The War on Drugs in the Cotton Kingdom: Historical Coercion, Racial Animus, and the Enforcement of Drug Laws in the U.S. South*

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Abstract

This paper examines the historical origins of racial disparities in the enforcement of drug laws in the U.S. South. I test the hypothesis that the coercive labor practices of antebellum cotton plantations fostered punitive attitudes towards Black individuals that persisted across generations and shaped the local response to the War on Drugs. Combining county-level arrest records for 1974 to 2007 with data from the 1860 Census of Agriculture, harmonized to modern county boundaries, I find that the Black–White gap in drug-possession arrests widened by 25.8 per 100,000 residents more in former cotton counties after the campaign began. The results hold under a triple-difference design, an instrumental-variables strategy based on land suitability for cotton, and a within-state comparison. To identify the mechanism, I show that the response is confined to offenses in which officers exercise discretion, that it is organized by pre-1982 measures of anti-Black animus, and that a formal decomposition attributes it to inherited animus rather than to economic, policing, or geographic factors.

JEL codes: J15, K42, N31, N32, Z13.
Keywords: War on Drugs, racial disparity, cultural persistence, slavery, law enforcement, cotton.

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1 Introduction

In October 1982, the federal government of the United States announced the War on Drugs, a campaign with an ostensibly race-neutral premise. In practice, its enforcement fell disproportionately on Black communities. In the decade following its initiation, the drug-related arrest rate for Black individuals was at least four times higher than for white offenders (Neal and Rick (2016)), with Southern states like Alabama, Louisiana, and Mississippi showing some of the highest disparities (The Sentencing Project, 2018). The disparity, moreover, was far from uniform within the South: the widening of the Black–White arrest gap after 1982 varied considerably across Southern counties, and the counties in which it widened most share a common history that predates the campaign by more than a century. While racial disparities in law enforcement, along with other aspects of social life, are well-documented in the U.S. South, little is known about the root causes of these differences.

My interest is in explaining the origins of this variation. Specifically, I test the hypothesis, drawn from the historian Edward Baptist's account of labor conditions on antebellum cotton plantations, that the punitive attitudes formed under cotton slavery persisted across generations and resurfaced in drug-law enforcement more than a century later. Baptist argues that the rise of cotton as the dominant raw material of the nineteenth century incentivized planters to increase pressure on enslaved workers to boost productivity through a system of punishment and negative incentives, which he calls the pushing system. In this system, dynamic quotas were imposed on enslaved workers, forcing them to exert greater effort or face severe punishment. This system stood in sharp contrast to the task-based system prevalent in non-cotton regions of the South, where enslaved workers were assigned fixed daily tasks and additional effort was negotiated between workers and plantation owners.

The coercion that sustained the pushing system plausibly had consequences beyond the plantation economy itself. Repeated over decades, it fostered a punitive attitude towards Black individuals that became embedded in the culture of the counties in which cotton was grown, and socially learned attitudes of this kind tend to persist long after the circumstances that produced them have disappeared. When the War on Drugs increased the financial returns to discretionary drug arrests a century later, counties that had inherited such attitudes would be expected to respond more strongly with respect to Black offenders. The hypothesis therefore yields a clear geographic prediction: after 1982, the racial gap in drug arrests should have widened most in the counties with the greatest historical intensity of cotton production.

To formalize this argument, I develop a simple model of the persistence of punitive attitudes and law enforcement intensity based on historical cotton plantations in the region.

This model will provide a set of predictions regarding differential arrest rates in former cotton counties compared to non-cotton counties. I then combine the FBI's Uniform Crime Reporting (UCR), which has detailed records of arrests made in the U.S. at the county level from 1974 to 2020, with historical cotton output data from the U.S. Census of Agriculture of 1860 to test the predictions of the model using a triple difference model, pooled difference-in-differences model, and a fixed effects model with a continuous measure of cotton production intensity.

Consistent with the predictions of the model, I find a robust positive link between historical cotton plantation intensity and the arrest rate of Black individuals after the start of the War on Drugs. More specifically, I find that there exists a wider racial gap in the arrest rates of Black and White drug offenders in former cotton counties. Furthermore, I find that Black individuals were arrested at higher rates in former cotton counties compared to those in non-cotton counties. Finally, I find that the intensity of anti-drug law enforcement of Blacks is increasing in cotton production intensity in 1860.

Although these findings align with the historical narrative and support the model's predictions, they are also consistent with alternative interpretations. For instance, it is plausible that former cotton counties might have Black communities struggling with legitimate employment for reasons unrelated to racial animus and discrimination. To address these concerns, I control for observable characteristics such as per capita income, unemployment rate, education levels, and poverty at the county level, as well as geographic characteristics that determine cotton output (e.g., average precipitation, temperature, latitude, and longitude). I show that the results are robust to controlling for these factors.

To further mitigate concerns regarding unobserved confounders, I use land suitability for cotton plantations as an instrument for cotton plantation intensity in 1860. I construct the instrument directly from the source rasters of FAO's Global Agro-Ecological Zones (GAEZ) project, extracting county-mean attainable cotton yields by zonal statistics over modern county polygons; the resulting first stage is strong ($F = 44$). I show that if anything, the baseline results are biased downward, and the true effect of cotton plantation intensity is greater than initially estimated.

The final contribution of the paper is to identify the mechanism. Beyond confirming the model's reduced-form predictions, I subject the animus interpretation to a series of tests designed to distinguish it from rival explanations: the arrest response appears only where officers hold discretion and is absent from victim-anchored offenses; it is organized by a pre-1982 geography of anti-Black animus measured three independent ways (lynchings, 1960s Klan activity, and the 1968 Wallace vote); and a formal decomposition shows that observable economic, policing, and geographic channels jointly predict a smaller, not larger,

cotton-county response, while a direct animus index absorbs half of the effect and leaves the remainder statistically indistinguishable from zero.

Given the possibility that cross-county comparisons might be biased by state-level characteristics, I focus on cross-county comparisons within Arkansas, which, due to its geography, has a balanced mix of cotton and non-cotton counties. This micro-level analysis allows for within-state variation in arrest rates, confirming the initial results of the paper.

To further illustrate the persistence of regional racial animus long after the cotton boom, I document that Black school segregation over the past two decades is markedly higher in former cotton counties than elsewhere in the South, and that it shows no meaningful decline there. The historical expressions of the same animus, namely lynchings between 1882 and 1930, Klan chapters in the 1960s, and the segregationist vote of 1968, enter the analysis formally as the components of a county-level animus index used to identify the mechanism.

This paper provides a novel evaluation of the War on Drugs policy, contributing to the growing body of literature on its impact on the arrests and incarceration of drug offenders. Numerous studies have examined the broad consequences of this campaign on racial disparities in the criminal justice system. For example, Schoenfeld (2012) andRaphael and Stoll (2013) highlight how the War on Drugs disproportionately affected African Americans, particularly through the implementation of policies such as mandatory minimum sentencing laws, which led to a dramatic increase in incarceration rates. Schoenfeld (2012) further argues that these legislative changes exacerbated existing racial inequalities, disproportionately targeting Black communities.

Recent scholarship, such as Alexander (2020) and Cohen et al. (2022), explores how these policies reinforced racialized policing practices, with a particular focus on Black and Hispanic communities. Alexander (2020), building on her foundational work on mass incarceration, provides a critical analysis of how drug-related arrests became a tool for racial control, while Cohen et al. (2022) examine the socio-economic effects of these policies on marginalized communities. Additionally, Neal and Rick (2016) focus on the racial disparities in drug-related arrests, demonstrating how African Americans were disproportionately arrested compared to their white counterparts for similar drug offenses.

Cox and Cunningham (2021), in their study on the financing of the War on Drugs, specifically examine the role of the Byrne Memorial Justice Assistance Grant Program, which provided financial incentives to local law enforcement agencies to increase drug-related arrests. Their analysis shows how this federal funding mechanism led to a significant rise in arrests of African Americans, particularly in economically disadvantaged communities, where local police departments heavily relied on this financial support to boost their budgets. This focus on the financial incentives created by the Byrne Memorial grants highlights how the

War on Drugs intensified racial disparities, as law enforcement agencies in both the North and South responded to these incentives by prioritizing drug arrests in Black communities.

While many studies primarily address the abrupt surge in arrests and incarceration of African Americans following the War on Drugs, this paper is the first in the literature to examine the campaign's disparate effects on African Americans across various counties in the South. The implications of these findings could significantly influence policy decisions at both the federal and local levels. Recognizing the varying impacts of the policy in different counties may discourage the adoption of a uniform federal approach to law enforcement and other punitive social programs.

Additionally, this paper contributes to the growing body of research in economic history that explores the deep-rooted origins of cultural values and attitudes. Cook (2014) explores the impacts of race riots and lynchings from 1870 to 1940, revealing that these events reduced Black patenting activity by over 15% annually between 1882 and 1940. Williams (2022) provides county-level data on lynching events between 1882 and 1930 and their correlation with lower voter registration rates among Black populations. Acharya et al. (2016) examine how slavery's legacy continues to shape the political and racial attitudes of white individuals, particularly in counties that historically relied on slavery. These regions are more inclined to support the Republican Party today and oppose policies aimed at assisting Black communities, such as affirmative action. Logan (2019) documents a decline in tax revenues in counties where violence against Black politicians was prevalent between 1870 and 1880. Collins and Margo (2007) analyze the effects of race riots during the late 1960s, finding long-lasting negative effects on Black incomes, employment rates, and property values. Additionally, Williams (2021) identifies a connection between Confederate street names and poorer labor market outcomes for Black residents in the surrounding areas. Further research, particularly beyond the U.S., highlights the negative impacts of exposure to violence, coercion, and exploitation (Nunn and Wantchekon (2011); Bautista et al. (2015); Alsan and Wanamaker (2018); Archibong and Obikili (2020); Archibong and Annan (2021); Bautista et al. (2015).

The remainder of the paper is organized as follows. Section 2 describes the War on Drugs campaign and the historical background of cotton plantations. Section 3 develops the conceptual framework. Section 4 describes the data, the harmonization of 1860 county boundaries to modern ones, the construction of the land-suitability instrument, and the measurement of historical animus. Section 5 presents the empirical strategy and the main results, including sensitivity checks. Section 6 reports the within-Arkansas case study. Section 7 presents direct evidence on the persistence of racial animus in the form of contemporary school segregation patterns. Section 8 identifies the mechanism: structural tests of the arrest-cost

model, the offense-discretion ladder, the animus gradients, and a formal decomposition of the cotton-county arrest response. Section 9 concludes.

2 Policy Background and Historical Context

2.1 The War on Drugs Campaign

In October 1982, President Reagan officially declared the initiation of the War on Drugs, a comprehensive effort by the administration to address drug-related crime through direct and indirect measures. Directly, substantial increases in funding were allocated to key federal agencies, including the FBI and DEA, resulting in a rapid escalation of budgets for federal law enforcement agencies. In her book, Alexander (2020) outlines some relevant facts and figures. She notes that, between 1980 and 1984, FBI anti-drug funding surged from $8 million to $95 million, and Department of Defense antidrug allocations rose from $33 million in 1981 to $1,042 million in 1991. Concurrently, DEA antidrug spending grew from $86 million to $1,026 million (Alexander, 2020).

The indirect strategy involved financial incentives for state and local law enforcement agencies to prioritize drug-related crime. Two primary incentives emerged: cash grants and asset forfeiture policies. Cash grants, notably exemplified by the Edward Byrne Memorial State and Local Law Enforcement Assistance grants, were provided to jurisdictions demonstrating active involvement in the War on Drugs. The grants, mandated to be matched by states at 25 percent, facilitated a 3 to 1 federal contribution to local policing initiatives (Cox and Cunningham, 2021). During the 1987 fiscal year, Byrne grants received an appropriation of $178 million under block grants and $46 million for discretionary grants, with the program doubling in size by 1990, reaching a total allocation of $445 million. Subsequent years saw a slight increase, reaching a total of $500 million in 1995.

The second indirect incentive, "asset forfeiture" policies, introduced in the mid-1980s, empowered state and local law enforcement agencies to retain, for their use, the majority of cash and assets seized during drug war operations. This policy shift significantly vested state and local police in the War on Drugs. Between 1988 and 1992 alone, drug task forces seized over $1 billion in assets, excluding figures from DEA or other federal agency-funded task forces (Alexander, 2020). These actions as components of the War on Drugs campaign, as will be shown in subsequent sections, led to the rise of drug-related arrest rates for both Black and white offenders.

2.2 Historical Background

Industrial Revolution in the UK: Before 1760, cotton textiles were a negligible part of the English economy. In fact, they were so insignificant that Adam Smith did not mention them in his 1776 book, *The Wealth of Nations*. However, by 1850, the UK produced half of the world's cotton textiles output (Clark, 2014). By the mid-nineteenth century, 3.5 million people in the UK (roughly a quarter of the labor force) were employed by the country's cotton industry (Beckert, 2014). This rise in the significance of cotton textiles is a consequence of mechanical innovations in the spinning and weaving industries in the mid-18th century in the north of the country, making cotton textile products much cheaper than before (Clark, 2014). Therefore, the rise of the textile industry and the popularity of cotton products in the UK gradually increased demand in the global market for cotton between the mid-18th to mid-19th centuries.

Rise of the Cotton Kingdom: In 1790, the US South was in transition. Tobacco, its primary cash crop, had lost economic significance, making room for a more profitable crop. However, growing alternatives, such as rice and cotton, were limited in scale and initially confined to coastal areas (Boyer et al., 2013). The invention of the cotton gin in 1793 facilitated mass production of cotton, and the Louisiana Purchase of 1803 allowed the westward migration of planters and enslaved workers to lands more suitable for cotton plantations. The robust global demand for cotton, mainly from the UK, motivated many to purchase suitable land and enslave workers to produce cotton in the so-called "Cotton Kingdom" (see Figure 1). Cotton plantations became so profitable between 1800 and 1860 that it could "make poor men prosperous and rich men kings" (Boyer et al., 2013).

2.3 Cotton Output Growth and the Source of Productivity Gains

The increasing demand for cotton and the incentive to maximize profit meant that output had to grow over time. However, with a limited supply of land and enslaved labor, output growth could only come from productivity growth.

