(12.41)	(7.771)	(8.048)
Theater capacity	1521.4	2064.9	2040.3
	(964.5)	(1151.5)	(1149.3)
Number of films screened	1.026	1.350	1.335
	(0.164)	(0.488)	(0.483)
Observations	47124		

Table 2: Weekly theater revenue and desegregation of DC theaters

	(1)	(2)	(3)	(4)	(5)
Post*DC	-0.11***	-0.11***	-0.13***	-0.096***	-0.087***
	(0.025)	(0.025)	(0.029)	(0.018)	(0.016)
	[0.068]	[0.068]	[0.068]	[0.068]	[0.068]
Post desegregation	-0.016		0.024	-0.0024	-0.015
	(0.066)		(0.099)	(0.066)	(0.064)
Log high price					0.25***
					(0.057)
Log low price					0.030
					(0.040)
Log theater capacity	0.45***	0.46***	0.45***		
	(0.066)	(0.066)	(0.066)		
Log film age (months)	-0.12***	-0.13***	-0.12***	-0.11***	-0.10***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
Log weeks film at theater	-0.34***	-0.33***	-0.34***	-0.33***	-0.33***
	(0.037)	(0.037)	(0.038)	(0.027)	(0.027)
Max temp.	-0.0027**	-0.0030***	-0.0013*	-0.0022**	-0.0024**
	(0.00071)	(0.00075)	(0.00059)	(0.00064)	(0.00065)
Min temp.	0.0027**	0.0024*	0.0019**	0.0025**	0.0026**
	(0.00086)	(0.0010)	(0.00062)	(0.00077)	(0.00075)
Snowfall in.	-0.040**	-0.035**	-0.050***	-0.036*	-0.038**
	(0.012)	(0.011)	(0.0095)	(0.013)	(0.012)
Snow depth in.	0.0073**	0.0046*	0.011***	0.0071**	0.0076**
	(0.0025)	(0.0021)	(0.0027)	(0.0023)	(0.0022)
Precip. in.	0.0047	0.018	0.0032	0.0021	0.0029
	(0.014)	(0.011)	(0.014)	(0.011)	(0.011)
Year*week effects		X			
Year*month effects	X		X	X	X
Theater effects				X	X
City specific seasons			X		
Observations	46669	46669	46669	46669	46669
R-Squared	0.75	0.76	0.75	0.84	0.84

The dependent variable is the log of revenue earned by a theater. The unit of observation is a theater-week. All specifications include film dummy variables and city*year effects.

In parenthesis is the asymptotic standard error clustered by city. In brackets is the p-value obtained by randomized inference.

*,**,*** denote significance at the 10%, 5%, and 1% level, respectively, and refers to inference using the asymptotic standard error.

Table 3: Weekly theater ticket price and desegregation of DC theaters

	(1)	(2)	(3)	(4)
Post*DC	-0.022	-0.022	-0.025	-0.028**
	(0.011)	(0.011)	(0.013)	(0.0083)
	[0.250]	[0.250]	[0.341]	[0.250]
Post desegregation	0.048**		0.054*	0.040*
	(0.017)		(0.022)	(0.018)
Log theater capacity	0.012	0.012	0.012	
	(0.021)	(0.021)	(0.021)	
Log film age (months)	-0.015**	-0.016**	-0.015**	-0.0098**
	(0.0045)	(0.0050)	(0.0045)	(0.0030)
Log weeks film at theater	0.015*	0.016*	0.016*	0.0035
	(0.0063)	(0.0065)	(0.0064)	(0.0035)
Max temp.	0.00022	0.00014	0.00020	0.00025
	(0.00015)	(0.00017)	(0.00017)	(0.00019)
Min temp.	-0.00023	-0.000084	-0.000058	-0.00023
	(0.00020)	(0.00028)	(0.00020)	(0.00023)
Snowfall in.	0.0064	0.0057	0.0043	0.0071
	(0.0037)	(0.0040)	(0.0035)	(0.0035)
Snow depth in.	-0.00098	-0.00077	-0.00063	-0.0017
	(0.00096)	(0.0010)	(0.0013)	(0.00086)
Precip. in.	-0.0043	-0.0043	-0.0044	-0.0044
	(0.0045)	(0.0047)	(0.0036)	(0.0035)
Year*week effects		X		
Year*month effects	X		X	X
Theater effects				X
City specific seasons			X	
Observations	46669	46669	46669	46669
R-Squared	0.86	0.86	0.86	0.90

The dependent variable is the log of the weekly high price charged by a theater. The unit of observation is a theater-week. All specifications include film dummy variables and city*year effects.
In parenthesis is the asymptotic standard error clustered by city. In brackets is the p-value obtained by randomized inference.
*,**,*** denote significance at the 10%, 5%, and 1% level, respectively, and refers to inference using the asymptotic standard error.

Table 4: Racial bias and films with black actors

	Weeks at theater		Log total revenue			Run > 1 wk	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black actor*Racially biased	-0.28***	-0.12***	-0.23***	-0.11***	-0.030	-0.073**	-0.045
	(0.038)	(0.029)	(0.052)	(0.042)	(0.054)	(0.030)	(0.036)
Black actor	-0.014	0.019	0.023	0.068	0.055	0.019	-0.0018
	(0.067)	(0.053)	(0.089)	(0.081)	(0.058)	(0.042)	(0.021)
Black actor*Black pop. share	3.39***	1.10**	2.00***	0.091	-0.65	0.57**	0.66***
	(0.67)	(0.49)	(0.69)	(0.64)	(0.59)	(0.24)	(0.16)
Run length in weeks					0.68***		
					(0.023)		
Log first week revenue							0.42***
							(0.063)
City effects	X		X				
Theater effects		X		X	X	X	X
Observations	92074	92074	92070	92070	92070	92074	92036
R-Squared	0.16	0.29	0.41	0.56	0.83	0.24	0.36

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 theater release year. The racially biased variable is an indicator for the racial bias index being above the average. In parenthesis is the asymptotic standard error clustered by city.
*,**,*** denote significance at the 10%, 5%, and 1% level, respectively.

A Appendix

Table A1: Films with significant black actors by major studios

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