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Can We Legislate Tolerance? Investigating the Impact of State Anti-Discrimination Laws on Racial Attitudes (1945-1965)

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Abstract

Can We Legislate Tolerance? Investigating the Impact of State Anti-Discrimination Laws on Racial Attitudes (1945-1965)

By Jamila Spears

This study strives to explore the relationship between the adoption of local anti-discrimination laws concerning fair employment, public accommodation, and private housing with the power of enforcement and the percentage of positive racial attitudes observed in a given state. Since different states adopt laws at different times, I employ a staggered Difference in Differences (DiD) design both with and without an efficient estimator. I also conduct correlational analysis on the association between adoption time and racial attitudes, in addition to whether there is a statistically significant difference between the attitudes of treated and never-treated states. The findings of this study suggest that while there is some evidence that anti-discrimination laws may have contributed to increased racial tolerance, the presence of pre-treatment differences, high variance in the data, and issues of small sample size limit causal claims. These results emphasize the need for further research to address data limitations.

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1 Introduction

There has, since the inception of the United States, existed a great irony in this country. Although the Declaration of Independence spoke of the purported equality of all men[1], slavery was still deeply rooted in the fabric of U.S. society by the time the Constitution was drafted in 1787 (Alexander 2010, 42)

The ramifications of the four centuries during which slavery prospered in America have endured long past the Emancipation Proclamation, and in fact are still very much present today. The legacy of slavery in America can be seen in the institutionalization of white supremacy, a term referring to the systematic subjugation of Black Americans in tandem with the preservation of the privileges of whiteness.

Much like Patrick Henry's declaration "give me liberty, or give me death," [2](Currier & Ives 1876) the Civil Rights Movement was born from the passion of Black Americans who would have rather died than continue to be treated as undesirables in a country made wealthy by their forced servitude. It was time for another revolution. The movement hinged on two key targets: desegregation and voting rights. Civil Rights leaders recognized that equal opportunity in jobs and education was essential for the economic and social mobility of African Americans. W.E.B. Du Bois was one of the most prominent champions of Black suffrage in this time, and the National Association for the Advancement of Colored People (NAACP) played a pivotal role in legal challenges against voter suppression (Lawson 2015, 100). Black veterans returning from World War II echoed this sentiment, with one Georgia veteran stating, "peace is not the absence of war but the presence of justice which may be obtained, first, by your becoming a citizen and registered voter" (97).

Due to the reluctance of governments to enforce early anti-discrimination laws, such as the 1944 Supreme Court decision preventing white-only primaries and Brown v. Board of Education

1. "We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain unalienable Rights, that among these are Life, Liberty and the pursuit of Happiness," 1776

2. A patriot during the Revolutionary War, he is famously quoted as having ended a speech to the Virginia Assembly with the phrase "Give me Liberty, or give me death!" which became the movement's rallying cry, 1775

(1954) which mandated school desegregation, some of the greatest legal victories of the Civil Rights Movement were not as fruitful as proponents had hoped. By 1966, most Southern schools remained segregated and millions of Black Americans were still disenfranchised by voter suppression tactics such as literacy tests and poll taxes (Lockard 1968, 6).

A common argument against enforceable Civil Rights legislation was that even if these laws were adopted, they would fail to change discriminatory behaviors and attitudes. This assertion was particularly hypocritical in the South, where Jim Crow laws had systematically enforced discrimination, proving that laws do shape behavior and social norms (7). Although it is well established that laws can compel behavioral changes, the question of whether they can shape the way people think has proved more complex. Duane Lockard, author of *Toward Equal Opportunity: A study of State and Local Antidiscrimination Laws*, refutes the claim that laws cannot influence attitudes, arguing that "laws do so constantly" (8). Employers who resisted laws on Fair Employment practices initially feared economic losses from integrating their workplaces, but once these laws were enforced and the expected social upheaval failed to materialize, their attitudes shifted. The same occurred with employees, who after more direct interactions with African American coworkers, recognized that the commonly accepted negative racial stereotypes were baseless (9).

Thus, the central question remains: Can legal interventions change not just behavior, but public opinion? Although the Civil Rights Movement is often associated with federal legislation, many of its early successes came at the state level, with the earliest enforceable Fair Employment laws passed in 1945 (24). In this study, I seek to examine whether the implementation of certain of these state-level anti-discrimination laws influenced racial attitudes, or if they merely forced compliance without shifting underlying beliefs. Survey data is used to measure public opinion on discriminatory employment practices, segregation in schools, and other relevant subjects.

Of course, when dealing with survey data there are limitations to keep in mind. The impact of desegregation on public attitudes is complex. Historian Gavin Wright, in his book *Sharing the Prize: The Economics of the Civil Rights Revolution in the American South*, includes this remark from a Black southerner: “when [segregation] ended you can’t find a single white person who

remembers it" (Wright 2013, 2). This willful amnesia of past racism suggests that people may not openly acknowledge past discriminatory beliefs once they become socially unacceptable. If this is the case, measuring true shifts in attitudes could prove difficult, as individuals may be reluctant to admit previously held prejudices.

Three types of state anti-discrimination laws form the basis of this study: Fair Employment Laws (prohibiting racial discrimination in hiring and workplace treatment), Public Accommodations Laws (banning racial segregation in businesses such as restaurants, hotels, and theaters), and Private Housing Laws (preventing racial discrimination in housing sales and rentals) (Lockard 1968, 24). These laws went into effect with enforcement provisions at different times, making a staggered Difference-in-Differences (DiD) approach appropriate. The analysis is conducted using both models which allow for heterogeneous treatment effects in DiD settings, and models using an efficient estimator to further reduce variance.

If anti-discrimination laws are effective, we should expect to see shifts in racial attitudes over time. However, there are reasons to be cautious in interpreting the results, such as the fact that shifts in attitudes may take longer than changes in behavior, confounding variables such as migration patterns may bias treatment effects, and the inherent limitations of survey data.

This paper contributes to the literature on policy effectiveness by examining whether laws designed to promote racial equality had broader social consequences beyond forced compliance. It addresses the longstanding debate over whether legal mandates can shape social norms and influence deeply held beliefs. The findings have implications for contemporary policy debates, particularly in areas where legislation seeks to challenge entrenched biases. If laws foster genuine changes in attitudes, then they can serve as powerful instruments for social progress. If not, additional strategies may be required to shift public opinion.

2 Historical Background

2.1 A Nation on the Brink

The changing racial demography and increasing demand for labor caused by the United States' precipitous charge into World War II did little to resolve the deep-seated racial inequalities in American society (Seltzer 2022, 39). Rather, the wartime period highlighted and, in many cases, exacerbated racial tensions. The need for laborers due to the war effort stood in direct conflict with entrenched racial discrimination in the job market, leading to mounting frustration among Black Americans over their persistent lack of economic opportunity. A. Philip Randolph, president of the Brotherhood of Sleeping Car Porters, and Walter White, executive secretary of the NAACP, emerged as central figures in the fight for equal employment opportunities during the war (Lockard 1968, 17). When the Roosevelt administration learned that at least 100,000 demonstrators planned to march on the capital in July of 1941 in a protest planned by White and Randolph, the president signed Executive Order 8802, establishing the Committee on Fair Employment Practices (FEPC). This legislation temporarily placated Civil Rights leaders into canceling the demonstration, but when the FEPC proved to be a weak instrument which lacked substantive enforcement power, Randolph and White began to threaten another march on Washington (17). Their dissatisfaction was well-founded, as racial tensions in the labor market only continued to escalate. The influx of approximately 400,000 new residents into Detroit during the war, and resulting competition over jobs, culminated in violent race riots in June 1943 (Seltzer 2022, 39).

Beyond the domestic labor market, World War II posed a unique opportunity for the fight against discrimination in the defense industry, as United States racial hypocrisy was put on full display on the world stage. Despite the nation's ideological battle against Nazi Germany's racist policies, segregation within the U.S. military remained firmly in place and legally enforced (40). Black soldiers in segregated units were asked to risk their lives in a war against fascism while being denied fundamental rights at home (Lawson 2015, 97). Civil rights advocacy groups emphasized

this contradiction, drawing explicit connections between the war effort and the ongoing domestic struggle for racial equality (Lawson 2015, 102). Federal legislators, too, recognized the potential international ramifications of domestic racial discrimination, fearing that such injustices could be exploited by enemy propagandists, particularly Japan (101). Their concerns were substantiated by a 1942 Office of War Information study, which found that 18% of African Americans believed they would receive better treatment under Japanese rule, while an additional 31% percent felt they would be treated no differently than they were in the United States (101–102).

The postwar period did little to alleviate these tensions. Black veterans returned from battlefields in Europe and the Pacific only to become victims of the same racial discrimination they had fought against abroad. In one of the most horrific incidents of this, Isaac Woodard Jr., a Black veteran, had his eyes gouged out by white South Carolina police officers while he was still in uniform, not 6 hours after receiving his honorable discharge (133–134). While Woodard's case was far from the only instance of extreme violence against Black veterans, it was the one to gain national attention, even sparking sympathy from President Harry Truman after NAACP representatives confronted him with the case. Two years later, on July 26, 1948, Truman signed Executive Order 9981, effectively ending racial segregation in the U.S. military (Lawson 2015, 146; Seltzer 2022, 40). This marked a significant, though incomplete, victory in the broader struggle for Civil Rights.

2.2 The Civil Rights Movement

The start of the modern Civil Rights Movement is often said to be 1954, with the landmark Brown v. Board of Education Supreme Court decision, which declared racial segregation in public schools to be unconstitutional (Seltzer 2022, 55). However, this ruling did not take immediate effect and was met with staunch resistance. In an effort to force desegregation, the NAACP arranged for nine Black students to enroll at Little Rock Central High School in Arkansas. In response, the governor of Arkansas deployed the National Guard to block their entry, sparking a violent conflict. President Dwight D. Eisenhower was forced to intervene, which he did by federalizing the Arkansas National Guard and ordering part of the 101st Airborne Division to protect the students. Only then

were the Little Rock Nine able to begin school (Seltzer 2022, 55).

The shocking torture and murder of Black 14-year-old Emmett Till in August of the following year forced many Americans, Black and white, young and old, to face the devastating consequences of racism in the South. Till was visiting relatives in Mississippi when he allegedly whistled at Carolyn Bryant, a white woman. That night, Bryant's husband and brother-in-law entered Till's uncle's house and kidnapped him (Library of Congress, n.d.[a]). Days later, the boy's body was found in the Tallahatchie River, mutilated almost beyond recognition. In a press release, an officer of the NAACP is reported as saying "It would appear from this lynching that the State of Mississippi has decided to maintain white supremacy by murdering children" (Library of Congress, n.d.[b]). Despite nationwide outrage, the two men who kidnapped Till were acquitted of all charges, the injustice of this decision only further radicalizing Civil Rights activists and motivating more citizens to join their cause (Library of Congress, n.d.[a]).

1955 was also the year in which Rosa Parks refused to give up her seat on a segregated bus in Montgomery, Alabama, her ensuing arrest inciting the Montgomery Bus Boycott, which lasted for 381 days. This economic protest catapulted Martin Luther King Jr., its leader, into the national spotlight as a central figure in the battle for Civil Rights (Seltzer 2022, 55). In 1957, King and other religious leaders established the Southern Christian Leadership Conference (SCLC), a group dedicated to "achieving full integration of the Negro in all aspects of American life" (Lawson 2015, 136). Later that year, the first major Civil Rights march on Washington, "A Prayer Pilgrimage for Freedom," drew roughly 25,000 people to the Lincoln Memorial. On that day, King stressed that the path to true citizenship for Black Americans was through the right to vote (97).

The movement intensified in February 1960 when four Black students in Greensboro, North Carolina, staged a sit-in at Woolworth's segregated lunch counter. The students, who sat at the counter and bowed their heads in silence, were forced to endure violent physical assaults from white patrons and counter-protesters while police officers outside of the restaurant stood by and watched (138). Similar sit-ins soon spread nationwide, and the Student Nonviolent Coordinating Committee (SNCC) was born (Seltzer 2022, 71). The SNCC was a militant group that operated in

areas of extreme racial subjugation and saw suffrage more as a means of rallying people to their larger cause: radical change to the foundations of U.S. political institutions (Lawson 2015, 101).

The empowerment given to ordinary Black citizens through collective organizing proved transformative. Fannie Lou Hamer, a Mississippi resident who joined the Black suffrage movement in 1962, remarked that white residents who had previously menaced her home with guns began to show her respect once she engaged in political activism: "They respect people who respect themselves" (102). Yet, by 1960, only four states had anti-discrimination laws all three areas: Fair Employment, Public Accommodations, and Private Housing (Lockard 1968, 24), and there was still no federal legislation banning racial violence and discrimination in the South (Lawson 2015, 139).

May 1961 saw the first Freedom Riders set off, a name for the white and Black activists who rode in buses together across state lines to challenge segregation in interstate travel (Seltzer 2022, 72). They faced severe beatings and mob violence throughout Alabama before finally being arrested in Jackson, Mississippi. The plight of the Freedom Riders garnered sympathy across the nation, and ultimately pressured President John F. Kennedy to demand bus integration (72).

2.3 Dreams and Disillusions

Kennedy, though the success of his 1960 presidential bid likely hinged on his role in obtaining the release of Martin Luther King after he was arrested for a traffic violation, was hesitant to fully embrace Civil Rights legislation once in office (Lawson 2015, 139–140). His administration did not condone civil disobedience and only intervened when there seemed to be no alternative (141). Recognizing Kennedy's complacency, Civil Rights leaders escalated demonstrations to force his hand.

In 1963, a wave of devastating racial violence erupted, most notably in Birmingham, Alabama, where Martin Luther King Jr.'s peaceful protesters were met with "attack dogs, water hoses, clubs, and cattle prods" (142). The SCLC headquarters and King's brother's home were firebombed, which led Black Americans to exact violent retribution on white citizens, and NAACP Field

Secretary Medgar Evers was assassinated—almost resulting in another race riot. All of this conflict came to a head with the "March on Washington for Jobs and Freedom" on August 28, 1963, where an estimated 250,000 people gathered at the Lincoln Memorial in Washington, D.C. to hear King's famous "I Have a Dream" speech (Seltzer 2022, 72). A month later, the bombing of the 16th Street Baptist Church in Birmingham killed four Black children and injured many others (72).