Unlike the textile industry in the UK during the Industrial Revolution, the US South saw minimal technological advancement during the cotton boom, and yet it experienced a staggering 400% increase in slave productivity from 1800 to 1860, which matched that of workers in Manchester cotton mills (Baptist, 2016)[1]. These productivity gains came at a se-

[1]Technological advancement, here, refers to one that would make enslaved labor more productive. Since the main task of enslaved blacks was to pick cotton, a relevant technological advancement would, for instance, be mechanical cotton picker which was not invented until late 1930s. Olmstead and Rhode (2008) argue that introduction of a new variant of cotton led to an increase in cotton picking and labor efficiency. This argument, however, is inadequate as the new cotton variant would only increase the potential for higher

vere cost: the relentless use of whippings and other punishments. As Baptist (Baptist, 2016) argues, enslaved Blacks had little incentive to work ever harder to meet market demands. To address this, plantation owners implemented a system known as the"pushing system," which stood in stark contrast to the earlier "task system" that allowed enslaved workers some leisure time after completing their assigned tasks[2].

Under the pushing system, white planters set daily quotas for enslaved cotton pickers. Overseers, armed with lamps, scales, and whips, would appear at the end of each day to weigh each worker's basket. Those failing to meet the quotas faced severe punishment. These quotas, moreover, were not static; they increased year after year in response to rising cotton demand (Baptist, 2016). As Baptist describes further: "What plantation owners used to enhance productivity was a system of measurement and negative incentives...daily quotas are set for slaves... A white man (usually on a horse) acts as an overseer monitoring work...and whips the ones that shirk[3]. At the end of the day...each basket of cotton is submitted and then carefully weighed. The slave with a submitted basket below the set quota is whipped for falling short" (Baptist, 2016). In effect, productivity rose because cotton picking increased, and cotton picking increased because quotas rose (see Figure 2). However, this growth came at the expense of harsher punishments and increased whippings (Baptist, 2016)[4].

labor productivity. The enslaved labor still must, somehow, be convinced to work harder to actualize the potential.

[2]Fogel and Engerman (1977) point to the higher (enslaved) labor productivity in the south compared to the (free) North. They argue that this seemingly puzzling superiority of the southern labor, probably, has to do with more intensive employment of labor in the southern farms with more extended hours of work.

[3]This was characteristic of cotton plantation to the extent that plantation owners were called "lords of the lash" Beckert (2014).

[4]Beckert (2014) quotes two formerly enslaved African Americans, illustrating the brutality of work in the cotton fields: "John Brown, a fugitive slave, remembered in 1854 how he was "flogged...with the cow-hide," and how overseers "hunt[ed] 'stray niggers.' " "When the price [of cotton] rises in the English market," he remembered, "the poor slaves immediately feel the effects, for they are harder driven, and the whip is kept more constantly going." Henry Bibb, another slave, remembered the fearful violence: "At the sound of the overseer's horn, all the slaves came forward and witnessed my punishment. My clothing was stripped off and I was compelled to lie down on the ground with my face to the earth. Four stakes were driven in the ground, to which my hands and feet were tied. Then the overseer stood over me with the lash."

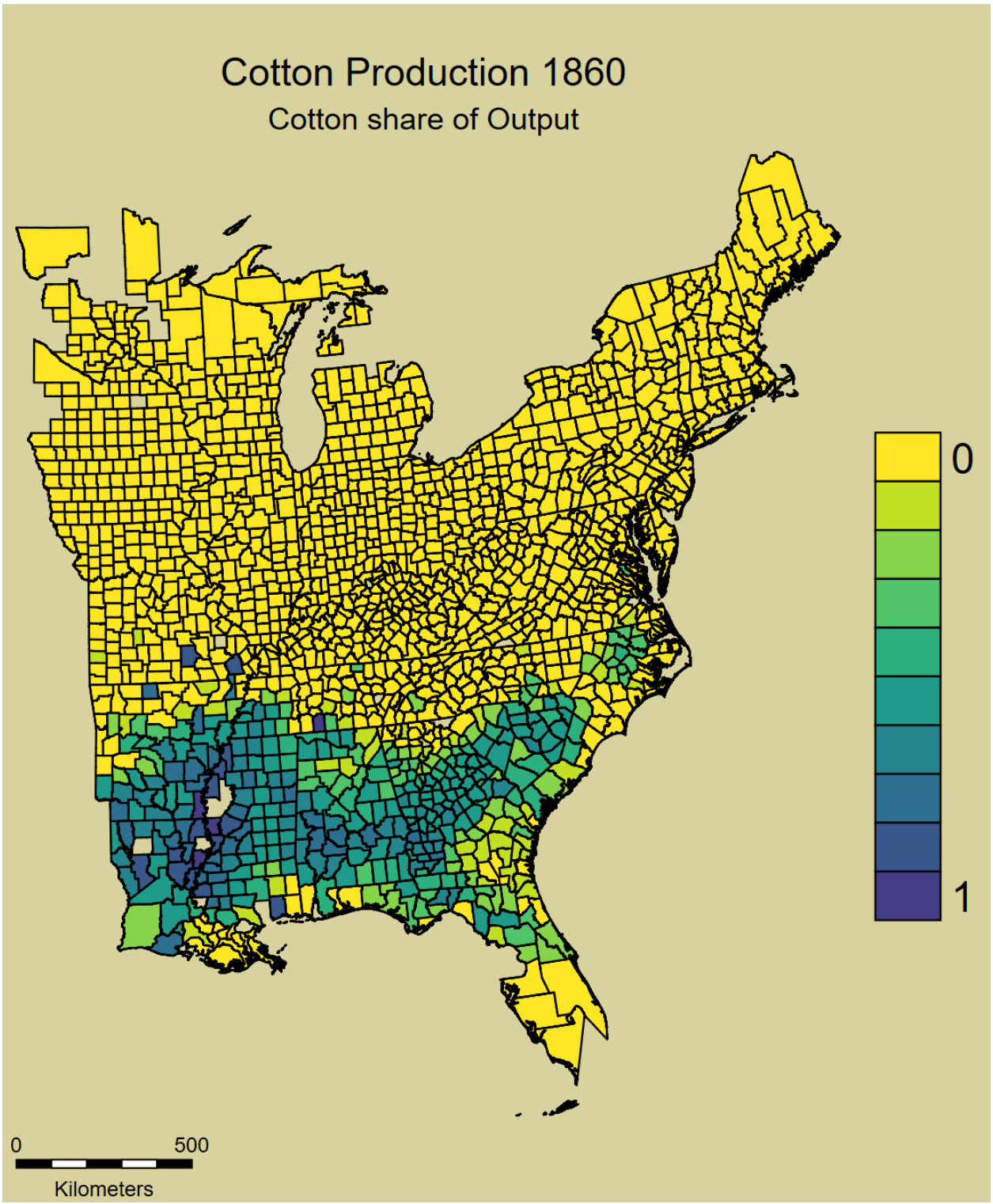


Figure 1: Distribution of cotton production across the US counties in 1860.
Note: the measure used to show distribution in the map is cotton share of output in 1860.

3 Conceptual Framework

3.1 Transmission Mechanism

I follow the argument from the seminal work of Rindos et al. (1985) on the role of culture in social information acquisition and intergenerational transmission. Given the costly nature of information acquisition, individuals find it optimal to imitate others in their surroundings and thus turn to social norms, beliefs, and practices for codes of routinized daily actions. The preexisting cultural norms and codes of action are created to allow adaptation to the environment and to facilitate collective survival (Rindos et al. (1985)). This framework is particularly relevant for understanding the persistence of racial hierarchies in the American South, where environmental conditions, specifically land suitable for cotton plantations along with robust global demand for cotton, fueled the creation of a harsh system of slavery.

The South's insatiable demand for cotton intensified the brutality of these coercive labor practices, exemplified by the quota-driven "pushing system," where enslaved workers were subjected to severe punishments, including whippings, for failing to meet increasingly demanding daily picking targets (Baptist, 2016). This system not only drove productivity but also widened the racial divide by normalizing a punitive culture rooted in domination and violent subjugation of Black people. These harsh practices became embedded in the fabrics of the counties' culture, fostering norms of racial hierarchy and a deep-rooted notion of racial superiority.

As argued by Nunn (2008), socially learned norms and beliefs tend to persist over time as long as the environment remains relatively unchanged. Even when the environment changes, individuals often continue to follow traditions that are no longer adaptive. In the context of the American South, even after the abolition of slavery and the end of the cotton-driven economy, the punitive attitudes towards African Americans remained embedded in social institutions. This informal institution of domination and violence did not cease with the Civil War but instead adapted to new forms of social control.
The enduring cultural legacy of this punitive system became especially evident during the War on Drugs campaign, which introduced new financial incentives, such as the Byrne Memorial Grant and asset forfeiture policies, aimed at motivating law enforcement agencies to prioritize drug-related arrests. These financial incentives provided an exogenous shock to law enforcement practices, encouraging agencies across the country to increase anti-drug efforts. However, in counties with a historical legacy of cotton cultivation, where racial animus and punitive attitudes towards Black people had persisted since the era of slavery, these

incentives had a particularly pronounced effect.

In these "cotton counties," law enforcement agents, already influenced by deeply rooted racial hierarchies and punitive norms, responded to the financial incentives by disproportionately targeting Black drug offenders. The pre-existing racial attitudes meant that, when given additional resources and motivation to pursue drug-related arrests, law enforcement in these areas were more likely to act on biases against Black individuals, intensifying racial disparities in arrests. Thus, the combination of historical racial animus and modern financial incentives during the War on Drugs campaign led to significantly higher arrest rates for Black drug offenders in former cotton-producing regions, reinforcing and perpetuating long-standing patterns of racial discrimination in law enforcement.

3.2 Model

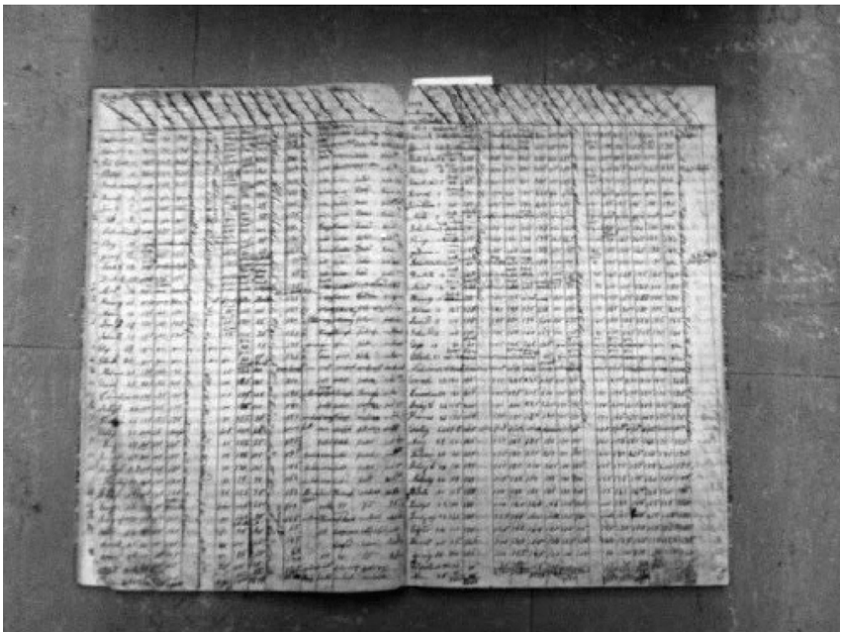
In this section, I introduce a theoretical framework to explore the historical development of the Cotton Kingdom, catalyzed by the Louisiana Purchase and the westward expansion of southern planters. The immense profitability of cotton in the global market created unprecedented demands for plantation labor, driving the need for increased productivity from enslaved workers. Unable to achieve this through voluntary means, plantation owners institutionalized violence as a mechanism of control, normalizing the use of harsh punishments to extract labor. To formalize this idea, consider the following simple agricultural production function:

$$Y = zf(L, N, \eta(H)), \tag{1}$$

where the inputs of production are land L, labor N, and effort η, which itself is an increasing function of coercion H. With a fixed amount of land and enslaved labor and in the absence of significant technological advancements in the agricultural South, productivity gains came from increased effort, which was involuntarily extracted through heightened coercion.

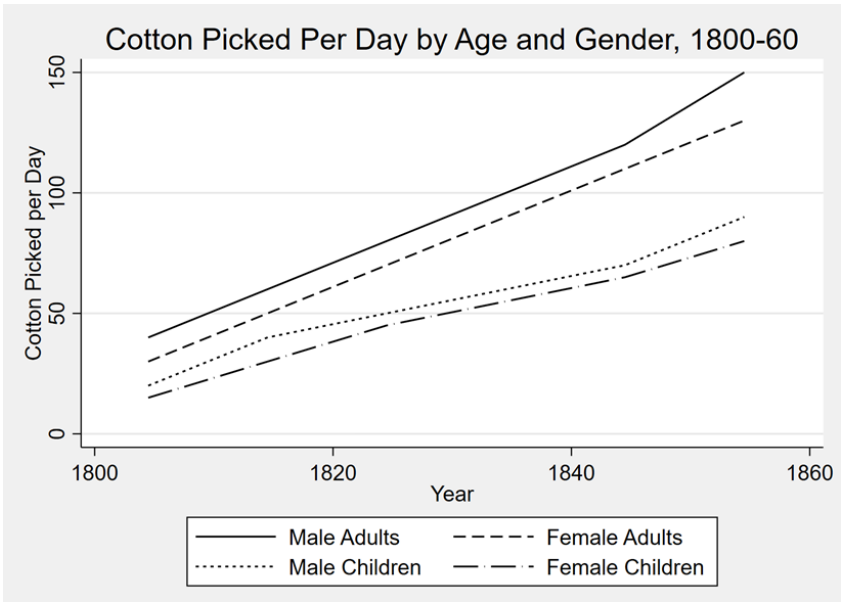
This system of forced labor and brutal enforcement not only shaped economic production but also affected the region's culture, reinforcing ideologies of racial hierarchy. Over time, these violent practices became organic components of the environment, contributing to the development of rigid structures of racial segregation and subjugation that persisted beyond slavery.

To incorporate the cultural persistence of this attitude into the model, let A_0 denote the initial punitive attitude formed by antebellum labor-enhancing coercive mechanisms so that:



Panel A:

Example of Plantation Journals and Records of Cotton Picked.
Source: Baptist (2016)



Panel B:

Slave Productivity Growth 1800-1860
Note: On average slave productivity grew 400 percent between 1800-1860
Source:Olmstead and Rhode (2008)

Figure 2: Historical Evidence

$$A_0 = \psi(\eta(H)), \tag{2}$$

and

$$\frac{dA(\eta(H))}{dH} > 0. \tag{3}$$

The persistence of this attitude beyond the Civil War can be modeled as:

$$A_{t+1} = \alpha A_t + \epsilon, \tag{4}$$

where A_t is the punitive attitude towards Black people at time t, $\alpha \in (0, 1)$ is the persistence parameter capturing the extent to which the attitude persists across generations, and ϵ represents other potential factors that affect the attitude (e.g., civil rights movement, education, etc.).

