Inequality and Racial Backlash: Evidence from the Reconstruction Era and the Freedmen's Bureau*

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September 11, 2024

Abstract

How do majority groups respond to a narrowing of inequality in racially polarized environments? We study this question by examining the effects of the Freedmen's Bureau, an agency created after the U.S. Civil War to provide aid to former slaves and launch institutional reform in the South. We use new historical records and an event study approach to estimate impacts of the Bureau on political economy in the South. In the decade immediately after the war, counties with Bureau field offices had reduced vote shares for Democrats, the major political party that previously championed slavery and opposed Black civil rights during Reconstruction. The decrease in the Democrat vote share dissipates alongside the decline of the Bureau's activities and federal oversight of the South. In the longer-run, we find evidence of backlash in the form of increases in several forms of racial violence, including lynchings and attacks against Black schools. This backlash extends through the twentieth century, when we find that counties that once had a Bureau field office have higher rates of second-wave and third-wave Ku Klux Klan activity and lower rates of intergenerational economic mobility. Overall, our results suggest that the initial impacts of the Freedmen's Bureau stimulated countervailing responses by White majorities who sought to offset social progress of Black Americans.

JEL Codes: J15, N31, D72, D74, I31.

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"The slave went free; stood a brief moment in the sun; then moved back again toward slavery."

—W. E. B. Du Bois in *Black Reconstruction in America* (1935)

1 Introduction

Nearly four million formerly enslaved Black Americans faced a daunting transition from slavery to freedom at the conclusion of the U.S. Civil War. The vast majority were landless and many had life experiences largely confined to the plantations and farms in which they were held in bondage. Moreover, newly emancipated Black Americans, dubbed freedmen, lived in Southern communities where many Whites held prejudiced attitudes and resented the full rights of citizenship secured for Black individuals by the Northern victory.

In response to these challenges, the Republican Congress passed legislation in 1865 that created the Freedmen's Bureau as an institution to provide material aid throughout the South and broadly promote the independence of formerly enslaved populations. Du Bois (1903) described the Bureau as "one of the most singular and interesting of the attempts made by a great nation to grapple with vast problems of race and social condition" (p. 10). In addition to distributing food rations and providing medical care, the Bureau opened schools for Black children, worked toward the creation of a free labor market, and supported the political enfranchisement of Black Americans before its operations ended in 1872. Historical scholarship over the last century has generated little consensus on whether the Bureau had any material effect (Foner, 2014), in large part because of data limitations and a lack of clear empirical benchmarks.

This paper studies the short and long-run impacts of the Freedmen's Bureau on social progress and racial backlash using an array of novel and pre-existing data sources. First, we compile a list of the locations of the Bureau's field offices that operated in selected Southern counties using newly available records from the National Archives. This provides

us with a measure of the intensity of exposure to the Freedmen's Bureau's policies that is motivated by historical narratives which suggest proximity had important impacts on the extent to which the Bureau could assist newly freed Black Americans (Mugleston and Hopkins, 1978; Rodrigue, 2001). Second, we combine the location of field offices with county-level measures of voting as well as historical records on Black voter registration and office holding during the Reconstruction Era. The combined data allow us to study important measures of social progress for Black Americans. Third, we study violence against Black Americans using canonical measures of lynching analyzed in prior work, recently compiled records on attacks against African-American schools, newly digitized Bureau records on crimes against Black Americans, and an original collection of newspaper-based measures of county-level violence.

While the data available for our study is comprehensive, an additional challenge confronting our goal of assessing the Freedmen's Bureau is concern over the selective nature in which field office locations were chosen during the Reconstruction Era. The Bureau was tasked with delivering services to freedmen immediately, and it lacked sufficient resources to address all possible areas of need. This created an environment in which there were many equally-suitable counties, only some of which received a field office. However, existing reviews of the Bureau's archives highlight that officials often selected towns and cities for offices based on their transportation infrastructure and the size of their Black population (Bean, 2016). In line with these accounts, our data show that counties with field offices were more likely to have railroad lines and larger Black population in the pre-war years. These patterns imply that naive comparisons of counties with and without Bureau offices might not isolate differences in the intensity of exposure to Bureau services if the factors driving office selection were correlated with the propensity for political engagement or racial violence in local communities.

To address concerns over selection in our analysis, we follow recent scholarship that iden-

tifies suitable comparison counties using a historically-motivated matching-based approach (e.g., Dippel and Heblich, 2021). In particular, we use six variables—1860 log White population, log free Black population, log slave population, log urban population, presence of a railroad, and distance to the nearest navigable river—and show that this approach identifies a set of comparison counties that are well-balanced not only on a broader set of untargeted 1860 characteristics, but also on changes in characteristics from 1850 to 1860. We also are able to study electoral outcome at the county-level using an event-study approach that compares counties over time. The event-study approach yields two important tests on our research design: we find no evidence of differential pre-trends before 1860, and our matching-based approach yields results that are very similar to those obtained when including county fixed effects to control for time-invariant, unobserved characteristics of counties.

Counties exposed to the Bureau experienced political progress that emerged immediately. Event study estimates reveal that the vote share for Republicans increased by 5 percentage points in the 1868 presidential election in counties exposed to Bureau field offices. This result is consistent with Black political empowerment given that the Republican Party was the major political force that supported civic rights for Black individuals during this period. As additional measures of short-term political progress, we study whether the Bureau influenced Black voter registration rates from 1867–1869 for state constitutional conventions and the number of Black civic officeholders, who mostly entered office before 1872. We find evidence of quantitatively important, positive effects: Having a Bureau office increased the Black voter registration rate by nearly 14 percentage points (18% relative to the mean level for counties without a Bureau office), increased the share of registered voters who were Black by 3 percentage points (7%), and more than doubled the number of local Black officeholders within a county. These impacts are consistent with early-century historical scholarship emphasizing that the Bureau may have fostered Black political empowerment by regulating elections and supporting important political organizations such as the Union League (Peirce, 1904; Taylor,

1926; Du Bois, 1935; Bentley, 1955).

However, the political progress facilitated by the Bureau did not last. The increase in the Republican vote share reached its peak in 1868, and subsequently dissipated until the effect was negligible by the late 1880s. This elimination of the effect of the Bureau occurs as the Bureau's activities were curtailed, the federal government exercised less oversight of the South, and Southern states enacted Jim Crow voting restrictions such as literacy tests and poll taxes (Kousser, 1974).

Given the evidence on political impacts, we next turn to consider the effects of the Bureau on racial violence. The Reconstruction Era has long been recognized as a period of intense racial conflict and intimidation (Wells, 1892, 1895; Du Bois, 1903; Raper, 1933). Most prominently, this period witnessed the emergence of the Ku Klux Klan, a White supremacist group that sought to launch a "reign of terror" on freedmen and undermine Republican political leaders of both races in the South. Our focus is on testing whether the Bureau—potentially due to its impacts on Black empowerment—amplified rates of violence against Black individuals using a range of newly available data sources.

We find widespread evidence that direct exposure to Bureau offices increased racial violence in the years following the establishment of the Bureau. As an initial approach, we rely on a novel annual county-level measure of newspaper reports of lynchings before and after the end of the Civil War. Using the within-county variation through an event-study approach, we find increases in lynching reports in treated counties in the years following the establishment of the Bureau. We also study additional measures of violence and racial intimidation that are only observed after the creation of the Bureau. This analysis reveals that counties treated with field offices had more Black lynchings, more executions of Black individuals, and experienced a higher number of attacks on Black schools—a key institution that threatened antebellum norms (Woodson, 1919).

To conclude our main analysis, we also test whether the Bureau had persistent impacts.

While the impacts on political outcomes were short-lived, the impacts on racial violence were not. We find that counties with historical Bureau field office exposure have higher amounts of racially-motivated killings from 1930–1955, more institutional racial violence in the form of executions of Black individuals from 1900 to 2003, higher amounts of second-wave and third-wave Ku Klux Klan activity (around 1915 and 1960, respectively), and a higher number of monuments built to honor the Confederacy. These impacts are quantitatively large: Counties with historical exposure to the Bureau have rates of racial violence in the 20th century that are almost twice as large as counties that were historically comparable.

Throughout our analysis, we address several possible confounds. A primary concern is that federal troops occupied parts of the South until 1877, which raises the possibility that our results could be explained by federal troops being located in the same counties as the Freedmen's Bureau. We address this concern using recently available data on the historical positions of troops (Downs, 2015*a*). While there is a positive correlation between a county having a Bureau field office and the number of troops, this association is modest (correlation coefficient: 0.17). Moreover, we show that our estimated effects of Bureau field offices are essentially unchanged after augmenting our specification to control for the number of troops in a county. These exercises rule out the most immediate concern of omitted variable bias, though the many connections between the Freedmen's Bureau and the army in terms of staffing and operations imply that these two organizations had closely linked effects. We take a similar approach to address the Freedman's Savings Bank, a distinct Reconstruction Era institution, and again find that our results are robust to controlling for bank branch presence (Celerier and Tak, 2024).

Overall, this paper most broadly contributes to the literature on the political economy of institutions (Acemoglu and Robinson, 2006, 2008; Martinez-Bravo, Mukherjee and Stegmann, 2017; Dell, Lane and Querubin, 2018). We provide comprehensive quantitative evidence of the effects of the Freedmen's Bureau—an agency endowed with both resources and

broad legal authority to reform important social and economic relationships in the immediate aftermath of the U.S. Civil War. Our results provide unique empirical evidence that is consistent with theoretical models that highlight the persistence of long-established institutions despite fundamental changes in the political environment (Acemoglu and Robinson, 2008) and related predictions from frameworks that stress backlash by majority groups in a racially stratified environment (Darity, 2022). In this way, our analysis creates new connections between modern political economy research and historical scholarship on the Reconstruction Era that has largely relied on qualitative arguments to characterize the Bureau's impacts (e.g., Du Bois, 1903, 1935; Foner, 2014; Litwack, 1980; Peirce, 1904).

Our work also contributes to an emerging literature studying oppression and racial animus in U.S. history. Recent scholarship has documented important impacts of slavery (Hornbeck and Logan, 2023), the Jim Crow system (Sacerdote, 2005; Naidu, 2010; Althoff and Reichardt, 2024), and racial violence (Cook, 2014; Logan, 2020). Broadly, our work is most closely related to contemporaneous work by Testa and Williams (2023) which provides evidence that Black lynchings increased notably when Democrats narrowly lost elections during the Reconstruction period. Collectively, the findings from Testa and Williams (2023) and our results provide some of the first credible evidence on the origins of racial violence in the aftermath of the Civil War. Relative to their analysis, we innovate by providing evidence on how the creation of a specific social institution, the Freedmen's Bureau, and its positive impacts on Black empowerment may have had the unintended consequence of increasing violence during the Reconstruction Era due to White backlash.

Finally, this paper connects with a large literature on how culture and institutions can have long-lasting legacies (Nunn, 2009). Closely related to our work are studies providing evidence that present-day Black and White voting behavior can be traced back to historical experiences with slavery and racial violence (Acharya, Blackwell and Sen, 2016; Williams, Logan and Hardy, 2021; Williams, 2022). Relative to this work, our findings on the associ-

ation between Bureau field offices and 20th century outcomes provide novel evidence that racial backlash arising from a short-lived social policy may have long-lasting impacts.

2 The Freedmen's Bureau and Potential Effects

2.1 Historical Background

After four years of fighting to preserve the Union and end slavery, Northern leaders turned their focus in 1865 to the task of rebuilding the South. Republicans in Congress and abolitionist groups were especially concerned with the challenges facing four million former slaves. The daunting tasks confronted by policymakers included the creation of a new economic system in which Black Americans were free workers and a new social system in which Black Americans were free from violence. The hostility and violence that freedmen faced throughout this period underscored the need for a broad federal intervention.[1]

President Abraham Lincoln signed legislation establishing the Freedmen's Bureau, formally known as the Bureau of Refugees, Freedman, and Abandoned Lands, a month before Confederate General Robert E. Lee surrendered in Appomattox, Virginia (Congress, 1865). Created as a division of the War Department, the law endowed the Bureau with a broad mission to provide aid to former slaves while also managing lands abandoned throughout the South. To lead the Bureau, Lincoln called on Major General Oliver Otis Howard, a man with close ties to freedmen aid societies in the North (Foner, 2014). After his successful appointment, Howard staffed the Bureau largely by relying on military officers to serve as his assistant commissioners and local agents. The Bureau legally operated during 1865–1872, although funding cuts largely reduced its operations as of January 1869.[2]

[1]Violence and incidents of racial intimidation arose from economic disputes and the assertion of White supremacy (Foner, 2014). For example, according to a Freedmen Bureau report from 1866, a white man in Grayson County, Texas shot and killed a Black man, Jack Stone, for failing to tip his hat (Registered Reports of Murders and Outrages 1866).

[2]The Freedmen's Bureau was distinct from the Freedmen's Savings Bank, which was a government-sponsored private bank that grew to have 37 branches, including several in the North, before its collapse in 1874 (Stein and Yannelis, 2020; Celerier and Tak, 2024). It is also distinct from refugee camps that existed

The Freedmen's Bureau undertook a range of operations to support its goal of improving the economic and social standing of Black Americans. The Bureau distributed millions of food rations and provided medical care to hundreds of thousands of patients (Du Bois, 1903; Lieberman, 1994).[3] In addition, the Bureau actively attempted to reform and create new institutions in the South. Bureau officials worked to establish a free labor market by overseeing contracts and resolving disputes between planters and freedmen. Promoting Black education by supporting the opening of thousands of Black schools—many of which continued after the Bureau closed—was also a major priority. At its high-point in 1870, nearly 150,000 Black children were enrolled in Bureau schools (Du Bois, 1935).

The Bureau also was active in supporting the Black civil and legal rights created by Congress in the late 1860s. To be readmitted to the Union, the first Reconstruction Act of 1867 required each former Confederate state to adopt a new constitution that guaranteed enfranchisement of Black men (Congress, 1867). This legislation stipulated that Black men had the right to vote in the election of delegates to the conventions that wrote these constitutions. Officials of the Freedmen's Bureau took important steps to enforce these provisions of the Reconstruction Act. Bureau agents advised newly enfranchised Black individuals and promised them protection (Du Bois, 1935).[4,5] Moreover, Bureau officials often served on voting boards that were tasked with visiting each electoral precinct and registering eligible voters (Peirce, 1904).

The Bureau opened field offices throughout the South to execute its operations and provide assistance to freedmen. A key consideration in choosing the location of these offices

during the Civil War (Ramos-Toro, 2024).

[3]Each ration was meant to serve one person for a week. An estimated one-third of these rations were provided to White individuals.

[4]Appendix Figure A1 provides an example of the official instructions that Bureau officers received regarding their orders to support political participation of freedmen in 1867.

[5]The Bureau was widely recognized as having a role providing protection to freedmen. Appendix Figure A2 is an illustration from Harper's Weekly discussing the Bureau in 1868. The image depicts a Bureau agent standing between crowds of White and Black Americans.

was maximizing the impact of relief efforts. As a result, Bureau offices were typically located in areas that had large Black populations and could be easily accessed by train or other modes of transportation (Bean, 2016). Freedmen often traveled to field offices to seek help mediating labor disputes or protection from violence (Foner, 2014). In addition, Bureau agents worked out of these offices to oversee local government officials and planters and to coordinate with charities and service organizations such as the American Missionary Association and the Union League (Peirce, 1904; Du Bois, 1935; Bentley, 1955; Richardson, 1999). Appendix Cprovides detail on total annual expenditures on the Bureau (from the Secretary of War's Annual Reports to the President) and personnel from the Official Registers of the United States (a resource also used by, e.g., Aneja and Xu (2022*a*,*b*); Mastrorocco and Teso (2023)) and Bureau state reports (NARA), while also highlighting the fundamental challenge of quantifying the scope of resources/staffing from these official sources. The Bureau was tasked with delivering services to freedmen immediately, and it lacked sufficient resources to address all possible areas of need. This created an environment in which there were many equally-suitable counties, only some of which received a field office.

2.2 The Potential Political and Social Effects of the Freedmen's Bureau

The potential political and social effects of the Bureau depend on how the Black and White communities in the South responded to its relief and reform efforts. On the one hand, the Bureau's efforts to register and protect Black voters should have promoted political engagement. This channel implies the Bureau should have increased support for the Republican Party, which was aligned with the abolition cause in the antebellum period and Black civil rights in the subsequent Reconstruction Era. On the other hand, the responses of the White community may have blunted or reversed any gains by Republicans in the South for at least two reasons. First, recent theories of political change predict that elites may use their wealth and influence (i.e., forms of de facto political power) to counter reforms that expand

legal and civil rights to new groups (Acemoglu and Robinson, 2006, 2008). Second, existing frameworks for understanding intergroup inequality (Darity, 2022) and rank preferences (Kuziemko et al., 2014) suggest that this type of opposition could emerge not only from elites, but also from a broader group of individuals who oppose a narrowing of economic and social disparities. Any of these factors would predict that the Bureau may have indirectly increased support for the Democratic Party, which campaigned against Black rights during Reconstruction, while also unintendedly raising levels of violence experienced by Black communities.

The broad historical narrative surrounding Reconstruction suggests that the opposing effects of the Bureau on Black empowerment and White backlash are plausible. As noted by Du Bois (1935) and Foner (2014), the Black community embraced their new voting rights created through the Reconstruction Acts. In this environment, anecdotal evidence suggests Bureau agents had clear demand for their efforts to educate and protect freedmen in the voting process. Historical accounts also emphasize how the advancement of Black individuals could threaten the "public and psychological wage" that White individuals received (Du Bois, 1935).[6] Consistent with this interpretation, a large contingent of the White community worked to undermine Black social and political progress during the Reconstruction Era. Notably, state legislatures in the South passed laws known as the Black Codes which sought to ensure that freedmen remained employed in agriculture at low wages.[7] Moreover, the Ku Klux Klan (KKK) emerged during the Reconstruction Era and launched a violent terror campaign throughout the South. Congressional testimony from witnesses and victims highlight that the Klan's violence had political motivations and sought to intimidate

[6]With respect to status, Du Bois (1935) noted that "white groups of laborers, while they received a low wage, were compensated in part by a sort of public and psychological wage. They were given public deference and titles of courtesy because they were white" (p. 700) and, "every problem of labor advance in the South was skillfully turned by demagogues into a matter of inter-racial jealousy" (p. 701).

[7]Specific provisions initially passed by some states required Black individuals sign year-long employment contracts, imposed fines on employers who offered work to an individual already under contract, and prevented Black individuals from renting land in urban areas (Foner, 2014). See also Naidu (2010).

Republican politicians, their allies, and Black voters in general (Congress, 1872).