November of 1963 saw John F. Kennedy's assassination, but there was no stalling the momentum of the Civil Rights Movement at that point. The Civil Rights Act was finally passed on July 2nd, 1964, and signed into law by President Lyndon Johnson, prohibiting racial discrimination in public spaces, schools, and employment ("Civil Rights Act of 1964," n.d.).

Despite this political victory, violent repression persisted. On "Bloody Sunday," March 7, 1965, some 600 peaceful demonstrators marching from Selma to Montgomery to advocate for voting rights were stopped at the border (Seltzer 2022, 72). Roughly 150 Alabama State Troopers had lain in wait for them, and only one minute and five seconds after demonstrators were given a two-minute warning to disperse, the state troopers attacked using weapons such as bullwhips and barbed wire. According to his testimony from a March 1965 hearing, John Lewis, then chairman of the SNCC, was knocked down with a club, then beaten again when he tried to rise. It was at this crux that the Voting Rights Act of 1965 was finally signed into law on August 6 by President Johnson, banning discriminatory voting restrictions (National Archives, n.d.).

The fight for Civil Rights was far from over in 1965, but the movement soon began to fracture. One major source of division was the Vietnam War. As the U.S. intensified its involvement in Southeast Asia, federal resources and political attention increasingly shifted away from Civil Rights. By 1968, the government had redirected funds toward the war effort, depriving Civil Rights organizations of the federal support they had once leveraged for legislative victories. Furthermore, the war deepened ideological rifts within the movement itself. Activists such as Martin Luther King Jr. condemned U.S. involvement, linking it to systemic oppression at home, while others, including members of the NAACP, were hesitant to alienate potential allies in Washington by taking a strong anti-war stance (Lawson 2015, 122).

At the same time, the assassinations of key Civil Rights leaders, including Malcolm X in 1965 and Martin Luther King Jr. in 1968, only worsened matters. King's assassination, in particular, sparked a wave of unrest, with race riots erupting across the country resulting in 43 deaths. The systematic suppression of radical advocacy groups further weakened the movement, such as the FBI's targeting of the Black Panther Party. The Party ultimately suffered violent crackdowns, culminating in the 1969 assassinations of leaders Fred Hampton and Mark Clark in their sleep by the Chicago police and the eventual dissolution of the organization in the 1970s (Seltzer 2022, 72–73).

Evidently, while state and federal legislation marked historic victories for Civil Rights, systemic racism persisted and continues to persist. At every step toward progress, Civil Rights activists faced fierce resistance, not only from segregationists but also from federal and state authorities. The systematic suppression of the movement through state-sanctioned violence, brutal crackdowns on protests, and the assassinations of key leaders demonstrates that government institutions were often more invested in returning to the status quo as quickly as possible rather than in supporting meaningful racial reform.

This history provides crucial context into the racial climate during the Civil Rights period and the varying success and failures of anti-discrimination legislation. The persistence of racial violence, even after landmark federal actions such as Executive Order 9981 and Brown v. Board of Education, underscore the limitations of policy intervention alone. As the movement for racial justice gained momentum, state-level policies would play an increasingly important role in either reinforcing or challenging prevailing racial attitudes. The extent to which such policies influenced broader public sentiment remains a critical question in evaluating the efficacy of state anti-discrimination legislation during the Civil Rights era.

3 Literature Review

The effectiveness of anti-discrimination legislation in improving various social, economic, and judicial outcomes for historically marginalized communities has long been the subject of debate. There is much less empirical work, however, on how these policies influenced public perceptions of race and equality, or if they did at all. The aim of this study is to address this question.

Over the years, the Civil Rights Act of 1964 has become one of the most studied pieces of anti-discrimination legislation. However, much less emphasis has been placed on the role of state laws, which had already existed in 22 states by the time the federal law was passed. Collins (2001) emphasizes that measuring the impact of state anti-discrimination policies on labor market outcomes is essential to forming a deeper understanding if the Civil Rights Movement improved economic mobility for African Americans and if so, by how much.

Collins uses a difference-in-difference-in-difference (DDD) framework to estimate the impact of Fair Employment laws on annual income as well as unemployment and labor force participation. He finds that, particularly under the Fair Employment laws of the 1940's, the income of Black workers did marginally increase, though the racial income gap was still very much present. Additionally, in the 1940's he does not find evidence of higher unemployment rates for Black Americans as a result of their relative increase in income. This study expands on Collins' work by measuring the impact of state-level anti-discrimination legislation on racial attitudes, thus contributing to the literature if these marked achievements of the Civil Rights Movement actually made progress in influencing broader perceptions of race and equality.

Another relevant study is Cook et al. (2023), which examines racial discrimination in Public Accommodations using data from the *Negro Motorist Green Books* (1936–1966), more familiarly known as the Green Books, which provided Black travelers with a list of Black-friendly establishments across the country. A key focus of the study is the relationship between the number of state anti-discrimination laws and the number of Green Book listings in that state. They find the number of anti-discrimination laws in a state correlated with more Green Book listings, indicating that this

legislation was useful in reducing discriminatory behavior. However, the study concludes in order to truly end racial discrimination in public accommodations federal legislation is a necessity. This finding underscores the fact that anti-discrimination legislation can be very effective in terms of changing behavior, and that these behavioral changes can have a positive impact on the lives of Black Americans. I seek to extend these findings, and investigate if these laws lead to changes in public opinion.

Gil and Marion (2018) explore the effect of racial bias and segregation in residential areas on the number of movie theaters catering to Black patrons that entered these local markets in the 1950's. They use public opinion surveys and historical data on lynching rates to build two measures of racial hostility in a market. Their findings suggest that, while Black businesses and entrepreneurs were able to expand much more in areas of high segregation, they experienced limited growth in areas with high historical rates of racial bias. In net, racial discrimination appears to have reduced the market entry of Black businesses even despite some increases in market-share due to segregation, highlighting the direct impact of unchecked discrimination on Black Americans. In contrast, my study shifts the focus from economic outcomes to the attitudinal effects of anti-discrimination legislation. While Gil and Marion (2018) use public opinion data as a measure of racial bias affecting business entry, I examine it as an outcome variable—assessing whether anti-discrimination laws lead to shifts in racial attitudes. Furthermore, my study offers implications for contemporary policy debates, specifically if policy can be used to shape public opinion. This contribution helps bridge the gap between studies on structural discrimination, economic mobility, and those analyzing the social impact of policy interventions.

4 Data

4.1 Data Sources

The Gallup Polls

Public opinion microdata capturing attitudes towards racial discrimination and segregation in the United States from 1945-1965 was collected from Roper iPoll, a subsidiary of Cornell University's Roper Center for Public Opinion Research. The popularity of public opinion polling in the United States surged in the mid 1930's, primarily due to its usefulness as a tool for election forecasting. As pollsters were well versed in market research and its corresponding quota based sampling methods, populations who were historically not thought of as having much purchasing power, such as African Americans, were often severely underrepresented in surveys (Seltzer 2022, 2). To overcome this, the Berinsky & Schickler collection housed in Roper iPoll contains over 500 public opinion polls that have been re-weighted to correct for this sampling bias (The Roper Center, n.d.[b]). As such, all polls used in this study conducted prior to 1952 were collected from Berinsky & Schickler.

Gallup began to move away from these biased sampling methods in the 1950's, instead randomly assigning interviewers to zones of equal population size and instructing them to "select individuals within households by asking for the youngest man or oldest woman in each household" (The Roper Center, n.d.[c]). These surveys were then weighted a second time through card (row) duplication according to modern methods.

Despite this work to re-weight the surveys, the underlying data was by no means easy to work with. As remarked on by Richard Seltzer in the introduction to his book *US Public Opinion Since the 1930s: Galluping Through History*,

> *It was often very difficult to load the data. Gallup (and other organizations) had*

> *different forms that had to be combined (or decide not to combine). Furthermore, it was sometimes very difficult to map the data. A record might encompass seven or more lines (cards) in the ASCII file and the documentation was not always easy to decipher. My contact at Roper also noted that the interns they sometimes used made errors.* (Seltzer 2022, 10)

Loading the data was only one of the particular challenges introduced by the Gallup polls. In addition, questions that repeated over time were not always consistent in name nor wording. The collection of demographic information about the sample was also incredibly inconsistent. For example, in 1955 Survey 0541 classified respondents as either "White" or "Non-white," but in 1954, Survey 0532 classified respondents as "White" and "Black." Of course race would be a very interesting explanatory variable, but due to data limitations its use is not feasible in this case.

To identify questions of interest, the search terms "Negro," "Race," "Racial," "Black," "Civil," "Colored," and "Martin" were used. However, just because a question included one of these terms did not mean that it could be counted as a measure of racial attitudes.
For that reason, this question is considered a measure of racial attitudes:

> "The U.S. Supreme Court has ruled that racial segregation in all public schools is illegal. This means that all children, no matter what their race, must be allowed to go to the same schools. Do you approve or disapprove of this decision?" (1954-0531)

and this one, which refers to states rights rather than directly to an individual's racial attitudes, is not:

> "Some people say we should have a national law requiring employers to hire people without regard to color or race. Other people say that it should be left up to each state to decide on this for itself. With which side do you, yourself, agree?" (1952-0495)

Toward Equal Opportunity

Toward Equal Opportunity: A Study of State and Local Anti-discrimination Laws is a book by Duane Lockard that explores anti-discrimination legislation at the state and local level, rather than the federal one. Within the chapter: "The Politics of Enacting Anti-discrimination Laws," Lockard provides a table containing information on when states passed Fair Employment, Public Accommodations, and Private Housing anti-discrimination legislation with provisions for enforcement[3]. Thus, this data does not always reflect the year that the legislation was originally passed, but rather when it gained enforcement power.

4.2 Data Cleaning and Preparation

The data set used for this study contains observations at the state level. The outcome variable is the percentage of survey participants whose responses reflect positive racial attitudes in state i in year t. This is a continuous variable, bounded between 0 and 100. There are also three treatment variables denoting the three types of anti-discrimination law. These variables equal the year in which treatment went into effect for treated states and NA for states that never received treatment.

A total of 22 unique questions compiled from 37 surveys are used to construct to the dependent variable[4]. As referenced earlier, the majority of the 37 surveys used were ASCII files, and as such required manual parsing into R as fixed-width files with unique specifications for each survey according to the provided documentation. From each survey a long-format data set was created, containing the following columns for a given state:

1. Region

2. Demographic information (age, race, etc.)

3. Questions of interest (raw data)

3. This table can be found in Appendix D.
4. A full list of questions and surveys used is included in Appendix E

4. Count of number of responses for each possible answer choice for a given question, such as the number of respondents in favor (n_q7k_favor) or opposed (n_q7k_oppose).

5. A count of the number of individuals who answered each question

6. A count of the number of individuals included in the sample (including those who did not respond to the question)

Next, questions were arbitrarily renamed so that they could be tracked over time, and responses were grouped as either representing positive, negative, or neutral attitudes. Then the final outcome variable, percentage positive attitudes, was calculated by dividing the number of positive responses by the number of individuals who actually answered the corresponding question. This step is essential to ensure the outcome variable is comparable across states. The same was done for answers categorized as negative and neutral, but these responses are not used in the final analysis.

Demographic information for each survey was also aggregated up to the state level and data from all surveys was merged, so that questions which had multiple observations of percentage positive attitudes within a given year were averaged, and distinct questions were not. Lastly, data on the anti-discrimination laws was joined to the survey data.

The final panel data set contains region, response (positive, negative, or neutral), year, question (survey question), percentage, aggregated demographic information (education, gender, age, etc.) and three treatment columns (Fair Employment, Public Accommodations, and Private Housing) which indicate in what year enforceable legislation went into effect in each state.

4.3 Descriptive Statistics

Despite re-weighting, the sample of individuals included in this study is overwhelmingly white, and only the South is made up of more than 10% non-white respondents (Table 1). Individuals are also generally middle-aged and not highly educated. The sample is pretty evenly divided between male and female, with only a slightly higher percentage of women across regions.

Table 1: Sample Demographics

Region	White	HS Graduate	Some College	Age	Male	Female
Midwest	0.940	0.272	0.0711	45.3	0.490	0.509
Northeast	0.941	0.285	0.0904	44.3	0.488	0.512
South	0.833	0.180	0.0702	45.2	0.485	0.514
West	0.962	0.270	0.0891	44.5	0.485	0.508

These variables provide insight into the sample population, but they are less useful as potential controls in the DiD models. Table 2 explores the the between-group variance (variation in each variable across states, averaged over time) and the within-group variance (variation in each variable within each state over time).

Table 2: Between-Group and Within-Group Variance of Covariates

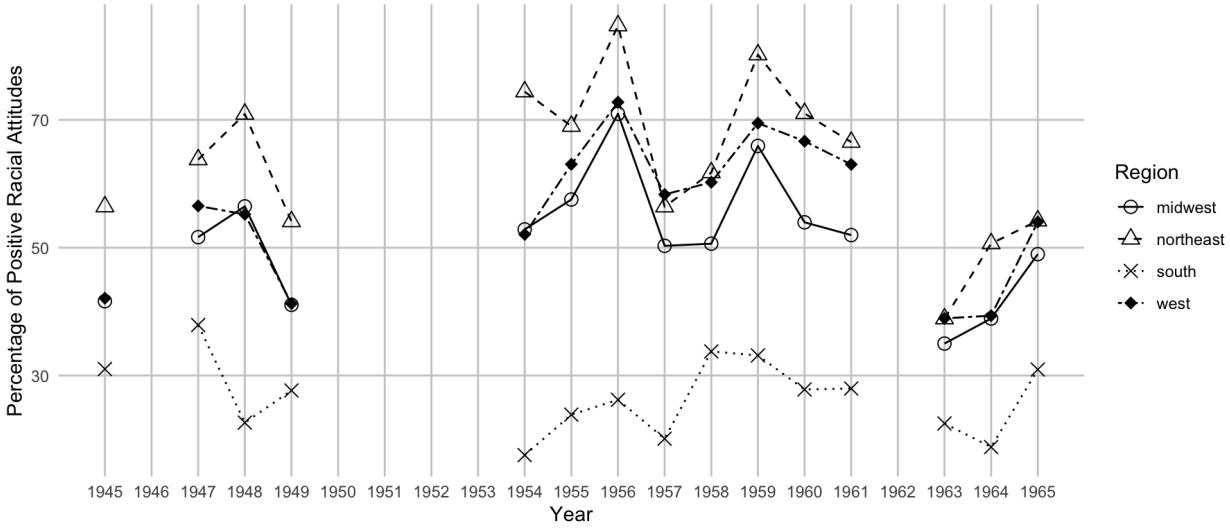
Variable	**Between Variance**	**Within Variance**
White (%)	0.4959	0.0190
High School Grad (%)	0.3355	0.0123
Some College (%)	0.3069	0.0240
Age (Mean)	0.2218	0.1618
Male (%)	0.1143	0.0298

The variation of the chosen variables over time seems to be mostly static. The variation across states is generally higher, but still not very large. This is likely to introduce collinearity issues with the implicit state and year fixed effects needed to estimate an ATT in a staggered adoption framework. Additionally, the primary goal of including controls in this study would be to explain some of the differences in trends between states. As demonstrated by Tables 1 and 2, these variables capturing the demographic information of the sample do not vary much between states or over time, so they will be excluded from the analysis.