Given the higher levels of coercion in the South's cotton-intensive areas ($H^C > H^{NC}$), the model implies that the initial punitive attitude was more pronounced in cotton counties ($A_0^C > A_0^{NC}$). Holding α fixed, the attitude in cotton counties is expected to diminish at a later time, implying:

$$A_{t+1}^C > A_{t+1}^{NC}, \tag{5}$$

since:

$$H^C > H^{NC}. \tag{6}$$

Next, consider Southern law enforcement agents in county $j \in \{C, NC\}$. On any given day, the law enforcement agent decides whether to arrest an individual of race $r \in \{B, W\}$ for a minor drug-related offense (e.g., possession of marijuana). Arresting a minor offender is costly, with two types of costs:

(i). Financial cost F, which captures the opportunity cost of resources required to arrest an offender (e.g., time, equipment, logistic support, etc.).

(ii). Psychological cost P, which represents the disutility suffered by the agent from making the arrest. P is a decreasing function of the punitive attitude A, and thus (indirectly) a decreasing function of historical coercion H:

$$P^{(r,j)} = P(A(H)). \tag{7}$$

The law enforcement agent arrests the offender if:

$$B_t \geq F + P^{(r,j)}, \tag{8}$$

where B_t is the benefit of making the arrest in period $t \in \{\text{pre}, \text{post}\}$. Rearranging, the condition for an arrest is:

$$P^{(r,j)} \leq B_t - F. \tag{9}$$

The arrest rate (or probability of arrest) is then expressed as the cumulative distribution function (CDF) of P, evaluated at $B_t - F$:

$$\text{Arrest Rate} = \Phi_P(B_t - F). \tag{10}$$

Suppose the financial cost F is constant across all groups. Further, suppose the psychological cost P is lower for Black offenders and even lower in cotton counties. If P follows a log-normal distribution with mean μ and standard deviation σ, then:

$$\mu_{\text{Black, Cotton}} < \mu_{\text{Black, Non-Cotton}} < \mu_{\text{White}}. \tag{11}$$

Because of this differential in psychological cost of arresting offenders, at any level of financial benefit and cost of arrest there exists a gap in the arrest rates between different groups. The arrest rate gap between Black offenders in cotton versus non-cotton counties is:

$$\Delta_{\text{Black, Cotton - Non-Cotton}} = \Phi_{P^{\text{Black, Cotton}}}(B_t - F) - \Phi_{P^{\text{Black, Non-Cotton}}}(B_t - F). \tag{12}$$

Similarly, the arrest rate gap between Black and White offenders is:

$$\Delta_{\text{Black - White}} = \Phi_{P^{\text{Black}}}(B_t - F) - \Phi_{P^{\text{White}}}(B_t - F). \tag{13}$$

Before the start of the campaign the net benefit of arresting drug offenders is positive and thus there already exists a racial gap between black and white individuals and between blacks in cotton and non-cotton counties.

$$\Phi_{B,j}(B_0 - F) - \Phi_{W,j}(B_{Pre} - F) \geq 0$$

and

$$\Phi_{B,C}(B_0 - F) - \Phi_{B,NC}(B_{Pre} - F) \geq 0$$

Following the start of the campaign, the financial benefit of arresting offenders increases significantly. To determine the response to the financial incentives one can differentiate the

these gaps with respect to the benefit B_t:

$$\frac{\partial \Delta_{\text{Black, Cotton - Non-Cotton}}}{\partial B_t} = \phi_{P^{\text{Black, Cotton}}}(B_t - F) - \phi_{P^{\text{Black, Non-Cotton}}}(B_t - F), \tag{14}$$

$$\frac{\partial \Delta_{\text{Black - White}}}{\partial B_t} = \phi_{P^{\text{Black}}}(B_t - F) - \phi_{P^{\text{White}}}(B_t - F), \tag{15}$$

where $\phi_P(x)$ is the probability density function (PDF) of P. Because the log-normal distribution is log-concave , the CDF Φ_P is convex for values below the median [5]. An increase to the financial benefit in the convex region of the CDF , increases the racial gap:

$$\phi_{P^{\text{Black, Cotton}}}(B_t - F) - \phi_{P^{\text{Black, Non-Cotton}}}(B_t - F) > 0, \tag{16}$$

and

$$\phi_{P^{\text{Black}}}(B_t - F) - \phi_{P^{\text{White}}}(B_t - F) > 0, \tag{17}$$

Figure 3 shows the racial gap in arrest rate between Black and White offenders in cotton and non-cotton counties. As one can see, with an increase in benefit of arrest, the black-white gap in arrest rates grows faster in cotton counties for the convex portion of the corresponding CDFs. Thus there is a differential in the racial gap in arrest rates and across counties (see Figure 3). Following the start of the campaign and with introduction of the financial incentives, the model makes the following predictions:

1. Arrests increase for offenders of all races and in all counties, as all arrest rate are increasing in the financial benefit.

2. The arrest rate of Black offenders in cotton counties rises most dramatically, followed by Black offenders in non-cotton counties, and finally White offenders, implying a wider racial gap in cotton counties.

3. A differential exists in the arrest rate of Black offenders between cotton and non-cotton counties, with no significant differential for White offenders.

4. The disparity in arrest rates increases with pre-1860 coercion levels.

The first prediction is consistent with empirical evidence in the literature. I test the remaining predictions in Section 5.

[5]Note that the only the convex portion of CDF is relevant for the purposes of this model since only a small fraction (i.e., below median) of all offenders are arrested in any crime category

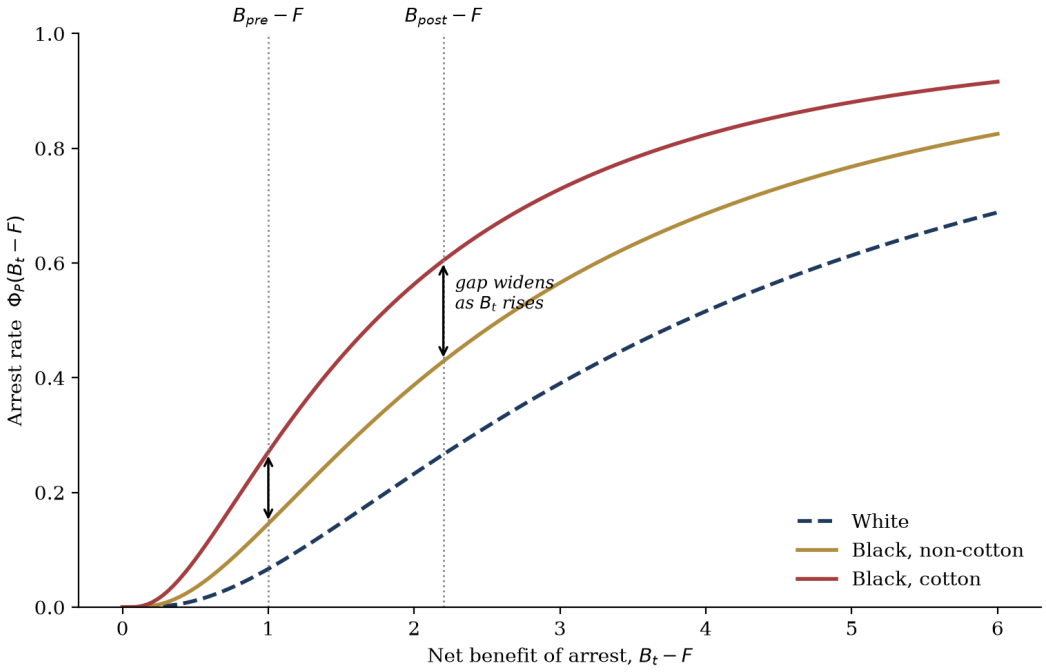


Figure 3: Racial Gap in Arrest Rates in Cotton and Non-Cotton Counties
Note: the gap between the CDFs captures the racial gap in arrest rates.

4 Data

4.1 Sources and summary statistics

There are multiple publicly available datasets used for this project. The data on agricultural output comes from the 1860 Census of Agriculture. The census data and the GIS shapefiles are publicly available at the IPUMS NHGIS (National Historical Geographic Information System) website. Panel A of Table 1 provides summary statistics on agricultural products as share of output. It is noteworthy that the main crop in non-cotton counties is corn with an average share in output of 0.4, followed by wheat and cotton. This is while the dominant crop in the cotton counties is cotton with an average output share of 0.66 followed by corn and sweet potato.

The police arrest data used in this paper comes from the FBI's Uniform Crime Reporting (UCR) program, which is publicly available for download on the FBI's website. Specifically, I utilize the arrest by age, race, and sex (ASR) version of the data, as collected and made available by Kaplan (2021). This dataset provides highly detailed information on the number of individuals arrested for various crimes, including drug-related offenses, covering the years 1974 to 2018. Panel B of Table 1 presents summary statistics for different crime measures. Notably, the highest arrest rates are observed for drug-related crimes. Within this category,

Table 1: Summary Statistics

Panel A: Agricultural Share of Output Across Counties in the US South
Source: The 1860 Census of Agriculture

	Cotton Counties	Non-Cotton Counties
Cotton	0.657	0.137
Wheat	0.026	0.139
Rye	0.001	0.008
Corn	0.219	0.400
Oat	0.005	0.026
Rice	0.000	0.019
Tobacco	0.000	0.047
Wool	0.002	0.014
Peas	0.021	0.025
Potato	0.002	0.011
Sweet Potato	0.040	0.072
Barley	0.000	0.001
Buckwheat	0.000	0.001

Panel B: Crime Rate (Per 100,000 Persons) Across Counties in the US South
Source: Uniform Crime Reporting (UCR) Data, estimation sample 1974-2007

	Cotton Counties	Non-Cotton Counties	US South
Total Drug	289.05	312.50	304.70
Drug Sales	67.54	57.06	60.54
Drug Possession	204.04	233.94	224.00
Aggravated Assault	119.86	109.20	112.74
Arson	4.18	4.56	4.43
Murder	6.47	4.63	5.24
Rape	8.73	7.66	8.01
Robbery	23.76	20.04	21.28
Theft	329.08	342.42	337.99

Panel C: Labour Market Variables Across Counties in the US South
Source: Bureau of Economic Analysis (BEA), IPUMS NHGIS, and the 1860 Census of Population

	Cotton Counties	Non-Cotton Counties	US South
Log Income Per Capita (2000)	10.017	10.094	10.068
Slaves in 1860 per 10K Population	4883	2398	3223
Share with Less than High School (%)	37.57	35.25	36.02
Black Population Share	0.366	0.120	0.202
Total Count	171	344	515

Note: Panels B and C are computed on the 515-county harmonized estimation panel (Section 4.2), 171 cotton and 344 non-cotton counties, over 1974–2007. Panel A describes 1860 agricultural output shares under the original census county definitions.

arrests for drug possession outpace arrests for drug sales. The arrest rates are similar between cotton and non-cotton counties.

The index of land suitability for cotton plantation is derived from the FAO's Global Agro-Ecological Zones (GAEZ) project, which offers information on the maximum attainable yield for various crops, including cotton, measured in tons per hectare per year. This index is based on data related to soil characteristics, climate, and precipitation, with estimates calculated from average conditions between 1961 and 1990. The key assumption behind using contemporary data on historical land suitability is that land suitability for cotton between 1860 and 1960 either remained largely unchanged or any changes occurred uniformly across the region.

Labor market variables, which are used as controls in the empirical analysis, include per capita income, the unemployment rate, and education levels at the county level. These data are provided by the Bureau of Labor Statistics (BLS) and the Bureau of Economic Analysis (BEA). Additionally, school attendance data by race, used to examine patterns of school segregation, is sourced from the National Center for Education Statistics (NCES) Common Core of Data. Geographic controls, such as average precipitation, average temperature, and distance from ports, are taken from Cervellati et al. (2017). County police employment, which enters the decomposition of Section 8.4 as a measure of policing capacity, is taken from the UCR Police Employee data and comprises sworn officers and patrol personnel per capita. Finally, the 1860 slave share of the county population, which serves as the direct measure of historical coercion in Section 8, is drawn from the 1860 Census of Population, also available through IPUMS NHGIS.

Panel C of Table 1 provides summary statistics for county characteristics on the estimation sample. Cotton and non-cotton counties show similar levels of per capita income, and educational attainment is only slightly lower in cotton counties. The differences that stand out are historical and demographic: the 1860 slave share was twice as high in cotton counties, and the Black population share remains three times as high today. Both facts matter later in the paper, the first for the coercion tests of Section 8 and the second for the interpretation of the decomposition in Section 8.4.

The lynching data used in this study originates from Tolnay and Beck (1995), which compiles information on lynchings across ten Southern states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee. The dataset was initially developed using records from the NAACP, the *Chicago Tribune*, and the Tuskegee Institute's lynching archives. To ensure accuracy, the authors validated each lynching event by cross-referencing contemporary newspaper accounts. For an event to be classified as a lynching, it must meet four specific criteria: (1) evidence must confirm the

individual's death; (2) the death must have occurred unlawfully; (3) at least three individuals must have actively participated; and (4) the act must have been justified under a pretense of enforcing justice, judgment, or tradition. The third criterion is particularly important, as it highlights the mob-like nature of lynchings. The dataset includes 2,805 lynchings documented at the county level between 1882 and 1930. Finally, the KKK activity data come from Ang (2023) and contain the location of Klan chapters ("klaverns") along with the date each chapter was first mentioned.

4.2 Harmonizing 1860 counties to modern boundaries

County boundaries changed substantially between 1860 and the modern period: counties split, merged, and shifted, so assigning 1860 cotton output to the counties that report arrests a century later is not innocuous. I harmonize the historical census to modern geography using GIS areal interpolation, following the procedure of Fiszbein (2017): 1860 census quantities are distributed from historical county polygons onto the modern county grid in proportion to intersected area, so that each modern county receives the cotton output of the historical territory it actually occupies. The harmonized cotton measure correlates at 0.70 with the naive same-name assignment, and about nine percent of sample counties change treatment status under the harmonized definition, a difference that is quantitatively consequential. All results in this paper use the harmonized measure; a county is classified as a cotton county if its harmonized 1860 cotton share lies in the top tertile of the Southern distribution, and the continuous specifications use the harmonized share directly. The 1860 tobacco share, which serves as the placebo crop in Section 5, is harmonized in the same manner. The estimation panel covers 515 Southern counties from 1974 to 2007, of which 171 are cotton counties.

4.3 Constructing the land-suitability instrument

The instrument for 1860 cotton intensity is built directly from the source data of FAO's Global Agro-Ecological Zones project rather than from pre-processed county aggregates. GAEZ distributes a global raster of maximum attainable cotton yield (tons per hectare) at 5-arc-minute resolution, computed from soil, terrain, and 1961–1990 climate under rain-fed, low-input conditions. I extract the county-level instrument by zonal statistics: the mean attainable yield over all raster cells intersecting each modern county polygon. The resulting measure predicts harmonized 1860 cotton intensity strongly (the first-stage F statistic is 44), which matters because coarser, pre-aggregated suitability indices used in earlier drafts produced first stages too weak to use. The identifying assumptions are discussed with the results in Section 5.