While the general historical record suggests the Bureau could have had potential impacts on political and social outcomes, the lack of detailed data on its performance and operations within states has prevented rigorous empirical evaluation. Instead, a large body of Reconstruction and Bureau historical scholarship has offered mixed assessments based on distinct and hard-to-reconcile qualitative approaches. For example, early literature was often critical of the Bureau's effectiveness based on selective accounts of the partisan interests of some Bureau agents (e.g., Bentley, 1955). In contrast, Du Bois (1935) and some later scholars offered more optimistic assessments. Our study aims to provide clarity in debates over the Bureau by providing a comprehensive analysis of its effects on political and social outcomes based on explicit quantitative comparisons and benchmarks.

3 Data

Our analysis is based on the creation of county-level samples that combine information from archival records of the Freedmen's Bureau with data on political outcomes, violence committed against Black Americans, and county characteristics measured in the U.S. Decennial Census. This section provides details on the sources of these data. We map all variables for our analysis to 1900 county boundaries and harmonize county-level longitudinal outcomes using the crosswalks provided by Ferrara, Testa and Zhou (2024). The samples for some outcomes of interest vary due to differences in data coverage, as explained in Appendix Section B.1.

Freedmen's Bureau Field Offices: Information on the presence of Freedmen's Bureau field offices comes from records made available by the National Archives. Records from Bureau state offices provide details on the location of each field office. Appendix Table B1 provides a detailed list of the microfilm records that are the underlying basis of information on

county field offices. Field offices were located in the states that comprised the Confederate States of America as well as the District of Columbia, Maryland, and West Virginia. We focus on the former Confederate states because the institutional and political environments in these states were substantially different than in the other states with a field office.

National Elections: We study the effects of the Freedmen's Bureau on political economy using data on Presidential and Congressional elections from 1840–1900. The earlier elections in this window cover a period when slavery was a key issue prior to both the Civil War (Foner, 1996) and the establishment of the Freedmen's Bureau. Our analysis examines elections through the year 1900 to cover the period following the traditional dating of the end of the Reconstruction Era in 1877 (Foner, 2014). All county-level election data come from Clubb, Flanigan and Zingale (2006). Our first main election outcome is the share of votes cast for the Democratic Party Presidential candidate, as Democrats were the major political party that supported slavery as an institution in the South and the Democratic Party existed throughout the entirety of this period. The second main election outcome that we study is the Republican vote share beginning with 1856, the first Presidential election after the party was founded by anti-slavery politicians and activists. As secondary outcomes, we also study Democratic and Republican vote shares for Congress. Our analysis does not include South Carolina since the state legislature chose representatives for the Electoral College and there was no popular vote for Presidential elections prior to the Civil War (Grodzins and Moss, 2024). In addition, county-level elections are not defined for years in which the county did not participate in a given election. This selectively occurs during the Reconstruction Era in the election years when former Confederate states had not yet been readmitted to the Union.

Black Political Outcomes: Information on Black voter registration and the election of Black politicians during the Reconstruction Era is available from two historical sources. Hume and Gough (2008) is the source of county-level Black voter registration for the period 1867–

1869. We construct the voter registration rate by dividing these county-level registration totals by one-fourth of the total 1860 Black population (to account for the fact that only adult men could vote at this time).[8] Foner (1996) provides individual-level information on Black officeholders during the Reconstruction Era. Officeholders included in the data typically hold positions as a state house representative although the data include others who held a range of positions including aldermen or judges. Over 80% of Black politicians in these data entered office in or before 1872, the final year before the Bureau was abolished. Using the information on each official's location of service, we construct a county-level count of the number of Black officials. The data from Hume and Gough (2008) and Foner (1996) were obtained from the Logan (2020) ICPSR replication files.

Annual Reports of Lynchings: We create an original annual county-level measure of the frequency of lynchings reported in newspapers from the `newspapers.com` database. Our approach follows recent historical studies such as Testa and Williams (2023), Masera, Rosenberg and Walker (2022), Ottinger and Winkler (2022) to create annual measures of newspaper content. We link each newspaper to its county using information on historical counties from the Census Place Project (Berkes, Karger and Nencka, 2023). For a given county and year, we count the total number of pages that include the word "lynching" across all newspapers. Using this total, we define a lynching rate by dividing by the total number of newspaper pages for the given county in that year. Our analysis sample is a county-level panel covering the period January 1, 1860 through December 31, 1899. Appendix B.2 provides more detail on the construction of this measure, including examples shown in Appendix Figure B1.

Confirmed Black Lynchings: Data on confirmed Black lynching cases comes from the Historical American Lynching Data Collection Project (Project HAL; Hines and Steelwater

[8]We include both free and enslaved Black individuals in constructing total Black population in 1860. Our use of one-fourth as an adjustment factor is motivated by statistics from the 1860 Census, in which 26% of free Black individuals were men age 18 and above.

(2006)) and Seguin and Rigby (2019). These data contain individual-level information on lynching cases including the location of the crime and the race of the victim.[9] The earliest lynching in the data occurred in 1882, and the latest lynching occurred in 1936. Based on these data, we create a county-level measure of all lynchings that occurred from 1882 to 1900 to align with the period covered in our analysis of elections.

Black Executions: Data on executions of Black individuals performed under civil authority (i.e. death penalities) come from Espy and Smykla (2016). This data has also been used by Grosjean, Masera and Yousaf (2022) as a measure of institutional racial violence in a heterogeneity analysis.[10]". The data contain individual-level information on executions cases, including the age, race, name, sex, and occupation of the individual, place, jurisdiction, date, and method of execution, and the crime for which the individual was convicted. The data include executions from 1608 to the end of 2002. We aggregate executions between 1865 to the end of 1899 for our short-run analysis and from 1900 to the end of 2002 for the long-run analysis.

Attacks on Black Institutions: Scribner (2020) provides reports of 574 attacks on Black schools in former slaveholding states during the Reconstruction Era.[11] Information on these attacks was collected based on original review of newspaper reports, published findings from a congressional committee investigating the rise of the first Ku Klux Klan, and records from the Freedmen's Bureau. We construct a county-level measure of all such attacks that

[9] As stated on the Project HAL site, they follow the NAACP definition of lynching, which requires that: 1) there must be evidence that someone was killed; 2) the killing must have occurred illegally; 3) three or more persons must have taken part in the killing; and 4) the killers must have claimed to be serving justice or tradition.

[10] Tolnay and Beck (1995) note, "*Statistics on legal executions in the South clearly indicate that Blacks were especially vulnerable to the death penalty. Raper even refers to the state-sanctioned execution of Blacks as "legal lynchings" – a sentiment echoed by Harry S. Truman's Committee on Civil Rights. In the majority of cases, the formal justice system dealt harshly enough with African-Americans accused of crimes to satisfy the punitive interests of most whites. Sometimes mobs took lynch victims from police custody, however, often after conviction. Clearly, Whites lynched even after the wheels of justice had begun to grind*

[11] We drop a handful of attacks recorded as occurring during Civil War years.

occurred during the Reconstruction Era. The data cover attacks that occurred between 1865 and 1878. We create a single measure for each county of the total number of attacks on Black schools during this period.

Freedmen's Bureau Records on Violence: We create a novel dataset of crimes reported by Freedmen's Bureau officers and agents in their regular reports from 1865 to 1868. The vast majority of the reported crimes were committed against Black individuals. The underlying sources for the data are records from the National Archives' holdings for the Freedmen's Bureau. The format of the original report is a tabular or narrative description of murders or other violent crime committed (commonly referred to as an "outrage"). We used a combination of automated and manual data collection and review steps to standardize and create tabular versions of these data. The main outcome of interest is the total number of outrages reported by the Freedmen's Bureau in each county. Appendix B.3 provides additional details on our data processing and the original microfilm record.

Census, Transportation, Bank, and Army Data: Our analysis uses a variety of measures from Census Bureau data. We construct county-level measures of total Black population from 1850 to 1900 using complete count census data (Ruggles et al., 2024*b*,*a*) for the free Black population and county tabulations of census data (Manson et al., 2023) of the slave population for 1850 and 1860. We also use the complete count census data to construct measures of the share of Black individuals age 5–19 who are attending school.[12,13] Data from Haines (2005) provide the number of 1860 residents that are a slave, a free Black individual, a White individual, and living in an urban area. The data from Atack (2016) and Atack (2015) allow us to identify whether each county had a railroad in operation as of 1865 and the distance between the geographic centroid of each county and the nearest steamboat-

[12]For 1850 and 1860 we assume that no slaves attended school.

[13]The original 1890 Census records were destroyed in a fire so we cannot use individual-level data to create a measure of school attendance in this year.

navigated river as of 1865, respectively. We obtain data on Freedman's Savings Bank branch locations from Celerier and Tak (2024) who source these from digitized registers on Familysearch.org. We also use archival-based data from Downs (2015*b*) to measure the median number of Union troops in a county between 1865–1872.

Long-Run Outcomes: We use seven categories of long-run outcomes. First, we use newly compiled measures of racially motivated killings of African Americans from 1930 to 1955. These data were produced by the Civil Rights and Restorative Justice Project (CRRJ) at Northeastern University School of Law (the Burnham-Nobles CRRJ Archive (2023)).[14] In total, they capture 973 well-documented killings. Second, we use executions of Black indvididuals from 1900 to the end of 2002 (Espy and Smykla, 2016). Third, we use data on locations of the second KKK. These data come from Ang (2023) which sources it from Kneebone and Torres (2015). Based on Klan records and publications, the dataset includes the location of a chapter (known as a "klavern") and the year (ranging from 1915 to 1942) each chapter was first mentioned. Fourth, we use data on locations of the third KKK from Ang (2023), which he sources from Mazumder (2018)'s compilation of locations of active klaverns noted in House Un-American Activities Committee reports. Fifth, we use data on the presence of Confederate Monuments from the Southern Poverty Law Center's "Whose Heritage?" database, which includes 2,292 monuments in the former Confederate states that have latitudes and longitudes. Of these, roughly half note the year of dedication, ranging from the 1800s to 2021 (mean: 1915, median: 1926). Finally, our sixth and seventh measures are sourced from the Opportunity Atlas (Chetty et al., 2018). We study upward mobility as the later-life rank in the nationwide income distribution for children in the 1978–1983 birth

[14]The CRRJ notes, "In most incidents, but not all, the killings also conform to the prevailing NAACP definition of lynchings earlier discussed. In some cases, the perpetrator(s) are unknown; and in others, fewer than three known perpetrators are involved." The data includes deaths at the hands of law enforcement, though there is some ambiguity around the inclusion of these cases in the NAACP's adopted lynching definition.

cohorts whose parents were in the 25th percentile of the nationwide income distribution—this is measured using IRS administrative records on income from 2014–2015 (when the respective cohorts were aged 31-–37). We also study incarceration (based on the 2010 Census short form).

4 Empirical Strategy

This section describes the research design that we use to understand the effects of direct exposure to the Freedmen's Bureau. Our strategy relies on identifying a comparison group of counties that were not "treated" with a Bureau field office but had otherwise similar characteristics. Based on accounts of the priorities for Bureau office placement, we create a historically-informed matched sample of comparison counties using a limited set of 1860 characteristics. As a validation exercise, we report results using the matched sample that demonstrate balance on un-targeted 1860 characteristics and 1850–1860 trends. For our main analysis, we use event study models for outcomes that are measurable before and after the Bureau existed and cross-sectional models for outcomes that are only measurable afterwards. The remainder of this section provides further details on our approaches.

4.1 Identifying a Comparison Group of Counties

Figure 1 illustrates the cross-county variation in our main sample of elections by providing a map of the 341 counties that had at least one field office during the Bureau's existence and the remaining 543 counties without offices.[15] Bureau offices are spread throughout the former Confederate states. The key goal of our research design is to identify a comparison group of counties that are comparable to the locations exposed to field offices.

Table 1 compares characteristics from the 1860 Census for the counties treated with exposure to the Freedmen Bureau field offices and various samples of counties that were not

[15]The vast majority (93%) of treated counties have a single field office.

directly exposed. The statistics in columns 1 and 2 show that there are important differences in the county-level averages between the counties that had a Bureau field office and the remaining Confederate counties that did not. In line with the historical record, the summary statistics show that counties with Bureau offices had better transportation access and larger populations, particularly with respect to the size of the Black population. To facilitate the comparison of counties, columns 3 and 4 report the raw difference and an adjusted difference that accounts for state fixed effects. Overall, the large contrasts between these groups of counties highlight the difficulty inherent in any evaluation of the effects of exposure to the Bureau. Basic comparisons of the political and social outcomes of counties that did and did not have a field office may reflect both the impact of the Bureau *and* important differences in other pre-existing county characteristics.

To address concerns over the selective nature of Bureau field office placement, we rely on a matching-based approach that is motivated by historical narratives on Bureau operations discussed in Section 2 and supported by evidence in the data. This approach focuses on a set of six core controls listed in Panel A of Table 1 that are chosen based on the Bureau's demographic and transportation-related priorities for the location of field offices. For demographics, we include measures of the size of the slave and free Black populations in the pre-Bureau period using the 1860 Census. This accounts for the fact that Bureau officials often wanted to concentrate their aid and reform efforts in areas where there was greater demand for relief. We also include the size of a county's White and urban populations to account for the fact that the Bureau prioritized existing population centers to facilitate oversight of local officials and collaboration with charitable groups serving freedmen. Finally, we include two proxies for transportation conditions in counties: whether a county had access to an active rail line and the distance between each county's centroid and the nearest river. These account for the fact that agents chose locations to facilitate transportation with other government officials, volunteers from the North, and aid.

We use the core controls to construct a matched sample of similar counties with (treated) and without (control) Bureau field offices. The matched sample is created using a propensity score estimated from a probit regression of the Bureau treatment indicator, FB_c, on the set of six core Bureau office selection variables. We always match counties in the same state. Using the propensity score, we determine the set of control counties by matching (with replacement) each treated county to its five nearest neighbors in propensity score space. Appendix Figure A3 displays the estimated propensity scores, which are well balanced in the matched sample.

The remaining columns of Table 1 show that using the matched sample and reweighting based solely on the core controls improves balance results notably. Column 5 demonstrates the importance of selecting suitable comparisons by showing that the magnitude of the treatment-control difference decreases when balance is only assessed on the sample of matched counties. Importantly, the results in column 6 show that any remaining differences shrink further in magnitude and lose statistical significance after reweighting based *only* on the core demographic and infrastructure-related measures. The results for the additional county-level characteristics listed in Panel B are particularly notable as differences for economic and political characteristics that are not directly related to the core control variables are no longer significant as well.[16] Moreover, Appendix Table A1 shows that there are no significant differences in the changes of key demographic variables from 1850 to 1860 after reweighting.

4.2 Event Study Specification

We use an event study approach to study outcomes that are measurable both before and after the establishment of the Freedmen's Bureau. Motivated by the institutional context and results in Table 1, we implement this approach using the sample of counties matched

[16] Of the 19 variables considered in Table 1, only one coefficient in column 6 is statistically significant at the 5% level, which is consistent with the presence of random sampling variability.

based on 1860 characteristics. This choice eliminates counties with treatment propensity scores based on the core controls outside of the region of common support. In other words, our event study analysis estimates the effects of Bureau exposure by examining changes in outcomes over time between treatment and comparable control units.

Formally, we estimate the following model that allows us to examine the evolution of relative outcomes while controlling for fixed differences across counties and broader time trends:

$$Y_{c,t} = \sum_{j \in \mathcal{T}^{\text{Pre}}} \pi_j \; FB_c \times 1\{t = j\} + \sum_{j \in \mathcal{T}^{\text{Post}}} \gamma_j \; FB_c \times 1\{t = j\} + X_c^{1860} \beta_t + \delta_{s(c),t} + \alpha_c + \epsilon_{c,t}, \quad (1)$$

where $Y_{c,t}$ is the outcome of interest for county c in year t. The variable FB_c is an indicator for whether county c had a Freedmen's Bureau field office and $1\{t = j\}$ is an indicator for year t being equal to the index value j. The index sets $\mathcal{T}^{\text{Pre}}$ and $\mathcal{T}^{\text{Post}}$ denote the years before and after the establishment of the Bureau, respectively. The years included in these sets vary based on the outcome variable (e.g., elections occur every four years). The vector X_c^{1860} consists of the core controls in Panel A of Table 1. We allow the coefficients on these controls to vary over time to allow for the possibility that outcomes evolved differently over time in counties with different characteristics. We also include state-by-year fixed effects, $\delta_{s(c),t}$, to focus on within-state comparisons of treated and control counties, and the model includes a county fixed effect, α_c, to absorb time-invariant differences across counties. The error term is denoted by $\epsilon_{c,t}$. We weight all results based on equation (1) using the treatment-on-the-treated weight.[17] For inference, we use and report heteroskedasticity robust standard errors clustered at the county level.

The main parameters of interest, π_j and γ_j, are the coefficients on the indicators for

[17]The treatment-on-the-treated weight is equal to 1 for treated counties. For control counties, the weight is equal to $\hat{p}(X_c^{1860})/(1-\hat{p}(X_c^{1860}))$, where $\hat{p}(X_c^{1860})$ is the estimated propensity score. In some specifications, we add a vector of time-varying control variables to assess robustness.

treatment by relative time terms. For each outcome, we set the reference year by excluding from each regression the term for the closest year before the Bureau was created. For example, in our analysis of Presidential elections, the omitted reference period for all counties is 1860 as this is the final election which occurred before the creation of the Bureau. Our setup implies the estimates of π_j and γ_j represent the difference in an outcome between year j and the reference period for treated counties relative to that of the same difference over time among matched control counties.[18]

Estimates of γ_j represent the reduced form impact of being treated with greater exposure to the Bureau as measured by the presence of a field office. The prevailing historical narrative implies that the Bureau plausibly had the greatest impact on the communities closest to its field offices. For example, freedmen often approached their local field office to report assaults or seek protection from violence (Foner, 2014). Moreover, Bureau reports often noted that Black workers often would seek mediation and dispute unfair labor practices by directly appealing to Bureau field agents at their offices (e.g., Mugleston and Hopkins, 1978; Rodrigue, 2001). At the same time, traveling to even adjacent counties represented a considerable effort for freedmen due to high transportation costs and a lack of safety.

Notably, two features of our approach allow us to address the potential concern that differential trends affect the interpretation of our comparisons. First, our reliance on the matched sample ensures that we are comparing counties with similar 1860 characteristics. This potentially reduces the scope for differential trends between counties with and without field offices (Miller, 2023). Second, the estimates of π_j from the event study provide a natural and important falsification test. For example, if political outcomes were evolving similarly in treated and the matched control counties, we would expect the estimates of π_j to be small and not statistically significant.

[18]Because our measure of treatment is non-staggered, our event study and difference-in-difference estimates do not require the adjustments that may be necessary with staggered treatments (Roth et al., 2023).