Figure 1 illustrates the stark regional differences in trends of positive racial attitudes from 1945-1965. Unsurprisingly, Northeastern states consistently display the highest rates of positive attitudes over the course of the series, even nearing 90% in the mid and late 1950's. Conversely, the South

exhibits the lowest percentages by a wide margin, not exceeding 40% during the whole period. The West and Midwest follow similar trends, beginning to diverge more in the late 1950's. Interestingly enough, the non-Southern states all experience a sharp decline in attitudes between 1961 and 1963, meaning that the difference in attitudes between all regions is significantly lessened.

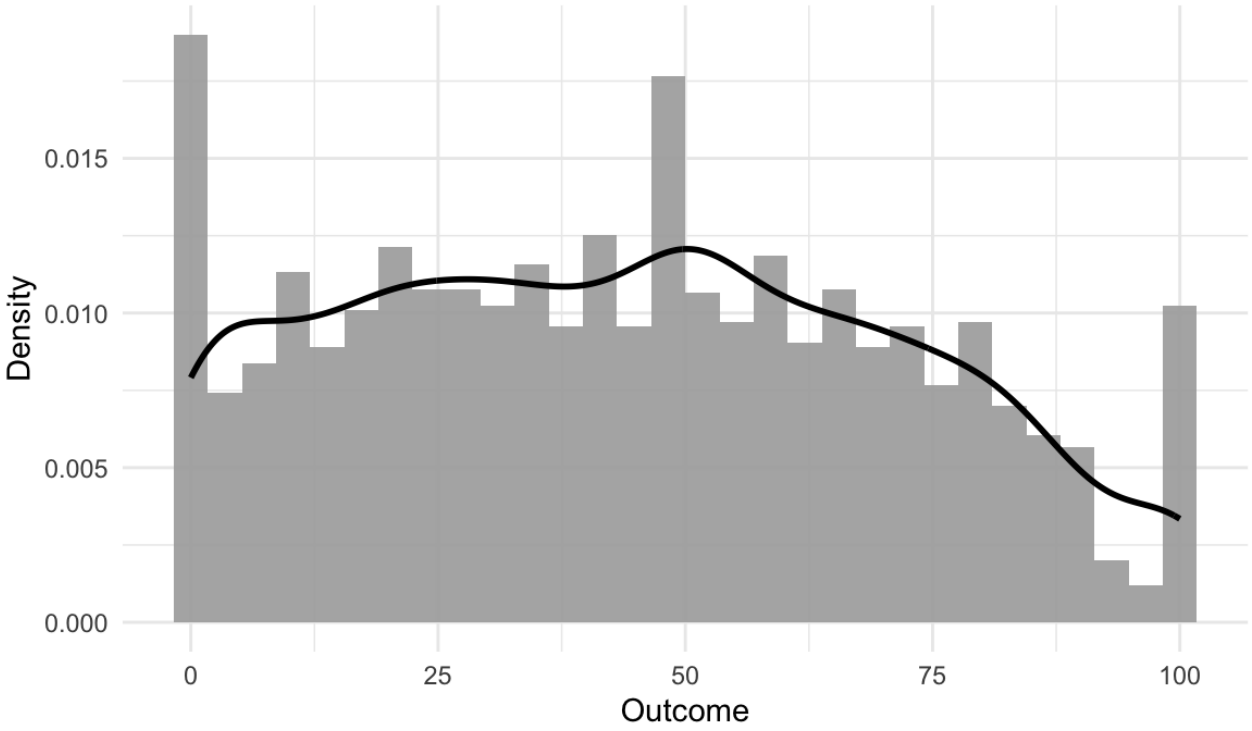
Figure 1: Percent of Positive Racial Attitudes by Region



There are many reasons why we may be seeing this dip in the early 1960's. Through a historical lens, race riots and civil disobedience campaigns before and after the passage of federal Civil Rights legislation displeased white Americans who saw them as a threat to order. It is also possible that some resistance to federal Civil Rights laws is reflected in the recorded racial attitudes.

A characteristic of this series that complicates causal inference is the non-normality of the distribution as seen in Figure 2. The distribution is relatively uniform, with wide spread and some clustering around zero and 50%. There is also an intriguing gap in density roughly between 90 and 100%.

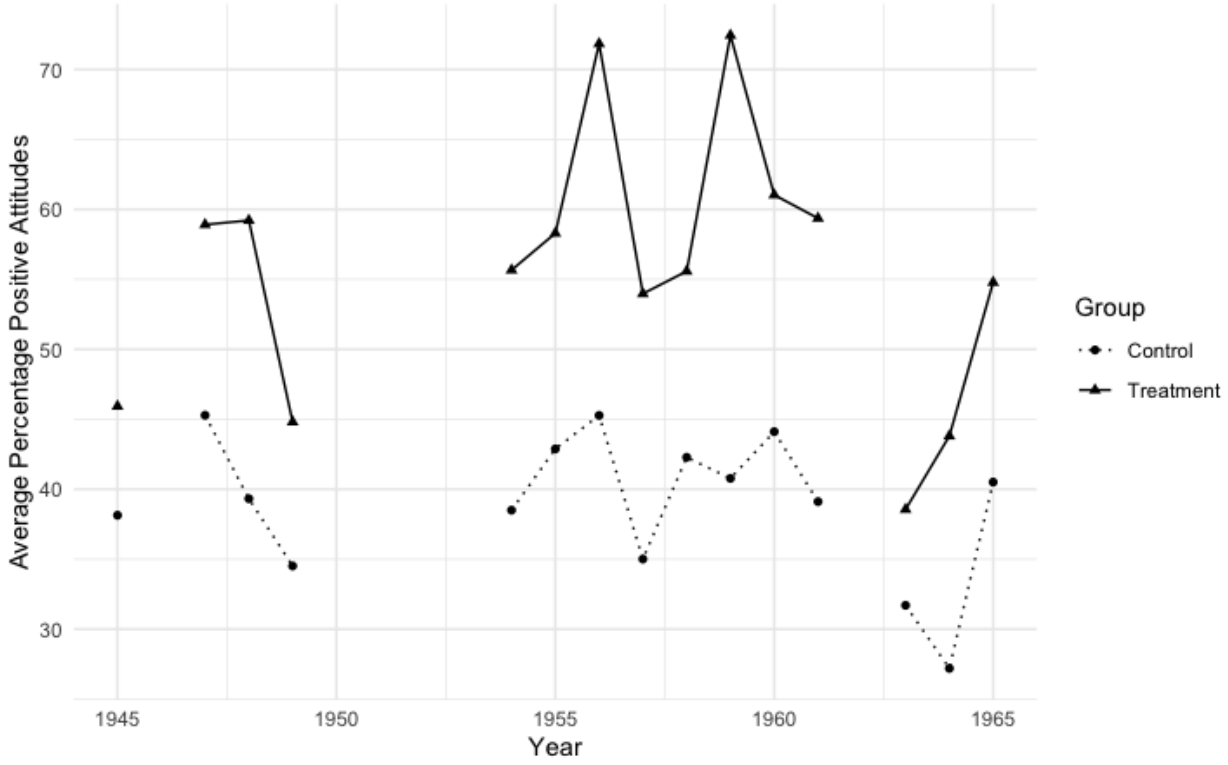
Figure 2: Distribution of Positive Racial Attitudes



The high variance of the distribution could lead to unstable and noisy estimates. If variance differs a lot between treatment groups, this could bias standard errors and confidence intervals in either direction, making estimates less reliable.

Lastly, I plot the aggregate trend of positive attitudes for treatment and control states over time, regardless of treatment timing. Figure 3 visualizes a comparison of positive attitudes among treatment and control groups, where treated states passed a Fair Employment law with enforcement.

Figure 3: Comparison of Treatment and Control Trends: Fair Employment

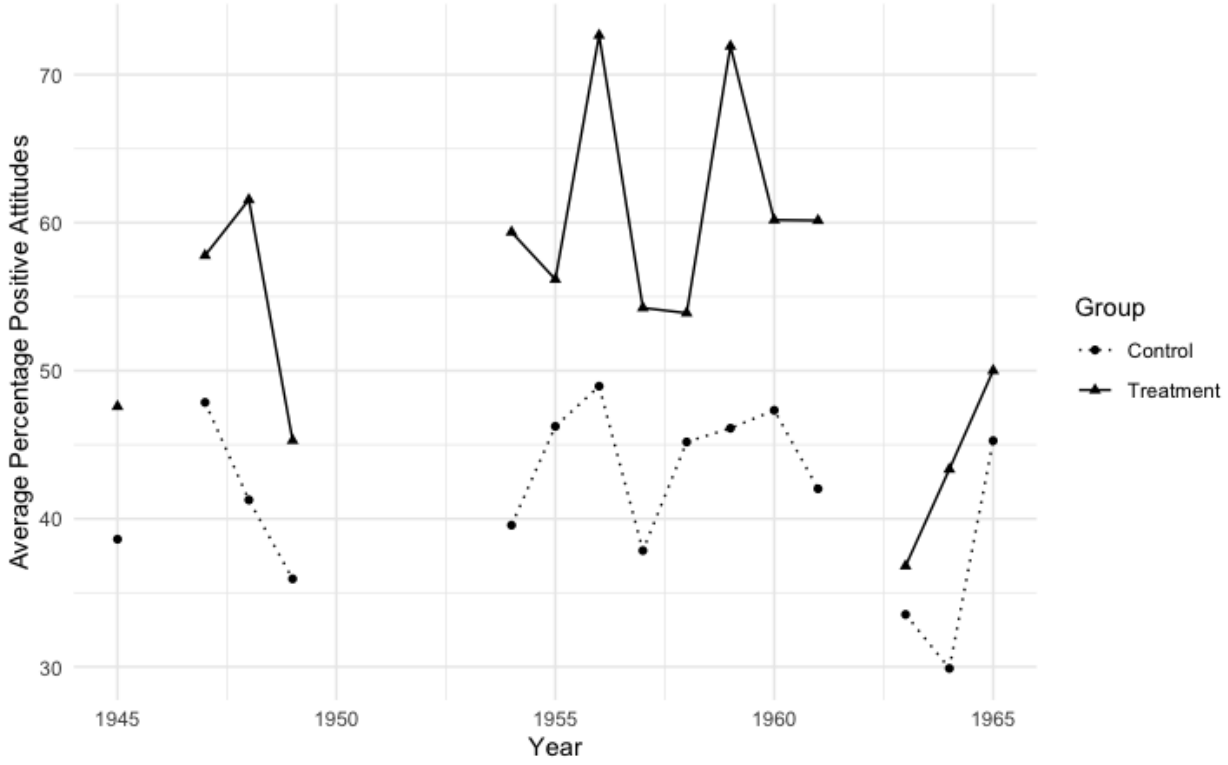


Treatment States: Alaska, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Indiana, Kansas, Massachusetts, Michigan, Minnesota, Missouri, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Washington, Wisconsin

Control States: Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Kentucky, Louisiana, Maine, Maryland, Mississippi, Montana, Nebraska, Nevada, New Hampshire, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, West Virginia, Wyoming

The percentage of positive attitudes definitely seems more stable over time for the control group, while the treatment group exhibits much larger swings in values post-1954. By 1965, the gap between treatment and control outcomes has narrowed, but the treatment group still shows higher attitudes.

Figure 4: Comparison of Treatment and Control Trends: Public Accommodations

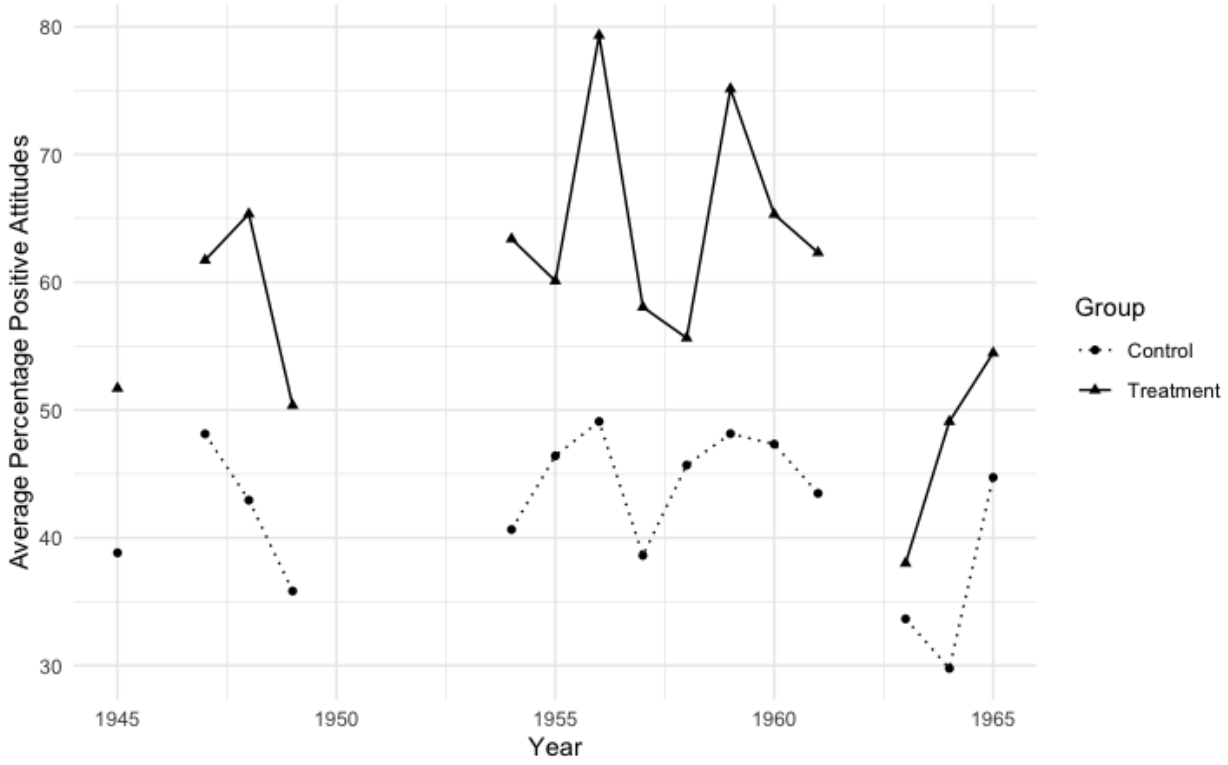


Treatment States: Alaska, Colorado, Connecticut, Delaware, Indiana, Kansas, Maryland, Massachusetts, Michigan, New Jersey, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Washington

Control States: Alabama, Arizona, Arkansas, California, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, West Virginia, Wisconsin, Wyoming

Figures 4 and 5, showing trends in outcome for the treatment and control groups of Public Accommodations and Private Housing respectively, very closely resemble Figure 3. For all three law types outcomes for the treated group are more volatile, and the the evolution of the trend in percentage of positive attitudes between the late 40's and mid 50's is impossible to guess at due to missing data.

