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

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Abstract

How do majority groups respond to narrowing inequality in racially polarized environments? We study this by examining the Freedmen's Bureau, an agency created after the U.S. Civil War to aid former slaves and launch institutional reform in the South. Using new historical records and an event-study approach, we estimate the Bureau's impact on political, social, and economic outcomes. Vote shares for Democrats, the party that previously championed slavery and opposed Black civil rights, initially eroded in Bureau counties, but these impacts faded as federal oversight weakened. Racial backlash emerged rapidly and persisted, suggesting White majorities sought to offset Black progress.

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) largely due to data limitations and a lack of clear empirical benchmarks.

This paper examines 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 analyze important measures of social progress for Black Americans. Third, we test for potential backlash by studying 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 establishing offices in an environment with tight time and budgetary constraints—pressures that were intensified by the urgency and uncertainty faced by an occupying federal government in a post-war context. As a result, the Bureau was unable to place field offices in all in-need areas. Existing reviews of archival records highlight that officials often prioritized selecting towns and cities for offices based on their transportation infrastructure and the size of their Black population (Bean, 2016). In line with these accounts, we 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

identifies suitable comparison counties using a historically-motivated matching-based approach (e.g., Dippel and Heblich, 2021). We show that a limited set of demographic and transportation-related county characteristics identifies a set of comparison counties that are well-balanced not only on a broader set of non-targeted 1860 characteristics, but also on changes in characteristics from 1850 to 1860. Moreover, our event-study approach that compares electoral outcomes over time yields two additional pieces of evidence on the validity of our research design. First, we use the matched sample and find no evidence of differential pre-trends before 1860 in a standard event-study specification. Second, our matching-based approach yields results for political outcomes 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 Democrats decreased by 5 percentage points in the 1868 presidential election in counties exposed to Bureau field offices. There is an equal-in-magnitude increase in the Republican vote share. These results are 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 13 percentage points (17% relative to the mean level for counties without a Bureau office) and almost tripled 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 effects on Democratic and Republican vote shares reached their peak in 1868, and subsequently dissipated until the effects were 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, particularly as an effort to politically intimidate Black Americans (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).

Our analysis concludes with two exercises that examine long-run measures of racial backlash in the 20th century and study potential mechanisms that may drive the Bureau's impacts. First, 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 1900–2003, higher amounts of second-wave and third-wave Ku Klux Klan activity (around 1915 and 1960, respectively), and a larger number of monuments built to honor the Confederacy. In sum, the Bureau's impacts on political outcomes were short-lived, but the large effects on racial violence were not. Second, as one key test of mechanisms, we study county-level outcomes measured in Census data using our event-study framework. Most notably, there are positive impacts on school attendance and literacy rates of children, important measures of Black progress given the relatively high rates of illiteracy and limited educational access for Black Americans at the turn of the century.

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: 0.18). 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 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 institutions and responses to attempts at institutional reform (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 Black progress and racial animus in U.S. history (e.g., Sacerdote, 2005; Naidu, 2010; Cook, 2014; Logan, 2020, 2023; Aneja and Xu, 2022; Ang, 2023; Jones and Schmick, 2023; Hornbeck and Logan, 2023; Frieden, Grossman and Lowery, 2024; Althoff and Reichardt, 2024).[1] Broadly, our findings for the Freedmen's Bureau are 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

[1]Our work also relates to an emerging set of findings on more contemporary backlash against social policy (Abdelgadir and Fouka, 2020; Ang, 2019; Bernini et al., 2023; Fouka, 2020; Wheaton, 2022).

providing evidence on how the creation of 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 historical 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 highlight that a sizable portion of 20th century racial violence can be traced to long-lasting backlash to a short-lived policy.

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.[2]

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).

[2]Violence and incidents of racial intimidation arose from economic disputes and the assertion of White supremacy (Foner, 2014). For example, according to a Freedmen's 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).

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.[3] The Bureau legally operated during 1865–1872, although funding cuts largely reduced its operations as of January 1869.[4]

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).[5] 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.[6] 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

[3]Appendix C provides details on total annual expenditures for the Bureau and personnel, while also highlighting the challenge of quantifying the extent of resources and staffing from these official sources.

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

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

[6]Historians have at times noted that some Bureau agents were susceptible to the influence of White landowners and served their interest over those of freedmen (Litwack, 1980).

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).[7,8] 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 challenge facing the Bureau was a lack of sufficient resources to open offices in all locations where freedmen needed relief. With respect to choosing office sites, General Otis Howard wrote, "it was impossible at the outset to do more than lay down a *few general principles* to guide the officers assigned as assistant commissioners [emphasis in italics added here and below]" (Howard, 1908, p. 363).