While the event study results are a key focus, we also summarize results using flexible difference-in-difference specifications where the main independent variables of interest are interactions between the Freedmen's Bureau field office indicator, FB_c, and broad time indicators. For example, in our analysis of political outcomes, we group together election years 1868–1884 and 1886–1900 to pool the effects in these periods. The choice of time periods in the difference-in-difference models is based on the dynamic pattern of effects that we document in the event study specification.

4.3 Cross-Sectional Specification

A number of important outcomes can only be observed in the period following the Civil War and the establishment of Bureau offices. For example, Black enfranchisement began in 1867 (two years after the Bureau's founding) and the earliest lynchings recorded in standard archival-based records of lynching—i.e., the HAL and Seguin and Rigby databases—begin in 1882. To consider these outcomes, we rely on cross-sectional comparisons between counties with and without Bureau field offices.

Our preferred cross-sectional approach also relies on the matched sample which eliminates control counties with treatment propensity scores based on the core controls outside of the region of common support. Using this sample, we estimate reduced-form impacts by regressing a given outcome on FB_c, the indicator for the presence of a Bureau field office. Based on the patterns observed in Table 1, we use an inverse probability weighting and 1860 covariate regression adjustment to estimate the treatment on the treated effect.[19] We use the core controls listed in Panel A of Table 1 and state fixed effects for propensity score estimation and regression adjustment in our main specification. All standard errors based on this approach are heteroskedasticity-robust.

[19]This approach features the "doubly-robust" property of consistency under correct specification of either the model for the conditional mean of the outcome variable or the propensity score (e.g., Bang and Robins, 2005).

A natural concern for our matching approach is that the core controls and state fixed effects might be insufficient to fully account for unobserved, time-invariant differences between counties with and without Bureau field offices. To shed light on this issue, we conduct sensitivity analysis using event study specifications and political elections as an outcome. Specifically, we examine whether estimates of π_j and γ_j change meaningfully when we exclude county fixed effects from the model. In the event study, these county fixed effects absorb all time-invariant differences across counties. Such differences would be a key source of potential bias that could threaten comparisons between counties with and without Bureau field offices for outcomes that we do not observe prior to the establishment of the Bureau. Of course, a limitation of this robustness check is that it is only feasible for outcomes which are observed before and after the Bureau was constructed. As a result, we also explore the robustness of our cross-sectional specification to including 1860 characteristics beyond those in Panel A of Table 1.

5 Results

In this section, we present estimates of the effects of the Freedmen's Bureau on a range of political and social outcomes. We start by documenting the electoral consequences of exposure to the Bureau using event-study and difference-in-difference designs with county-level data on Presidential and Congressional elections. We find that the Democratic vote share initially declines in counties with a Freedmen's Bureau field office and that this detectable change dissipates alongside the end of Reconstruction. We then turn to a specific examination of underlying mechanisms driving these electoral dynamics by studying Black voter registration and the election of Black officeholders. Next, we document backlash by estimating impacts of the Bureau on violence against Black citizens (lynchings, school attacks, and crimes reported to the Bureau). We then consider long-run outcomes, such as measures of racial animus during the 20th century and rates of intergenerational mobility

for children raised in these counties more than a hundred years later, before concluding with an assessment of the robustness of our results.

5.1 Elections

We begin by presenting a graphical summary of the impacts of Freedmen's Bureau office locations on election outcomes. Figure 2 reports event-study estimates of equation (1) for Presidential elections spanning 1840 to 1900. As discussed in Section 4.2, these results are based on the matched sample, and each point estimate reflects the treatment-control difference between a given election year's vote share and the omitted reference year, which is the last pre-Civil War election in 1860.[20]

Panel A shows results for the Democratic vote share, for which we have five pre-treatment elections (1840 to 1856), while Panel B shows the Republican vote share, with the sole pre-treatment year corresponding to the first Presidential election (1856) after the party's founding by anti-slavery activists in 1854. Our main results are based on equation 1 and appear as a solid black line. Reassuringly, the results show no evidence of pre-trends, with all coefficients being small and statistically insignificant.

In the first post-treatment election of 1868, we see a significant increase in the Republican party vote share (5 percentage points) in counties with a Freedmen's Bureau office, relative to the comparison group. We also see a corresponding decrease in the Democratic party vote share of a similar amount. These changes attenuate starting with the 1872 election, when most of the Freedmen's Bureau operations were curtailed, and are small and indistinguishable from zero by 1892.

Table 2 presents difference-in-difference estimates for these same Presidential elections, as well as Congressional elections. Motivated by the results in Figure 2, we combine the elections

[20]Note that the 1864 election is omitted from event time due to the Civil War and secession of the Confederate states that make up our sample. Since the Freedmen's Bureau was created in 1865, the first post-treatment election is 1868 for the states that participated in this election (Mississippi, Texas, and Virginia had not yet re-entered the union at this time).

from 1868 to 1884 that had sizable negative point estimates for the Democratic vote share into an earlier period and the elections from 1886 to 1900 that had point estimates closer to zero into a later period. Column 1 of Table 2, Panel A shows that Bureau exposure results in a 3.5 percentage point drop in Democratic Presidential vote shares on average from 1868–1884. This corresponds to a 6% drop in this vote share relative to the pre-treatment matched control group mean (62%). The decrease in the Democratic vote share in Congressional elections, shown in column 2, is very similar, at 3.1 percentage points. As with the event-study, the results in columns 3 and 4 for the Republican vote share have the opposite sign and a similar magnitude. We estimate an increase in the Republican vote share of 2.4 percentage points (9%) for Presidential elections and 2.5 percentage points (11%) for Congressional elections. Together, these results suggest that the Freedmen's Bureau generated immediate electoral gains for Republicans and corresponding losses for Democrats.

The estimates for elections from 1886 to 1900 are small and indistinguishable from zero. This period coincides with the implementation of poll taxes, literacy tests, and other forms of Black voter suppression under state-level Jim Crow legislation. These state laws drove Black voter participation rates close to zero, which would have increased the Democratic vote share relative to its value in the early 1870s.[21] Ultimately, while the Freedmen's Bureau offices resulted in an increase in the relative standing of the Republican party in these treated counties, this change could not be sustained and was eroded alongside the reduction of federal oversight of the South.

As previewed above, we also report results in Figure 2 that shed light on potential concerns for a cross-sectional approach to estimating impacts of Bureau exposure. Notably, we find that the estimated impacts of Bureau exposure are very similar in a version of equation 1 that omits county fixed effects (green line). This result suggests that the historically-

[21]For example, the number of Black registered voters in Louisiana dropped from 130,344 in 1896 to 5,320 in 1900 after the enactment of the disfranchising code in that state (Lewinson, 1932, p. 81).

motivated variables we use to identify comparable control counties adequately account for unobserved, time-invariant characteristics of counties.

5.2 Black Political Participation and Representation

We next examine mechanisms that may explain both the temporary rise in Republican vote shares and any subsequent backlash. First, as described in Section 2, part of the Bureau's duties included assisting in voter registration and election oversight. As such, if the Freedmen's Bureau was more successful in protecting the voting rights of newly enfranchised freedmen in treated counties, then we should see an increase in Black voter registration. The Bureau also could have increased Black voter registration through its broader efforts, which might have increased Black men's civic engagement or perception of safety. Because Black men did not vote or hold political office in the South before the establishment of the Bureau, we use the cross-sectional approach described in Section 4.3 for this analysis.

The results in Table 3, Panel A indicate that the Freedmen's Bureau increased the Black voter registration rate. As a benchmark, column 1 reports results from an OLS regression that uses the full sample and controls for state fixed effects and the core control variables listed in Table 1, Panel A. We find that the Freedmen's Bureau increased the Black voter registration rate by 17.3 percentage points. Results are somewhat smaller but still sizable when we limit the analysis to the sample of matched counties (15.1 percentage points; column 2) and when using our preferred approach of also adjusting for differences between treated and control counties using inverse propensity score weights (13.9 percentage points; column 3). Our preferred estimate amounts to 18% of the control county mean.

A natural question is whether White voters also mobilized in treated counties, and thus whether treatment resulted in an increase in the share of registered voters who were Black. Increases in White voter registration could be due to racial threat, e.g. in the contemporary period, Enos (2016) finds that White voter turnout responds to a major shift in the presence

of Black neighbors. We find that the Freedmen's Bureau does not affect the White voter registration rate (point estimate from our preferred approach is 0.007, with a standard error of 0.016), which implies that the share of voters who are Black rose in counties with a Bureau field office. To examine this more directly, Panel B reports results from regressions where the dependent variable is the share of the voters within a county who were Black (vs. White). Our preferred estimate is an increase in the Black share of registered voters of 2.6 percentage points (7%). This increase in Black voter registration alone can explain the majority of the 3.4pp decrease in Democratic (and 2.6pp increase in Republican) vote shares shown in Table 2.

The increase in Black political participation was accompanied by an increase in representation. Table 3, Panel C examines effects on the number of Black political officeholders elected during Reconstruction (over 80% of whom entered office before 1872). All specifications point to a sizable increase in the number of Black officeholders, with our preferred estimate in column 3 implying that the existence of a Freedmen's Bureau office translated into an extra 1.3 elected officials in each county, which is over twice as large as the control group mean of 0.6.

The increase in Black voter registration is a natural explanation for the increase in the Republican vote share, as the vast majority of Black voters supported the Republican party at this time (Foner, 2014). In turn, higher participation of Black voters is a natural explanation for the increased rate of officeholding by Black politicians and officials. While these improvements in political representation were likely to be valuable to Black individuals, they may have spurred changes that were used to motivate backlash. Logan (2020) shows that Black politicians during Reconstruction increased per capita tax revenue (and also increased Black literacy and land tenancy), and Logan (2023) shows that tax increases translated into violence against Black politicians. Motivated by this evidence, we turn to a direct and comprehensive examination of whether the Freedmen's Bureau was associated with changes in

violence against Black people.

5.3 Violence

Measuring racial violence during this period is particularly difficult because many Southern White officeholders worked to undermine the civil rights of Black individuals, many Southern White citizens either implicitly or explicitly supported such violence, and modern systems to record vital statistics and crime did not exist. We address this challenge by compiling a variety of different measures, each of which has its own strengths and weaknesses.

We begin by examining media mentions of lynchings during his period, using data from newspapers.com. This approach is appealing because we can measure lynching reports *before* the opening of the Freedmen's Bureau, which enables us to use our event study and related difference-in-differences specifications. We use data covering 1860–1864 as the pre-treatment period and 1865–1900 for the post-treatment period. The key outcome of interest is a county-by-year measure of the rate of newspaper pages mentioning the keyword "lynching."

Table 4 shows that reports of lynchings were higher from 1865–1900 in counties with a Bureau field office. The estimate in column 1 implies that a field office led to 0.6 additional mentions of "lynching" per 100 pages of newspaper stories, which is a 39% increase relative to the control group mean of 1.5. When estimating separate effects for the periods in which the Republican vote share was or was not elevated (1865–1884 and 1885–1900), we see that both periods are marked by increases in mentions of lynching in treated areas. The coefficient is 84% larger in the later period, but the period-specific control mean also rises over time from 0.6 lynchings per 100 pages for 1865–1884 to 2.6 lynchings for 1890–1900, so the effect size relative to the control mean is larger in the earlier period. Appendix Figure A4 shows results from the more flexible event study specification. Notably, the increase in reported lynchings begins shortly after the Bureau is created and is positive in nearly every year after 1865.

Of course, newspaper mentions of lynchings suffer from several shortcomings, including the issue that they may be reporting on violence in other locations or periods of time and may be valenced differently across areas (e.g., condoning versus reporting on the acts).[22] In Table 5, we instead rely on datasets that document investigated incidents of violence in the form of lynchings and attacks on Black schools. Because these data only exist after the opening of the Freedmen's Bureau, we estimate cross-sectional models for this analysis. Our preferred estimate in Table 5, Panel A, shows that counties with a Bureau field office experienced 0.6 additional lynchings from 1882–1900, a 22% increase over the control county mean. Although we cannot directly compare the number of newspaper reports of lynchings with the number of verified lynchings, it is reassuring that we estimate a similar treatment effect relative to the control group mean for both outcomes.

Panel B shows that attacks on Black schools were higher in counties with Bureau offices. The estimated treatment effect from our preferred specification is an increase of 0.7 attacks, which is over twice as large as the control group mean and significant at the 1% level. In interpreting this result, it is important to note that educational opportunities for Black Americans were limited prior to the Civil War due to state laws that forbade the creation of institutions to educate slaves and harsh punishments frequently pursued by slave owners (Woodson, 1919). Against this backdrop, the creation of schools for Black individuals—which was spearheaded by the Freedmen's Bureau—represented a highly visible change in the social status of Black individuals.

During the height of the Bureau's operations from 1865 to 1868, agents collected detailed reports of murders and other violent crimes against Black citizens, an outcome we turn to in Panel C. These reports capture the broader types of terrorism enacted against Black residents, beyond the narrow category of lynching captured in the Project HAL and Seguin and Rigby datasets (see footnote 9). While Bureau agents received and investigated reports

[22] Appendix Figure B1 shows examples of both of these types of issues.

of violence in surrounding areas, a caveat to this analysis is that Bureau agents may have been more likely to record violence in areas with Bureau offices. Across all specifications, we see a large and significant increase in this measure of violence, with about 1.3 extra acts of violence in treated counties, almost doubling the total compared to control counties. Finally, in Panel D we turn to a measure of institutional racial violence: death penalties executed under civil authority (Espy and Smykla, 2016), a measure similarly interpreted by Grosjean, Masera and Yousaf (2022). We again see a significant increase of roughly 0.5 executions between 1865 to 1900, roughly a 70% increase over the control mean.

In sum, the results in Tables 4 and 5 indicate that both private and institutional violence against Black individuals rose immediately in counties with a Freedmen's Bureau field office and persisted for at least two decades. One possible explanation for this backlash is the increase in political rights exercised by Black individuals that we have documented. Another potential explanation is a change in the standing of Black individuals, and we explore this issue next.

5.4 Census Outcomes

The improved opportunities for Black Americans facilitated by the Freedmen's Bureau may have spurred shifts in Black demographic outcomes that subsequently triggered increased White hostility in treated areas. The role of White fears of even modest Black progress or demographic expansion threatening their status has long been discussed in political science, most notably with Key (1949). In sociology, Blumer (1958) emphasized relative group position as a source of prejudice, a topic covered within stratification economics (Darity, 2022), as noted in Section 2.

Appendix Table A2 studies Black demographic shifts observed in Decennial Censuses. We begin by examining the effects on the share of the population that is Black in column 1. Our motivation is that even small increases in the Black population share could trigger the

"symbolic potency" of racial threat (Key, 1949). After finding no evidence of pre-trends in the 1840 and 1850 Censuses, we see that relative to the 1860 Census, the Black population share was 1.2 percentage points higher in 1870 in counties with a Freedmen's Bureau office. The relative increase in treated counties peaks in 1880 at an increase of 2.0 percentage points (6% of the control mean) before declining in magnitude for 1890 and 1900. Notably, the Black population share remains slightly elevated in 1890 and 1900 even though there is little evidence of an impact on vote shares during this period. This is consistent with Jim Crow laws severely limiting Black individuals' ability to vote. The elevated Black population share may also have motivated the short-run increase in racial violence.

Another potential catalyst of White backlash is the prospect for improved economic outcomes and dignity signaled by improved schooling for Black children. We examine school attendance rates, as the Bureau's efforts in promoting schools are recognized by many historians (Du Bois, 1903; Taylor, 1924; Foner, 2014). Our dependent variable of interest is the share of Black children between ages 6–19 that were attending school. We see an increase in school attendance of 1.2 percentage points (11% of the control group mean) in counties with a Bureau field office in 1870. This effect is even larger in 1880, when we estimate that the school attendance rate for Black children was 1.6 percentage points higher in counties with a field office. The effect is also apparent in 1900. Evidence of positive impacts in 1880 and 1900 is not surprising because many schools created by the Bureau were operated by other organizations after its closure in 1872. A key takeaway is that the improved educational opportunities for Black children could have contributed to the observed backlash.

5.5 Long-run Impacts on 20th Century Racial Animus and Contemporary Upward Mobility

Finally, we test whether the Freedmen's Bureau and the resulting backlash during Reconstruction had lasting effects up to the present day. To do so, we first examine measures of anti-Black animus and violence that extend into the middle of the 20th century. These

measures cover a period that is well after the immediate backlash to Reconstruction, and they may be reflections and extensions of the attitudes and institutions built up in treated counties during that time. Then, we turn to a range of outcomes in the Opportunity Atlas (Chetty et al., 2018) for children raised in these counties roughly a hundred years later to examine whether the lasting legacy of the backlash still resonates in the economic and social outcomes of the present day.

First, we examine newly compiled measures of racially motivated killings of African Americans in the Jim Crow South beginning at the end of the Project HAL lynching data (1930) and extending to 1955. Table 6, Panel A examines how the presence of a Freedmen's Bureau field office affects this measure of racial violence more than 50 years later. Results are quite similar across specifications, with the final column showing that counties with a Bureau field office had 0.9 more killings. The control group mean of this variable is 0.7 killings, which means that these effects amount to a roughly 140% increase in racially motivated killings. In line with this increase in private racial violence, Panel B shows that institutional racial violence in the form of executions also rose by 1.6 executions during the Twentieth Century, a 75% increase. These results suggest that the increase in lynchings observed in treated counties in the late 1800s did not stop there but instead continued into the subsequent century.

Our next set of results expands on our analysis of the rise of racial backlash by examining whether there was a greater amount of second and third wave KKK activity in counties with a Bureau field office. The role of the first KKK in the anti-Black violence studied in Section 5.3 has been well-studied qualitatively, but the lack of historical data on their chapter locations precludes us from studying their role precisely. However, better data are available on later waves of the KKK, which could be influenced by historical Bureau offices through persistent social and political views. At its peak in 1924, the second KKK—at roughly 4 or 5 million members—had nearly 10 times the enrollment of the first KKK, before diminishing to 30,000

members by 1930 (Lay, 2005; Baudouin, 2011; Ang, 2023). The third iteration of the KKK emerged again in the 1950s and 1960s, as an amalgam of numerous initially independent local groups. While the third KKK was much smaller than the second KKK (roughly a third of the number of total chapters, and perhaps as few as 10,000 members), it had a significant presence in the South, with even more chapters than the second KKK in the geographic coverage of our data.

Table 6, Panel C shows that counties with a Bureau field office had 0.4 more second-wave KKK chapters, a 100% increase over the control group mean. This estimate is significant at the 1% level and robust to different specifications. In Panel A of Appendix Table A3 we instead use a binary indicator for whether the KKK had established a chapter by 1930, to match the way this outcome is parameterized in Ang (2023). We find that a Bureau field office increases the likelihood of a klavern by 17.3 percentage points, which is around 60% of the control group mean. While not directly comparable due to Ang (2023)'s focus on the entire U.S., this is roughly 25% of his preferred instrumental variable estimate of the effect of screenings of *Birth of a Nation*. Panel D of Table 6 shows that counties with a Bureau field office have about 0.4 more third wave KKK klaverns, roughly a 70% increase over the control mean. Again, estimates are statistically significant and robust across specifications. Panel B of Appendix Table A3 shows that we continue to see a positive effect when using a binary measure for the presence of any third wave KKK klavern. These results show the remarkable persistence of the backlash almost a hundred years after the operation of the Freedmen's Bureau.