Figure 5: Comparison of Treatment and Control Trends: Private Housing



Treatment States: Alaska, California, Colorado, Connecticut, Massachusetts, Michigan, Minnesota, New Jersey, New York, Oregon, Pennsylvania

Control States: Alabama, Arizona, Arkansas, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming

The large amount of missing data for the outcome variable before 1954, including the five years (1949-1953) where there are no observations at all, are a cause for concern. The sparsity of the data pre 1954 and potential lack of overlap of pre and post treatment outcomes could introduce bias in a staggered DiD framework. Due to this fact, 1945-1953 will be excluded from the causal analysis, but included in all other settings.

5 Methodology

I begin by conducting two correlational analyses: the Spearman Correlation Test to see if there is a relationship between treatment adoption timing and racial attitudes and a Wilcoxon Rank-Sum Test to evaluate whether there is a significant difference in the distributions of the treatment and control groups. Then, as the intent of this study is to uncover whether there is a causal relationship between state-level anti-discrimination legislation and racial attitudes, I specify three Difference in Differences (DiD) models with staggered treatment timing. In staggered adoption settings, different units (states in this case) are first treated at different times, rather than simultaneously. Unlike a traditional DiD approach with a single treatment period, a staggered DiD framework allows for the estimation of treatment effects while accounting for variations in treatment timing. This approach is crucial for this study because anti-discrimination laws were implemented with enforcement in different years across states, and failing to account for this variation would lead to biased estimates. My first causal model is a standard staggered DiD approach with provisions for heterogeneous treatment effects. Next, I use a staggered DiD method with an efficient estimator. I finish with a robustness check of the efficient estimator model by repeating the analysis with a different control group.

Now to take a more in-depth look at these methods. I use Spearman correlation to statistically test the direction and strength of the relationship between adoption timing for each type of anti-discrimination law and positive racial attitudes. I choose this ranked test instead of a standard Pearson correlation test because it is robust to differences in scale and non-linear relationships.

Law adoption time is calculated by subtracting the year that an observation received treatment from the year of the observation. Never treated states are assigned a placeholder value of $year + 50$. Observations for both law adoption timing and racial attitude percentages are then standardized to a comparable scale [0, 1] using rank normalization. Early adoption timing for each kind of law is indicated by values near zero and positive racial attitudes are noted by values near 1.

Spearman correlation is computed by applying the Pearson correlation formula to the ranked

variables. Given two variables X (ranked law adoption timing) and Y (ranked racial attitudes), the Spearman correlation coefficient is calculated as:

$$\rho = 1 - \frac{6 \sum d_i^2}{n(n^2 - 1)} \tag{1}$$

where $d_i = \text{rank}(X_i) - \text{rank}(Y_i)$ is the difference between the ranks of each paired observation and n is the number of observations.

To test the statistical significance of ρ, I compute the t-statistic:

$$t = \rho \sqrt{\frac{n-2}{1-\rho^2}} \tag{2}$$

Under the null hypothesis of no association ($H_0 : \rho = 0$), this test statistic follows a Student's t-distribution with $n - 2$ degrees of freedom. The value of ρ ranges from -1 to 1. In this case, since an X closer to 0 indicates early adoption, and a Y closer to 1 indicates high racial attitudes, a negative t-statistic will indicate that earlier adoption is associated with higher racial attitudes.

Next, to assess whether there is a significant difference in positive racial attitudes between the treated and never-treated states, I employ the Wilcoxon rank-sum test. This is an alternative to the t-test designed for non-normal distributions. This test, equivalent to the Mann-Whitney U test, ranks all observations from both groups together and evaluates whether the ranks of one group tend to be systematically higher or lower than those of the other.

Let $X_1, X_2, ..., X_m$ be the racial attitude scores of the never-treated group and $Y_1, Y_2, ..., Y_n$ be those of the treated group. The test statistic W is computed as the sum of ranks assigned to observations from one of the two groups:

$$W = \sum_{i=1}^{m} R(X_i)$$

where $R(X_i)$ represents the rank of observation X_i in the pooled sample of size $m + n$.

A significant p-value ($p < 0.05$) indicates that there is a noteworthy difference in the distributions of racial attitudes between the treated and never-treated groups.

The first of the causal frameworks is the standard Difference-in-Differences (DiD) approach with provisions for heterogeneous treatment effects. I build my first three models using the Callaway & Sant'Anna (2021) DiD package in R, which allows for dynamic treatment effects over time. This approach estimates the group-time average treatment effect (ATT) for each cohort of treated states at each point in time and does not impose homogeneous treatment effect assumptions. In addition, I compare treated states to never-treated states in order to properly estimate the causal effects of these policies. This allows for the construction of a valid counterfactual for each treated unit and strengthens causal identification.

The model is fully specified under these assumptions:

1. No Treatment in the First Period and Irreversibility of Treatment
2. Random Sampling
3. No Anticipation Effects Beyond a Known Horizon
4. Conditional or Unconditional Parallel Trends
5. Overlap Condition

The majority of these assumptions are met in this study. Once a state is treated, it remains treated. States treated in the first period are excluded from the analysis. The outcome variable is constructed from survey responses which have been re-weighted to better reflect population characteristics. Anticipation effects are unknown in this experiment, so failure to meet this assumption may introduce bias into estimates. However, I posit that since interracial interaction has been acknowledged to influence racial attitudes, the year that the law goes into effect is more relevant than any anticipation of the law in this case. I use a never-treated group as the control without conditioning on covariates, which simplifies to the unconditional parallel trends assumption:

$$\mathbb{E}[Y_t(0) - Y_{t-1}(0) \mid G_g = 1] = \mathbb{E}[Y_t(0) - Y_{t-1}(0) \mid G_g = 0] \tag{3}$$

A test of parallel trends assumption is conducted for each model. Finally, only years where treatment went into effect were used to calculate the ATT.

To formally estimate the effect of the three types of anti-discrimination laws on positive racial attitudes, I specify the following staggered DiD models:

Model 1: Fair Employment Laws

$$ATT_{g,t}^{FE} = \mathbb{E}[Y_t(g) - Y_t(0) \mid G_g = 1] \tag{4}$$

Model 2: Public Accommodations Laws

$$ATT_{g,t}^{PA} = \mathbb{E}[Y_t(g) - Y_t(0) \mid G_g = 1] \tag{5}$$

Model 3: Private Housing Laws

$$ATT_{g,t}^{PH} = \mathbb{E}[Y_t(g) - Y_t(0) \mid G_g = 1] \tag{6}$$

where: $ATT_{g,t}$ is the ATT for a given policy, group g, and time t, $Y_t(g)$ is the observed outcome for a state in cohort g at time t, $Y_t(0)$ is the counterfactual outcome for the same state had it not received the treatment, and $G_g = 1$ indicates that this state belongs to a treatment cohort.

Each model estimates the group-time average treatment effects using the $did :: att_g t$ function, where the control group is the never treated states, there are zero known anticipation periods, and the estimation method is doubly robust. Then these group-time effects are aggregated to obtain a single ATT estimate for each group-time, and an overall ATT estimate for the model. Event study plots are then generated to visualize the treatment effects and validate the parallel trends assumption.

Next, the three models are constructed again, now using the Roth and Sant'Anna (2023) efficient estimator, which adjusts the basic DiD model $\hat{\theta}_\beta$ for differences in pre-treatment outcomes using the $\overline{X}$ pre-treatment adjustment term and the $\hat{\beta}^*$ coefficient to minimize variance. In this study

I use the default scalar $\hat{X}$ such that $\beta = 1$ and the control group is formed from the not-yet and never-treated states.

This model relies on two key assumptions:

1. Timing of treatment is quasi-randomly assigned

2. The timing of treatment does not affect pre-treatment outcomes.

I acknowledge that one or both of the assumptions of this model may be violated by my data, which could include potential anticipation effects and where more liberal states tended to adopt treatment sooner. Because of this, the results of this model will be used to give a broad idea of the underlying relationships in the data, without assuming unbiasedness.

For each of the three categories of anti-discrimination law (Fair Employment, Public Accommodations, and Private Housing), I estimate a simple treatment effect across all cohorts and time periods:

$$\hat{\tau}_{tg}^{Eff} = (\hat{\tau}_{t,g_\infty} - \hat{\tau}_{g-1,g_\infty}) - (\hat{\tau}_{g-1,g\infty}), \tag{7}$$

where $(\hat{\tau}_{g-1,g\infty})$ is the pre-treatment difference in means and $\hat{\tau}_{t,g_\infty}$ is the difference between the treated and control groups at time t.

and an event study:

$$\hat{\tau}_{lg}^{Eff} = (\overline{Y}_{g+l,g} - \overline{Y}_{g+l,\infty}) - (\overline{Y}_{g-1,g} - \overline{Y}_{g-1,\infty}), \tag{8}$$

where $l = t - g$ = event time, $\overline{Y}_{g+l,g}$ is the mean outcome at event time l for cohort treated at g, $\overline{Y}_{g+l,\infty}$ is the mean outcome of the never-treated group at event time l, and $\overline{Y}_{g-1,g} - \overline{Y}_{g-1,\infty}$ is the pre-treatment differences at time $g - 1$.

Finally, I consider that there may be systematic differences between the treated and never-treated

states and thus the latter states may not serve as a good counterfactual. To implement this, I specify the efficient estimator models once again, this time only using not-yet treated units as controls.

6 Results

6.1 Correlation Analysis

The results of the Spearman Correlation Test are shown in Table 3. The p-values are significant for all three policy types and though the correlation estimates are quite small, they still reveal that states who adopted treatment earlier are associated with higher degrees of racial tolerance. This correlation is largest for Fair Employment laws, but only by a small degree.

Table 3: Correlation Results for Fair Employment, Public Accommodations, and Private Housing

Law	**Correlation Estimate**	**p-value**
Fair Employment	-0.335	3.59×10^{-19}
Public Accommodations	-0.257	1.05×10^{-11}
Private Housing	-0.298	2.41×10^{-15}

As this is only a correlational exercise, it is still unclear whether higher percentages of positive racial attitudes are really a result of the timing of the adoption of laws or due to inherent differences between the treatment and control groups.

Next, the Wilcoxon Rank Test is conducted and its findings suggest that the distribution of racial attitudes is significantly different between treated and never-treated groups. More specifically, the treatment group is associated with higher percentages of positive racial attitudes. The results from this test can be seen in Table 4.

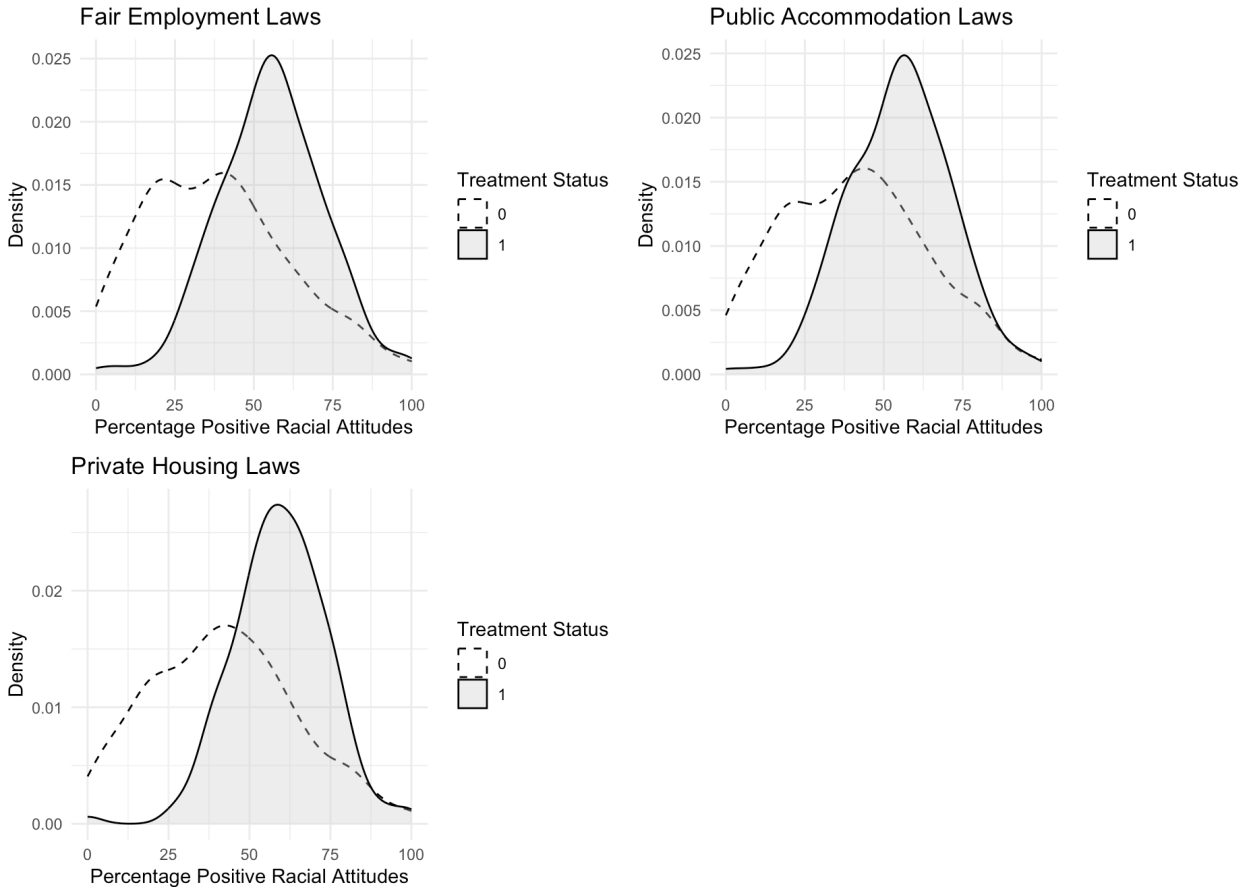
Table 4: Results from the Wilcoxon Rank-Sum Test

Policy Type	**Test Statistic (W)**	**p-value**
Fair Employment	29,918	$< 2.2 \times 10^{-16}$
Public Accommodations	30,699	8.437×10^{-16}
Private Housing	19,888	$< 2.2 \times 10^{-16}$

To visualize this difference, I include plots of the the distributions of the treated and never-treated groups for each law type in Figure 6. In each case, the never-treated group exhibits a peak

at lower racial attitude percentages, suggesting that many of these states had generally less positive racial attitudes. In contrast, treated states tend to have higher racial attitudes, with their distribution more concentrated at higher values. This pattern aligns with the Wilcoxon rank-sum test results, confirming that the difference between the two groups is statistically significant.