Existing reviews of the Bureau's archives highlight that officials often selected towns and cities to maximize impacts of relief and reform efforts. As a result, officials prioritized areas with large Black populations and those that could be easily accessed by train or other modes of transportation. For example, Bean (2016) notes that Bureau authorities "assigned SACs [subassistant commissioners] to places of *greatest need or importance*," while other agents also "had assignments at the *main hubs for shipments* of crops to market from the interior," and the "remaining SACs received appointments to regions with *large black populations*."[9] Accessibility was a key consideration given that freedmen often traveled to field offices to seek help mediating labor disputes or protection from violence (Foner, 2014). In addition,

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

[8]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.

[9]Bean (2016) summarizes, "Bureau headquarters required the office to be in a *convenient and accessible* location. Many men established their offices at the *county seat or most important city* in their subdistrict. In districts comprising more than one county, however, the office was in the *most populous* and important county in the district."

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).

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).[10] 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.[11] 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 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.

[10]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).

[11]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).

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. The county-level samples rely on available data for all states that comprised the Confederate States of America except for South Carolina (which does not have relevant election data prior to the Civil War).[12] 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, Kentucky, 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.[13]

[12]Since South Carolina cannot be analyzed in our political analysis, we also exclude South Carolina counties from the main analysis of other outcomes for consistency. Our main conclusions do not change when we include South Carolina in the sample (Appendix Table A10).

[13]We explored a separate analysis of Freedmen's Bureau field offices in the few states outside of the former Confederacy. However, the resulting sample size was much smaller and our matching approach did not yield treatment and control counties that are well-balanced.

National Elections: We study the effects of the Freedmen's Bureau on political outcomes 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.[14] 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.[15] 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). Note that 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

[14]In the 1860 election, Stephen Douglas was the candidate of the Democratic Party, and John Breckinridge was the candidate of the Southern Democratic Party. The latter received a larger share of votes in the South, and we combine the two vote shares for our analysis.

[15]In 1856, the Republican Party Presidential Candidate, John Fremont, received a small share of Southern votes. Instead, the main opposition to the Democratic Party in the South was the Know Nothing Party. Abraham Lincoln received an even smaller share of Southern votes in 1860, when the main opposition to the Democratic Party in the South was the Constitutional Union Party. As a robustness test, we estimate our main event-study regression (described below) when using vote shares for these alternate parties in 1856 and 1860.

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).[16] 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), and 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.

[16] 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.

Confirmed Black Lynchings: Data on confirmed Black lynching cases comes from the Historical American Lynching Data Collection Project (Project HAL; Hines and Steelwater (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.[17] 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 penalties) come from Espy and Smykla (2016). The data includes the county of conviction which we use to geolocate the decision. This data has also been used by Grosjean, Masera and Yousaf (2022) as a measure of institutional racial violence.[18] We aggregate executions of Black individuals 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.[19] Information on these attacks was collected based on original reviews 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 occurred during the Reconstruction Era. The data cover attacks that occurred between 1865

[17] 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.

[18] Tolnay and Beck (1995) note that: "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."

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

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 shares of Black individuals age 5–19 who are attending school or are literate.[20,21] 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-navigated river as of 1865, respectively. We obtain data on Freedman's Savings

[20]For 1850 and 1860 we assume that no slaves attended school or were literate.

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

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)).[22] In total, they capture 973 well-documented killings. Second, we use executions of Black individuals 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 cohorts whose parents were in the 25th percentile of the nationwide income distribution—

[22]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.

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 empirical approaches that we use to understand the effects of direct exposure to the Freedmen's Bureau. Our strategies rely 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 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.[23] As noted in Section 2, the Bureau was created in the aftermath of the Civil War and prioritized establishing its field offices in areas where they could have the most impact. However, many communities where freedmen could have benefited from relief were not selected due to the limited budget and resources available to the Bureau. The key to our research design is to identify a comparison group of counties

[23]The vast majority (93.6%) of treated counties have a single field office. For completeness, Appendix Figure A3 displays an analogous figure without imposing our baseline restriction that all counties have non-missing data for 1860 Presidential election vote shares, which notably adds South Carolina.

that met the Bureau's priorities for placement but were not chosen to host a field office.

Table 1 compares characteristics from the 1860 Census for the counties treated with exposure to the Freedmen's Bureau field offices and various samples of counties that were not 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 noted in Section 2, the summary statistics show that counties with Bureau offices had larger populations, particularly with respect to the size of the Black population, and better transportation access. 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, our empirical approaches rely on a matching-based approach that is motivated by historical narratives on Bureau operations and supported by evidence in the data. This approach focuses on a set of seven 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 population as well as the Black population share in the pre-Bureau period using the 1860 Census. This accounts for the fact that Bureau officials 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 logit regression of the Bureau treatment indicator, FB_c, on the set of seven 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. Panels A and B of Figure 2 display the estimated propensity score in the full and matched samples and illustrate how the matching process reshapes the distributions of the estimated treatment probabilities.