In Panel E of Table 6 we examine the presence of Confederate monuments in a county as another long-run expression of racial attitudes, following Ang (2023) and also studied by Bazzi et al. (2023). We find a substantial effect of Bureau office locations on the establishment of these monuments, with column 3 showing an additional 1.5 monuments, almost doubling the number found in control counties.

Finally, we turn to outcomes of young adults in the present day from the Opportunity Atlas (Chetty et al., 2018). Using our state fixed effects and matching-style specifications, we compare children born between 1978 and 1983 who were raised in treated and control counties.[23] In Table 7, Panel A examines measures of intergenerational mobility for children born to parents whose income placed them at the 25th percentile of the nationwide distribution. We find that children raised in counties that long ago had a Freedmen's Bureau field office face a reduction of 0.5 percentiles in their later-life income rank.[24] Relative to the control group mean income rank (38.4 percentiles), this is roughly a 1% drop and amounts to around $1,000 of household income per year.[25] Panel B shows a 0.1 percentage point increase in incarceration, roughly a 4% increase relative to the control mean. In particular for the interpretation of these results, we note that a change in the composition of residents is one channel through which the Bureau offices could have long-run effects. Despite the Bureau only operating for a few years, the backlash produced and upheld institutions that had detrimental effects on children roughly a century later.

5.6 Robustness

This section presents several exercises to assess the sensitivity of our results. We begin with concerns related to the event study approach. While there is no evidence of detectable differences in outcomes between treated and control counties prior to the Bureau's creation,

[23] The underlying sample is not restricted to individuals who spent their entire childhood in these counties. The Opportunity Atlas weights individuals' outcomes based on the amount of time they spend in each county to produce an exposure-adjusted estimate of economic mobility.

[24] One benchmark for this 0.5 percentile impact of the Bureau can be obtained from estimates of the effects of racial segregation on upward mobility. Chyn, Haggag and Stuart (2022) use an instrumental variable strategy based on historical railroad placement (Ananat, 2011) and find that a one standard deviation increase in contemporaneous racial segregation reduces upward mobility by 4 percentiles.

[25] The Opportunity Atlas also provides a measure of teenage pregnancy (based on whether IRS records indicate that a woman claimed a dependent when they were between the ages of 13 and 19) as an additional long-run outcome. Appendix Table A4 presents an analysis of the effects of the Bureau on teenage pregnancy for children from low-income families and shows that we generally find small, positive point estimates (indicating increases in teen fertility) although these are not statistically significant in our preferred specification.

a potential issue is that post-treatment differences could partly reflect shocks to social or economic conditions unrelated to the Bureau's impact. We conclude with a discussion of additional sensitivity analysis for our cross-sectional estimates.

5.6.1 Event Study Tests

We take three approaches to address concerns about time-varying confounds. First, we explore the potential importance of federal troop levels in a county. Prior historical work highlights that the Northern military was an important institution in the South during the Reconstruction Era (Downs, 2015*a*), and the Bureau often coordinated with the military.[26] That said, the historical record also emphasizes that the Bureau had its own independent impacts. For example, in an 1866 meeting with federal generals sent to the South by President Johnson, a large crowd of nearly 1,000 freedmen in Wilmington, North Carolina unanimously told the military leaders that, if forced to choose, they would prefer to have the Bureau remain in the South over the Army (Foner, 2014). To empirically clarify the role of the military for our results, we augment our event study specification with controls for the median number of Union troops stationed in a county during 1865–1872, the Bureau's years of operations. This is achieved by including interactions between this troop measure and a full set of time indicators in equation (1).

Second, we examine the influence of the Freedman's Savings Bank, a distinct Reconstruction institution that also aimed at improving conditions for newly-emancipated Black Americans. Chartered by Congress on March 3, 1865, by 1874 the bank grew to encompass over 100,000 accounts across 17 states. While Stein and Yannelis (2020) estimate positive short-term effects of the Bureau on Black individuals' education, employment, and income, Celerier and Tak (2024) argue these effects cannot be separated from the positive selection of Bank branches into areas with higher Black wealth. The Freedman's Bank also interacted

[26]Within political science, Troop presence has been correlated with measures of political and social outcomes of Black Americans (Chacón, Jensen and Yntiso, 2021; Stewart and Kitchens, 2021).

with other social institutions, including the use of Freedmen's Bureau school teachers for outreach to gather bank deposits. Given the entanglement between the Bureau and the Bank, we take a similar approach as we do with studying troops. In particular, we use data from Celerier and Tak (2024) which contains the location of the 28 bank branches whose registers were preserved and add interactions between an indicator for a county having a bank branch and the full set of time indicators. As with the robustness check for troops, we acknowledge that the Bureau may have influenced the location of bank branches or the number of bank depositors in an area, so that controlling for location of bank branches is a potential "bad control" (Angrist and Pischke, 2009; Montgomery, Nyhan and Torres, 2018).[27]

Our third and final event study exercise focuses on the potential role of other confounding post-treatment shocks as measured by demographic or economic conditions. We follow standard approaches and augment equation (1) by including time-varying measures of county-level population and economic conditions (i.e., the log of total population, the log of manufacturing employment, the log of the total value of manufacturing, or the log of the total value of farms) from the Decennial Census.[28] Our main interest is examining whether the inclusion of these additional time-varying measures affects the Bureau event study estimates.

Appendix Figure A5 reports reassuring results from these three exercises for electoral outcomes. The figure reproduces our main estimates (black lines) alongside the Bureau event study coefficients from the augmented models. The Bureau impacts from models that include troop measures (teal squares) or bank branch locations (red squares) are nearly unchanged. The figure similarly shows that our results are even less sensitive to the inclusion of time-

[27]The unconditional correlation of Bureau offices with the presence of a bank branch is modest at 0.04, as is the correlation with the total number of depositors at 0.09. After implementing the matching and weighting, these relationships are insignificant.

[28]To do this, we associate each election with the county-level characteristics from the most recent previous Decennial Census. For example, we use the 1870 Census county characteristics for all elections that occurred during the 1870s. Of course, it is possible that during the post-treatment years some of these variables were influenced by the presence of a Freedmen's Bureau field office, and thus constitute "bad controls". Given this, we interpret our results from this robustness analysis cautiously and do not include time-varying controls in our primary specifications.

varying county-level characteristics.

We provide similar sensitivity results for our analysis of the newspaper-based measure of lynchings in Appendix Table A5. Again, columns 2 and 3 demonstrate that the results for the Bureau impacts are not substantively affected by including interactions between the post-treatment period indicators and the federal troop measure or the Freedman's Bank branch indicator. Columns 4-7 provide additional reassuring evidence that our lynching results are robust to including time-varying measures of economic and demographic measures.

5.6.2 Cross-Sectional Tests

We also conduct related robustness exercises for our matching approach. First, to assess the importance of the U.S. Army presence, we augment the cross-sectional model to include the troop measure as an additional control variable in the propensity score and regression adjustment steps. Second, we augment the model to include the Freedman's Bank branch indicator in a similar manner. Finally, we use the full set of controls listed in both Panels A and B of Table 1. The results in Appendix Table A6 are quite similar from all of these augmented models, which provides further reassurance on the validity of our results.

6 Conclusion

In this paper, we study one of the most ambitious attempts by a government to reform long-standing institutions. Specifically, we examine the political economy of the Freedmen's Bureau, an agency tasked with the broad mission of aiding and promoting the independence of former slaves after the Civil War. Our analysis shows that counties exposed to Bureau field offices saw substantially increased Black political participation and representation, as well as vote shares for the abolitionist Republican party relative to comparable counties without a Bureau office.

However, the counties treated with greater Bureau exposure exhibited a swift and se-

vere backlash against efforts to reform the pre-existing political and social institutions that promoted White supremacy. Vote shares for the pro-slavery Democratic party rebounded to their pre-Civil War levels by the end of the 1880s. Moreover, we find evidence of important social backlash in the form of elevated levels of violence enacted on the Black citizens of treated counties in the years during and after the closure of the Bureau. Our concluding analysis shows a remarkable persistence of the harmful effects of this backlash, with higher rates of Ku Klux Klan activity in treated counties in the twentieth century, elevated levels of private and institutional racial violence, and lower rates of intergenerational mobility over a century after the closure of Bureau offices.

These findings offer two main contributions. First, we relate to a large literature that has theoretically and empirically sought to explain and quantify the persistence of institutions (Acemoglu and Robinson, 2006, 2008; Martinez-Bravo, Mukherjee and Stegmann, 2017; Dell, Lane and Querubin, 2018). Indeed, the U.S. South and its particular manifestation of White supremacy via slavery and Jim Crow laws has long been highlighted as an example of institutional persistence through varied means (e.g., de jure vs. de facto power).

Relative to prior work, our analysis goes beyond demonstrating the persistence of social and political institutions. Instead, the data available for our analysis allow us to study the dynamics of a large coordinated effort to reform established institutions. We find that the reform efforts generated meaningful political and social progress in the years when the Freedmen's Bureau operated and the federal government was committed to Reconstruction. This initial progress was accompanied by a significant degree of racial violence. After the federal government's commitment to reforming the South lapsed, backlash and the spread of institutional mechanisms such as politically-motivated violence undermined and suppressed the initial progress. A leading explanation for both the initial racial violence and the longer-run backlash is that the federal government did not provide the sustained resources necessary to meet the enormous challenge of reshaping the South. In this sense, the experience of the

U.S. South differs from the radical reform associated with the French Revolution, which had lasting consequences on local areas (Acemoglu et al., 2011).

Second, our findings add clarity to historical debates on the extent and dynamics of social progress during the Reconstruction Era. Within the existing literature, whether the federal government efforts to aid and rebuild the South had any meaningful effect is an open question.[29] In the early 1900s, historians of the Dunning School described the Reconstruction period as "the darkest page in the saga of American history" because of the view that Black individuals were incapable of properly exercising the new political rights they received (Foner, 2014, p. xxvii). Du Bois (1903, 1935) provided a more balanced view of the merits and limitations of social and political reforms launched after the end of slavery, although his work was largely ignored by the profession at the time. Revisionist work in the 1960s re-interpreted essentially every aspect of the Dunning School analysis and viewed Reconstruction as "a time of extraordinary social and political progress for Blacks" (Foner, 2014, p. xx). Subsequent work in the 1970s and 1980s "questioned whether anything of enduring importance happened at all" during this time (Foner, 2014, p. xxi). More recent scholarship by Foner (2014), which follows in the tradition of Du Bois, sees "Black participation in Southern public life after 1867 [as] the most radical redevelopment" and a "massive experiment in interracial democracy without precedent" (Foner, 2014, p. xxiii). Motivated in part by these changing evaluations based largely on qualitative approaches, our work provides the first quantitative analysis that documents important intended and unintended consequences of the federal government's most prominent effort to reform Southern institutions and improve conditions for Black Americans during the Reconstruction Era.

[29] A recent contribution to this literature is Jones and Schmick (2023), who find that Reconstruction-era improvements in schools for Black children had lasting consequences.

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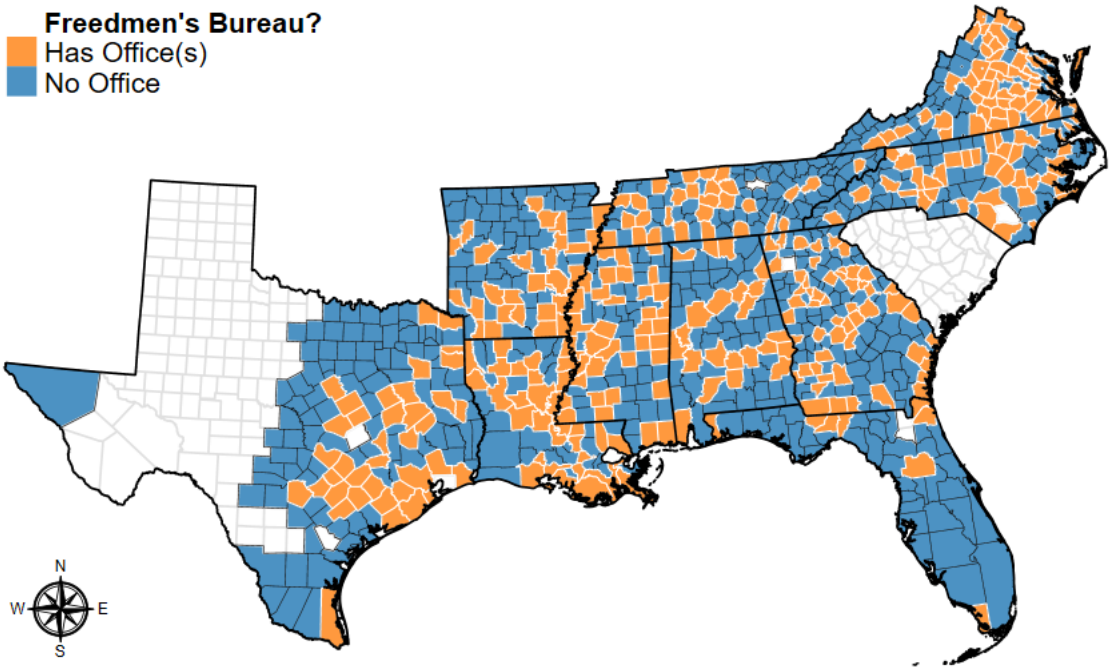
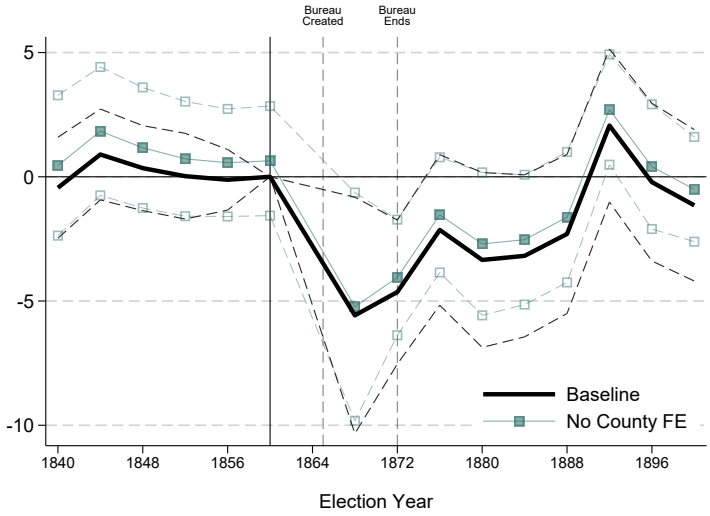
Figure 1: Map of Freedmen's Bureau Field Offices, Main Sample



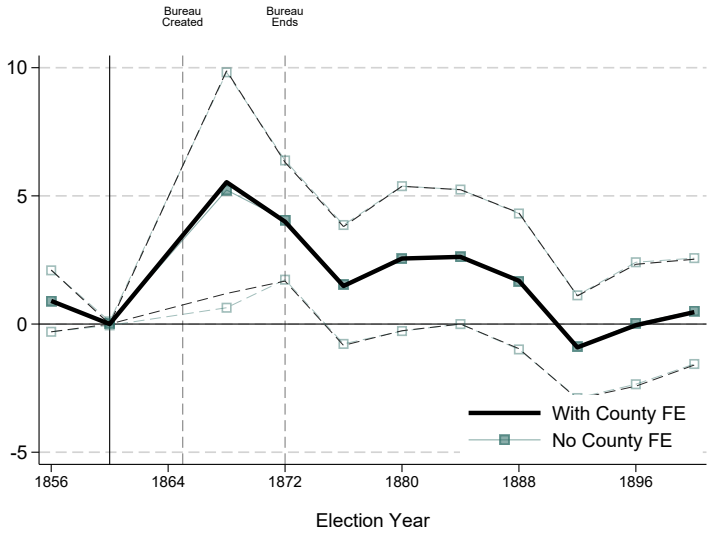
Notes: This map shows historical (1900) counties in the 11 Southern states that formed the Confederate States of America. Counties included in our election sample are shaded. Counties that had at least one Freedmen's Bureau field office are colored in orange shading, while blue shading indicates that the county had no offices. All counties in South Carolina are excluded from our election sample since there were no popular elections held prior to the Civil War. We exclude many counties in the western portion of Texas because a single sparsely populated county (Bexar) encompassed essentially all of this land during parts of the 19th century and county-level election data are not available for this area.

Figure 2: Impacts on Presidential Vote Shares, Event Study Estimates

(a) Democratic Vote Share (1840–1900)



(b) Republican Vote Share (1856–1900)



Notes: Panels A and B report event-study estimates of the impacts of having at least one Freedmen's Bureau field office on county-level Presidential Democratic and Republican Party vote shares, respectively. The baseline results are based on a regression that includes interactions between the county having any Freedmen's Bureau field office and year indicators, interactions between year indicators and state indicators, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The omitted reference year is 1860. There are no Republican vote shares before 1856 since the party was not formed until 1854. The sample is the set of elections for states that formed the Confederacy (excluding South Carolina due to its lack of a popular vote prior to the Civil War). The 1864 election is not included due to the Civil War and secession of Confederate states. The dashed black lines surrounding the point estimates represent the 95% confidence interval based on standard errors clustered at the county level.