Figure 6: Density Plot of Racial Attitudes by Treatment Status



However, since these two tests are purely correlational, no causal conclusions can be made. Policy adoption may not be the sole driver of racial attitude changes, as some never-treated states still exhibit mid-range and high attitudes. This suggests that factors beyond policy adoption—such as regional or cultural influences—could have played a role in shaping racial attitudes, independent of whether or when a state adopted Fair Employment laws.

6.2 Staggered DiD

Moving on to causal analysis, I begin with a staggered DiD model that does not assume homogeneous treatment effects. The p-values of the parallel trends assumption for each model are reported in Table 5. All of the p-values are greater than 0.05, which indicates that there is not a statistically significant difference in outcomes pre-treatment.

Table 5: Staggered DiD: P-values for Parallel Trends Test

Fair Employment	Public Accommodations	Private Housing
0.650	0.8289	0.685

Since the parallel trends assumption has been met, I continue with my analysis. The aggregated treatment effect for Fair Employment laws is reported in Table 6. The ATT is estimated as -2.0564, with a standard error of 5.0061. The large standard error and range of the 95% confidence interval indicate a high level of uncertainty in this estimate. In addition, because the confidence interval covers zero, I conclude that this estimate is not statistically significant.

Table 6: Staggered DiD: Aggregated Effect of Fair Employment Laws

ATT	Std. Error	95% Conf. Int. (Lower)	95% Conf. Int. (Upper)
-2.0564	5.0061	-11.8683	7.7554

The event study plot (Figure 7) and estimates (Table 7) do not provide much further clarity, as all the confidence intervals are wide and cover zero. Though the estimates fluctuate over time, indicating some variability in the effect of treatment, due to their statistical insignificance, none are very informative. This reflects the high variance in the data, which is likely obscuring the true effect of the policy on racial attitudes.

Figure 7: Staggered DiD: Effect of Fair Employment Laws Over Time

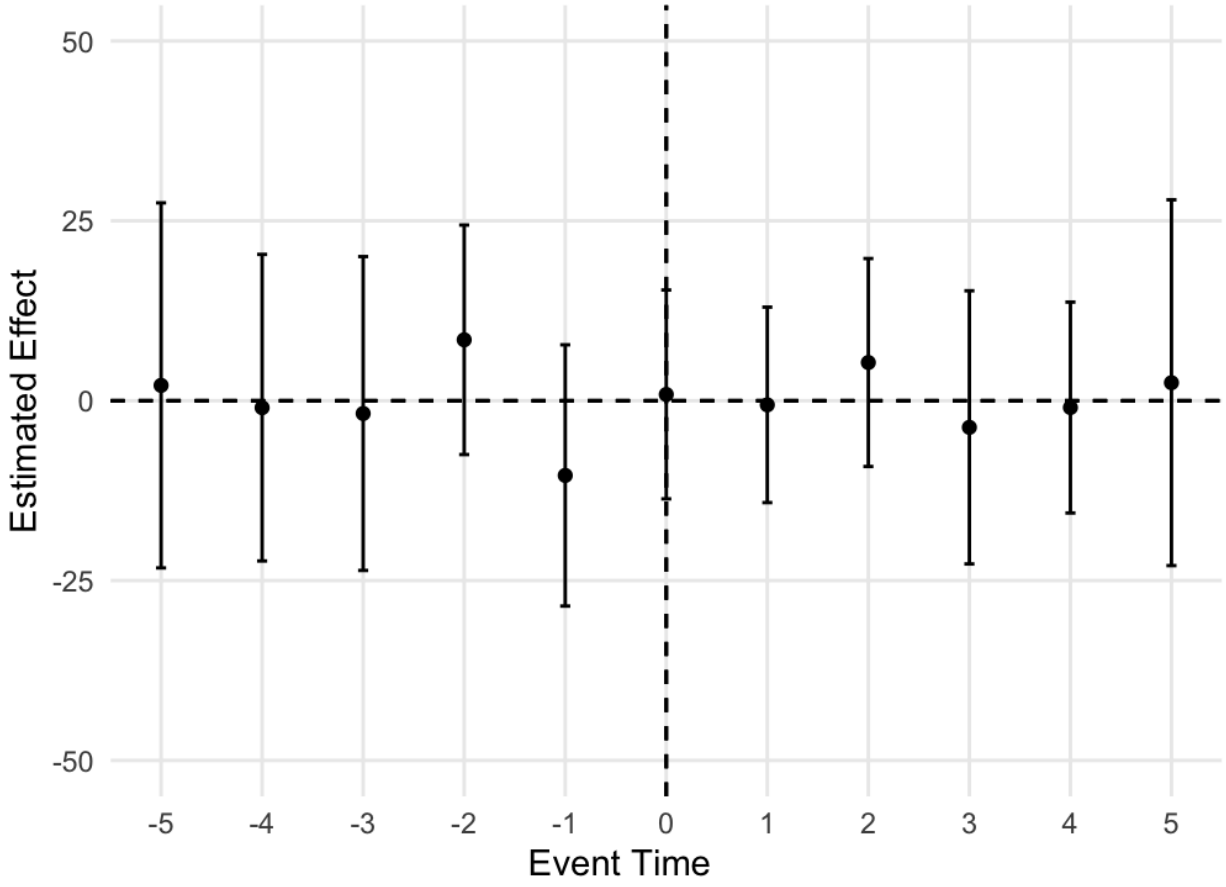


Table 7: Staggered DiD: Estimates of Effect of Fair Employment Laws Over Time

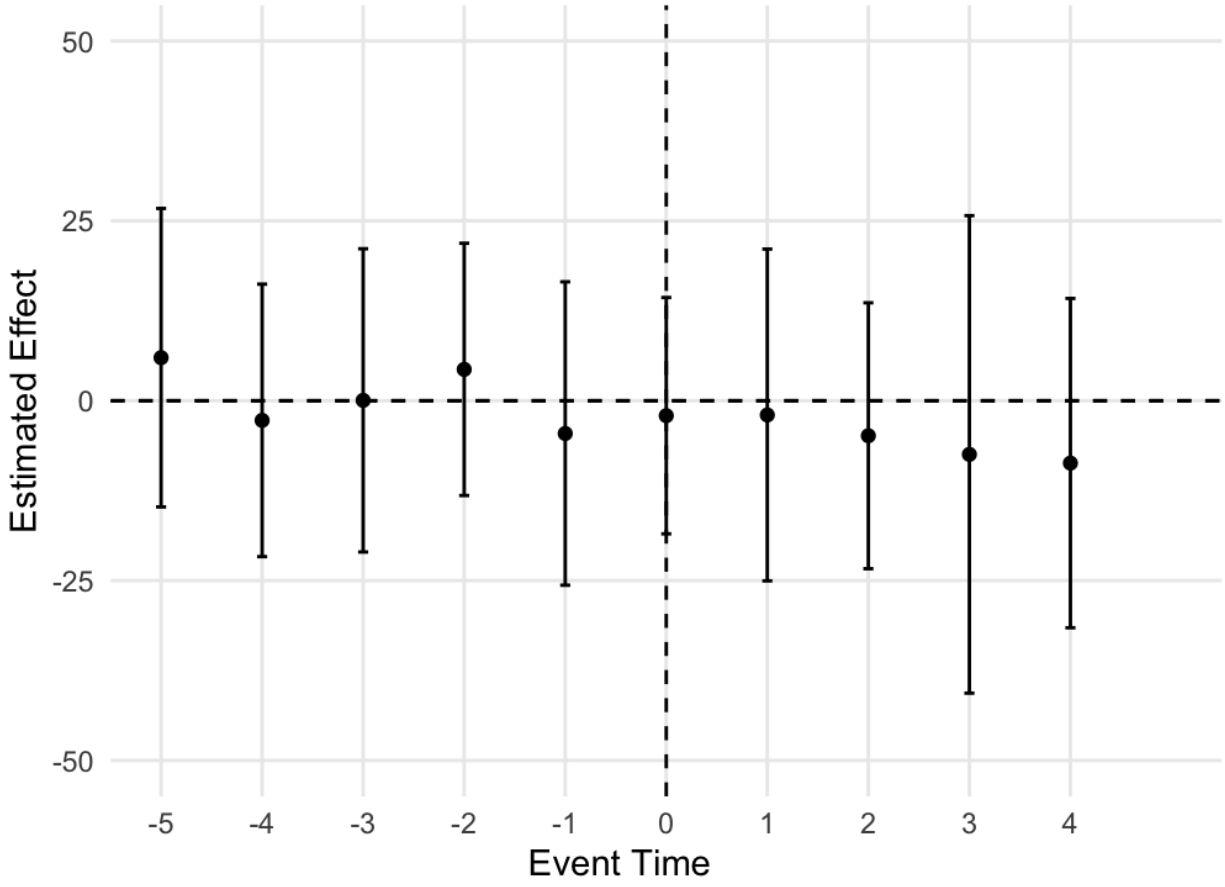
Event Time	Estimate	Std. Error	95% Simult. Conf. Band (Lower)	95% Simult. Conf. Band (Upper)
-5	2.1308	8.2728	-20.8770	25.1387
-4	-0.9688	7.1289	-20.7953	18.8578
-3	-1.7787	7.1540	-21.6749	18.1174
-2	8.4647	6.0473	-8.3537	25.2831
-1	-10.3870	6.5317	-28.5524	7.7785
0	0.8710	4.9966	-13.0253	14.7673
1	-0.5778	4.6499	-13.5098	12.3543
2	5.3088	5.0526	-8.7431	19.3606
3	-3.7054	6.5809	-22.0077	14.5969
4	-0.9489	5.0988	-15.1294	13.2316
5	2.5021	8.9688	-22.4414	27.4455

The estimates for Public Accommodations follow much the same pattern. The aggregated ATT (Table 8) for Public Accommodations laws is estimated at -2.2042, very close to the Fair Employment estimate. However, with a standard error of 8.9747, the true value of the estimate is still very uncertain. In Figure 8 there does seem to be a slight downward trend in estimates post treatment, but with the error bars as wide as they are, this is by no means confirmed.

Table 8: Staggered DiD: Aggregated Effect of Public Accommodations Laws

ATT	Std. Error	95% Conf. Int. (Lower)	95% Conf. Int. (Upper)
-2.2042	8.9747	-19.7943	15.3858

Figure 8: Staggered DiD: Effect of Public Accommodations Laws Over Time



Finally, the results for Private Housing laws are reported in Table 9 and Figure 9. The aggregated ATT, estimated at 0.6933, is actually positive here, which is a divergence from the previous two laws I have examined. However, as with the past two models, the standard error is much too high to confidently attribute the adoption of the law as the reason for an increase in attitudes.

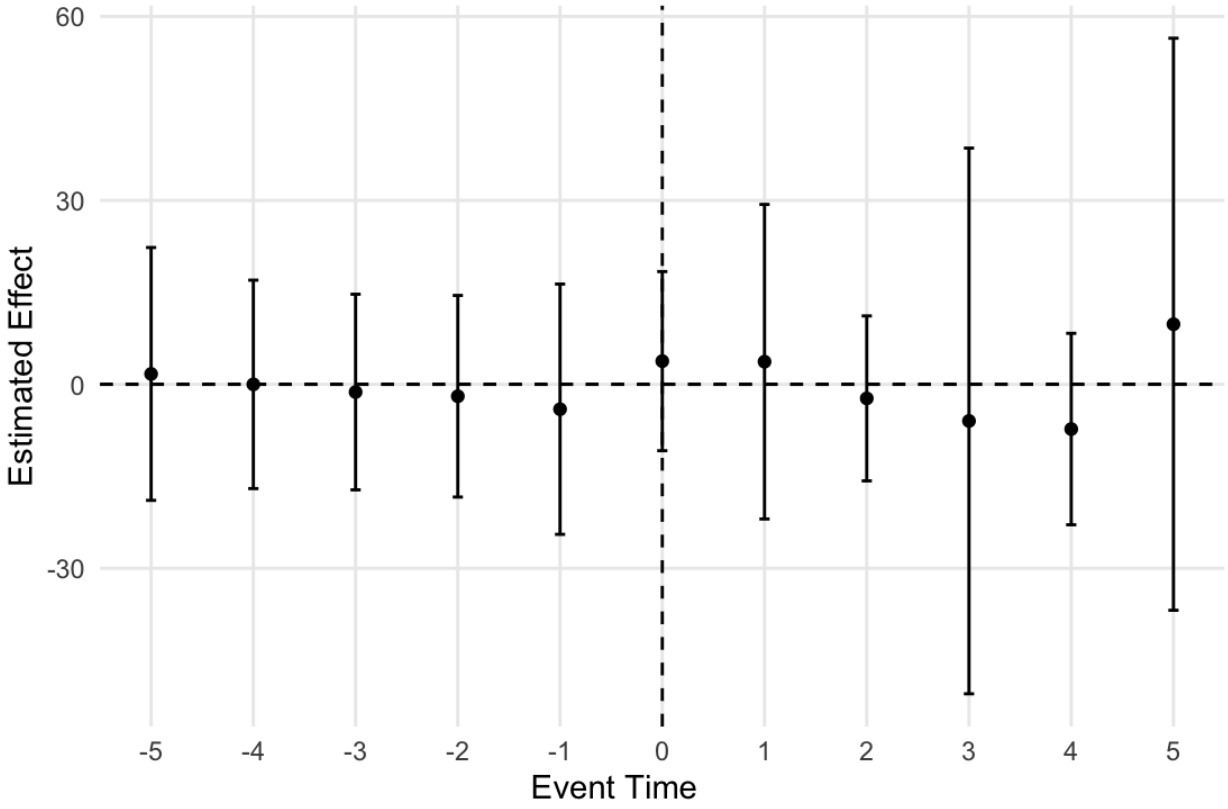
Table 9: Staggered DiD: Aggregated Effect of Private Housing Laws

ATT	Std. Error	95% Conf. Int. (Lower)	95% Conf. Int. (Upper)
0.6933	5.4615	-10.011	11.3977

The event study plot suggests some positive post-treatment estimates, but the confidence bands are too wide to draw definitive conclusions. See Appendix A for event study estimates for Public

Accommodations and Private Housing laws.