4.4 Measuring historical racial animus

The mechanism tests of Section 8 require county-level measures of anti-Black animus that predate the War on Drugs. I use three, each capturing a different manifestation of the same underlying attitude. The first is the count of Black lynchings between 1882 and 1930 from Tolnay and Beck (1995), animus revealed through mob violence. The second is the presence of Ku Klux Klan chapters ("klaverns") in the 1960s from Ang (2023), animus revealed through organized mobilization against the Civil Rights Movement. The third is the county vote share of George Wallace in the 1968 presidential election, from the county returns assembled by Algara and Amlani (2021): a ballot cast for an explicitly segregationist platform is a revealed preference with no social-desirability filter, expressed by the general white electorate rather than the violent fringe, fourteen years before the campaign began. Wallace coverage is universal across all Southern counties; in Alabama, where Wallace held the Democratic ballot line, his vote is taken from the Democratic column, and the resulting shares reproduce published statewide totals to within a few hundredths of a percentage point in eleven of fourteen states. The three proxies correlate only weakly with one another (pairwise 0.18–0.20), which is what one wants from an index: averaging them cancels instrument-specific noise. The animus index is the standardized mean of the three standardized components. The Klan operated in three historical waves: a Reconstruction-era first wave, a second wave that peaked in the 1920s, and a third wave that mobilized in the South during the 1960s against the Civil Rights Movement. Only the third wave enters the index. Chapters of the 1920s second Klan are deliberately excluded and reserved as a placebo, because that wave was a largely Midwestern nativist movement whose targets extended to Catholics, Jews, and immigrants, and the 1860 slave share predicts its presence with the wrong sign. Two further measures, the racially charged Google-search index of Stephens-Davidowitz (2014), assigned from media markets to counties through a publicly available county-to-market crosswalk, and pre-1982 Confederate monuments from the Southern Poverty Law Center (2019) inventory, serve as independent modern validations in Section 8.5.

5 Empirical Specification and Results

First, I estimate a difference-in-difference-in-differences (DDD) model. This model allows me to assess the impact of historical cotton plantations on the drug-related arrest rates of Black compared to White individuals in cotton counties following the War on Drugs campaign. The regression model is as follows:

$$y_{rjt} = \gamma_{rt} + \theta_{jt} + \tau_{jr} + \beta(Race \times Cotton \times Post) + X'_{jt}\lambda + \epsilon_{rjt} \tag{18}$$

where y_{rjt} is the arrest rate of individuals of race r (either Black or White) in county j (either a cotton or non-cotton county)[6] at time t (either before or after the start of the campaign). γ_{rt} captures race-time fixed effects, θ_{jt} captures county-time fixed effects, and τ_{jr} captures county-race fixed effects. The coefficient on the triple interaction captures the difference in the Black and White racial gap in arrest rates in cotton counties relative to non-cotton counties, after the start of the campaign (see Figure 3). X_{jt} denotes a vector of county-level covariates that includes income, education, unemployment rate, and poverty levels[7].

The DDD approach compares the racial arrest gap (Black versus White) in cotton counties to the corresponding gap in non-cotton counties, before and after the start of the War on Drugs campaign. This triple interaction captures whether the racial disparity in arrest rates widened more in cotton counties than in comparable non-cotton counties following the policy shock. A key advantage of the DDD framework is that it allows me to difference out unobserved factors that vary across time, race, and counties but are common within one of these dimensions. In practice, the inclusion of race-time, county-time, and county-race fixed effects means that any factor affecting all individuals of a given race over time (for example, national trends in drug enforcement), any factor affecting all individuals in a given county over time (for example, county-level policing resources), or any persistent differences between racial groups across counties is absorbed. What remains is the differential policy impact on the racial arrest gap that is specific to cotton compared to non-cotton counties.

The main identifying assumption is that, absent the War on Drugs campaign, the racial arrest gap in cotton counties would have evolved in parallel to that in non-cotton counties. The policy was introduced in October 1982, and I assume this marks the start of the post-treatment period. Accordingly, the time dummy in the model captures years after 1982. By exploiting the historical classification of cotton counties, defined as counties where cotton production in 1860 accounted for more than 50 percent of agricultural output, I rely on variation that is plausibly exogenous to modern law enforcement practices. Since this historical measure predates the War on Drugs by more than a century, it is unlikely to be correlated with unobserved, contemporaneous shocks to drug-related arrests.

Table 2 presents the regression results. Rather than fold all drug offenses into a single measure, I split the outcome into possession and sales, because the two categories differ

[6]Let q_{cot} and Q denote cotton and total agricultural output in 1860, harmonized to modern county boundaries (Section 4.2). A county is a cotton county if q_{cot}/Q lies in the top tertile of the Southern distribution.

[7]The share of blacks and whites in counties are controlled for in all specifications of the paper.

sharply in how much discretion an officer brings to the arrest. Columns 1 and 2 report the estimates without additional controls; Columns 3 and 4 add the full set of county-level covariates. The estimation sample stacks the arrest rates of Black and White offenders, so the unit of observation is the county-year-race cell. With 515 counties observed over 1974 to 2007 for two racial groups, and after excluding the small number of county-years with unreported arrests, this yields the 35,000 observations reported in the table.

The estimate for possession is of particular interest. In former cotton counties the Black-White gap in possession arrests widens by 25.8 per 100,000 after the campaign begins (Column 1), an effect significant at the five percent level. Because the specification carries race-time, county-time, and county-race fixed effects, this coefficient isolates the change in the racial gap that is specific to cotton counties, net of any national trend in drug enforcement, any county-specific shock over time, and any fixed difference between racial groups within a county.

The distinction between possession and sales is itself informative. Possession is a low-evidence charge that leaves considerable room for officer discretion, whereas a sale is more difficult to observe and to establish. If an inherited punitive attitude drives the results, one would expect the possession margin to respond most strongly, and this is indeed what the estimates show. The possession coefficient is 25.8, more than double the 12.3 estimated for sales (Column 2), and both are precisely estimated.

The inclusion of additional county-level controls in Columns 3 and 4 leaves the estimates essentially unchanged. This is what one should expect rather than a surprise: the covariates vary at the county level and are nearly orthogonal to a triple interaction that already conditions on county, race, and time, so little is left for them to absorb. A more substantive caveat concerns the nature of these covariates. Several of them, most notably contemporary income and educational attainment, are themselves plausibly consequences of the historical treatment: if the cotton legacy depressed Black economic opportunity, then these variables are outcomes rather than confounders, and conditioning on them amounts to conditioning on post-treatment variables, which can bias the coefficient of interest (Angrist and Pischke, 2009). The specifications without controls are therefore the preferred estimates. I report the controlled specifications, in keeping with common practice, as a sensitivity check, and the fact that the estimates are essentially identical across the two indicates that this concern has little practical consequence in the present setting.

It is also critical to note that the standard errors in all specifications are clustered at the county level, which is the appropriate level of clustering given the structure of the data. Since the treatment varies at the county level, clustering at this level accounts for potential intra-county correlation in arrest rates, ensuring that the standard errors are robust and do

Table 2: The Impact of Historical Cotton Production on the Drug-related Arrest Rate of Blacks Relative to Whites

	(1) Possession	(2) Sales	(3) Possession	(4) Sales
Cotton x				
Black x Post	25.78**	12.29***	25.78**	12.29***
	(11.20)	(2.62)	(11.20)	(2.62)
Time-Race FE	✓	✓	✓	✓
Race-County FE	✓	✓	✓	✓
Time-County FE	✓	✓	✓	✓
Additional Controls			✓	✓
Observations	35,000	35,000	35,000	35,000
R^2	0.138	0.065	0.173	0.071

Observations are county-year-race cells: 515 counties, 1974–2007, two racial groups.
Clustered standard errors at county level in parentheses
* $p < 0.10$, ** $p < 0.05, ***p < 0.01$

not overstate the precision of the estimates.

To further evaluate the sensitivity of the findings, I re-estimated the triple-difference model after expanding the set of county-level covariates. In addition to the baseline labour market measures, which include a broad range of variables such as per capita income, unemployment rate, and other county-level labour market indicators, I incorporated a more comprehensive list of geographic characteristics, including average temperature, latitude, longitude, distance to the nearest river and railroad, elevation above sea level, and average rainfall levels. The triple interaction for possession remains highly significant across these specifications, and the point estimate barely moves, staying close to 26 per 100,000. This demonstrates that the main results are not sensitive to the inclusion of either additional labour market covariates or a wide set of county-level geographic controls.

I also tested the sensitivity of the results to different subsamples by estimating the model separately for each Southern state. Leaving out one Southern state at a time, the possession triple difference stays positive in every case, ranging from roughly 17 to 46 per 100,000. These leave-one-out estimates are less precise, reflecting smaller sample sizes and unbalanced distributions of cotton versus non-cotton counties, but the consistency of the effects indicates that the results are not driven by any single state. Section 6 highlights Arkansas as a useful case study: the state's geography provides a relatively balanced mix of cotton and non-cotton counties, making it a particularly credible subsample for comparison.

Inference is a related concern, since the sample covers only twelve states. As a design-

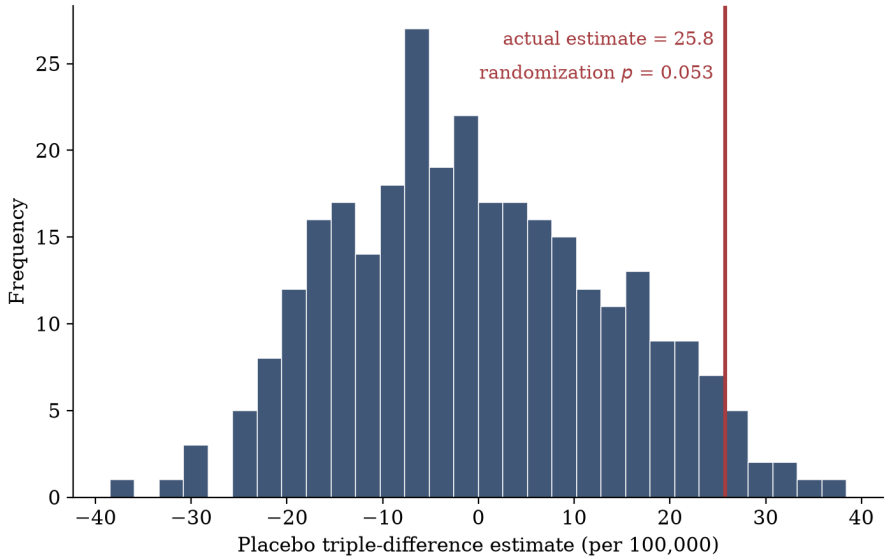


Figure 4: Randomization Inference for the Possession Triple Difference
Note: the histogram shows 300 placebo estimates obtained by randomly reassigning cotton status across counties, holding the number of cotton counties fixed. The vertical line marks the actual estimate of 25.8 (randomization $p = 0.053$).

based check, I randomly reassign cotton status across counties, holding the number of cotton counties fixed, and re-estimate the possession triple difference three hundred times. The actual estimate of 25.8 exceeds, in absolute value, 94.7 percent of the placebo estimates (randomization $p = 0.053$), in line with the county-clustered analytic inference. Figure 4 plots the distribution of the placebo estimates against the actual estimate.

A related objection is that any legacy of plantation agriculture, rather than of cotton specifically, could produce the same pattern. Tobacco provides the natural placebo crop: it was the South's other major plantation staple, but its labor regime did not develop the escalating daily quotas and systematic violence that defined the cotton pushing system. Table 3 re-estimates the triple difference using the harmonized 1860 tobacco share, alone and in a horse race against cotton. Tobacco intensity alone predicts no widening of the racial arrest gap for either offense category. The cotton gradient is essentially unchanged when tobacco enters, and it remains two to four times the size of the tobacco coefficient; only in the possession specification does a marginally significant tobacco coefficient appear, and it has no counterpart for sales. The arrest response is therefore specific to the crop whose production regime generated the coercion that the model takes as its starting point.

Going forward, I test the hypothesis that Black individuals in cotton counties were more severely affected by the campaign compared to those in non-cotton counties[8]. To do this, I

[8]I estimate the same model for white individuals as well. This is to provide a point of reference showing

Table 3: Crop Specificity: Cotton versus Tobacco

	Possession			Sales		
	(1)	(2)	(3)	(4)	(5)	(6)
Cotton × Post × Black	15.07***		16.89***	4.46***		4.68***
	(3.92)		(4.35)	(1.12)		(1.18)
Tobacco × Post × Black		6.12	9.35**		0.27	1.16
		(3.93)	(4.31)		(1.02)	(1.07)
Observations	35,000					

Race-long panel; harmonized 1860 crop shares, standardized (coefficients per SD). Clustered SEs at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

estimate a pooled difference-in-differences model:

$$y_{rjt} = \beta_{0r} + \beta_{1r} POST_t + \beta_{2r} COT_j + \beta_{3r} POST_t \times COT_j + X_{jt}\beta_4 + \omega_{rjt} \tag{19}$$

where y_{rjt} is the arrest rate of an individual of race r in county j and year t, POST and COT are indicator variables for year 1982 and cotton county, respectively. The coefficient on the interaction between these two indicator variables captures the difference in arrest rate of Black individuals across the two sets of counties following the start of the campaign. Similar to the DDD model, X_{jt} is the vector of control variables.

Table 4 reports the interaction of interest, the cotton indicator interacted with the post-1982 indicator, estimated separately for each race and offense type. The pattern the triple difference implied now shows directly. For Black possession arrests the cotton-by-post coefficient is 18.8 per 100,000 and highly significant (Column 1); for Black sales it is 6.4 (Column 3). Both carry the same message: after 1982, Black arrest rates rose in former cotton counties relative to the rest of the South.

The magnitude is substantial. An additional 18.8 Black possession arrests per 100,000 residents, sustained across the post-1982 period, compounds into a wide gap between counties that differ mainly in a farming pattern fixed more than a century before the policy.

In contrast, arrest rates for Whites followed a different trend. Column 2 shows that the White possession differential between cotton and non-cotton counties is statistically insignificant, and the White sales estimate in Column 4 is, if anything, negative. Nothing in the White series suggests that cotton counties simply became more punitive across the board; the response is specific to Black offenders.

The inclusion of controls in this table ensures robustness and further contextualizes the findings. The results suggest that, even after accounting for various county-level characteristics, historical factors tied to cotton production are critical in explaining these differential

that the higher arrest rates in cotton counties is not a result of more punitive law enforcement in general.