Table 1: Summary Statistics and Covariate Balance

	(1)	(2)	(3)	(4)	(5)	(6)
	Treated Mean	Control Mean	Treated − Control Difference			
Panel A. Core Controls						
Log White Population	8.507	8.157	0.350***	0.308***	0.153***	-0.038
			(0.054)	(0.049)	(0.047)	(0.036)
Log Slave Population	8.225	6.891	1.334***	1.141***	0.654***	0.039
			(0.088)	(0.081)	(0.077)	(0.034)
Log Free Black Population	3.425	2.311	1.114***	0.728***	0.462***	-0.009
			(0.150)	(0.095)	(0.098)	(0.051)
Log Urban Population	1.129	0.323	0.806***	0.789***	0.665***	-0.229
			(0.170)	(0.181)	(0.194)	(0.168)
Has Railroad Line	0.581	0.258	0.323***	0.303***	0.231***	-0.008
			(0.033)	(0.032)	(0.035)	(0.013)
Distance to Nearest River (km)	18.850	35.035	-16.185***	-12.542***	-4.169*	0.864
			(2.803)	(2.467)	(2.335)	(0.892)
Panel B. Additional Characteristics						
Log Total Population	9.195	8.543	0.652***	0.552***	0.328***	0.013
			(0.053)	(0.048)	(0.044)	(0.032)
Log Foreign Population	4.203	3.127	1.075***	1.093***	0.781***	0.152
			(0.108)	(0.101)	(0.106)	(0.115)
Slave Population Share	0.440	0.276	0.164***	0.137***	0.091***	0.018**
			(0.014)	(0.013)	(0.013)	(0.009)
Free Black Population Share	0.017	0.008	0.009***	0.005***	0.004**	0.002
			(0.002)	(0.001)	(0.002)	(0.002)
Urban Population Share	0.055	0.012	0.043***	0.043***	0.040***	0.001
			(0.010)	(0.010)	(0.010)	(0.014)
Foreign Poulation Share	0.023	0.025	-0.001	0.004	0.005	0.003
			(0.005)	(0.004)	(0.005)	(0.004)
Log Farm Value	14.337	13.354	0.982***	0.778***	0.388***	-0.011
			(0.115)	(0.098)	(0.094)	(0.085)
Log Number of Large Farms	1.422	0.730	0.692***	0.561***	0.404***	0.024
			(0.071)	(0.067)	(0.073)	(0.070)
Log Agricultural Output	13.082	12.052	1.030***	0.826***	0.407***	-0.025
			(0.118)	(0.101)	(0.099)	(0.078)
Log Cotton Output	9.875	8.509	1.366***	1.785***	0.902***	0.319
			(0.333)	(0.233)	(0.241)	(0.259)
Log Manufacturing Output	10.288	8.900	1.387***	1.177***	0.552**	-0.404
			(0.275)	(0.261)	(0.275)	(0.268)
Log Number of Churches	2.809	2.445	0.364***	0.294***	0.118**	-0.024
			(0.059)	(0.051)	(0.051)	(0.040)
Democratic Vote Share	54.301	55.040	-0.739	-2.861***	-1.882*	0.188
			(1.349)	(0.971)	(1.044)	(1.046)
State Fixed Effects	–	–	No	Yes	Yes	Yes
Matched Sample	–	–	No	No	Yes	Yes
Reweighted	–	–	No	No	No	Yes
Counties	341	543	884	884	701	701

Notes: This table shows means and differences in means for characteristics for counties in the 1860 Census. Column 1 shows means for treated counties that had at least one Freedmen's Bureau field office, while column 2 shows this for control counties with no office. Columns 3 to 6 report coefficients from regressions of the characteristic on a treatment indicator, with robust standard errors clustered at the county level in parentheses. Column 3 reports the simple difference (i.e., OLS with no controls), column 4 reports the coefficient from a specification including state fixed effects, column 5 runs this regression using just the matched sample, and finally column 6 layers on the inverse propensity score weighting. Panel A is composed of the core control variables upon which the propensity score matching is based, while Panel B includes additional control variables that are not included in the matching set. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Impacts on Elections, Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)
	Democratic		Republican	
	President	Congress	President	Congress
Post '68-'84 × 1(Freedmen Office)	-3.480**	-3.076*	2.438**	2.539**
	(1.383)	(1.634)	(1.099)	(1.086)
Post '86-'00 × 1(Freedmen Office)	-0.309	-0.931	-0.135	0.659
	(1.343)	(1.699)	(0.951)	(1.208)
Control Mean	61.99	60.39	27.75	22.51
Observations	7397	11779	7397	11815
County FE	Yes	Yes	Yes	Yes

Notes: This table reports estimates of a difference-in-difference specification in which vote shares are regressed on interactions between the county having any Freedmen's Bureau field office and indicators for being in a post-treatment period (split into 1868 to 1884 and 1886 to 1900), interactions between these period indicators and state fixed effects, interactions between these period indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. Columns 1 and 3 report vote shares for nine post-period Presidential elections (1868, 1872, 1876, 1880, 1884, 1888, 1892, 1896, 1900), where the pre-period encompasses the 1856 and 1860 elections. Columns 2 and 4 report results for post-treatment Congressional elections (every two years from 1868 to 1900) with pre-treatment elections being 1856 and 1858. Control mean is calculated over all years. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: Impacts on Black Political Outcomes, OLS and Matched Sample Estimates

	(1)	(2)	(3)
Panel A. Black Voter Registration Rate			
1(Freedmen Office)	0.173***	0.151***	0.139***
	(0.038)	(0.035)	(0.033)
Control Mean	0.763	0.754	0.754
Observations	778	627	627
Panel B. Black Voter Share			
1(Freedmen Office)	0.0463***	0.0379***	0.0260***
	(0.009)	(0.009)	(0.008)
Control Mean	0.328	0.396	0.396
Observations	772	620	620
Panel C. Number of Black Officeholders			
1(Freedmen Office)	0.945***	0.943***	1.312***
	(0.206)	(0.213)	(0.304)
Control Mean	0.432	0.635	0.635
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports OLS regressions of Black political outcomes on an indicator for the county having any Freedmen's Bureau field office, state fixed effects, and the six core control variables listed in Panel A of Table 1. Columns 2 and 3 are limited to a sample of similar counties matched on these core variables. Column 3 applies inverse propensity score weighting. The outcome in Panel A is the Black male voter registration rate (1867 to 1869), constructed as the number of registered voters divided by one-fourth the 1860 Black population (to account for the fact that only adult men could register to vote). The outcome in Panel B is the share of the county's registered voters who are Black (vs. White) in that same time period. The outcome in Panel C is the number of Black officeholders elected in Reconstruction. Note that voter registration data is not available for all counties (see Appendix Section B.1 for details). Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4: Impacts on Newspaper Reports of Lynchings, Difference-in-Difference Estimates

Lynching Report Rate Per 100 Pages		
	(1)	(2)
Post '65-'00 × 1(Freedmen Office)	0.571***	
	(0.169)	
Post '65-'84 × 1(Freedmen Office)		0.421***
		(0.142)
Post '85-'00 × 1(Freedmen Office)		0.774**
		(0.328)
County FE	Yes	Yes
Observations	4299	4299
Control Mean	1.469	1.469

Notes: This table reports estimates of a difference-in-difference specification in which the rate of newspaper reports of lynchings (100 times the number of pages on which the term is found, divided by the total number of newspaper pages) is regressed on interactions between the county having any Freedmen's Bureau field office and indicators for being in a post-treatment period (defined as 1865–1900 in column 1 and separated as 1865–1884 and 1885–1900 in column 2), interactions between the 1865–1884 and 1885–1900 period indicators, interactions between these period indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The pre-treatment period is January 1, 1860 to December 31, 1864. Control mean is calculated over all years. Note that newspaper data is not available for all counties (see Appendix Section B.1 for details). Sample is limited to counties with at least 15 years of non-missing newspaper data. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: Impacts on Racial Violence Outcomes, OLS and Matched Sample Estimates

	(1)	(2)	(3)
Panel A. Number of Black Lynchings Recorded (1882–1900)			
1(Freedmen Office)	0.587**	0.489*	0.604**
	(0.265)	(0.273)	(0.274)
Control Mean	2.259	2.719	2.719
Observations	884	701	701
Panel B. Number of Black School Attacks (1865–1878)			
1(Freedmen Office)	0.497***	0.536***	0.718***
	(0.133)	(0.136)	(0.164)
Control Mean	0.297	0.315	0.315
Observations	884	701	701
Panel C. Number of Outrages Reported (1865–1868)			
1(Freedmen Office)	1.347***	1.267***	1.318***
	(0.432)	(0.468)	(0.477)
Control Mean	1.220	1.723	1.723
Observations	691	550	550
Panel D. Number of Black Executions (1865–1900)			
1(Freedmen Office)	0.485***	0.474***	0.542***
	(0.134)	(0.135)	(0.155)
Control Mean	0.607	0.738	0.738
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports OLS regressions of anti-Black violence outcomes on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the six core control variables listed in Panel A of Table 1. Columns 2 and 3 are limited to a sample of similar counties matched on these core variables. Column 3 applies inverse propensity score weighting. The outcome in Panel A is the total number of lynchings recorded between 1882 and 1900, while in Panel B it is the total number of attacks on Black schools (1865–1878), and in Panel C it is the total number of "outrages" (instances of violence or crimes) reported to the Freedmen's Bureau (1865–1872). Note that outrage data on murders and assaults is not available for all counties (see Appendix Section B.1 for details). The outcome in Panel D is the number of executions of Black individuals performed under civil authority from 1865 to 1900. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 6: Impacts on Twentieth Century Black Killings, Black Executions, KKK Klaverns, & Confederate Monuments

	(1)	(2)	(3)
Panel A. Number of Mid-Century Black Killings			
1(Freedmen Office)	0.930*	0.900**	0.916***
	(0.481)	(0.450)	(0.311)
Control Mean	0.633	0.661	0.661
Panel B. Number of Black Executions (1900 to 2003)			
1(Freedmen Office)	1.377***	1.427***	1.624***
	(0.494)	(0.469)	(0.488)
Control Mean	1.971	2.156	2.156
Panel C. Number of Second Wave KKK Klaverns			
1(Freedmen Office)	0.331***	0.362***	0.395***
	(0.075)	(0.074)	(0.069)
Control Mean	0.471	0.392	0.392
Panel D. Number of Third Wave KKK Klaverns			
1(Freedmen Office)	0.374***	0.356***	0.430***
	(0.118)	(0.123)	(0.128)
Control Mean	0.501	0.614	0.614
Panel E. Number of Confederate Monuments			
1(Freedmen Office)	1.074***	1.103***	1.458***
	(0.294)	(0.302)	(0.428)
Control Mean	1.409	1.565	1.565
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports OLS regressions of twentieth century outcomes on an indicator for the county having any Freedmen's Bureau field office, state fixed effects, and the six core control variables listed in Panel A of Table 1. Columns 2 and 3 are limited to a sample of similar counties matched on these core variables. Column 3 applies inverse propensity score weighting. The outcome in Panel A is the number of Black killings reported in a county in the CRRJ data (1930–1955). Panel B is the number of executions of Black people performed under civil authority from 1900 to 2003. Panel C is the number of second wave KKK klaverns ever established in a county from 1915 to 1940. Panel D is the number of third wave KKK klaverns in the 1960s. Panel E is the number of Confederate monuments erected in a county. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Impacts on Long-Run Opportunity, OLS and Matched Sample Estimates

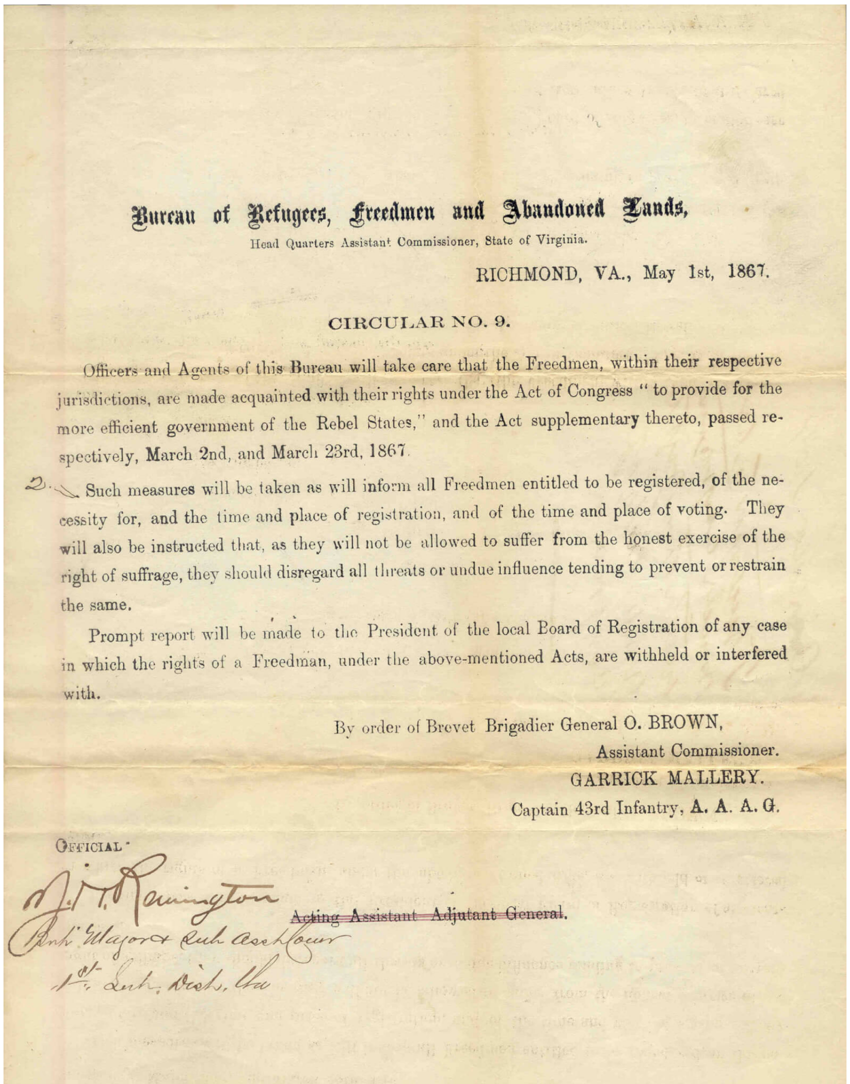
	(1)	(2)	(3)
Panel A. Upward Mobility (1978–1983 Cohorts)			
1(Freedmen Office)	-0.006***	-0.005***	-0.005**
	(0.002)	(0.002)	(0.002)
Control Mean	0.391	0.384	0.384
Panel B. Incarceration Rate (1978–1983 Cohorts)			
1(Freedmen Office)	0.001	0.001	0.001*
	(0.001)	(0.001)	(0.001)
Control Mean	0.026	0.027	0.027
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Matched Sample, Weighted	No	No	Yes

Notes: This table reports OLS regressions of long-run outcomes (taken from the Opportunity Atlas) on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the six core control variables listed in Panel A of Table 1. Columns 2 and 3 are limited to a sample of similar counties matched on these core variables. Column 3 applies inverse propensity score weighting. The outcome in Panel A measures upward mobility for children born to parents in the 25th percentile of the national income distribution. More specifically, it is the average later-life rank in the nationwide income distribution for children born from 1978–1983 using IRS administrative records on income from 2014–2015 (when the respective cohorts were aged 31—37). The outcome in Panel B is the incarceration rate for men, based on the 2010 Census short form. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Online Appendix

A Appendix Figures and Tables

Figure A1: Freedmen's Bureau Orders to Protect the Freedmen's Right to Vote (1867)



Bureau of Refugees, Freedmen and Abandoned Lands,

Head Quarters Assistant Commissioner, State of Virginia.

RICHMOND, VA., May 1st, 1867.

CIRCULAR NO. 9.

Officers and Agents of this Bureau will take care that the Freedmen, within their respective jurisdictions, are made acquainted with their rights under the Act of Congress "to provide for the more efficient government of the Rebel States," and the Act supplementary thereto, passed respectively, March 2nd, and March 23rd, 1867.

Such measures will be taken as will inform all Freedmen entitled to be registered, of the necessity for, and the time and place of registration, and of the time and place of voting. They will also be instructed that, as they will not be allowed to suffer from the honest exercise of the right of suffrage, they should disregard all threats or undue influence tending to prevent or restrain the same.

Prompt report will be made to the President of the local Board of Registration of any case in which the rights of a Freedman, under the above-mentioned Acts, are withheld or interfered with.

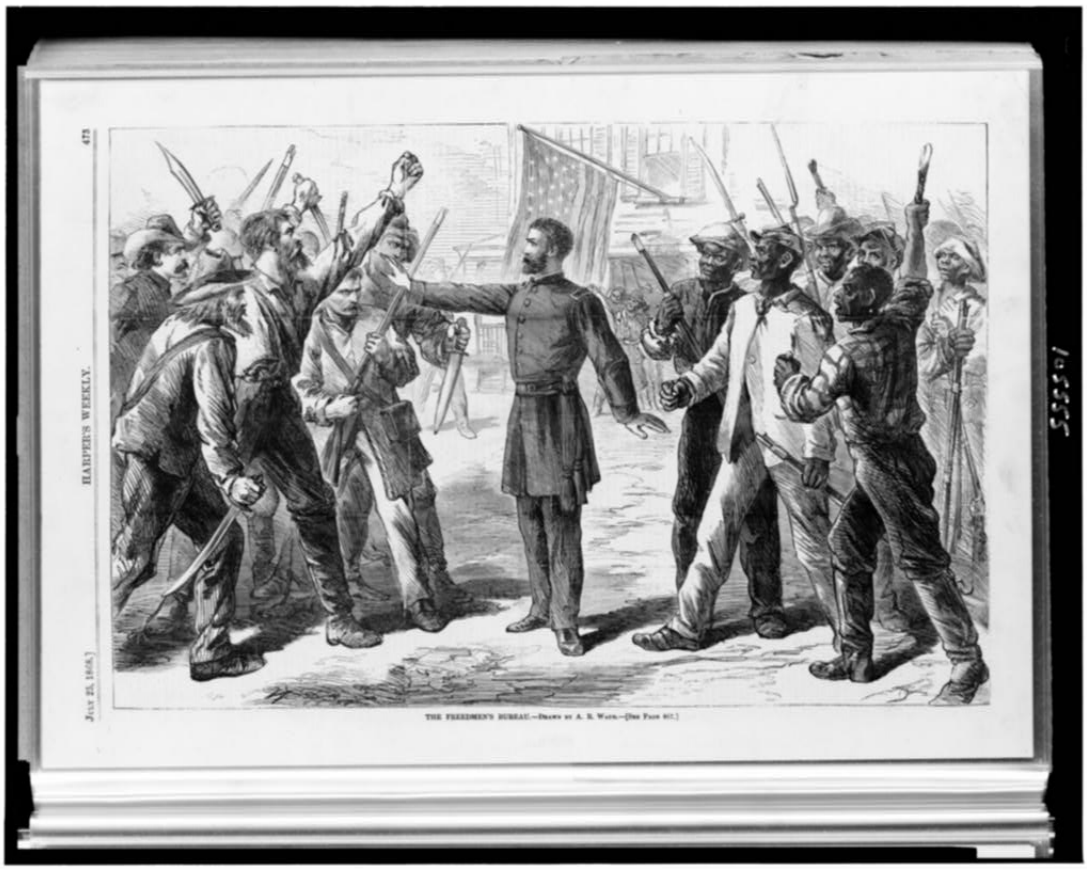
By order of Brevet Brigadier General O. BROWN,
Assistant Commissioner.
GARRICK MALLERY.
Captain 43rd Infantry, A. A. A. G.

OFFICIAL:

~~Acting Assistant Adjutant General.~~

Notes: This figure shows official Freedmen's Bureau orders made on May 1, 1867 regarding voting rights. Congress passed the First and Second Reconstruction Acts in March 1867. This legislation enfranchised Black men for the election of delegates to state constitutional conventions. Congress charged the Freedmen's Bureau with supervising elections in the Southern states. The orders reproduced in this image direct Bureau agents to inform freedmen of their voting rights and protect these rights. The image comes from the National Archives and Records Administration (Records of the Field Offices for the States of Virginia, Bureau of Refugees, Freedmen, and Abandoned Lands, 1865–1872, microfilm publication M1913, Roll 97).