Figure 9: Staggered DiD: Effect of Private Housing Laws Over Time



6.3 Staggered DiD with the Efficient Estimator and Never-Treated States as Control Group

Given the limitations of a standard staggered DiD framework, I now move on to the Roth & Sant'Anna (2021) efficient estimator. Differences between the previous estimates and those using the efficient estimator immediately become clear. For one, the standard error is much smaller for all models, indicating higher precision. In addition, the overall ATT estimates for all three models are significant at the 5% level. Nevertheless, the results of the Fisher permutation tests, which yield significant p-values for all three models, indicate that there is some evidence of pre-treatment differences in outcomes.

Table 10: Efficient Estimator: Aggregated Effect of Fair Employment Laws (Never-Treated)

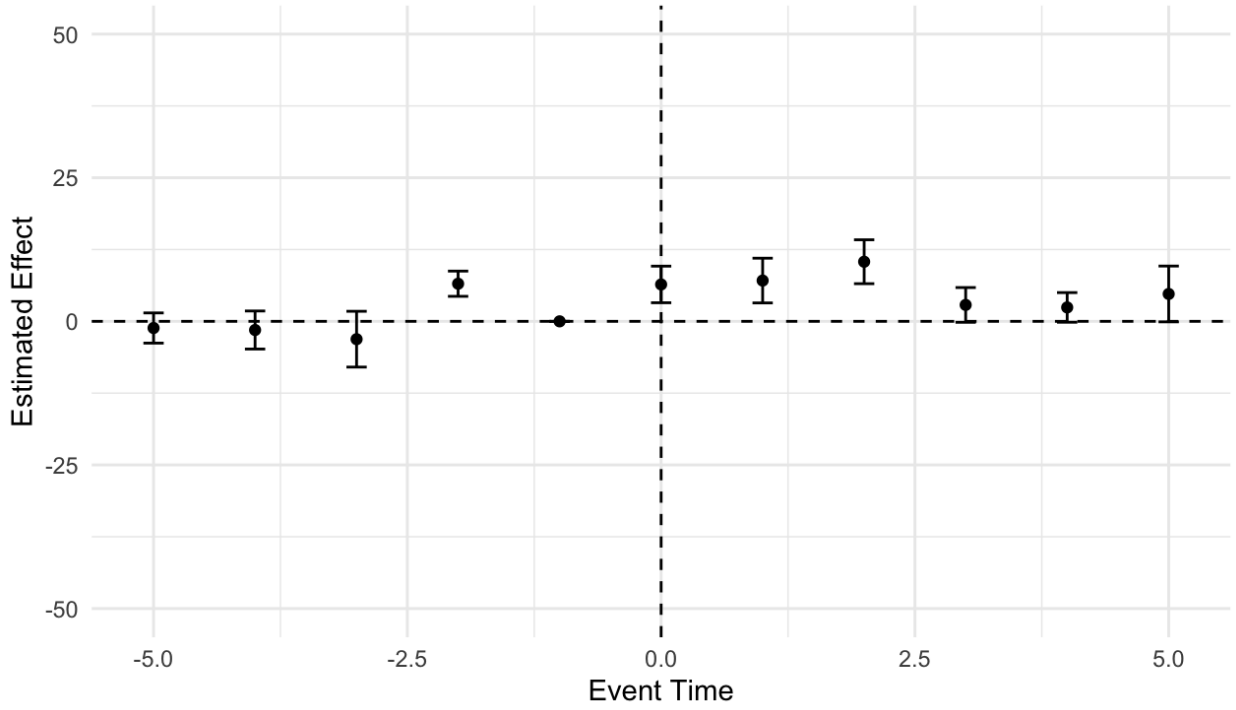
Estimate	Standard Error	Fisher Permutation Test p-value
9.584	2.255	0.012

In the case of Fair Employment laws, the overall ATT is 9.584 (2.255), as can be seen in Table 10. This suggests that the implementation of this anti-discrimination legislation with enforcement lead to an almost 10% increase in positive racial attitudes across the country. Regardless of magnitude, this positive estimate is consistent with Lockard's reasoning that in the case of desegregation in the workforce, daily interaction with Black workers aided in discrediting harmful stereotypes held by their white colleagues.

The event study plot[5] (Figure 10) provides insight into the relationship between the relative time of treatment and the treatment effect. Note that event study estimate tables for the efficient estimator can be found in Appendix B.

5. Note that in the Roth and Sant'Anna (2021) Staggered package, event time -1 is always set to zero in order to be used as a reference group

Figure 10: Efficient Estimator: Effect of Fair Employment Laws Over Time (Never-Treated)



This visualization does show a statistically significant increase in racial attitudes post-treatment, particularly at event times [0:2], with estimates of 6.421 (1.625), 7.098 (0.1.986), and 10.376 (0.949), respectively. This increase tapers off at event time 3, or three years post-treatment. I find the fact that a significant effect is visible so soon after treatment to be slightly surprising. I would have expected to see a slower shift in attitudes after the passage of the law. Interestingly, there do seem to be some anticipation effects, as seen at event time -2.

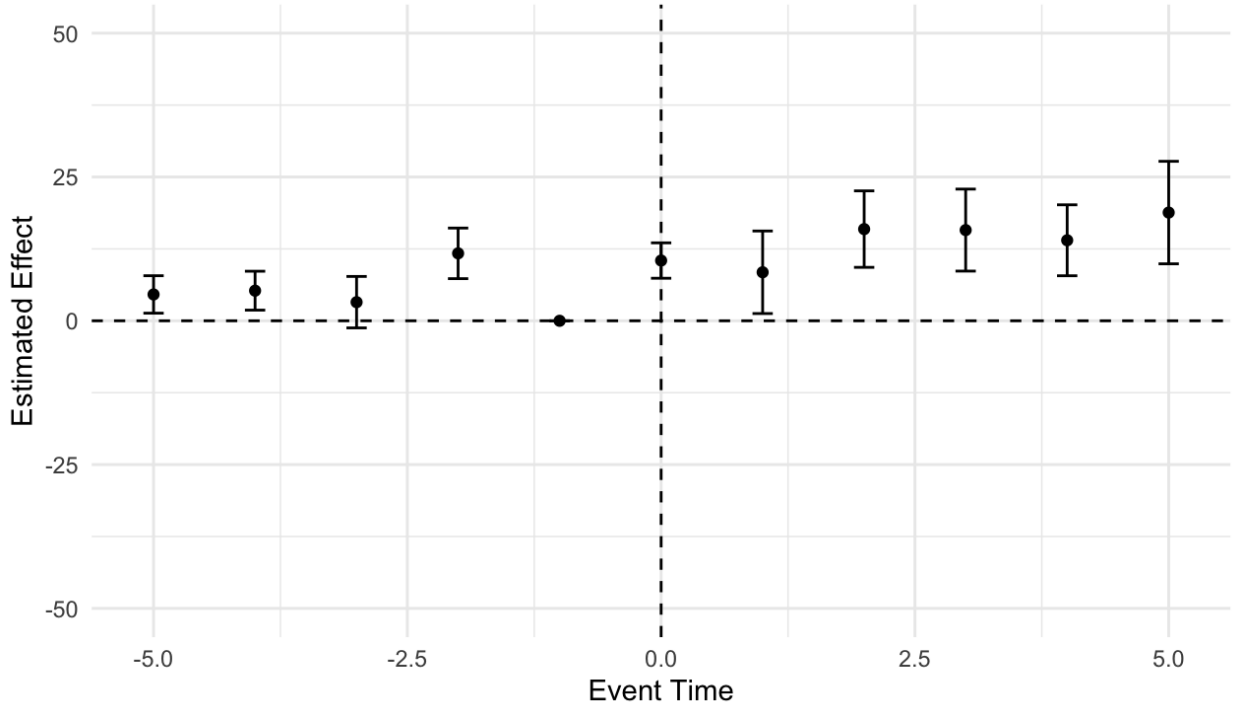
Turning to Public Accommodations laws, the aggregate estimate of 17.661 (3.263), seen in Table 11, is almost double that of Fair Employment. This larger effect is understandable, as desegregation in Public Accommodations meant that Black Americans were then able to utilize all manner of businesses and facilities, increasing social interaction between races.

Table 11: Efficient Estimator: Aggregated Effect of Public Accommodations Laws (Never-Treated)

Estimate	Standard Error	Fisher Permutation Test p-value
17.661	3.263	0.01

Looking first at post-treatment estimates, the effect of the policy appears to increase over time (Figure 11). Since treatment is when the law is passed with enforcement and it often takes several years for legislation like this to fully go into effect, it makes sense that the effect increased over time as more and more individuals are actually exposed to it.

Figure 11: Efficient Estimator: Effect of Public Accommodations Laws Over Time (Never-Treated)



Once again, though, these estimates must be interpreted with caution, because they are significantly different from zero at almost all event times. This means that the treatment and control groups were already experiencing different trends before the legislation was passed. Because of this, the increase in attitudes might not be solely attributable to the policy change, but instead driven by pre-existing trends.

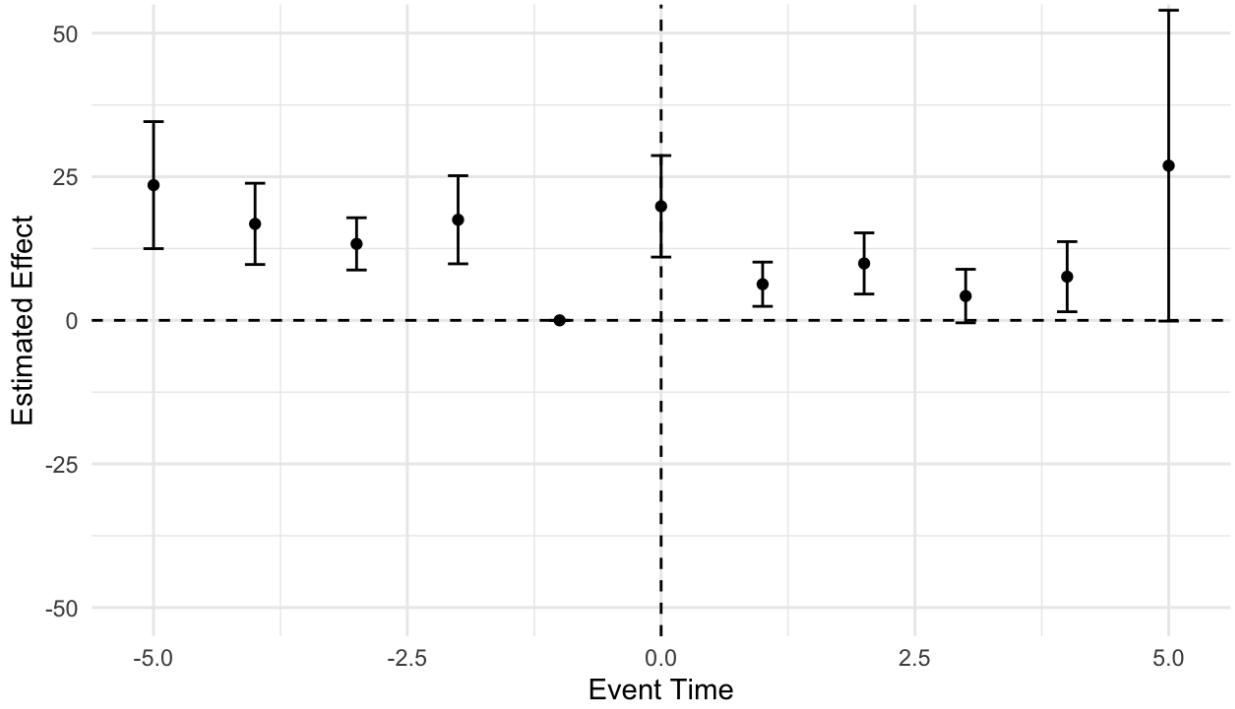
Looking finally at the aggregate treatment effect of Private Housing laws as seen in Table 12, the estimate is 13.553% (3.056), more moderate than that of Public Accommodations, but still greater than Fair Employment.

Table 12: Efficient Estimator: Aggregated Effect of Private Housing Laws (Never-Treated)

Estimate	Standard Error	Fisher Permutation Test p-value
13.552	3.056	0.016

Once again, as implied by the Fisher Permutation Test p-value, the presence of pre-trends is immediately obvious when looking at the event study plot (Figure 12). While the pre-treatment event study estimates for Public Accommodations were significant but at least close to zero, the same cannot be said for Private Housing. For this model, pre-treatment differences are quite large, as are some of their associated confidence intervals.