Table 4: The Impact of Historical Cotton Production on the Drug-related Arrest Rate of Blacks in Former Cotton Counties Relative to Non-Cotton Counties

	(1) Black Poss.	(2) White Poss.	(3) Black Sales	(4) White Sales
Cotton × Post	18.83***	-7.06	6.36***	-5.95**
	(2.90)	(10.99)	(1.54)	(2.45)
Controls	✓	✓	✓	✓
Observations	17,500	17,500	17,500	17,500
R^2	0.180	0.135	0.091	0.051

Observations are county-year cells for the indicated racial group.
Clustered standard errors at county level in parentheses
* $p < 0.10, ** p < 0.05, *** p < 0.01$

patterns. As with Table 2, the standard errors in Table 4 are clustered at the county level, aligning with the structure of the treatment and ensuring the robustness of the statistical inference. Given the county-level variation in the treatment (cotton production legacy) and the outcome (arrest rates), clustering at this level accounts for potential intra-county correlations and ensures that standard errors are not underestimated.

It is worth noting that instead of estimating equations (18) and (19) separately, one could fully expand the triple-difference model to incorporate the difference-in-differences coefficients directly. I chose, however, to estimate them separately in order to present the analysis in a stepwise fashion. In the first step, the results show that the racial arrest gap widens more sharply in former cotton counties following the onset of the campaign. In the second step, the evidence indicates that this widening gap is driven by a higher arrest rate of Black individuals in those counties rather than by a decline in the arrest rate of White individuals.

The parallel-trends assumption stated in the discussion of the triple-difference design can be assessed directly. To do so, I estimate the following event study model

$$y_{rjt} = \alpha_r + \sum_{\tau=q}^{-1} \beta_{r\tau}(\text{Time}_t \times \text{Treated}_j)_{j\tau} + \sum_{\tau=0}^{T} \beta_{r\tau}(\text{Time}_t \times \text{Treated}_j)_{j\tau} + \text{Time}_t + \mu_{rjt} \quad (20)$$

Where y_{rj} is the arrest rate of an individual of race r in county j and in year t, $Time_t$ captures year and $Treated_j$ is an indicator variable that equals 1 if the county is a cotton county. The results are presented in Figure 5, separately for possession (left panel) and sales (right panel). Both panels tell the same story. In the years before 1982 the cotton versus non-cotton differential in Black arrests is small and statistically indistinguishable from zero,

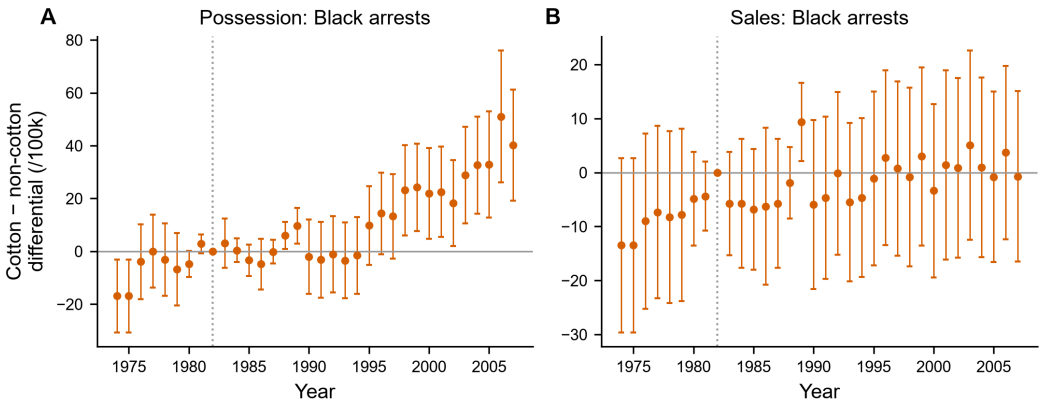


Figure 5: Event Study Arrest Trends for Blacks and Whites.
Note: time is reported in intervals of five years with 1980 normalized as base-year.

the flat pre-trend the design requires. After the campaign the differential turns positive and grows, becoming both significant and sizable by the late 1980s and into the 1990s. The event study therefore places the divergence at the policy rather than before it, which is what a causal reading needs.

The White series, by contrast, stay flat through the same window, as the pooled results in Table 4 already showed. The asymmetry between the two races is the sharpest visual evidence that the War on Drugs was enforced along racial lines in these counties.

Another important question is whether anti-drug law enforcement of Black offenders is increasing in the intensity of counties' historical cotton plantation levels. To check this, one can use a continuous measure of cotton plantation in a two-way fixed-effect regression:

$$y_{rjt} = \alpha_r + \lambda_t + \gamma_j + \beta_r(shcot_j \times Post_t) + \zeta_{rjt} \tag{21}$$

where y_{rjt} denotes drug-related arrest rate for individual of race r in county j in year t, and $shcot_j \times Post_t$ is the output share of cotton in county j in 1860 interacted with the indicator variable $Post_t$ (equal to 1 after 1982). The coefficient β_r captures the marginal impact of cotton plantation intensity on the arrest rate of Blacks and Whites after the start of the campaign.

The results, as reported in the OLS columns of Table 5, confirm that the intensity of cotton plantations in 1860 is associated with heterogeneous law enforcement patterns across the two races. A one-standard-deviation increase in harmonized 1860 cotton intensity raises Black possession arrests by 10.2 per 100,000 after the campaign begins and Black sales arrests by 2.6, both precisely estimated; the White point estimates are negative and imprecise. Accounting for county and year fixed effects ensures that the results are robust

to unobserved temporal and spatial heterogeneity.

To address potential endogeneity, I use FAO's Global Agro-Ecological Zones (GAEZ) land-suitability-for-cotton index as an instrument for historical cotton output. This approach has precedent in the literature. Acharya et al. (2016) uses this measure in studying the historical roots of political attitudes, while Fiszbein (2017) draws explicitly on FAO-GAEZ's crop-specific yield and land suitability data as instruments in historical economic analysis.

It is noteworthy, however, that the key identifying assumption in using this index as instrument is that contemporary FAO measures of land suitability provide a valid proxy for conditions in the 19th century. While soil quality and climate have evolved over time, the argument is that relative spatial differences in suitability for cotton production have remained largely stable, or that any changes occurred uniformly across regions. If this assumption holds, then the instrument can be considered exogenous.

Furthermore, it seems difficult to identify any plausible direct channel by which geographic features such as soil type, terrain, or climatic suitability for cotton would independently influence modern patterns of law enforcement or drug arrests. Therefore, it is reasonable to assume that exclusion restriction holds because these geographic characteristics shaped cotton production in the 19th century, which in turn influenced the development of racialized institutions and law enforcement practices. Absent that historical pathway, there is little reason to expect land suitability itself to have a direct effect on arrest rates today.

The instrument itself is constructed from the GAEZ source rasters by zonal statistics over modern county polygons, as described in Section 4.3. Figure 6 shows the first stage: county-mean attainable cotton yield predicts harmonized 1860 cotton intensity tightly, with a first-stage F statistic of 44, comfortably above conventional weak-instrument thresholds.

The IV columns of Table 5 report the estimated coefficients using the instrument alongside the benchmark OLS estimates. For Black individuals the coefficients are positive and statistically significant in both specifications and for both offense types, and the IV estimates are substantially larger than OLS (39.6 against 10.2 for possession; 19.0 against 2.6 for sales), implying that if anything the OLS results understate the effect, consistent with measurement error in the harmonized historical cotton measure. For White individuals the estimates are negative throughout, imprecise under OLS and significant under IV. The racial asymmetry, already visible in the reduced form, sharpens under the instrument.

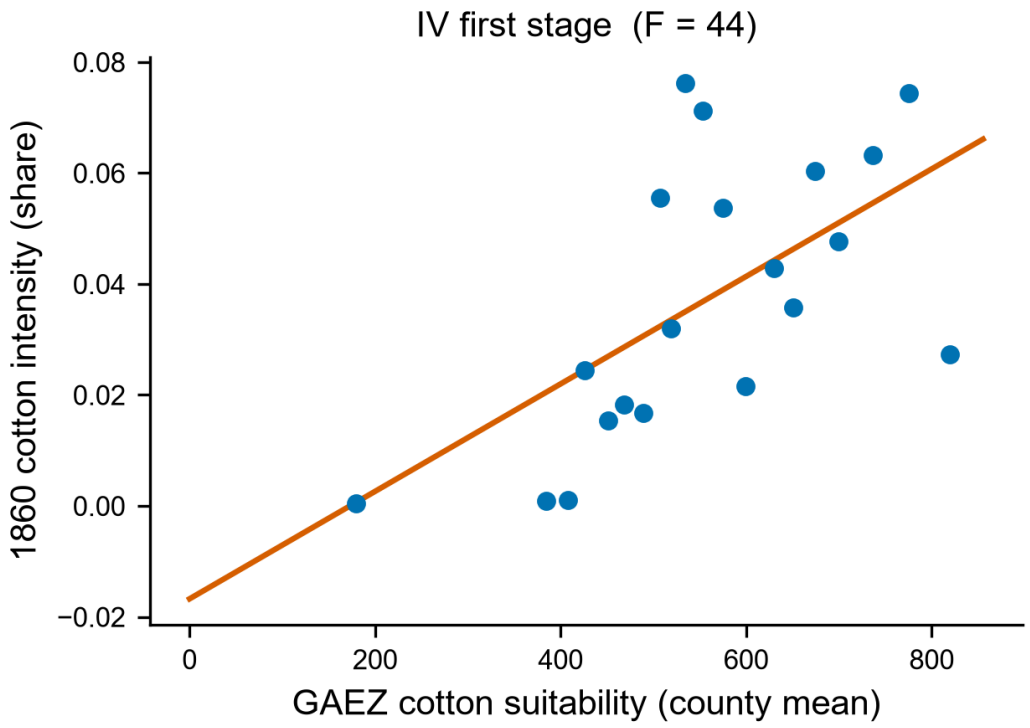


Figure 6: The first stage: GAEZ attainable cotton yield and harmonized 1860 cotton intensity.
Note: binned scatter of the standardized 1860 cotton share against the standardized county-mean GAEZ suitability; first-stage F = 44.

6 Case-Study Evidence: The Case of Arkansas

Another concern when comparing cotton and non-cotton counties in the south, is that most cotton and non-cotton counties are clustered together which has to do with land suitability for cotton plantation. In other words, it is difficult to find counties next to each other that are similar in most characteristics except for cotton plantation intensity in 1860. Consequently, states are either dominantly occupied with cotton counties or non-cotton counties. There is one state that, due to its specific geography, has a mix of cotton and non-cotton counties. Arkansas has lands low in elevation and high in fertility along the Mississippi river on the east and south of the state, which has counties that are suitable for cotton plantation. In contrast, there are mountainous regions with high elevation and poor fertility in the northwest (Ozark mountains) and southwest (Ouachita mountains) of the state, which have counties that are not suitable for cotton plantation McNeilly (2000). Panel (a) of Figure 7 shows the unique geography of Arkansas. As can be seen, the state is cut in half by the diagonal connecting the northeast to the southwest.

There were 55 counties in the state in 1860. Out of this universe of counties, I was able to

Table 5: The Impact of Cotton Plantation Intensity on Arrest Rate of Blacks and Whites

	Black		White	
	OLS	IV	OLS	IV
Cotton intensity × Post (Possession)	10.21***	39.57***	-4.91	-67.70***
	(1.67)	(4.77)	(3.61)	(22.10)
Cotton intensity × Post (Sales)	2.56***	19.04***	-1.91*	-18.92***
	(0.88)	(2.56)	(0.98)	(7.11)
County & Year FE	✓	✓	✓	✓
First-stage F		44.4		
Observations		17,442		
R^2		–		

Clustered standard errors at county level in parentheses

* $p < 0.10, ** p < 0.05, *** p < 0.01$

Table 6: The Impact of Historical Cotton Production on the Drug-related Arrest Rate of Blacks and Whites in Former Cotton Counties in Arkansas

	(1) Black Poss.	(2) White Poss.	(3) Black Sales	(4) White Sales
Cotton × Post	14.94*	-38.51	8.14**	-18.01**
	(8.57)	(33.07)	(3.93)	(7.77)
Controls	✓	✓	✓	✓
Observations	918	918	918	918
R^2	0.462	0.236	0.340	0.163

Standard errors in parentheses

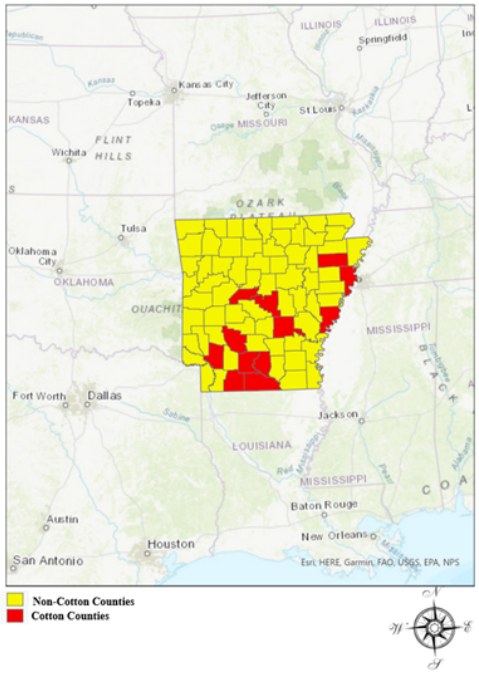
* $p < 0.10, ** p < 0.05, *** p < 0.01$

match the UCR data with historical counties in 27 instances. Out of these 27 counties, 12 are cotton counties and 15 are non-cotton counties. Panel (b) of Figure 7 shows the distribution of the (successfully matched) counties in the data. As one can see, these counties are located in the southeast of the state.

Table 6 reports the results for Arkansas, estimated separately for possession and sales. The pattern mirrors the full-sample results. Black possession arrests rise by 14.9 per 100,000 in Arkansas's cotton counties after the campaign, and Black sales arrests by 8.1, both statistically significant despite a sample of only 27 counties. The White estimates are negative throughout, and significantly so for sales. That the effect survives at all in a single state, with the state government, state police, and state law held fixed, indicates that the cross-county results are not an artifact of comparing different states' enforcement regimes.

(a) Geography of Arkansas

Note: The region in green has land suitable for cotton production.