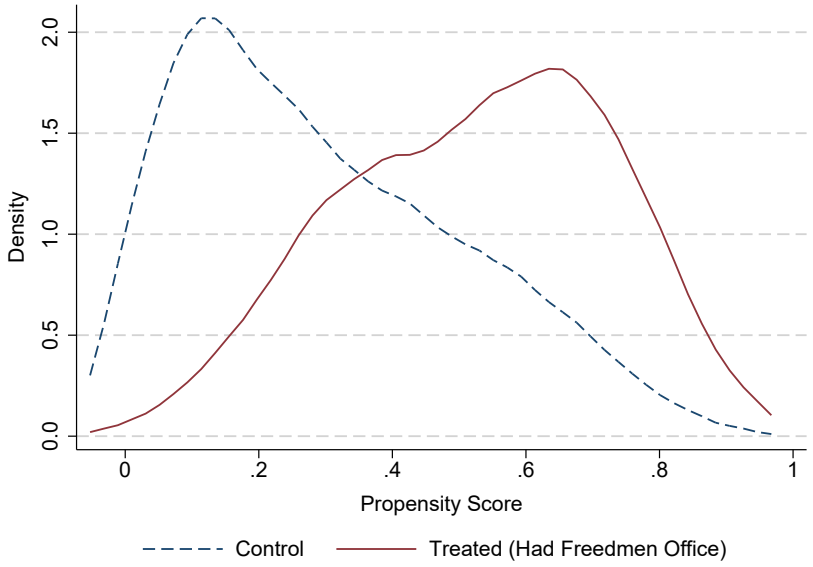
Figure A2: Freedmen's Bureau Illustration (Harper's Weekly)



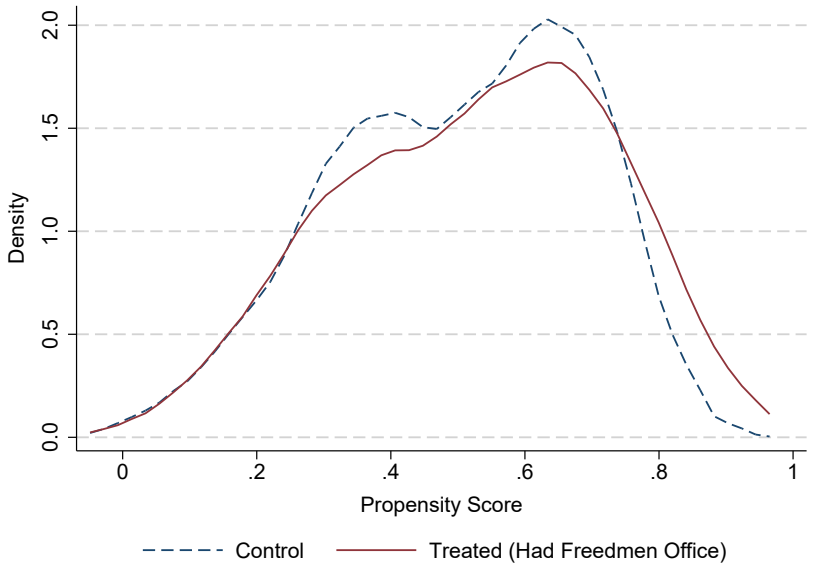
Notes: This figure reproduces an image of an agent of the Freedmen's Bureau standing between armed groups of White and Black Americans. Illustration created by A.R. Waud and appeared in the periodical Harper's Weekly, July 25, 1868 (Library of Congress).

Figure A3: Propensity Scores for Counties With and Without Bureau Offices

(a) Full Sample (N=884)

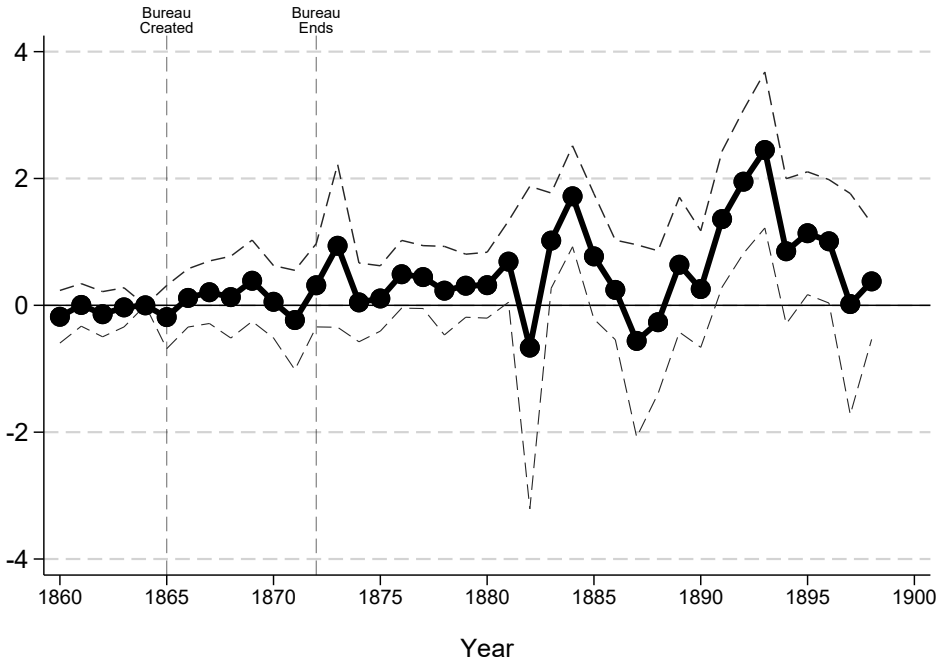


(b) Balanced Sample (N=701)



Notes: Panels A and B are kernel density plots of the propensity scores for the raw and balanced samples of counties, respectively. The propensity scores are estimated from a probit regression of the Bureau treatment indicator, FB_c, on the set of six core Bureau office selection variables listed in Table 1, Panel A. Using the propensity score, we determine the set of control counties by matching (with replacement) each treated county to its five nearest neighbors in propensity score space. All matching is conducted within each state in our sample.

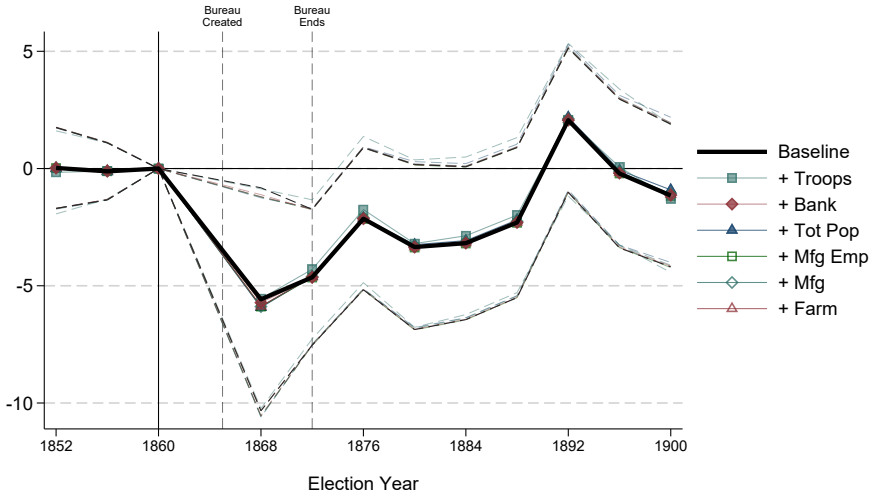
Figure A4: Impacts on Newspaper-Based Mentions of Lynchings



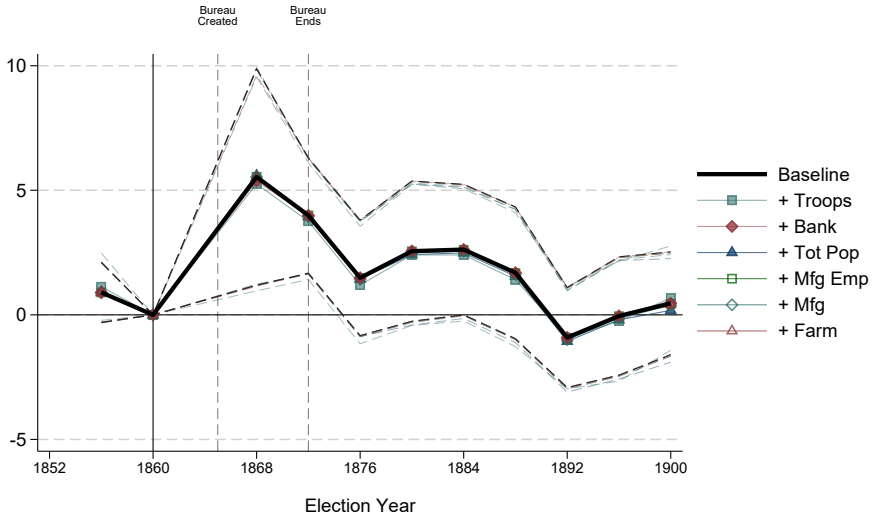
Notes: This figure reports event-study estimates (black dots) of the impacts of having at least one Freedmen's Bureau field office on county-level rates of newspaper mentions of lynching (100 times the number of pages on which the term lynching is found divided by the total number of newspaper pages). Results are based on a regression that includes interactions between the county having any Freedmen's Bureau field office and year indicators, year fixed effects, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The omitted reference year is 1864. The dashed black lines surrounding the point estimates represent the 95% confidence interval based on standard errors clustered at the county level. Sample is limited to counties with at least 15 years of non-missing newspaper data.

Figure A5: Impacts on Presidential Vote Shares, Event Study Robustness

(a) Democratic Vote Share (1852–1900)



(b) Republican Vote Share (1856–1900)



Notes: Panels A and B report event study estimates (black dots) of the effects of having at least one Freedmen's Bureau field office on county-level Presidential Democratic and Republican Party vote shares, respectively. The baseline results are based on a regression that includes interactions between the county having any Freedmen's Bureau field office and year indicators, interactions between year indicators and state indicators, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. The augmented specifications include additional controls for: interactions between year indicators and the median number of U.S. troops in a county between 1865–1872; interactions between year indicators and an indicator for the county having a Freedmen's Bank office; time-varying measures of (i) log total population, (ii) log manufacturing employment, (iii) log value of manufacturing output, or (iv) log value of farm output. Section 5.6 provides a detailed discussion of the augmented specifications. All regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The omitted reference year is 1860. For additional notes, see Figure 2.

Table A1: Summary Statistics and Covariate Balance, Supplemental Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Treated Mean	Control Mean	Treated − Control Difference			
Change in Log White Population, 1850–60	0.328	0.491	-0.163***	-0.061	-0.026	0.018
			(0.050)	(0.041)	(0.038)	(0.033)
Change in Log Slave Population, 1850–60	0.429	0.495	-0.066	0.040	0.000	-0.014
			(0.055)	(0.045)	(0.042)	(0.035)
Change in Log Free Black Population, 1850–60	0.009	-0.065	0.074	0.068	0.010	0.033
			(0.065)	(0.063)	(0.067)	(0.063)
Change in Log Urban Population, 1850–60	0.408	0.066	0.342***	0.382***	0.333***	0.108
			(0.108)	(0.117)	(0.124)	(0.134)
State Fixed Effects	–	–	No	Yes	Yes	Yes
Matched Sample	–	–	No	No	Yes	Yes
Reweighted	–	–	No	No	No	Yes
Counties	341	543	884	884	701	701

Notes: This table extends on the results from Table 1 by showing means and differences in means for additional characteristics of counties using changes observed between the 1850 and 1860 Census. Column 1 shows mean changes for treated counties with any Freedmen's Bureau field office, while column 2 shows this for control counties with no office. Columns 3 to 6 report coefficients from regressions of the characteristic on a treatment indicator, with robust standard errors clustered at the county level in parentheses. Column 3 reports the simple difference (i.e., OLS with no controls), column 4 reports the coefficient from a specification including state fixed effects, column 5 runs this regression using just the matched sample, and finally column 6 layers on the inverse propensity score weighting. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A2: Impacts on Black Population and Black Child School Attendance, Event Study Results

	Black Population Share (1)	Black Child School Attendance Rate (2)	Black Child Literacy Rate
1840 × 1(Freedmen Office)	0.006		
	(0.006)		
1850 × 1(Freedmen Office)	0.004	0.000	0.000
	(0.006)	(0.007)	(0.007)
1870 × 1(Freedmen Office)	0.012*	0.012*	0.003
	(0.006)	(0.007)	(0.007)
1880 × 1(Freedmen Office)	0.020***	0.016**	0.015**
	(0.006)	(0.007)	(0.007)
1890 × 1(Freedmen Office)	0.017***		
	(0.006)		
1900 × 1(Freedmen Office)	0.014**	0.016**	0.023***
	(0.006)	(0.007)	(0.007)
1910 × 1(Freedmen Office)	0.011*	0.014**	0.020***
	(0.006)	(0.007)	(0.007)
1920 × 1(Freedmen Office)	0.005	0.026***	0.018***
	(0.006)	(0.007)	(0.007)
Observations	6227	4898	4898
Control Mean	0.333	0.229	0.223

Notes: This table reports estimates of an event study specification in which the indicated dependent variable (Black population share, Black school attendance rate for children ages 5–19, literacy rate for Black children ages 5–19) is regressed on interactions between the county having any Freedmen's Bureau field office and year indicators, interactions between year indicators and state indicators, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The Black school attendance rate and Black child literacy rate are not measurable for 1840 or 1890. For 1850 and 1860, we construct the Black school attendance rate by assuming that all Black children who were slaves did not attend school (i.e., individuals who were slaves contribute only to the denominator of the school attendance rate), and we do the same for the Black child literacy rate. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Impacts on Binary Measures of Second and Third Wave KKK Klavern Locations

	(1)	(2)	(3)
Panel A. Second Wave KKK Klavern by 1930			
1(Freedmen Office)	0.153***	0.158***	0.173***
	(0.035)	(0.036)	(0.036)
Control Mean	0.301	0.276	0.276
Panel B. Third Wave KKK Klavern in 1960s			
1(Freedmen Office)	0.106***	0.0896**	0.106***
	(0.035)	(0.036)	(0.040)
Control Mean	0.254	0.318	0.318
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table provides alternative parameterizations of our analysis of the effects on second and third wave KKK klavern presence in a county. Panel A reports results from linear probability models where the dependent variable is an indicator for whether there were any KKK klaverns established in a county by 1930 (as an alternative to the total number of klaverns established before 1940 in Panel B of Table 6). In Panel B the dependent variable is an indicator for whether there were any third wave KKK klaverns established in a county (the binary version of the outcome in Panel C of Table 6). Robust standard errors clustered at the county level are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: Impacts on Long-Run Opportunity as Measured by Teenage Pregnancy, OLS and Matched Sample Estimates

	(1)	(2)	(3)
1(Freedmen Office)	0.009**	0.008*	0.006
	(0.004)	(0.004)	(0.004)
Control Mean	0.345	0.357	0.357
Observations	884	701	701
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports OLS regressions of long-run outcomes (taken from the Opportunity Atlas) on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the six core control variables listed in Panel A of Table 1. Columns 2 and 3 are limited to a sample of similar counties matched on these core variables. Column 3 applies inverse propensity score weighting. The dependent variable is teenage pregnancy (based on whether IRS records indicate that a woman claimed a dependent when they were between the ages of 13 and 19) for children born to parents in the 25th percentile of the national income distribution. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A5: Impacts on Newspaper Reports of Lynchings, Difference-in-Difference Estimates, Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Lynching Report Rate Per 100 Pages						
	Baseline	+ Troops	+ Bank	+ ln(Totpop)	+ ln(MfgEmp)	+ ln(Mfg)	+ ln(Farm)
Post '65-'84 × 1(Freedmen Office)	0.421***	0.403***	0.409***	0.391**	0.434***	0.461***	0.413***
	(0.142)	(0.144)	(0.140)	(0.166)	(0.162)	(0.149)	(0.148)
Post '85-'00 × 1(Freedmen Office)	0.774**	0.798**	0.776**	0.544*	0.696**	0.766**	0.745**
	(0.328)	(0.338)	(0.329)	(0.328)	(0.319)	(0.327)	(0.331)
Control Mean	1.469	1.469	1.469	1.469	1.469	1.469	1.469
Observations	4299	4299	4299	4299	4299	4299	4299

Notes: This table reports estimates of a difference-in-difference specification in which the rate of newspaper reports of lynchings (100 times the number of pages on which the term is found divided by the total number of newspaper pages) is regressed on interactions between the county having any Freedmen's Bureau field office and indicators for post-treatment periods, post-treatment period fixed effects, interactions between post-treatment period indicators and the core controls listed in Panel A of Table 1, and county fixed effects. Regressions are weighted by the ATT weights constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. The pre-treatment period is January 1, 1860 to December 31, 1864. Column 1 reports results from our baseline specification as reported in Table 4. Columns 2 and 3 report results from an augmented specification that controls for the interaction between the post-treatment period indicators and the median number of troops in a county between 1865–1872 (column 2) or an indicator for the presence of a Freedmen's Bank branch (column 3). Columns 4–7 report results from augmented specifications that control for time-varying measures of the log of total population, the log of manufacturing employment, the log of manufacturing output value, or the log of farm value from the most recent previous Decennial Census. Sample limited to counties with at least 15 years of non-missing newspaper data. Robust standard errors clustered at the county level are reported in parentheses. Statistical significance is denoted by:* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A6: Impacts on Reconstruction Era and Long-Run Outcomes, Robustness to Different Controls

Dependent variable	(1) DV Mean	(2) Baseline	(3) + Troops	(4) + Bank	(5) Full Controls
Black voter registration rate	0.754	0.139***	0.154***	0.138***	0.132***
		(0.033)	(0.035)	(0.033)	(0.034)
Black officeholders	0.635	1.312***	0.747**	1.312***	1.159***
		(0.304)	(0.293)	(0.303)	(0.312)
Black lynchings	2.719	0.604**	0.610**	0.605**	0.511*
		(0.274)	(0.288)	(0.273)	(0.283)
Black school attacks	0.315	0.718***	0.663***	0.718***	0.752***
		(0.164)	(0.161)	(0.164)	(0.163)
Outrages	1.723	1.318***	1.395***	1.345***	1.279**
		(0.477)	(0.492)	(0.475)	(0.520)
Black Executions (1865 to 1900)	0.738	0.542***	0.466***	0.542***	0.563***
		(0.155)	(0.147)	(0.155)	(0.147)
Black Executions (1900 to 2003)	2.156	1.624***	1.580***	1.631***	1.567***
		(0.488)	(0.489)	(0.484)	(0.554)
Mid-century killings	0.661	0.916***	0.752**	0.916***	0.857***
		(0.311)	(0.307)	(0.311)	(0.311)
2nd wave KKK	0.392	0.395***	0.388***	0.396***	0.380***
		(0.069)	(0.068)	(0.068)	(0.068)
3rd wave KKK	0.614	0.430***	0.444***	0.431***	0.439***
		(0.128)	(0.126)	(0.128)	(0.131)
Confederate monuments	1.565	1.458***	1.325***	1.465***	1.391***
		(0.428)	(0.446)	(0.424)	(0.424)
Income mobility	0.384	-0.005**	-0.006***	-0.005**	-0.004*
		(0.002)	(0.002)	(0.002)	(0.002)
Incarceration	0.027	0.001*	0.002**	0.001*	0.001*
		(0.001)	(0.001)	(0.001)	(0.001)

Notes: This table reports results from an augmented specification intended to assess the robustness of our matching approach. Column 1 reports the mean of each dependent variable. Column 2 reports our baseline specification. Column 3 reports results from an augmented specification in which we add the median number of troops stationed in a county during 1865–1872 (the years of the Bureau's operations) when constructing the propensity score and regression adjustment. In column 4 we augment the baseline specification by adding an indicator for a county having a Freedmen's Bank branch location in a similar manner. Column 5 instead uses the full set of controls in Panels A and B of Table 1. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B Data Appendix

B.1 Sample Details

The main sample that we use for our analysis of elections and violence is a set of 884 counties in Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Tennessee, Texas, and Virginia. As noted in Section 3, we do not include South Carolina since the state legislature chose representatives for the Electoral College and there was no popular vote for Presidential elections prior to the Civil War. In addition, several counties in the Western portion of Texas (see Figure 1) are excluded due to the lack of election data in the years preceding and following the Civil War.[30]

For three outcomes, we use alternative samples due to limited coverage of the original data sources. First, the Black voter registration data from Hume and Gough (2008) are available for 781 counties (88% of the main sample of 884 counties). Most counties from our main analysis sample that lack Black voter registration data are in Tennessee (81%). Second, the newspaper data for annual lynching reports is available for 381 counties (43% of the main sample). As discussed in Section B.2 below, we can only include county-year observations in this analysis if there is at least one newspaper publication in a given year recorded in the `newspapers.com` database. Third, the Bureau records on murders and violence ("outrages") are available for 691 counties (78% of the main sample).[31] Due to data limitations, the sample excludes all counties in the states of Arkansas, Mississippi, and Florida.