Figure 12: Efficient Estimator: Effect of Private Housing Laws Over Time (Never-Treated)



Vital context for this result is the fact that there are a very small number of treated states for Private Housing, only 11 in total, compared to the control group of 37. The estimates are thus based on fewer observations, making them more sensitive to outliers or shocks. Additionally, since the control group is much larger than the treated group, the weighted average treatment effect could

over-represent the control group. Since we know that the majority of the control group is made up of the Southern states, and the Southern states exhibit much lower racial attitudes on average, this is likely a factor in why the parallel trends assumption is not holding. The confidence interval for period 5 is particularly wide, reflecting the difficulty of estimating a long-run effect with a small sample.

6.4 Staggered DiD with the Efficient Estimator and Not-Yet-Treated States as Control Group

Given the results of the Fisher permutation tests and the fact that the majority of the never-treated states are the Southern states, I consider that regional differences may be contributing the difference in pre-treatment outcomes. Therefore, I conduct the same analysis but excluding the South and using not-yet-treated states as the control group. Event study estimates can be found in Appendix C. I find changing the control group results in the p-value for the Fisher permutation tests no longer indicating significant pre-trends, while the overall estimates shrink significantly and none are significant at the 5% level.

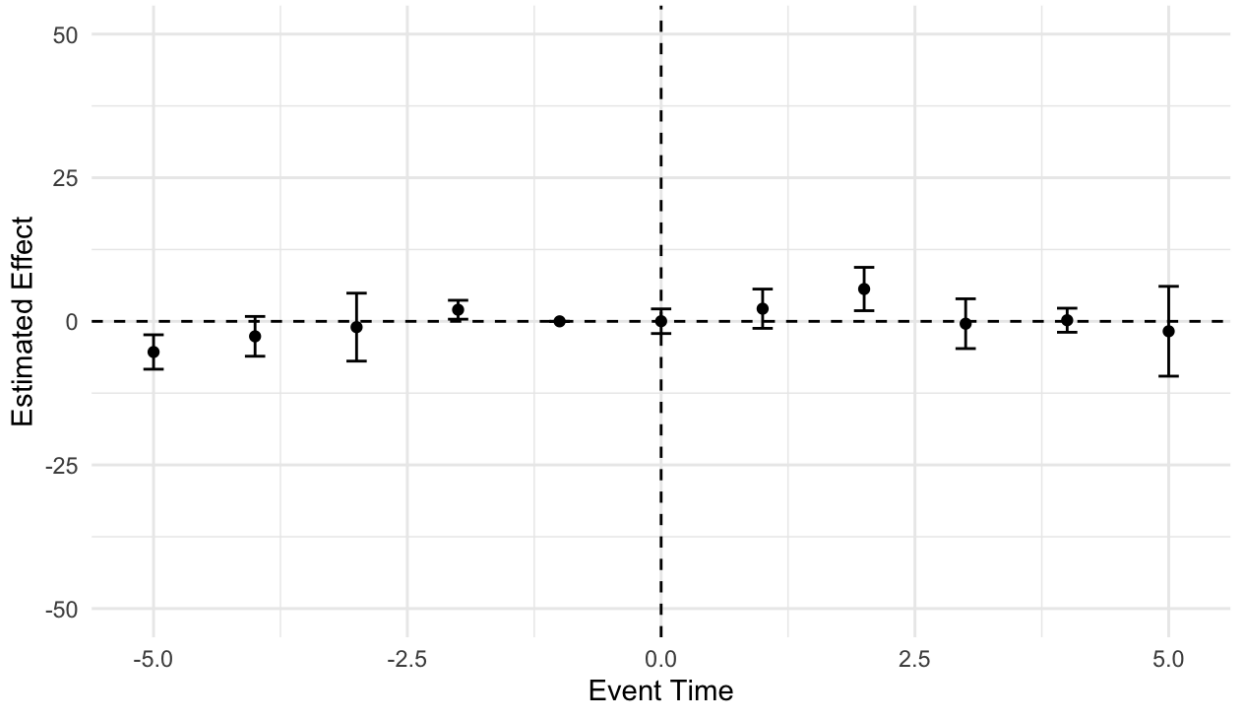
Table 13: Efficient Estimator: Aggregated Effect of Fair Employment Laws (Not-Yet-Treated)

Estimate	Standard Error	Fisher Permutation Test p-value
3.585	2.588	0.250

With the changed control group the overall treatment effect estimate for Fair Employment laws drops four percentage points to 3.585, and the standard error only slightly increases (Table 13). Though statistically insignificant, this estimate is more dependable and indicates that the large positive effects found by the efficient estimator models using the never-treated states were likely biased upwards.

The significant effect seen in period -5 of the event study (Figure 13) is likely attributable to statistical noise and is not necessarily a violation of parallel trends, especially since the Fisher Permutation test does not find any systematic deviations in the pre-period.

Figure 13: Efficient Estimator: Effect of Fair Employment Laws Over Time (Not-Yet-Treated)



Despite the fact that the aggregate treatment effect of treatment is insignificant, the event study shows that there is a significant estimate at period -2.

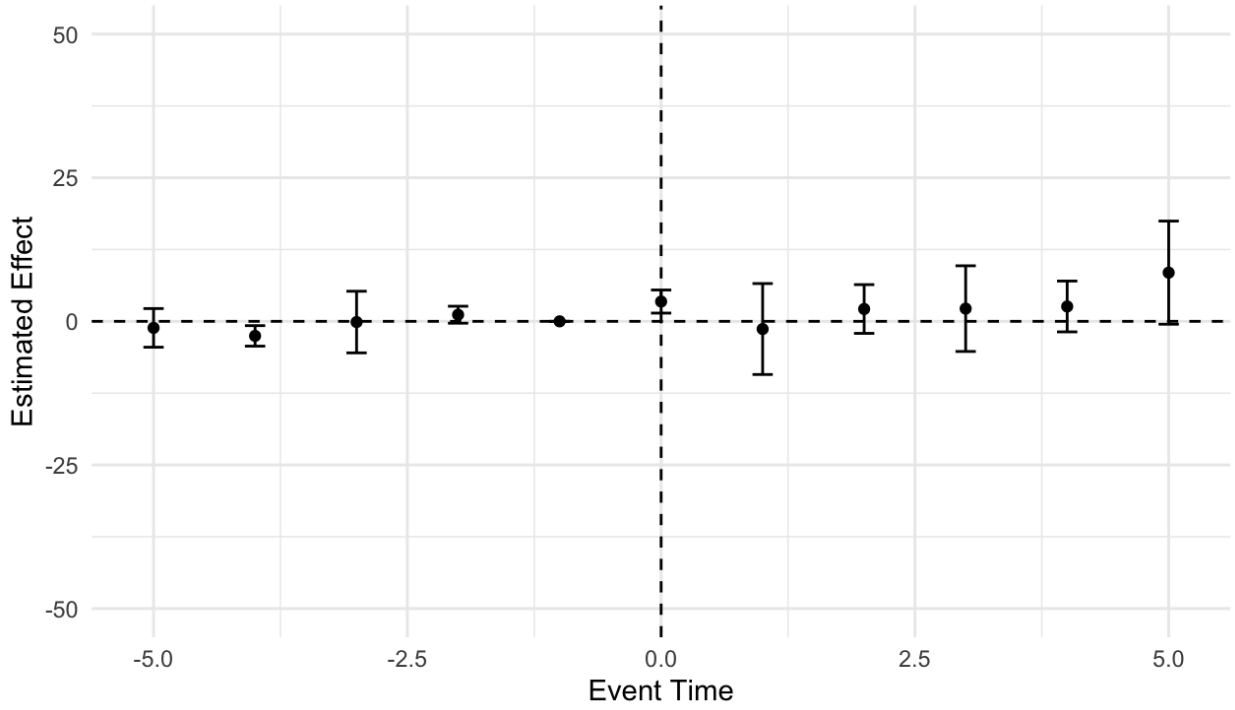
Public Accommodations follows much the same pattern. The aggregate treatment effect is small and insignificant, but also more reliable because the series meets the parallel trends assumption.

Table 14: Efficient Estimator: Aggregated Effect of Public Accommodations Laws (Not-Yet-Treated)

Estimate	Standard Error	Fisher Permutation Test p-value
4.646	2.811	0.170

In the event study shown in Figure 14, there is a significant effect of treatment at period 0. The rest of the estimates in the post period have much wider confidence intervals, meaning that the effect of treatment in later periods is unclear.

Figure 14: Efficient Estimator: Effect of Public Accommodations Laws Over Time (Not-Yet-Treated)



The overall estimate for Private Housing laws suggests a modest positive effect, but since it is not statistically significant no real conclusions can be made.

Table 15: Efficient Estimator: Aggregated Effect of Private Housing Laws (Not-Yet-Treated)

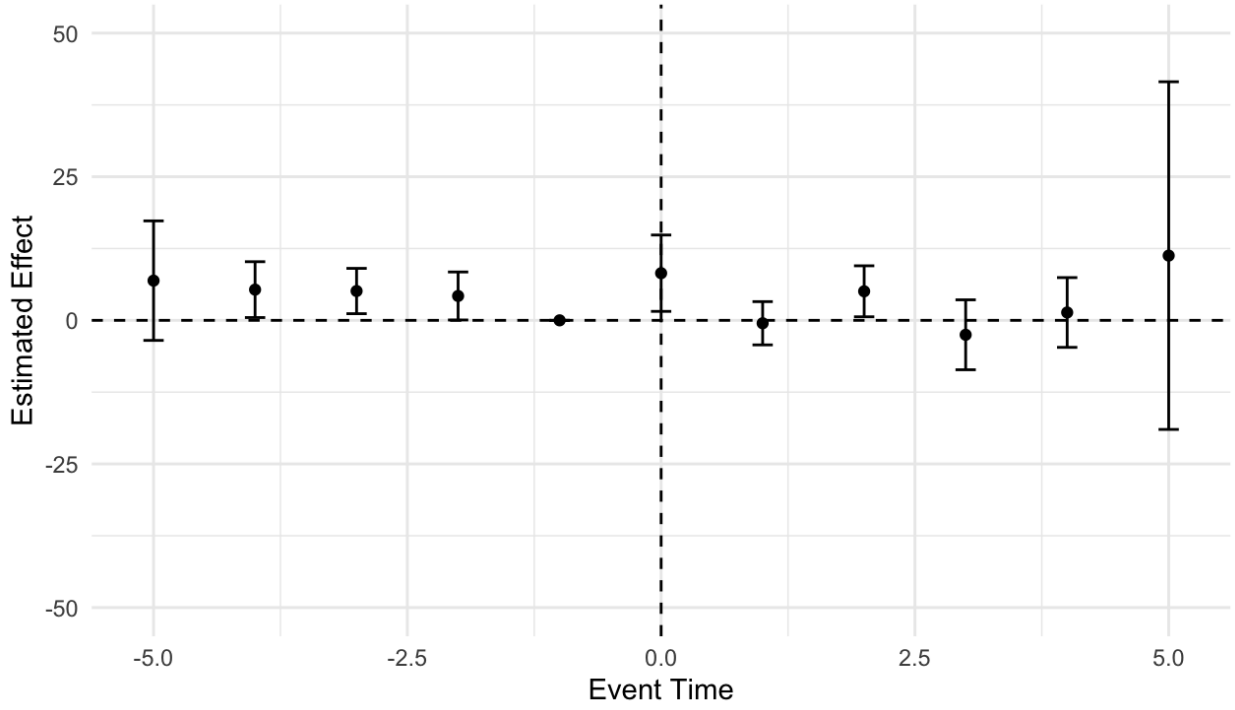
Estimate	Standard Error	Fisher Permutation Test p-value
3.943	2.978	0.490

Private Housing laws also report a significant treatment effect in period 0. However, both before and after treatment, the confidence intervals grow wider as the period gets farther from 0. This indicates that there may be less data points in the earlier and later periods, which makes estimates less reliable.

For Private Housing laws, the analysis indicates a significant treatment effect at period 0. However, as we move further away from the treatment period—both before and after—the confidence

intervals widen noticeably. This suggests that the precision and reliability of the estimates decreases in these more distant periods.

Figure 15: Efficient Estimator: Effect of Private Housing Laws Over Time (Not-Yet-Treated)



7 Discussion

The findings of this study provide insight into the relationship between anti-discrimination legislation and racial attitudes, though limitations presented by the underlying data prevent me from making any conclusions concerning causality.

The correlation tests do find that there is a statistically significant difference in racial attitudes between treated and non-treated states. In addition, earlier adoption is associated with higher racial attitudes. However, further study is needed in order to ascertain if these higher rates of positive racial attitudes are due to policy adoption rather than other social and economic trends.

The initial staggered DiD models failed to detect any significant effect of treatment, with high error for both the aggregate effect and event study estimates. These models also find no evidence of a violation of the parallel trends assumption, which is where an interesting discrepancy emerges between the two staggered DiD models which use never-treated states as controls. This is likely due to the fact that the standard staggered models use the Wald test to test for pre-trends, which assumes normally distributed error terms. The Fisher Permutation test (used by the efficient estimator models), however, is non-parametric, making it more robust to non-random treatment assignment. Because of this, I have more confidence in the finding of the Fisher test, and conclude that when the never-treated states are used to create the counterfactual, this creates pre-trend issues.

The initial efficient estimator models yielded more precise estimates for the overall treatment effects, which were statistically significant at 9.584 (2.255), 17.661 (3.263), and 13.552 (3.056) for Fair Employment, Public Accommodations, and Private Housing laws respectively. This implies that passing these different pieces of anti-discrimination legislation had large positive effects on the percentage of positive racial attitudes in a given state after adoption. However, even with the efficiency gains, concerns about selection bias and pre-treatment differences in trends persist. The imbalance in pre-treatment outcomes suggests that unobserved confounders may still be influencing the results, meaning that these estimates must be interpreted as biased at the very least.

Notably, the efficient estimator models using the not-yet-treated states as the control group

provide the most reliable results. Here, we still see efficiency gains, but this time the Fisher Permutation Test does not find evidence of a violation of the parallel trends assumption for any of the three types of anti-discrimination law. Unfortunately, due to a loss of statistical power by decreasing the sample size, all estimates are insignificant. However, these estimates of 3.585 (2.589) for Fair Employment, 4.646 (2.811) for Public Accommodations, and 3.943 (2.978) for Private Housing are also less biased and likely much closer to the true effect. Thus, the treatment effects seems to be either very small or non-existent.