The evidence in the remaining columns of Table 1 shows 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 assessed on the sample of matched counties. Importantly, the results in column 6 show that 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.

Moreover, Panels C and D of Figure 2 show that the matched treated and control counties are similar in terms of the distribution of log population and the Black population share—not just at the means. We also find that the distributions of several other key baseline variables—distance to the nearest river, urban population share, share of output from manufacturing,

churches per capita, and Democratic vote share—are very similar between the matched treated and control counties (Appendix Figure A4). Finally, we show that there are no significant differences in the changes of key demographic variables from 1850 to 1860 after reweighting in Appendix Table A1.

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 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 average treatment-on-the-treated (ATT) weight.[24] 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 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.[25]

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

[24]The ATT weight is equal to 1 for treated counties. For control counties, the weight is equal to $\hat{p}/(1-\hat{p})$, where $\hat{p}$ is the propensity score constructed from a logit regression of a county having a Freedmen's Bureau field office on state fixed effects and the core controls. In some specifications, we add a vector of time-varying control variables to assess robustness.

[25]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).

considerable effort for freedmen due to high transportation costs and a lack of safety (Elliot, 1952; May, 1968; Bean, 2016).

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 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. A caveat for this analysis is that political outcomes before 1860 may reflect different underlying dynamics than those after the Civil War given the changing political role of race and slavery, although the event-study comparison still identifies relative differences between treated and control counties.

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 years 1868–1872 and 1873–1900 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 crimes against freedmen reported to Bureau agents are only available starting in 1865. 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 counties without Bureau offices that had 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.[26] 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.

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 our event-study specifications. 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.

[26] 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).

5 Results: Political Progress

In this section, we present estimates of the effects of the Freedmen's Bureau on a range of political outcomes. We find that exposure to the Bureau led to gains in the Republican vote share in Presidential and Congressional elections, increases in the Black voter registration rate, and gains in the number of Black officeholders. These results suggest that the Bureau notably improved the political power of Black individuals in the short-term. However, we find evidence of these gains vanishing alongside the end of Reconstruction.

5.1 Elections

We begin by presenting a graphical summary of the impacts of Freedmen's Bureau office locations on election outcomes. Figure 3 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.[27]

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.[28] Our main results are based on equation (1) and appear as a thick black line. Reassuringly, the results show no evidence of pre-trends, with

[27]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). Our event-study estimates are similar when excluding these states from all years of the analysis.

[28]As previewed in Section 3, the vote shares for Republican Presidential candidates in the South were low in the 1856 and 1860 elections. As a robustness exercise, we combine the Know Nothing and Constitutional Union party vote shares with the Republican party vote shares in these elections and estimate our event-study model. These results are similar to our main estimates. We do not do this as our main analysis due to the substantial differences in these parties' platforms.

all coefficients being small and statistically insignificant.

In the first post-treatment election of 1868, we see a significant decrease in the Democratic party vote share (5 percentage points) in counties with a Freedmen's Bureau office, relative to the comparison group. We also see a corresponding increase in the Republican 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. Underlying these event-study effects is an increase in the Democratic vote share between 1860 and 1868 in control counties and a slight decrease in counties with a Bureau field office. The Democratic vote share fell in both sets of counties in the 1872 election that Ulysses S. Grant won by a large margin, then increased over time and converged—consistent with the fade-out of the event-study impacts (Appendix Figure A5).

Table 2 presents difference-in-difference estimates for Presidential and Congressional elections. Motivated by the results in Figure 3, we combine the elections from 1868 to 1872 that had sizable negative point estimates for the Democratic vote share into an earlier period and the elections from 1873 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 4.0 percentage point drop in Democratic Presidential vote shares on average from 1868–1872. This corresponds to a 7% drop in this vote share relative to the matched control group mean (61%). The decrease in the Democratic vote share in Congressional elections, shown in column 2, is similar, at 3.2 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 3.4 percentage points (12%) for Presidential elections and 2.3 percentage points (10%) for Congressional elections. Together, these results suggest that the Freedmen's Bureau generated immediate electoral gains for Republicans and corresponding losses for Democrats.[29]

[29]We find similar estimated impacts of exposure to the Bureau on gubernatorial elections (Appendix Table A2), although the available data for these elections have important limitations relative to the data

The estimates for elections from 1873 to 1900 are small and indistinguishable from zero.[30] This period includes 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.[31] Ultimately, while the Freedmen's Bureau offices increased 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 in the South.