(b) Distribution of Cotton Counties in Arkansas

Figure 7: Arkansas: geography and the distribution of cotton counties

7 Persistent Racial Animus: Evidence from School Segregation

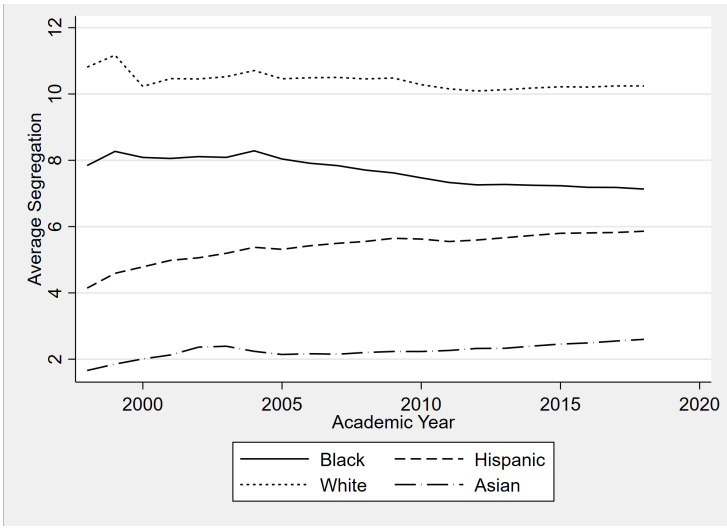
Another source of skepticism might concern the persistence of racial animus over such a long span of time. It could be argued that the harsh labor conditions in the cotton plantations ended more than 120 years before the start of the War on Drugs campaign, making it unlikely that the same collective sentiments are still detectable. The Jim-Crow era expressions of this animus, namely lynchings between 1882 and 1930, the Klan chapters of the 1960s, and the segregationist vote of 1968, enter the analysis formally in Section 8, where they are combined into a county-level animus index. In this section I present a demonstration of the same animus that extends into the contemporary period: the segregation of Black students in Southern schools.

Another strong indicator of racial animus is the segregation of races across different dimensions of society. The idea here is that a majority group with a distaste for a minority's presence would, to the extent possible, physically separate itself from the minority. With the end of the Jim Crow era, desegregation initiatives began, and in the absence of cultural persistence, one would expect segregation to have disappeared completely over time. Using data from the U.S. Department of Education's Common Core of Data (CCD), I investigate this hypothesis specifically within the U.S. South and across the two sets of counties. As illustrated in Panel A of Figure 8, although school segregation persists in the region, the Black series shows a downward trend over the past two decades. This initial observation might suggest progress; however, the narrative becomes more nuanced when we decompose the segregation trends further. Panel B of Figure 8 presents the Black segregation trends separately for cotton and non-cotton counties. There are two critical points to consider here.

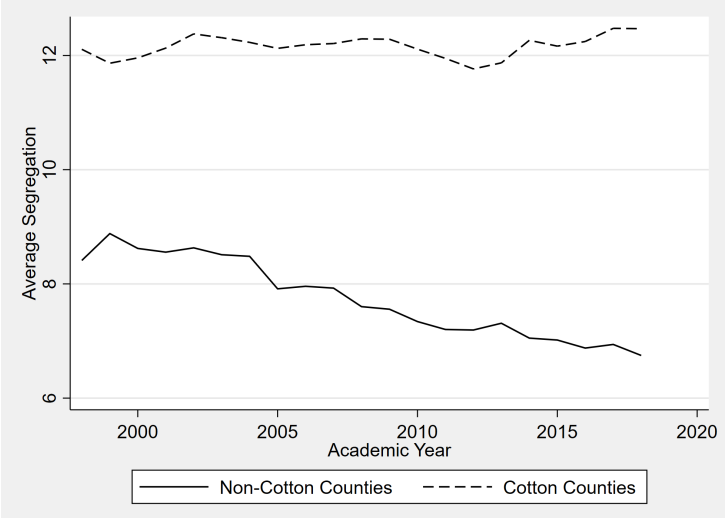
Firstly, the data indicates that schools in cotton counties are, on average, more segregated than those in non-cotton counties for African Americans. This suggests that historical economic factors, namely the intensity of cotton plantation in 1860, have a lasting impact on racial segregation.

Secondly, the downward trend in Panel A is driven almost entirely by the non-cotton counties. As Panel B shows, segregation in cotton counties exhibits no meaningful decline over the two decades and remains close to its level at the start of the period. This distinction underscores the complex and persistent nature of segregation in areas with deep historical ties to the cotton industry.

These two observations reinforce the previous findings of this paper. The first observation confirms the significant role of cotton in the existence and intensity of racial animus. The higher levels of segregation in cotton counties align with the historical importance of cotton



Panel A: Segregation by Community in the US South



Panel B: Black Segregation Patterns in Cotton and Non-Cotton Counties

Figure 8: Segregation Patterns in the US South

in shaping social and economic structures. The second observation highlights the enduring legacy of the cotton kingdom, demonstrating how historical economic foundations continue to influence racial segregation patterns.

8 Identifying the Mechanism

The empirical sections test what the model predicts: a wider racial arrest gap in cotton counties, a response specific to Black offenders, and an effect that grows with historical cotton intensity. Confirming those predictions is reassuring, but it constitutes a weak test of

the mechanism itself. Many alternative explanations predict a wider gap in cotton counties. Few, however, predict it for the specific reasons the model identifies, namely that the arrest rate is the cumulative distribution of an officer's psychological cost evaluated at the net benefit of an arrest (Arrest Rate $= \Phi_P(B_t - F)$), that this cost is lowest for Black offenders in cotton counties ($\mu_{\text{Black, Cotton}} < \mu_{\text{Black, Non-Cotton}} < \mu_{\text{White}}$), and that the low cost is the inherited residue of nineteenth-century coercion.

Before presenting the tests, it is worth being clear about what the classical tools of the discrimination literature can and cannot do here. The outcome test of Knowles et al. (2001) and Anwar and Fang (2006), which separates taste-based from statistical discrimination through search and contraband data, requires stop-level microdata that were never collected for rural Southern counties in the 1980s. The officer-composition test of Ba et al. (2021) requires force rosters by race that no survey covers for small rural departments. What remains feasible is a set of complementary tests: ruling out the rival explanations that the data can address, decomposing the effect into its observable channels, and showing that the residual behaves as animus would. Specifically, the effect appears only where discretion allows attitudes to operate, it is organized by the pre-existing geography of anti-Black animus, and it passes placebo tests that a generic Southern-county correlate would fail. This section presents these tests in turn.

8.1 The cost distribution behind the arrest rate

The behavioral core of the model is a single object: the arrest rate is the probability that an officer's psychological cost falls below the net benefit of an arrest, so it equals the CDF of that cost evaluated at $B_t - F$. Three features of that object are testable, and I test each on the drug-possession arrest rate, measured as arrests per 100,000 county residents.

The first is the cost ordering. I cannot observe an officer's psychological cost, but the model supplies its inverse: a higher arrest rate implies a lower cost. So I read the pre-1982 Black arrest rate off cotton and non-cotton counties and back out the implied cost. The ordering holds, and it holds before the War on Drugs even begins. Black possession arrests run at 14.7 per 100,000 in cotton counties against 3.4 in non-cotton counties, more than four times higher, exactly the lower cost the model assumes. The behavioral primitive is visible in the raw pre-period data.

The second feature is functional form, and it is the sharpest of the three. The model says the gap widens after 1982 because a convex CDF magnifies an otherwise fixed cost gap when the benefit rises. A skeptic can object that any convex transform of two rising series will look like a widening gap, so the effect might be an artifact of curvature rather than a real

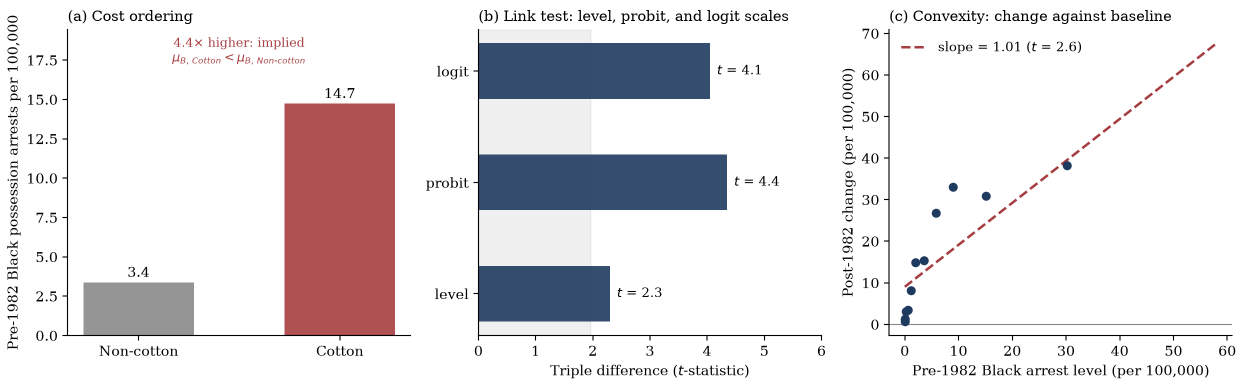


Figure 9: Structural tests of the arrest-cost model. Panel (a): the pre-1982 Black arrest rate is 4.4 times higher in cotton counties, implying the lower psychological cost the model assumes. Panel (b): the triple difference survives the probit and logit transforms, so it is not an artifact of the convex link. Panel (c): the post-1982 jump rises with the pre-1982 baseline, the signature of a convex CDF.

interaction. I can settle this. If I move the arrest rate back onto the latent index scale, using the inverse-normal transform that linearizes a normal or log-normal CDF, a pure curvature effect should vanish. It does not. The triple difference is 25.8 on the raw arrest scale and stays firmly positive on the probit scale (0.12, $t = 4.4$) and the logit scale (0.42, $t = 4.1$). The widening is a genuine interaction on the latent scale, not an accident of the link function. I read this as support for the reduced-form result rather than a defeat of the model: a fully curvature-driven effect would have washed out. The qualification is that the stylized convexity story does not by itself account for everything, and the true complementarity is, if anything, a little stronger than curvature alone would deliver.

The third feature is convexity itself. On a convex curve, a county already higher up sits on a steeper slope, so the same rise in benefit produces a larger jump. Counties with higher pre-1982 arrest levels should therefore show larger post-1982 increases. They do. Regressing the post-1982 jump on the pre-1982 baseline gives a slope of 1.01 ($t = 2.6$), the convex signature the model predicts, now visible across counties rather than assumed. Figure 9 collects the three tests.

8.2 The offense-discretion ladder

The first test relies on the observation that an officer's attitudes can affect arrest outcomes only where the officer exercises discretion. For offenses such as murder or robbery, the arrest is anchored to a reported crime, and the scope for discretion is limited. A possession arrest, in contrast, typically involves no complainant, so that whether an encounter results in an arrest is determined largely by the officer. If inherited animus is the mechanism, the cotton × post

Table 7: The offense-discretion ladder: Cotton × Post × Black by offense

Offense	DDD	SE	Pre-1982 Black mean
High discretion (proactive, no complainant)			
Drug possession	25.78**	(11.20)	7.16
Heroin/cocaine possession	16.35***	(2.23)	0.22
Drug sales	12.29***	(2.62)	3.02
Cannabis possession	2.21	(9.31)	6.70
Re-targeted margins (old public-order charges)			
Drunkenness	−363.05***	(39.68)	63.06
DUI	−81.76**	(38.23)	52.03
Disorderly conduct	−20.83*	(10.96)	27.68
Gambling	−7.58***	(1.46)	3.07
Victim-anchored (the offense forces the case)			
Murder	−0.71**	(0.35)	1.14
Manslaughter (negligent)	−0.28***	(0.10)	0.09
Rape	0.27	(0.30)	1.21
Robbery	0.65	(0.66)	3.64
Discretionary boundary of the violent class			
Aggravated assault	11.94***	(3.30)	15.12
Other (simple) assault	76.44***	(11.55)	23.98

Each row is a separate race-long triple difference, $N = 35{,}000$; SEs clustered at county level. Outcome: arrests per 100,000 county residents. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

× Black effect should be concentrated in high-discretion offenses and absent from victim-anchored ones. Neither rival explanation generates this prediction. Race-neutral differences in policing intensity would raise arrests across all offense categories, and an explanation based on differential offending would require that drug offending alone changed, in cotton counties alone, after 1982, while leaving victim-anchored offenses unaffected.

I estimate the triple difference of equation (18) separately for twenty-five offenses, every category the UCR master file carries by race, with each outcome constructed identically to the drug outcomes. Table 7 reports the key rows, Figure 10 the full ladder, and Table A1 in the Appendix the complete set of twenty-five estimates.

The estimates for the four unambiguously victim-anchored offenses are statistically indistinguishable from zero: murder at −0.71 per 100,000, negligent manslaughter at −0.28, rape at +0.27, robbery at +0.65. Against them, drug possession moves by +25.8, heroin and cocaine possession by +16.4 with a t-statistic above seven, and drug sales by +12.3. The murder estimate warrants a power calculation, since a null result from a sparse series is not informative on its own: Black murder arrests average 1.1 per 100,000 pre-1982, and the

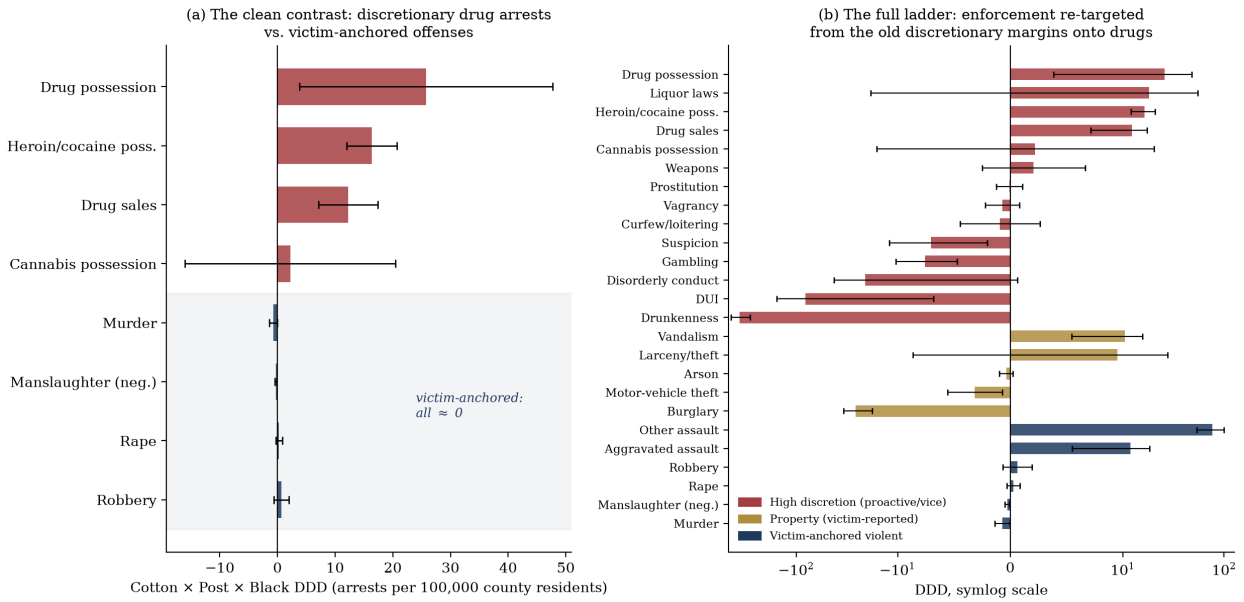


Figure 10: The offense-discretion ladder. Panel (a): the clean contrast between discretionary drug enforcement and the four unambiguously victim-anchored offenses. Panel (b): the full 25-offense ladder; the negative bars on drunkenness and DUI are the re-targeting of the old discretionary margins onto drugs.

design would detect an effect of about 0.98, so the null is informative relative to the size of the series, and the better-powered robbery and rape nulls say the same thing.