One of the key limitations of this study is the non-random nature of policy adoption. While the staggered DiD framework accounts for variation in adoption timing, it assumes that treatment assignment is as good as random either unconditionally or after conditioning on covariates. However, the presence of significant pre-trends suggests that policy adoption may have been influenced by prior shifts in public opinion or other unobserved factors, thus biasing the estimated treatment effects.

Additionally, the problems of sample size significantly affect the precision of estimates. When the data is aggregated by state and year and filtered for positive racial attitudes post-1954, the number of observations shrinks considerably (496 observations, and only 320 when excluding Southern states). This raises concerns about the generalizability of the findings and the reliability of estimates in detecting true effects.

Another limitation arises from measurement error in racial attitudes. Public opinion data is inherently noisy, as self-reported attitudes may be biased by survey design such as selection bias, or by a respondent's desire to be perceived in a certain way. Moreover, changes in attitudes may not be solely attributable to policy shifts but rather to broader societal changes, including migration patterns, media influence, and activism. The Spearman correlation results suggest that policy adoption is associated with more positive racial attitudes, but this does not establish causation.

It is important to acknowledge, however, that the lack of strong evidence for the effectiveness of these laws in influencing racial attitudes does not necessarily mean that they were ineffective in practice. Legal enforcement still enforced behavioral changes, and this, in complement with

other societal factors, likely played a critical role in shaping racial attitudes over time. The stronger estimated effects in Southern states suggest that these laws may have had a greater impact where racial attitudes were initially more resistant to change, but it remains difficult to disentangle these effects from other regional dynamics.

There are several ways that I could address these limitations in order to conduct a more robust analysis. The first is by collecting additional data on racial attitudes from different sources and over a longer time period to mitigate the issues with small sample size. More granular data on local enforcement of anti-discrimination laws and when exactly each law took effect in each state could also provide a richer understanding of how implementation affected attitudes. Gathering data on potential covariates is also essential. Control variables, such as economic indicators, demographic shifts, and other measures of political ideology may help account for confounding factors and improve model specification. These control variables could include state-level characteristics such as urbanization, racial composition, and economic conditions.

It is also important to consider that the effects of policy could appear gradually, and as such, a longer time horizon could reveal patterns that are not immediately apparent in short-term analyses. Studying generational shifts in attitudes could also provide a more complete picture of the impact of anti-discrimination laws.

The results of this study highlight both the potential and the limitations of state-level Civil Rights era anti-discrimination legislation in shaping racial attitudes. While there is some evidence that these laws contributed to increased racial tolerance, the presence of pre-existing trends, sample size constraints, and measurement issues limit the strength of causal claims. Nevertheless, the findings underscore the importance of policy in shaping social norms and attitudes, particularly in regions with strong resistance to racial integration. Future research should continue to explore these dynamics using more robust methods and additional data to clarify the complex relationship between law and public opinion.

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Appendix A

Appendix A.1: Event Study Estimates: Public Accommodations

Event Time	Estimate	Std. Error	95% Simult. Conf. Band (Lower)	95% Simult. Conf. Band (Upper)
-5	5.9795	7.8410	-14.7914	26.7504
-4	-2.7342	6.5175	-19.9993	14.5308
-3	0.0467	7.2709	-19.2141	19.3075
-2	4.3535	6.1969	-12.0623	20.7693
-1	-4.5598	7.1232	-23.4293	14.3097
0	-2.0793	6.0843	-18.1969	14.0382
1	-1.9825	8.2173	-23.7503	19.7853
2	-4.8714	6.7598	-22.7782	13.0355
3	-7.4637	12.0646	-39.4230	24.4956
4	-8.6728	8.4510	-31.0598	13.7141

Appendix A.2: Event Study Estimates: Private Housing

Event Time	Estimate	Std. Error	95% Simult. Conf. Band (Lower)	95% Simult. Conf. Band (Upper)
-5	1.6976	7.4076	-18.9189	22.3140
-4	-0.0042	6.1066	-16.9997	16.9913
-3	-1.2548	5.7319	-17.2075	14.6980
-2	-1.9397	5.9079	-18.3822	14.5029
-1	-4.0542	7.3339	-24.4655	16.3570
0	3.7857	5.2484	-10.8215	18.3929
1	3.6927	9.2164	-21.9580	29.3434
2	-2.2932	4.8330	-15.7441	11.1577
3	-5.9687	15.9890	-50.4685	38.5311
4	-7.2916	5.6091	-22.9026	8.3193
5	9.8018	16.7612	-36.8472	56.4509

Appendix B

Appendix B.1: Event Study Results: Fair Employment (95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	-1.163	1.341	0.494	-3.791	1.465
-4	-1.510	1.690	0.492	-4.823	1.802
-3	-3.107	2.478	0.392	-7.963	1.750
-2	**6.549***	1.121	0.010	4.353	8.745
-1	0.000	0.000	NA	0.000	0.000
0	**6.421***	1.625	0.182	3.236	9.606
1	**7.098***	1.986	0.204	3.206	10.991
2	**10.376***	1.949	0.130	6.556	14.197
3	2.857	1.544	0.322	-0.169	5.883
4	2.422	1.317	0.326	-0.159	5.003
5	4.766	2.478	0.166	-0.091	9.623

Appendix B.2: Event Study Results: Public Accommodations (95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	**4.580***	1.661	0.184	1.325	7.834
-4	**5.235***	1.725	0.142	1.854	8.617
-3	3.236	2.281	0.412	-1.236	7.708
-2	**11.722***	2.244	0.076	7.325	16.119
-1	0.000	0.000	NA	0.000	0.000
0	**10.747***	1.570	0.054	7.395	13.548
1	**8.434***	3.660	0.132	1.260	15.609
2	**15.939***	3.392	0.012	9.291	22.586
3	**15.768***	3.637	0.038	8.640	22.897
4	**13.995***	3.147	0.040	7.828	20.162
5	**18.814***	4.546	0.038	9.905	27.725

Appendix B.3: Event Study Results: Private Housing (95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	**23.538***	5.642	0.004	12.481	34.596
-4	**16.792***	3.609	0.006	9.718	23.865
-3	**13.310***	2.321	0.000	8.761	17.859
-2	**17.508***	3.913	0.024	9.838	25.178
-1	0.000	0.000	NA	0.000	0.000
0	**19.848***	4.508	0.046	11.012	28.684
1	**6.287***	1.966	0.074	2.435	10.138
2	**9.900***	2.717	0.054	4.574	15.226
3	4.227	2.378	0.234	-0.434	8.888
4	**7.596***	3.114	0.106	1.493	13.699
5	26.924	13.804	0.282	-0.131	53.979

Appendix C

Appendix C.1: Event Study Results: Fair Employment (Not-Yet-Treated as Control, 95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	**-5.339***	1.528	0.042	-8.333	-2.344
-4	-2.612	1.767	0.262	-6.075	0.851
-3	-1.011	3.018	0.802	-6.925	4.903
-2	**2.018***	0.840	0.156	0.372	3.665
-1	0.000	0.000	NA	0.000	0.000
0	0.021	1.093	0.992	-2.121	2.163
1	2.198	1.743	0.552	-1.218	5.614
2	**5.626***	1.925	0.354	1.852	9.400
3	-0.410	2.291	0.918	-4.741	3.921
4	0.192	1.069	0.942	-1.903	2.285
5	-1.728	3.989	0.770	-9.546	6.091

Appendix C.2: Event Study Results: Public Accommodations (Not-Yet-Treated as Control, 95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	-1.140	1.716	0.648	-4.503	2.223
-4	**-2.533***	0.911	0.060	-4.318	-0.748
-3	-0.126	2.738	0.976	-5.490	5.240
-2	**1.152***	0.753	0.404	-0.324	2.628
-1	0.000	0.000	NA	0.000	0.000
0	**3.445***	1.026	0.134	1.435	5.456
1	-1.340	4.039	0.792	-9.257	6.578
2	2.149	2.164	0.450	-2.094	6.391
3	2.212	3.805	0.678	-5.245	9.669
4	2.589	2.257	0.382	-1.834	7.012
5	8.476	4.581	0.184	-0.504	17.455

Appendix C.3: Event Study Results: Private Housing (Not-Yet-Treated as Control, 95% Confidence Intervals)

Event Time	Estimate	SE	Fisher P-Value	Lower CI	Upper CI
-5	6.910	5.307	0.288	-3.493	17.310
-4	**5.345***	2.480	0.090	0.486	10.205
-3	**5.102***	2.013	0.058	1.157	9.047
-2	**4.239***	2.131	0.288	0.063	8.418
-1	0.000	0.000	NA	0.000	0.000
0	**8.206***	3.391	0.362	1.560	14.853
1	-0.510	1.921	0.892	-4.275	3.257
2	**5.054***	2.264	0.386	0.616	9.493
3	-2.517	3.106	0.804	-8.603	3.570
4	1.368	3.098	0.818	-4.705	7.440
5	11.269	15.440	0.798	-18.993	41.531

Appendix D

Appendix D.1: Percentage Positive Responses by Region

Region	Mean	Standard Deviation	Min	Max	Quartile 1	Median	Quartile 3	N
Midwest	48.9	23.2	0	100	31.7	50	65.5	580
Northeast	60.6	27.2	0	100	43.7	66.7	81.3	388
South	26.8	23.1	0	100	9.09	22.5	40.1	757
West	54.1	25.8	0	100	35.4	53.8	75	427
All	44.293	27.874	0	100	21.089	43.951	66.484	2152

Appendix D.2: State Anti-Discrimination Legislation, 1940-1964

Year	Fair Employment	Public Accommodations	Private Housing
1945	New York, New Jersey		
1946	Massachusetts		
1947	Connecticut		
1948			
1949	New Mexico, Oregon, Rhode Island, Washington	Connecticut, New Jersey	
1950			
1951			
1952		New York, Rhode Island	
1953		Massachusetts, Oregon	
1954			
1955	Michigan, Minnesota, Pennsylvania		
1956			
1957	Wisconsin, Colorado	Washington, Colorado	
1958			
1959	California, Ohio		Massachusetts, Connecticut, Colorado, Oregon
1960	Delaware		
1961	Illinois, Kansas, Missouri	Ohio, Pennsylvania	New Jersey, Minnesota, New York, Pennsylvania
1962			
1963	Alaska, Indiana, Hawaii	Alaska, Indiana, Kansas, Michigan	Alaska, California, Michigan
1964		Delaware, Maryland	

Appendix E

Appendix E.1: List of Gallup Polls Used in Study

Gallup Poll # 1945-0349

Gallup Poll # 1947-0400

Gallup Poll # 1947-0407

Gallup Poll # 1948-0414

Gallup Poll # 1948-0433

Gallup Poll # 1949-0439

Gallup Poll # 1949-0450

Gallup Poll # 1954-0531

Gallup Poll # 1954-0532

Gallup Poll # 1955-0541

Gallup Poll # 1955-0546

Gallup Poll # 1955-0556

Gallup Poll # 1955-0557

Gallup Poll # 1956-0576

Gallup Poll # 1957-0577

Gallup Poll # 1957-0586

Gallup Poll # 1957-0589

Gallup Poll # 1958-0604

Gallup Poll # 1958-0605

Gallup Poll # 1959-0610

Gallup Poll # 1959-0611

Gallup Poll # 1960-0614

Gallup Poll # 1960-0622

Gallup Poll # 1961-0642

Gallup Poll # 1961-0646

Gallup Poll # 1961-0649

Gallup Poll # 1963-0667

Gallup Poll # 1963-0673

Gallup Poll # 1963-0674

Gallup Poll # 1963-0676

Gallup Poll # 1964-0633

Gallup Poll # 1964-0637

Gallup Poll # 1964-0683

Gallup Poll # 1965-0710

Gallup Poll # 1965-0711

Gallup Poll # 1965-0712

Gallup Poll # 1965-0714

Appendix E.2: List of Gallup Poll Questions Used in Study

1. If colored people came to live next door, would you move?
2. Would you move if colored people came to live in great numbers in your neighborhood?
3. Would you, yourself, have any objection to sending your children to a school where a few of the children are colored?
4. Would you, yourself, have any objection to sending your children to a school where half of the children are colored?
5. Would you, yourself, have any objection to sending your children to a school where more than half of the children are colored?
6. The U.S. Supreme Court has ruled that racial segregation in the public schools is illegal. This

means that all children, no matter what their race, must be allowed to go to the same schools. Do you approve or disapprove of this decision?

7. Would you object to having your children attend a school where the majority of pupils are Negroes?

8. If the federal government in Washington decides to give money to aid education, should this money go to all public schools, or should it be withheld from schools that fail to integrate white and Negro students?

9. How about communities in the South where white and colored children are separated? Should the government help these communities or refuse to help them build schools?

10. How would you feel about a law that would give all persons—Negro as well as white—the right to be served in public places such as hotels, restaurants, theaters, and similar establishments? Would you like to see Congress pass such a law, or not?

11. Do you think Negroes should or should not be required to occupy a separate part of a train or bus when traveling from one state to another?

12. The Supreme Court has also ruled that racial segregation on trains, buses, and in public waiting rooms must end. Do you approve or disapprove of this ruling?

13. How do you feel about (President) Truman's Civil Rights program? Do you think Congress should or should not pass the program as a whole?

14. Here is an interesting experiment. You notice that the boxes on this card go from the highest position of plus 5—for someone you like very much—all the way down to the lowest position of minus 5—for someone you dislike very much. Please tell me how far up the scale or how far down the scale you would rate the following men: Martin Luther King.

15. Do you favor or oppose a law in your state that would require employers to hire a person if he is qualified for the job regardless of his race or color?

16. Would you favor or oppose a state law that would require employees to work alongside persons of any race or color?

17. Do you favor or oppose a federal law (for all the states) that would require employers to hire a person if he is qualified for the job—regardless of his nationality, religion, race, or color?

18. As you know, a Civil Rights law was recently passed by Congress and signed by the President. In general, do you approve or disapprove of this law? (If disapprove, ask:) Well, do you disapprove because you think the Civil Rights law goes too far or not far enough?

19. If your party nominated a generally well-qualified man for president and if he happened to be a Negro, would you vote for him?

20. Would you, yourself, have any objections to living in a district where people of other religions, races, or colors also lived?

21. How far do you think the federal government should go in requiring employers to hire people without regard to their race, religion, color, or nationality?

22. Would you, yourself, be willing to work at your job alongside someone of a different religion, race, or color?