B.2 Newspaper Keyword Searches

As described in Section 3, we utilize newspaper reports of lynchings as an outcome variable in our analysis. Similar to Masera, Rosenberg and Walker (2022), Ottinger and Winkler (2022), and Testa and Williams (2023), we conduct keyword searches on `newspapers.com` to construct newspaper-year level measures of the number of pages that mention a specific keyword. Our rate of interest is equal to the number of pages that mention a keyword in each county-year divided by a proxy for the total number of pages in each county-year. This normalization ensures that we are not simply capturing the possibility that treated counties had more extensive newspapers. We follow Testa and Williams (2023) in using searches for "th*" (i.e., any word that starts with the letters "th") to proxy for the total number of pages.

To match newspapers to 1900 county codes, we utilize the city headquarters of the newspaper provided on `newspapers.com`. We use *reclink2* in Stata to match the text between the newspaper headquarters' city name and the city names in the Census Place Project data for 1900, providing us with the historical (1900) county and state FIPS codes. We find an exact match for 1,845 of the 2,015 newspapers. For the remaining 170 non-exact matches, we hand-inspect and preserve 7 (with a matching score above 0.99/1) that reflect slight spelling differences. We then use an alternative approach on the full set of 2,015 newspapers, matching the headquarters' city name to modern city names (and their lat/lon centroids) using

[30] Note that Texas was admitted into the Union in 1845. The Western portion of the state was sparsely populated during this period.

[31] See Appendix Section B.3 for further details on these records.

OpenCageGeo's reverse geocoding, and then using the *geoinpoly* command to geocode these coordinates to the NHGIS 1900 county shapefiles. The two approaches result in the exact same county match in 1,747 cases. We prioritize the Census Place Project in the 1,852 cases of exact matches (and slight misspellings) but use the latter approach for the remaining 163 newspapers whose headquarters could not be matched to the Census Place Project.

Our analysis is limited to county-year observations with a positive number of search results for "th*."[32] This is because our focus is on *rates* of mentioning a particular topic, and thus assuming a zero rate for areas without coverage would be misleading.

Appendix Figure B1 shows examples of searches on `newspapers.com` for the term *lynching*. One concern with interpreting this measure as a proxy for the extent of lynchings in a local area is that keyword searches of lynchings could either (i) report on an act in another county or (ii) describe the act in a more general way (e.g., in an opinion piece condemning the phenomenon).[33] Panel C shows an example of the latter issue, while Panel D shows the former. However, scholarship suggests these issues, while present, may not be widespread. On the former issue, Perloff (2000) notes that coverage was typically local (outside of the major national newspapers):

> "Newspapers in every region of the country provided graphic coverage of lynchings, especially those that occurred in their area. "When discussing a lynching in their particular area," noted Wright (1990) in a study of racial violence in Kentucky, "local newspapers gave all of the grisly details and, significantly, would often point out that the lynching was not the first one that had happened in their area" (p. 5). Major newspapers or metropolitan dailies sometimes described lynchings that occurred outside their geographical area (p. 5)" (p. 319).

On the latter issue, Perloff (2000) highlights the strong editorial incentives to simply report lynchings with minimal condemnation:

> "[I]n small Southern towns, the editor "ran the risk of bodily harm if he was too critical, especially if a sex crime against a female member of a good family had been punished" (Clark, 1964, p. 226). Big city newspaper editors also could reasonably expect to face violence from mobs and vigilante groups if they opposed lynching too vitriolically (Nerone, 1994). Thus, editors (that is, those who personally opposed lynching) were limited in what they could do. As human beings,

[32] We also drop newspaper-year observations in which there are more counts of certain keywords than what is found for "th*." We are left with 1,896 unique newspapers with pages, of which 1,728 newspapers (across 372 counties in all years and 363 counties after 1865) overlap with our Freedmen's Bureau data sample.

[33] We use the keyword "lynching" because it is a unique term capturing a specific type of racially-motivated violence. In terms of its etymology, Perloff (2000) notes, "*No one knows exactly where the word lynching or the related term lynch law came from, although theories abound. Cutler (1905) suggested that the term arose during the Revolutionary War when a Colonel Charles Lynch of Virginia administered punishments to Tory horse thieves. Legend has it that if the thief, after having received 39 lashes, refused to shout "Liberty forever!" he would be hung by his thumbs until he relented.*" We do not use the verb "lynch" in our search because it would return too many false positives via the common last name (e.g., as highlighted in this passage in the name of Charles Lynch).

> they worried about what would happen to themselves and to their families; as social animals, they feared social ostracism if they took too strong a stand" (p. 322).

Relatedly, one might be concerned that newspapers would want to censor coverage of such extra-judicial killings in areas where they were more prevalent. However, Perloff (2000) notes that this intuition does not apply to the period or phenomenon:

> "A late-20th-century observer schooled in hegemony or other theories that emphasize ways in which media enforce the status quo through subtle social control mechanisms (e.g., Shoemaker & Reese, 1996) might assume that the late-19th-century press refrained from covering lynchings in much the same way that the 20th-century media shied away from covering other phenomena that fell into the sphere of deviance (Hallin, 1986) such as the Holocaust or AIDS. However, this view assumes that lynchings fell outside the mainstream sphere of consensus, which, of course, they did not; public opinion and elites, particularly in Southern communities, frequently viewed lynchings as necessary mechanisms to enforce racial norms (Clark, 1964; Shapiro, 1988; Tolnay & Beck, 1995). Thus, to report on lynchings was akin to reporting on unpleasant acts of nature such as earthquakes or floods; the events were unfortunate but necessary aspects of the order of things and therefore grist for the newspaper's mill, particularly during an era in which news was developing ever more quickly into a commodity and sensational journalism was becoming a major force on the journalistic landscape (Baldasty, 1992; Dicken-Garcia, 1989). So it turns out that far from suppressing news about lynchings, newspapers embraced them, providing abundant, even graphic coverage of vigilante violence. As Clark (1964) observed in a book on the Southern country editor, "Many editors did not spare their readers' sensibilities. Whatever their motives, they [editors] wrote full, detailed accounts. Turning through many volumes for the period from 1875 to 1920 is somewhat like walking through a chamber of horrors" (p. 228).

We are able to analyze these data using a difference-in-difference specification because lynchings of Black people, while much less common, were a phenomenon during and even before the Civil War.

B.3 Historical Freedmen's Bureau Records

The National Archives and Records Administration (NARA) stores copies of records from the Freedmen's Bureau on microfilm.[34] Our analysis relies on the archived records that (i) provide information on the location of Freedmen's Bureau field offices and (ii) document reports of murders and violent crimes against freedmen (historically referred to as "outrages"). Appendix Table B1 is a list of the microfilm publications and rolls that provide information

[34] A detailed catalogue of their holdings is available at the following online URL: https://www.archives.gov/research/african-americans/freedmens-bureau (accessed March 23, 2024).

on the location of field offices. Appendix Table B2 is a list of the microfilm publications and rolls that provide information on murders and violence against freedmen.

We use a combination of automated and manual data collection steps to create a novel dataset of crimes reported by Freedmen's Bureau officers and agents in their regular reports from 1865 to 1868. The original Bureau reports have tabular or narrative formats. Appendix Figures B2 and B3 illustrate examples of tabular and narrative formatted reports, respectively. To standardize the data, we processed transcribed versions of the microfilm records in two main steps. First, we used large language models to create an initial tabular version of the Bureau record. Second, a team of researchers and research assistants manually reviewed the output to confirm that the correct number of outrages were recorded. Finally, we created a harmonized field that describes the nature of the outrage (murder, bodily harm, non-bodily harm, or other).

Importantly, the Bureau records of murders and other violent crimes contain information on the location where the reported crime was committed. The available location information is less standardized than what is available for newspapers, and so we adopt a distinct approach to identify the 1900 county in which each outrage took place. First, we modify the reported information to isolate a potential place (e.g., we change "near Athens" to "Athens" and remove words like "county" and "district"). Then, we identify exact matches between the location reported in the Freedmen's Bureau data and a list of county names in each state. We take the outrages that are not matched at this step and then attempt to construct exact matches to a list of names of current and historical names in each state from the United States Board on Geographic Names.[35] Finally, for outrages that do not have an exact match to county or place names, we attempt a fuzzy match to the list of county and place names. We select the match with the highest value of the Jaro-Winkler similarity score and require a similarity score of at least 0.8. Of the 1,652 outrages in our data for which some location information is provided, exact county name matches account for 74% of matches, exact place name matches account for 19%, fuzzy matches account for 6%, and less than 1% of outrages are not matched.

[35]We limit the sample of names in these data to those with a census, civil, or populated place feature class to reduce false positive matches.

Figure B1: *Newspapers.com* Keyword Search Results (*Lynching*)

(a) Full Search Results for Autauga County, Alabama

(b) Non-Valenced Report (Nov 29, 1898)

A LYNCHING.

Near Jones Switch Last Week.

A lynching occured in this county near Jones Switch on Thursday night of last week. There came into that section about a month ago a negro by the name of Joe White. One night last week White was caught under the bed of one of the best known and most repectable ladies of that section. The lady's husband was absent at the time, not having returned from his days duties. The negro was arrested on a warrant from the Justice of peace court, but was taken by a mob of infuriated citizens on Thursday night and made to pay the usual penalty by hanging to a limb of a tree. The lynching occurred about one mile from Jones Switch.

(c) Valenced Non-Report (Jun 16, 1896)

lot shall be turned in as cast.

LYNCHING.

We proclain our unqualified condemnation of the universal and barbarous practice well known as lynching or killing of human beings suspected or charged with crime without process of law.

NATIONAL ARBITRATION.

(d) Non-Local Report (Oct 20, 1893)

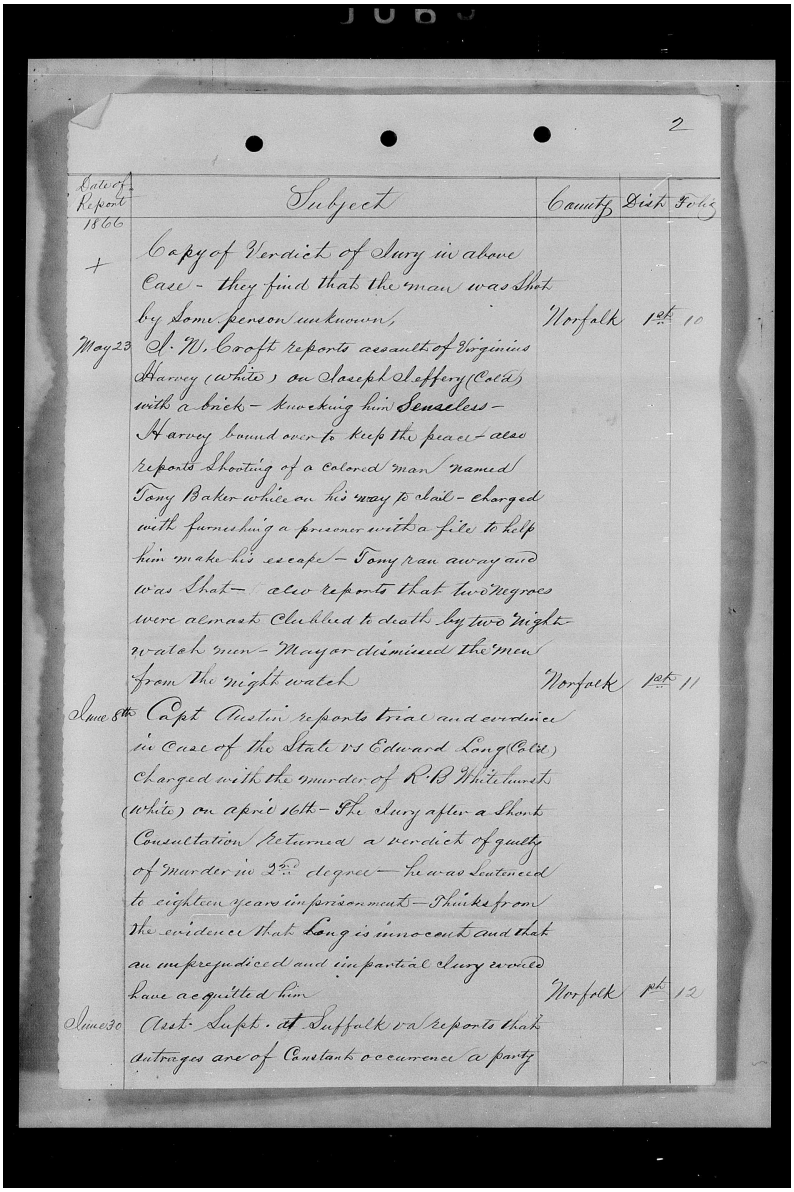
SOLDIERS OUT ON BOND.

They are Charged with the Lynching of Miner Drummond at Coal Creek.

A Knoxville, Tenn., special says: Judge Sneed, of the Knox county criminal court, rendered his decision Friday in the habeas corpus proceedings instituted before him for the release of the sixteen Coal Creek soldiers. They were charged with being implicated in the lynching of Richard Drummond, the purported assassinator of Private Laugherty at Briceville last August. He admitted the men to bond in the sum of $3,000 for the entire command. The bond was signed by Generals D. D. Anderson, John F. Fite, Major D. A. Carpenter, Captain Kellar Anderson and George W. Callahan, and the soldiers were released. The men, together with their comrades who had remained as witnesses, were paid off and disbanded. They left at once on a special train for Chattanooga, from which place they will scatter to their different homes throughout the state.

Notes: Panel A shows a graphical user interface search for "lynching" in Autauga County, Alabama from January 1, 1860 to December 31, 1899, which returned 101 newspaper pages containing the term. Autauga County is in the control group in our sample (i.e., it had no Freedmen's Bureau field office). Panels B–D show examples of the search results. Panel B shows a screenshot of a lynching report that occurred in Autauga County (at Jones Switch) in the week before the newspaper printing. Panels C and D show examples of the limitations of this broad search. Namely, Panel C shows a valenced/opinion item condemning lynchings, while Panel D discusses the release of men charged with lynching in a different county (Knox County, Tennessee).