The full set of estimates reveals an additional pattern. The older discretionary offense categories declined differentially in cotton counties after 1982: drunkenness arrests by −363 per 100,000, DUI by −82, disorderly conduct by −21, gambling by −7.6. Black drunkenness arrests were very high in the pre-period (63 per 100,000 on average, reflecting the public-order enforcement practices of the Jim Crow era), and they declined most in the counties where drug arrests rose most. This pattern suggests that discretionary enforcement did not expand uniformly but was redirected: when the campaign increased the returns to drug arrests, discretionary enforcement in cotton counties shifted from public-order offenses towards drug offenses, consistent with the model's emphasis on the benefit of arrest. Part of the decline in drunkenness arrests reflects the broader decriminalization of public intoxication over this period, so I do not attach weight to its magnitude; it is the pattern of redirection that is informative.

Two categories deserve further discussion. Aggravated assault (+11.9) and simple assault (+76.4) move, though they sit in the victim-anchored class. Assault arrests are widely regarded as the most discretionary among violent offenses, since the decision to arrest in an altercation involves considerable judgment, and the mandatory-arrest domestic-violence

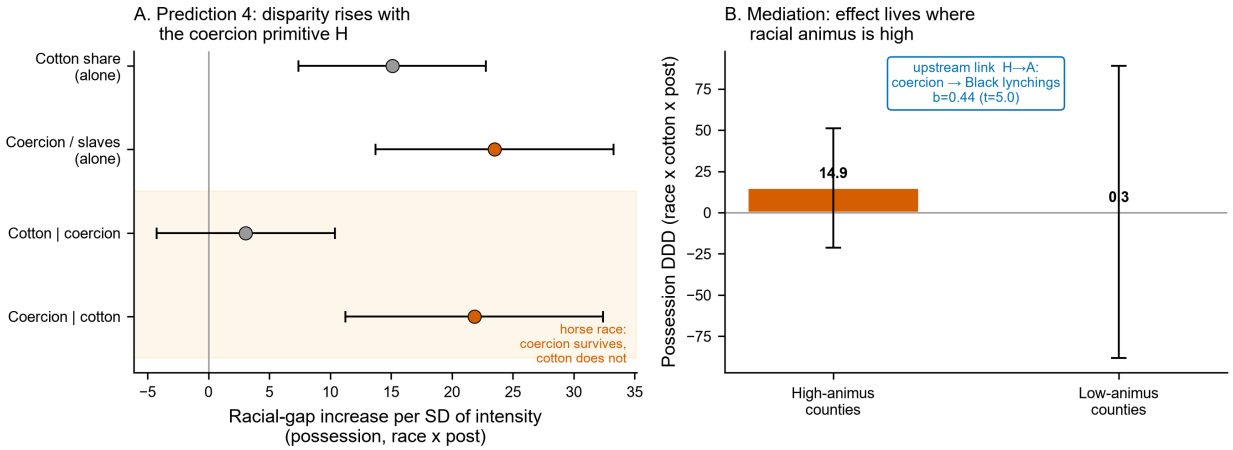


Figure 11: Prediction 4 and the coercion primitive. Panel (a): the racial-gap effect scales with the 1860 slave share, and in a horse race the coercion primitive survives while cotton output does not. Panel (b): the effect is concentrated in counties with a history of racial violence, a first, coarse look at the mediation channel that Figure 12 makes continuous.

statutes of the late 1980s operated through precisely this category. I classified offenses ex ante and report them as estimated rather than reclassifying offenses after seeing the results; the four offenses that are unambiguously victim-anchored serve as the placebo set, and the estimates for all four are indistinguishable from zero.

8.3 Coercion, animus, and the arrest response

The model's deeper claim is about where the low arrest cost comes from: coercion H on the cotton plantation formed a punitive attitude that persisted across generations. Two links in that chain can be measured. I proxy coercion with the 1860 slave share of the county population, a more direct measure of H than cotton output, and I proxy persistent animus with the three-component index of Section 4.4: lynchings, 1960s Klan chapters, and the 1968 Wallace vote.

Prediction 4 says the arrest disparity should rise with pre-1860 coercion, and it does, more strongly than with cotton output. The racial-gap effect scales with the 1860 slave share at 23.5 per standard deviation ($t = 4.7$), against 15.1 for cotton share ($t = 3.8$); in a horse race, coercion survives at 21.8 ($t = 4.0$) while cotton collapses to 3.1 and loses significance. The model never claims the crop matters for its own sake; it matters because cotton generated intense coercion, and once coercion is measured directly, it is what carries the modern effect. The caveat is that slave share and cotton share are closely correlated, so I read the horse race as strongly suggestive rather than decisive.

The mediation link is the harder one, and Table 8 takes it in two steps. Panel A shows

Table 8: The animus index: first stage and arrest gradients

	Coefficient	SE	t
Panel A: 1860 slave share (per SD) → animus proxy			
Black lynchings 1882–1930 (asinh)	0.179	(0.048)	3.74
1960s Klan klaverns (asinh)	0.125	(0.029)	4.31
Wallace 1968 vote share	0.059	(0.007)	8.20
Three-proxy animus index (SD units)	0.337	(0.043)	7.83
1920s Klan klaverns (asinh), placebo	−0.103	(0.024)	−4.22
Panel B: arrest gradient of the possession DDD (per SD of measure)			
Black lynchings	14.61	(4.31)	3.39
1960s Klan	20.06	(7.90)	2.54
Wallace 1968	4.54	(5.43)	0.83
1920s Klan (placebo)	−2.71	(6.16)	−0.44
Two-proxy index	22.58	(7.11)	3.17
Three-proxy index	19.30	(5.46)	3.54
Panel C: horse races			
Index | cotton	16.18	(6.24)	2.59
Cotton | index	10.09	(4.75)	2.12
Index | 1860 slave share	12.93	(6.03)	2.15
1860 slave share | index	19.18	(5.63)	3.40

Panel A: robust SEs, 515-county cross-section. Panels B–C: race-long panel, county FE, SEs clustered at county level. "A | B": coefficient on A with B's interaction in the model.

the upstream link: a one-standard-deviation increase in the 1860 slave share raises every Deep-South animus proxy (lynchings, $t = 3.7$; the 1960s Klan, $t = 4.3$; the Wallace vote, $t = 8.2$; the index, $t = 7.8$) and predicts the Midwestern 1920s Klan with the *wrong* sign ($t = -4.2$), consistent with its role as a placebo. Panel B shows the downstream link: the possession triple-difference scales with each violence-based proxy and with the index (22.6 per standard deviation, $t = 3.2$, for the two-proxy version; 19.3, $t = 3.5$, for the three-proxy version), while the 1920s Klan carries nothing (-2.7). In the horse race, the index carries 16.2 ($t = 2.6$) against cotton's residual 10.1 ($t = 2.1$): measured animus absorbs the larger share of the arrest response. The Wallace component is informative in its own right: alone, its arrest gradient is small (4.5, insignificant), suggesting that the violent expression of animus, rather than its prevalence in the electorate, is what predicts the enforcement response. The Wallace vote nevertheless contributes to the index by strengthening the coercion first stage and hedging against the objection that violence records measure record-keeping.

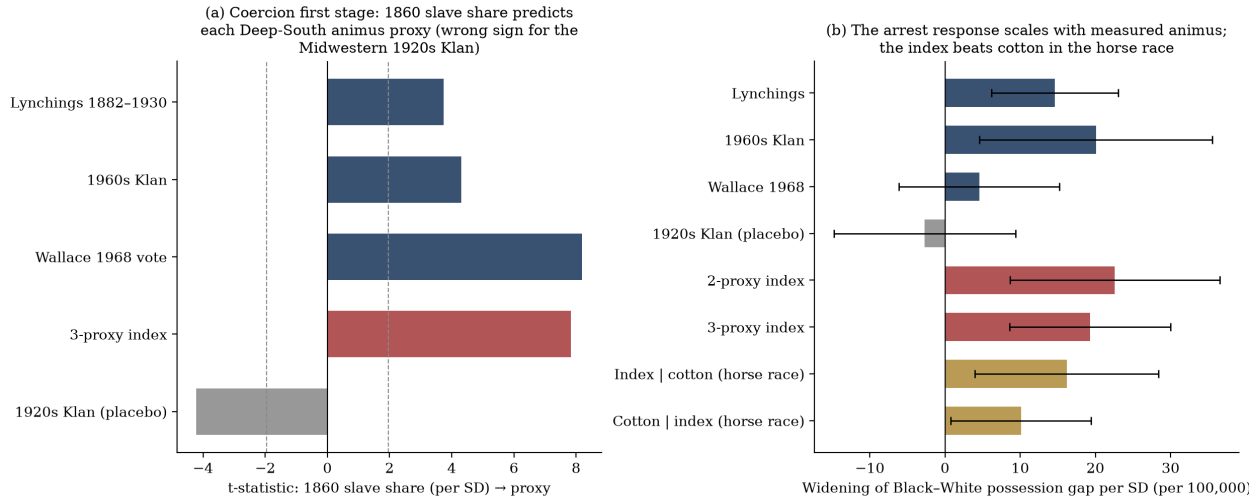


Figure 12: The three-proxy animus index. Panel (a): the 1860 slave share predicts each Deep-South animus proxy, the Wallace vote most strongly, and the Midwestern 1920s Klan with the wrong sign. Panel (b): the possession-arrest response scales with each proxy and with the index, and the index beats cotton in the horse race.

8.4 Decomposing the cotton-county arrest response

The tests so far show that the arrest response tracks animus. This subsection asks the complementary question: how much of the cotton-county racial arrest response can observable *rival* channels account for, and what happens to the residual when animus is measured directly? I answer it twice, with two workhorses that make different assumptions, then confront the residual with the animus index.

The first is a channel decomposition in the spirit of Gelbach (2016), in which every observable is allowed to operate on the same margin as the treatment and the movement of the cotton coefficient is allocated exactly across the competing channels; Appendix B details the construction. The rival channels all enter with the opposite sign: the economic channel (income and 1860 output per capita) contributes −6.9, police capacity (officers and patrols) −2.8, the vice-enforcement base (pre-existing drunkenness and DUI arrests) −8.2, and geography and climate −0.4. Jointly they predict a cotton-county racial response about 18 per 100,000 smaller than the sample average, so the unexplained component is, if anything, larger than the headline estimate. The differential racial arrest response in cotton counties is not proxying for poverty, police budgets, the pre-existing vice-arrest infrastructure, or geography.

The second workhorse is a Kitagawa–Oaxaca–Blinder decomposition (Oaxaca, 1973; Blinder, 1973; Neumark, 1988) of the county long-difference, the change in each county's Black–White possession gap from the pre-period mean to the post-period mean; Appendix B de-

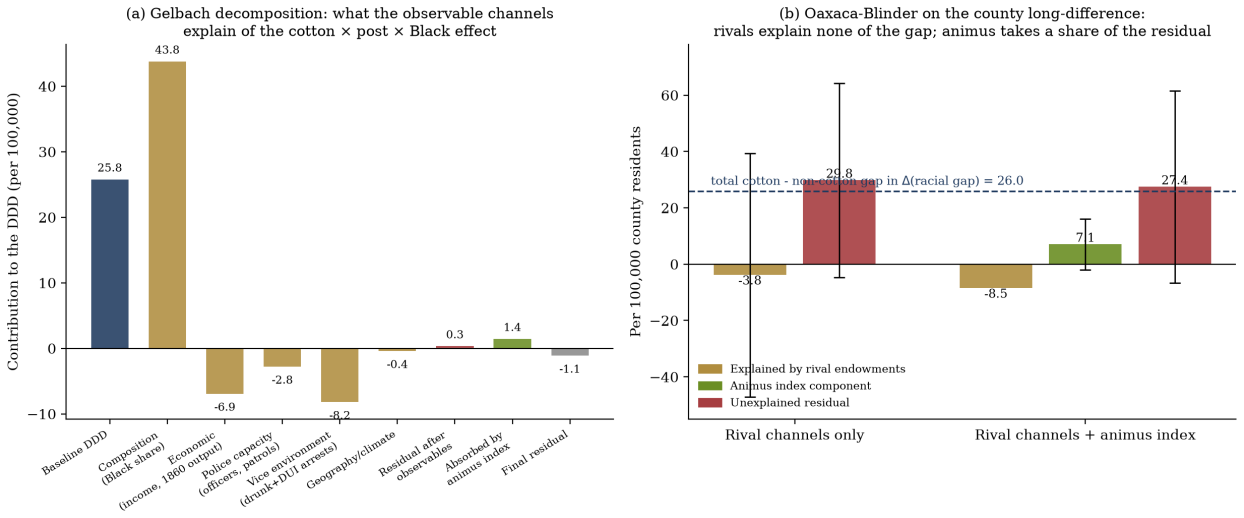


Figure 13: Decomposing the cotton-county arrest response. Panel (a): Gelbach channel accounting of the triple difference; the rival channels are negative, composition is mechanical, and the residual confronts the animus index. Panel (b): Oaxaca–Blinder on the county long-difference; rival endowments explain none of the cotton/non-cotton difference, and the animus index takes up a quarter of it.

scribes the construction and the bootstrap. The cotton minus non-cotton difference in that change is 26.0 per 100,000, which is the triple difference in cross-section form. The explained component is −3.8 (bootstrap SE 22.1): rival endowments explain none of the gap, and the same wrong-way prediction appears again. Adding the animus index to the endowment set, the index alone takes up 7.1 per 100,000 (bootstrap SE 4.6), roughly a quarter of the total, from a single county measure dated decades before the campaign.

The most direct test augments the baseline triple difference with the animus interaction. The cotton coefficient falls from 25.8 ($t = 2.3$) to 12.5 ($t = 0.9$) and is no longer statistically distinguishable from zero, an absorption of 52 percent, while the animus interaction itself enters at 17.1 with a t-statistic of 2.5 (Table 9, Panel C). This pattern suggests that the cotton indicator has been acting as a proxy for the underlying attitude: once that attitude is measured directly, it accounts for the bulk of the effect.

One channel requires careful interpretation: demographic composition. The share-Black channel absorbs more than the entire baseline, and conditional on it the cotton coefficient turns negative. This is the familiar consequence of conditioning on a post-treatment variable rather than evidence of leniency: the Black share of a Southern county in 1980 is the demographic footprint of the plantation system itself, the descendants of the enslaved workforce, so conditioning the cotton effect on it conditions a cause on its own consequence, and the outcome, being a per-county-resident rate, scales with composition by construction. The

Table 9: Decomposition of the cotton-county racial arrest response

	Contribution	Share of baseline
Panel A: Gelbach channel decomposition of the DDD (baseline = 25.78)		
Composition (Black population share)	+43.78	1.70
Economic (income, 1860 output p.c.)	−6.92	−0.27
Police capacity (officers, patrols)	−2.79	−0.11
Vice environment (drunkenness + DUI base)	−8.20	−0.32
Geography/climate	−0.41	−0.02
Panel B: Oaxaca–Blinder, county long-difference (total gap = 25.96)		
Explained by rival endowments	−3.82	[22.06]
Unexplained residual	29.78	[17.56]
Animus index component (when added)	7.05	[4.59]
Panel C: confronting the residual (race-long panel, $N = 35{,}000$)		
Baseline Cotton × Post × Black	25.78	(11.20)
…with animus channel included	12.49	(14.12)
Animus × Post × Black channel	17.13	(6.84)

Panel A: exact OVB accounting; channels enter as $X\times$ Post × Black with all lower-order terms. Panel B: pooled-reference twofold decomposition, 514 counties; bootstrap SEs (500 county resamples) in brackets. Panel C: clustered SEs at county level in parentheses.

composition channel does, however, clarify the limits of the design. Within county-level arrest data, the widening is unambiguously racial: it is organized by where Black communities live and where animus ran deepest, and by nothing economic, administrative, or geographic. The final partition between locally inherited enforcement animus and a nationally race-tilted campaign amplified by inherited demography, however, is beyond the reach of these data. Three pieces of evidence favor a substantial local component: the arrest response tracks the animus index conditional on county fixed effects; enforcement re-targeted from drunkenness to drugs county by county, which a purely national tilt does not predict; and the 1920s Klan placebo shows the organizing variable is anti-Black animus specifically.

8.5 Alternative animus measures

If the animus index were an artifact of how historical violence was recorded, independent measures built from different instruments, decades, and populations should not reproduce its gradient. Two public measures allow the check; survey-based alternatives do not, because the General Social Survey's county identifiers are restricted and its Southern sample is far too thin for county analysis. The first is the racially charged Google-search index of Stephens-Davidowitz (2014), the economics literature's standard revealed-preference animus measure,

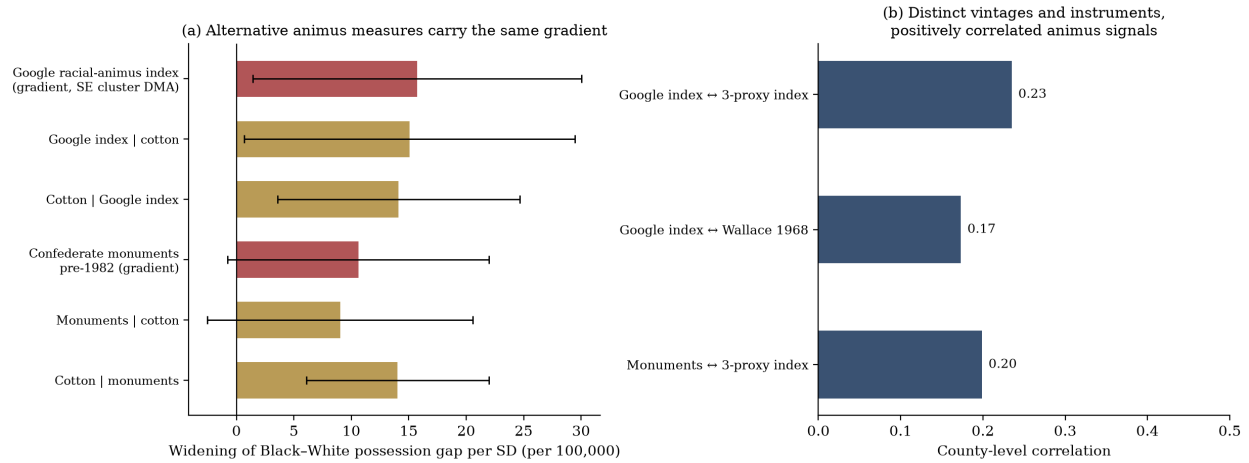


Figure 14: Alternative animus measures. Panel (a): the Google-search animus index and the pre-1982 Confederate-monument count both carry the possession-gap gradient, and the Google index survives the horse race against cotton. Panel (b): the alternative measures correlate positively with the historical three-proxy index despite being built from different instruments, decades, and populations.

observed at the media-market level in 2004–2007 and assigned to counties with market-clustered inference; because it postdates the campaign by two decades, it tests persistence as well as animus. The second is the count of Confederate monuments dedicated before 1982 and still standing then, from the Southern Poverty Law Center (2019) inventory: reflecting the commemorative choices of county elites decades earlier.

Both carry the gradient (Figure 14). The Google-index gradient is 15.7 per 100,000 per standard deviation ($t = 2.2$), and both the index (15.1, $t = 2.1$) and cotton (14.1, $t = 2.6$) survive the horse race: the market-level measure is too coarse to absorb county-level cotton variation, so the two are best read as complementary signals. The monument gradient is 10.6 ($t = 1.8$), marginal alone and subsumed by cotton, the expected behavior of a sparse count that is itself a consequence of the same history. The three measures correlate at 0.17 to 0.24 with the historical index and with each other: positive and modest, consistent with instruments of this diversity capturing a common underlying attitude. Although these measures are constructed from very different sources and time periods, they identify broadly the same set of counties, and the arrest response is consistent with each of them.

8.6 Summary of the mechanism evidence

Taken together, the evidence indicates that the differential racial arrest response in cotton counties is not explained by income, education, police capacity, the vice-enforcement base, geography, or climate; those channels jointly predict a smaller response in both decomposi-

tions. It is drug-specific and discretion-specific, absent from every offense where an officer cannot choose, and it was accompanied by a re-targeting of the discretionary margin away from the Jim-Crow-era public-order charges. It survives the probit and logit transforms, so it is not an artifact of a convex link. It is organized by the pre-1982 geography of anti-Black animus measured three independent ways, fails to load on the one Klan that was not an anti-Black institution, and is reproduced by animus measures built from Google searches and Confederate monuments. When the animus index enters the baseline specification directly, it absorbs half of the cotton effect and leaves the remainder statistically indistinguishable from zero while remaining significant itself. What no county-level arrest design can deliver is the strict taste-versus-belief separation of the Becker tradition, which requires search-level microdata that do not exist for the rural South of this era; I note this limitation explicitly. Within the limits of what the data can measure, the residual component of the cotton-county response behaves as inherited racial animus would be expected to behave, and not as any of the rival explanations would predict.

9 Conclusion

This paper contributes to the literature of cultural persistence of historically significant events and the role they play in shaping human behavior and determine outcomes decades after they cease to exist. While many papers in the recent years have studied the impact of such historical events on economic outcomes today (see for example Acemoglu et al. (2001), Nunn (2008) and Nunn and Wantchekon (2011), this paper focuses on the impact of cotton plantation prior to the civil war on the mass arrest of African Americans following the war on drugs campaign. In this paper, I show that the counties with higher rates of African American arrest are the same counties that were more intensively involved in the cotton plantation prior to the start of the country's civil war in 1861. This heterogeneity, then I try to argue, is due to a system of progressively negative incentives designed by plantation owners to increase slave productivity year after year to meet the global demand for cotton.

I use historical narrative and evidence from works of such people as Edward Baptist and Douglas Blackmon to argue that the incentive for productivity growth to meet the growing demand for cotton led to the use of severe and violent punishment system for slaves who did not meet the ever-increasing quotas set by plantations owners and overseers. This progressively violent and demeaning system of labor productivity enhancement widened the already existing gap between (enslaved) blacks and white (masters) and normalized punitive attitudes towards any deviation from what was perceived as acceptable by the dominantly white majority over time. This punitive attitude towards Black individuals, then, persisted

beyond cotton plantation and slavery and manifested itself in various forms during the Jim Crow era and as mass arrest and incarceration following the war on drugs campaign.

To pin down the causal nature of this relationship, I employ different strategies. I use a difference in difference in differences model (DDD) to estimate the heterogenous effect of historical cotton plantations on black and white drug offenders in former cotton counties after the start of the war on drugs campaign. Furthermore, I use a pooled difference in differences model to show the differential arrest rate of African Americans across counties based on historical cotton production. Next, I use land suitability for cotton plantation as an instrument for cotton plantation intensity in the county and verify that, if anything, OLS underestimates the impact of cotton plantation on the outcome variable. Finally, I limit my data to a single southern state (i.e., Arkansas) that, due to its unique geography, had a balanced mix of counties with and without intensive cotton plantation in 1860. I show that the historical impact of cotton plantation on arrest of Black offenders is strong enough to be detected even in such a small sample and in a single state.

Beyond establishing the effect, the paper identifies its mechanism. The arrest response is confined to the offenses where officers hold discretion and is absent from every victim-anchored crime; within the discretionary margin, enforcement shifted away from public-order offenses and towards drug offenses precisely when the campaign increased the returns to drug arrests. The response is organized by a pre-1982 geography of anti-Black animus measured from lynchings, Klan rosters, and ballots cast for segregation, and it fails to load on the one Klan that was not an anti-Black institution. In a formal decomposition, every observable rival channel (income, education, police capacity, the pre-existing vice-enforcement base, geography) predicts a smaller cotton-county response, not a larger one, while a direct animus index absorbs half of the effect and leaves the remainder statistically indistinguishable from zero. Independent animus measures built from Google searches two decades later and from Confederate monuments dedicated decades earlier carry the same gradient. What no county-level arrest design can deliver is the strict separation of officer-level taste from belief in the Becker tradition, which requires search-level microdata that do not exist for the rural South of the 1980s; within everything the data can measure, the evidence points to inherited racial animus, activated by federal financial incentives, as the operative mechanism. Overall, the findings of this paper indicate the importance of cotton plantation and slavery in creating beliefs, norms, and practices that became part of a region's culture and persisted beyond the civil war to affect Black individuals in more recent years. Further research in this area could shed light on the historical impact of cotton plantation and slavery on education and other labor market outcomes in the US south.

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Appendix

A The Full Offense Ladder

Table A1 reports the complete set of offense-level triple-difference estimates underlying Figure 10 and Table 7 in the text. Each row is a separate race-long triple difference with the outcome constructed identically to the drug outcomes: arrests per 100,000 county residents, race-attributed, 1974–2007, $N = 35{,}000$.

Table A1: The offense-discretion ladder: all twenty-five estimates

Offense	DDD	SE	t	Pre-1982 Black mean
High discretion (proactive, no complainant)				
Drug possession	25.78	(11.20)	2.30	7.16
Cannabis possession	2.21	(9.31)	0.24	6.70
Heroin/cocaine possession	16.35	(2.23)	7.35	0.22
Drug sales	12.29	(2.62)	4.70	3.02
Drunkenness	−363.05	(39.68)	−9.15	63.06
DUI	−81.76	(38.23)	−2.14	52.03
Liquor laws	18.18	(18.72)	0.97	6.83
Disorderly conduct	−20.83	(10.96)	−1.90	27.68
Vagrancy	−0.71	(0.77)	−0.92	0.62
Curfew and loitering	−0.91	(1.81)	−0.51	0.23
Suspicion	−7.05	(2.55)	−2.77	0.80
Prostitution	−0.08	(0.59)	−0.14	0.54
Gambling	−7.58	(1.46)	−5.19	3.07
Weapons	2.09	(2.34)	0.89	6.23
Property (victim-reported)				
Arson	−0.37	(0.31)	−1.20	0.37
Burglary	−25.95	(4.15)	−6.25	15.68
Larceny and theft	9.53	(9.27)	1.03	36.01
Motor-vehicle theft	−3.14	(1.24)	−2.53	2.15
Vandalism	10.53	(2.58)	4.09	2.98
Victim-anchored violent				
Murder	−0.71	(0.35)	−2.02	1.14
Manslaughter (negligent)	−0.28	(0.10)	−2.87	0.09
Rape	0.27	(0.30)	0.91	1.21
Robbery	0.65	(0.66)	0.99	3.64
Aggravated assault	11.94	(3.30)	3.62	15.12
Other (simple) assault	76.44	(11.55)	6.62	23.98

Clustered standard errors at county level. DDD is the coefficient on Cotton × Post × Black.

B Decomposition Details

The channel decomposition of Section 8.4 is constructed as follows. Each standardized covariate enters the race-long possession regression in levels and interacted with Post, with Black, and with Post × Black, so that every observable is allowed to generate its own differential racial enforcement response after the campaign. Following Gelbach (2016), the movement of the Cotton × Post × Black coefficient between the base specification and the full specification is allocated exactly across the added regressors by the omitted-variable-bias formula: the contribution of each regressor is the product of its coefficient in the full model and the coefficient on the treatment interaction in an auxiliary regression of that regressor on the treatment and the lower-order interaction terms. The contributions sum to the base-minus-full movement by construction, so the allocation involves no sequencing ambiguity. The channels are grouped as composition (the Black population share), economic conditions (log income per capita and 1860 output per capita), police capacity (officers and patrols per capita), the vice-enforcement base (the pre-existing drunkenness and DUI arrest rate), and geography and climate (latitude, longitude, temperature, and precipitation).

The county-level decomposition is a twofold Kitagawa–Oaxaca–Blinder decomposition with a pooled reference (Oaxaca, 1973; Blinder, 1973; Neumark, 1988). For each county I compute the long difference, the change in the Black–White possession-arrest gap from its pre-period mean to its post-period mean. The cotton minus non-cotton difference in this long difference is then split into an explained component, the part attributable to differences in pre-period county endowments evaluated at pooled coefficients, and an unexplained residual. The endowments are pre-period means of income, education, officers, patrols, the vice-arrest base, 1860 output per capita, and the geographic and climatic controls; the estimation sample is 514 counties. Standard errors for the components are computed by bootstrap over 500 county resamples. When the animus index is added to the endowment set, its detailed contribution is the component reported in the text.