Figure B2: Example of Freedmen's Bureau Reports of Murders and Violence (Part 1)

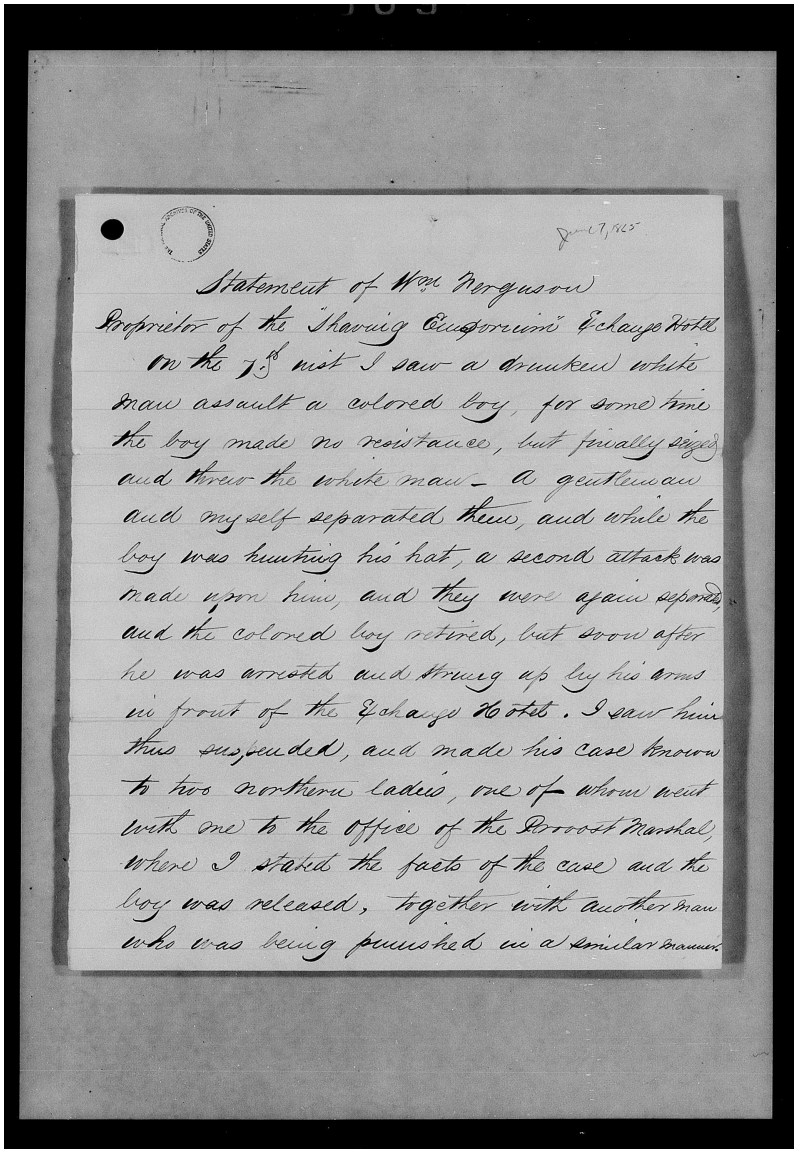


2

Date of Report 1866	Subject	County	Dist	Folio
+	Copy of Verdict of Jury in above Case - they find that the man was Shot by some person unknown,	Norfolk	1st	10
May 23	A. W. Croft reports assault of Virginius Harvey (white) on Joseph Jeffery (Colrd) with a brick - knocking him Senseless - Harvey bound over to keep the peace - also reports Shooting of a colored man named Tony Baker while on his way to Jail - charged with furnishing a prisoner with a file to help him make his escape - Tony ran away and was Shot - also reports that two Negroes were almost Clubbed to death by two Night watch men - Mayor dismissed the men from the Night watch	Norfolk	1st	11
June 8th	Capt Austin reports trial and evidence in Case of the State vs Edward Long (Colrd) charged with the murder of R. B. Whitehurst (white) on April 16th - The Jury after a Short Consultation Returned a verdict of guilty of murder in 2nd degree - he was Sentenced to eighteen years imprisonment - Thinks from the evidence that Long is innocent and that an unprejudiced and impartial Jury would have acquitted him	Norfolk	1st	12
June 30	Asst. Supt. at Suffolk Va reports that outrages are of Constant occurrence a party			

Notes: This image is a copy of a Freedmen's Bureau record on reports of murders and violence against Black Americans. The format of the record has a table structure. The image is from a collection for the Records of the Assistant Commissioner for the State of Virginia, Bureau of Refugees, Freedmen, and Abandoned Lands, 1865-1869 (Publication M1048, Roll 59).

Figure B3: Example of Freedmen's Bureau Reports of Murders and Violence (Part 2)



Statement of Wm Ferguson
Proprietor of the Shaving Emporium Exchange Hotel
On the 7th inst I saw a drunken white
man assault a colored boy, for some time
the boy made no resistance, but finally siezed
and threw the white man - a gentleman
and my self separated them, and while the
boy was hunting his hat, a second attack was
made upon him, and they were again separated
and the colored boy retired, but soon after
he was arrested and strung up by his arms
in front of the Exchange Hotel. I saw him
thus suspended, and made his case known
to two northern ladies, one of whom went
with me to the office of the Provost Marshal,
where I stated the facts of the case and the
boy was released, together with another man
who was being punished in a similar manner

Notes: This image is a copy of a Freedmen's Bureau record on reports of murders and violence against Black Americans. The format of the record is a narrative report. The image is from a collection for the Records of the Assistant Commissioner for the State of Virginia, Bureau of Refugees, Freedmen, and Abandoned Lands, 1865-1869 (Publication M1048, Roll 59).

Table B1: List of National Archives Records Used for Freedmen's Bureau Field Office Locations

Microfilm Publication	Rolls
M1869	14, 15
M1900	9, 11, 13, 15, 20, 21, 23, 27, 28, 31, 33, 34
M1901	6, 7, 8, 10, 11, 12, 13, 14, 16, 17, 18, 19, 22
M1902	13, 20
M1903	34, 39, 41, 45, 46, 50, 51, 52, 54, 55, 57, 58, 60, 61, 63, 64, 65, 66, 67, 70, 71, 72, 73, 74, 75, 77, 86, 87, 89, 90
M1904	87, 92, 93, 94, 96, 97, 98, 113, 123, 125, 126, 128, 133
M1905	52, 54, 58, 59, 64, 67, 68, 70, 71, 74, 79, 80, 81, 83, 84, 85, 87, 89, 90, 93, 96, 98, 100, 102, 103, 104, 105, 106
M1906	39, 41, 42
M1907	11, 12, 15, 16, 17, 18, 19, 20, 26, 29, 30, 32, 40, 41, 42, 43, 64
M1909	4, 5, 7, 10, 11, 12, 14, 15, 18, 22, 23, 25, 27, 28, 29, 31, 32, 33, 47, 48, 50, 53, 54, 56, 63, 64, 65
M1910	31, 36, 44, 51, 56, 62, 63, 64, 65, 67, 71, 75, 78, 84, 85, 86, 87, 90, 91, 94, 97, 98, 99, 100, 102, 104, 106
M1911	13, 18, 19, 21, 27, 77, 78, 86, 88, 89
M1912	12, 13, 14, 15, 16, 19, 21, 22, 23, 24, 25, 26, 27, 28
M1913	40, 56, 57, 58, 59, 61, 62, 64, 65, 67, 68, 69, 70, 71, 72, 73, 75, 76, 82, 89, 90, 97, 98, 99, 100, 101, 102, 103, 104, 105, 106, 110, 111, 112, 114, 131, 132, 149, 150, 160, 162, 172, 173, 176, 177, 178, 179, 180, 192, 193, 199

Notes: Records of the Freedmen's Bureau have been stored as microfilm by the National Archives. This table lists the microfilm publication and rolls used to obtain information on the counties of field offices.

Table B2: List of National Archives Records Used for Freedmen's Bureau Reports of Murders and Violence

Microfilm Publication	**Rolls**
M809	29
M798	32
M1027	34
M843	33
M869	34
M999	34
M821	32
M1048	59

Notes: Records of the Freedmen's Bureau have been stored as microfilm by the National Archives. This table lists the microfilm publication and rolls used to obtain information on murders and violence against Black Americans.

C Additional Description of Freedmen's Bureau Personnel and Expenditures

The full scope of the Freedmen's Bureau in terms of manpower and spending is difficult to quantify using official sources. As noted in Section 2, the Bureau helped coordinate (and utilized the resources of) other overlapping organizations during Reconstruction, including the American Missionary Association (in its schooling mission) and the Union League (in its political mission). It also relied on volunteers in its core functions. For example, in the administration of justice, the Bureau set up federal tribunals typically composed of three judges, only one of which would be a Bureau employee (agent), with the others being representatives of freedmen and planters.[36] As another example, the 1869 report of the South Carolina commissioner notes that "*honest and carefully selected agents, who would serve without salary, were to be appointed to superintend the distribution*" of food aid. Moreover, in the early years of operation, some agents were compensated by fees rather than being salaried employees. This appears to be the case in Georgia, as documented in the 1868 Report of the Commissioner, "*Owing to the limited number of officers under his command, and the impossibility of providing out of the bureau appropriation for a vast number of civilian agencies, he appointed agents from among the resident whites. These were to serve without salary, and to rely for remuneration upon fees they were authorized to exact from employers and freedmen for the witnessing and approval of contracts.*" The same report notes that the Commissioner then abolished this system, partly due to the "*suspicious manner in which it was coming to be regarded by freed people from whom fees were exacted.*" A similar dynamic played out in Tennessee as noted in their 1868 Report, "*By circular from this office, of January 24, the fee system was abolished, and agents were afterwards paid a regular salary. A better state of feeling resulted among the people, as agents no longer made charges for services rendered.*"

Nonetheless, to provide additional context, we first provide information on total spending by the War Department, as reported by General Oliver O. Howard in the Secretary of War's Annual Reports to the President. Table C1 compiles annual expenditures split by salaries of agents, salaries of clerks, and total expenditures, with each report capturing spending in roughly the prior fiscal year (e.g., from the 1867 report, "*my disbursing officer's report of the financial affairs of this bureau for the period of eleven (11) months ending August 31, 1867*").

Table C1 shows that spending on Bureau staff peaked in the fiscal year prior to October 24, 1868 at $864,743.91, and dwindled down to just $45,809.72 by October 20, 1871, at which point the Commissioner noted, "*The number of officers, agents, and clerks now on duty is*

[36] Similarly the size and scope of this court system also fluctuated across space and time. In President Andrew Johnson's successful veto of the re-authorization of the Freedmen's Bureau in 1866 (later followed by a House bill that successfully overturned Johnson's second veto vote), he complained of, "an *almost countless number of agents* established in every parish or county in nearly a third of the States of the Union, over whose decisions there is to be no supervision or control by the Federal courts." Many of these courts were shut down when State courts began to allow Black people to testify. For example, in January 1866, the Tennessee General Assembly passed a bill allowing Black testimony in state courts, which led the Bureau to close its courts, believing state courts would handle cases fairly. However, once it became clear that Black testimony was not being effectively enforced, the Bureau reopened the courts to continue protecting freedmen's rights.

Table C1: Annual Expenditures by War Department on Freedmen's Bureau

Date of Report	Salaries of Agents/Commissioners	Salaries of Clerks	Total Expenditures
November 1, 1866*	$9,657.73	$27,115 48	$88,190.70*
November 1, 1867	$184,336.19	$337,085.25	$3,279,497.43
October 24, 1868	$455,473.06	$409,270.85	$3,814,425.19
October 20, 1869	$278,691.07	$329,049.05	$2,170,211.52
October 20, 1870	$65,809.34	$91,805.37	$1,535,747.91
October 20, 1871	$23,886.08	$21,923.64	$362,263.42

Notes: Records of the Freedmen's Bureau expenditures are compiled from Annual Reports of the Secretary of War to the President from 1866 (page 715), 1867 (page 656), 1868 (page 1022), 1869 (page 522), 1870 (page 325), and 1871 (page 453); https://catalog.hathitrust.org/Record/000078451. All reports disaggregate salaries of clerks from other employees; however, between 1866 to 1868 the other category of employees is "Salaries of Commissioners and Sub-Assistant Commissioners" while from 1869 to 1871 it is simply "Salaries of Agents". *Unlike the other years, 1866 expenditure tabulations in the report are incongruous with text by the Commissioner reporting monthly spending in that year as roughly $350,000.

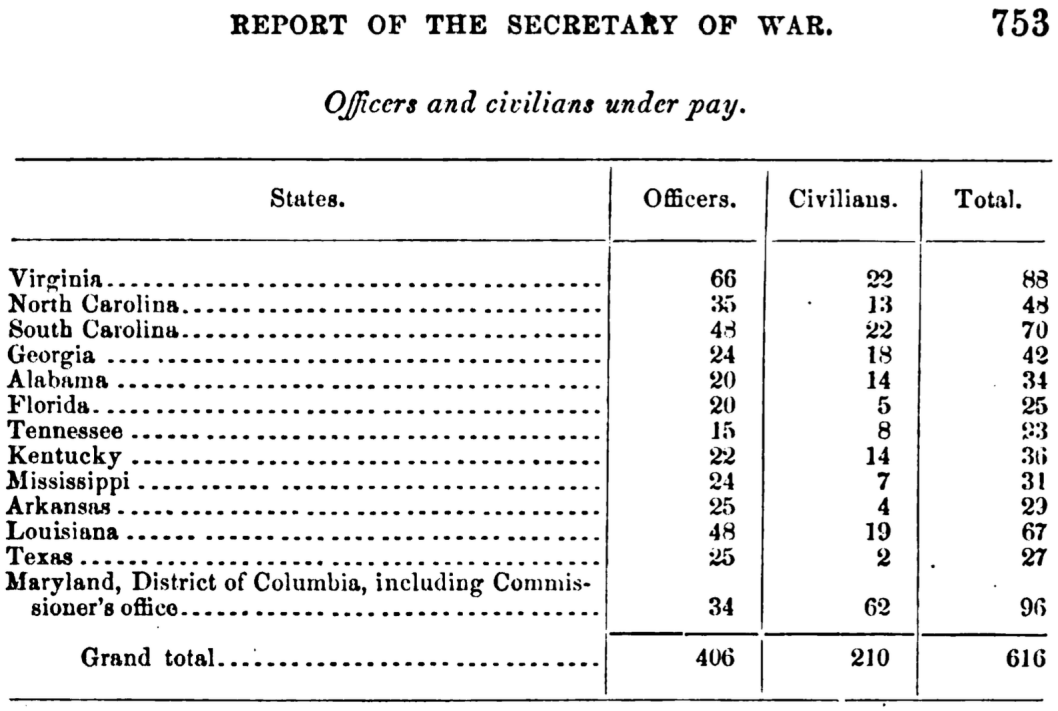
54, a reduction of 33 since the date of the last report. Of these 38 are employed in the office in Washington." The relatively small amount expended in the first year of operation in 1866 may reflect accounting changes in this first year.[37] While the annual reports do not note the number of agents in each year, the 1866 report does provide a breakdown of the (salaried) agents by state.

Figure C1 reproduces a table on state-level staff totals from the 1866 Annual Report. Limiting to our sample (i.e. excluding South Carolina, Maryland, and District of Columbia) would bring the total number of Officers and Civilians to 324 and 126 respectively, for a total salaried staff of 450. As our sample is composed of 341 counties with Bureau offices, this amounts to roughly 1.3 salaried staff members per treated county in 1866. Of course, the Bureau grew and shrank over our sample period, so we next turn to more comprehensive records of personnel from the biannual *Official Register of the United States* (1867, 1869, and 1871) produced by the US Department of Interior.

Table C2 shows state-level descriptive statistics on staff as recorded in the Official Register of 1867. We see a total of 493 staff across the 10 states, roughly evenly split between the 211 agents and 213 clerks (the remaining 68 are mostly composed of surgeons). These staff counts range from just 17 in Florida to 72 in Georgia. In total, staff were paid $570,292 in that year, with the average annual salary being $1,161. We also see that only a small minority of staff were born in the same state (10%) or any former Confederate state (19%). Arkansas and Virginia have the highest percentage of non-Southern born staff (91%) while Texas had no staff born in-state. The Official Registers of 1869 and 1871 coincide with the winding down of paid staff at the Bureaus, with just 76 (in 1869) and 11 (in 1871) staff

[37]The 1866 spending is incongruous with the Commissioner's summary which noted that "*The average expenditure per month is about three hundred and fifty thousand dollars, ($350,000)*").

Figure C1: Bureau Personnel as of November 1, 1866 Annual Report



REPORT OF THE SECRETARY OF WAR. 753

Officers and civilians under pay.

States.	Officers.	Civilians.	Total.
Virginia	66	22	88
North Carolina	35	13	48
South Carolina	48	22	70
Georgia	24	18	42
Alabama	20	14	34
Florida	20	5	25
Tennessee	15	8	23
Kentucky	22	14	36
Mississippi	24	7	31
Arkansas	25	4	29
Louisiana	48	19	67
Texas	25	2	27
Maryland, District of Columbia, including Commissioner's office	34	62	96
Grand total	406	210	616

Notes: This image is a screenshot taken from page 753 of the 1866 Annual Report of the Secretary of War to the President.

within our sample states.

While the Official Registers provide rich data (including birth state and compensation) on US government personnel in these odd years, it's unclear just how complete these are, notwithstanding the issues of unpaid staff/volunteers noted in the beginning of this section. One hint of the incomplete nature is the fact that there are only 254 unique locations for the staff in 1867, considerably less than the 341 unique treated counties in our estimation sample. For these reasons, we also turn to the National Archives (NARA) state reports from the Bureau (e.g., https://sova.si.edu/record/nmaahc.fb.m1913) which contain lists of staff employed across the entire duration of the Bureau's operation (including the start and end dates for that employment). We aggregate and restructure this data so that we can see all employees in these reports in a given year. The final column of Table C2 shows state-level counts of employees in the NARA records. We see significant differences in both directions across the two sources, e.g., Florida has 17 in the Registers but just 6 in NARA, while Virginia has 58 in the Registers and 75 in NARA. To take an example of a specific office, the Registers note a single employee in the Knoxville, TN office, a clerk named "John Walker". However, in the NARA records, there is similarly just one employee listed there; however, he is named "Samuel Walker" and listed as a subassistant commissioner rather than clerk. Ultimately we find 18 more employees in the NARA records than in the Official Registers; however, NARA

notes that its records are incomplete, as the preface to the employee lists notes that they contain records just for a subset of offices (e.g., "*this list provides the names and dates of service of known Freedmen's Bureau personnel at selected subordinate field offices for Virginia.*"). Moreover, none of the staff have their position listed as "clerk" (most staff are listed as Agent, Comissioner, Subassistant Comissioner, Assistant Subassistant Commissioner, or Superintendent).

We hope this section provides a bit more color on the personnel of the Bureau, while also highlighting the limitations of using staffing as a measure of the intensity of treatment. These results underscore that the Bureaus were tasked with carrying out a complex and ambitious mission with relatively little annual government spending, at roughly $80 million in total and $18 million in personnel (in inflation-adjusted terms) at its peak in 1868. Ultimately, due to both the uncertainty on total staff across these two official databases, and the issues of unpaid staff, we do not attempt an analysis that attempts to proxy for treatment intensity using these personnel records.

Table C2: 1867 Official Register Records of Freedmen's Bureau Personnel

State	Staff	Agents	Clerks	Total Salary	Avg. Salary	% Born in State	% Born in South	NARA Count
Alabama	45	22	23	$51,358	$1,141	16	29	16
Arkansas	53	18	28	$64,320	$1,214	4	9	45
Florida	17	9	3	$19,320	$1,136	6	18	6
Georgia	72	43	18	$88,500	$1,229	13	24	86
Louisiana	68	23	34	$79,900	$1,175	12	13	72
Mississippi	57	27	18	$58,200	$1,021	12	33	64
N. Carolina	44	9	28	$52,800	$1,200	11	16	52
Tennessee	34	16	14	$39,900	$1,174	24	26	33
Texas	45	34	11	$50,320	$1,170	0	18	62
Virginia	58	10	36	$65,674	$1,132	5	9	75
All	493	211	213	$570,292	$1,161	10	19	511

Notes: Records of the Freedmen's Bureau personnel are compiled from Official Register of 1867; https://catalog.hathitrust.org/Record/009557655. The table drops staff from DC (125), Kentucky (42), South Carolina (46), and Missouri (2) to match our estimation sample. These staff are employed at 258 unique field locations (e.g., 15 staff in Nashville, TN and 1 in Knoxville, TN). Staff not listed as Agent or Clerk are Acting Assistant Surgeon (49), Disbursing Officer (2), Laborer (12), Messenger (2), Superintendent of Schools (3), and Surgeon in Chief (1). The official registers of 1869 and 1871 have records for 76 and 11 staff respectively in our sample states. NARA count is the number of staff who appear in 1867 in National Archives reports for each state (typically covering agents/commissioners and no clerks).