As previewed above, we also report results in Figure 3 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 with square markers). This result suggests that the historically-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 impacts on measures of Black political participation and representation. 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

used for Presidential and Congressional elections.

[30] We find a similar pattern of no detectable impacts in the elections after 1900 (Appendix Figure A10).

[31] 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).

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 17.7 percentage points. Results are somewhat smaller but still sizable when we limit the analysis to the sample of matched counties (13.0 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 (12.9 percentage points; column 3). Our preferred estimate amounts to 17% of the control county mean.[32]

The increase in Black political participation was accompanied by an increase in representation. Table 3, Panel B 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 1.2 elected officials in each county, which is roughly 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 an 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

[32]We find no effect of the Freedmen's Bureau on the White voter registration rate (point estimate from our preferred approach is 0.004, with a standard error of 0.016). This suggests that White individuals did not increase their political engagement in state constitutional conventions in response to Bureau-induced increases in Black individuals' political power.

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.

6 Results: Racial Backlash

This section examines whether the political progress fostered by the Bureau was met with racial backlash. We begin by providing evidence that counties with a Bureau field office had elevated levels of violence against Black citizens in the 19th century, including the years after the Bureau ended its operations. Additional analyses show that racial backlash extended even into the 20th century.

6.1 Short-Run Racial 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 44% increase relative to the control group mean of 1.4. When estimating separate effects for the earlier and later periods of this time, we see that both periods have increased mentions of lynching in treated areas. The coefficient is almost three times larger in the later period, but the period-specific control mean also rises over time from 0.5 lynchings per 100 pages for 1865–1882 to 2.2 lynchings for 1883–1900, so the effect size relative to the control mean is larger in the earlier period. Figure 4 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. While lynchings increased over time in control counties, they increased by more in counties with a field office (Appendix Figure A6).

Of course, newspaper mentions of lynchings suffer from several shortcomings, including the issue that they could report on violence in other locations or periods of time. They also may be valenced differently across areas (e.g., condoning versus reporting on the acts).[33] In Table 5, we present results from several alternative measures of racial violence. Because these data only exist after the opening of the Freedmen's Bureau, we estimate cross-sectional models for this analysis.

During the height of the Bureau's operations, agents collected detailed reports of murders and other violent crimes against Black citizens, an outcome that we begin with in Panel A as it is our earliest cross-sectional measure in the post-Civil War period. 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 17). While Bureau agents received and investigated reports 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.[34] Across specifications, we see a large and significant increase in

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

[34]The historical record suggests that Bureau agents did record reports of violent crimes from other coun-

this measure of violence, with about 1.4 extra acts of violence in treated counties, reflecting an 80% increase over control counties.

Panel B shows that attacks on Black schools between 1865 and 1878 were higher in counties with Bureau offices. The estimated treatment effect from our preferred specification is an increase of 0.7 attacks, which is almost 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.

We next consider a measure of institutional racial violence: death penalties executed under civil authority between 1865 and 1900. Panel C shows a significant increase of roughly 0.53 executions between 1865 to 1900, a 71% increase over the control mean.[35] To complement our newspaper-based analysis of lynching, Panel D presents results for investigated lynchings between 1882 and 1900. Our preferred estimate shows that counties with a Bureau field office experienced 0.4 additional lynchings from 1882–1900, a 27% increase over the control county

ties. S.E. Melcher, the superintendent for LaGrange, Tennessee, wrote: "Complaints come in daily from the adjoining counties of Miss. of robbery, assaults &c, on the persons of Freedmen. Not less than one hundred & fifty have le[f]t Tippah and Marshall Co.'s, Miss. within the past week and came into Tenn. And all with their story." (National Archives Microfilm Publication M999, Roll 34, "Reports of Outrages, Riots and Murders," Jan. 15, 1866–Aug. 12, 1868).

[35] An open question is whether the effects on violence were higher during or immediately after the Bureau operated. We examine this by measuring the number of Black school attacks and Black executions in the years prior to when the Bureau was effectively defunded (1865–1869) and the immediately following years (1870–1878). If effects on our measures simply reflect better documentation of violence by Bureau agents, then we expect to see impacts only during the Bureau's years of operations. More substantively, if violence was carried out instrumentally to stall Black progress, then we may also see higher violence during Bureau operations. At the same time, there are also reasons to expect the opposite pattern. Bureau agents may have deterred violence if potential perpetrators feared a higher likelihood of punishment. While we cannot disentangle these mechanisms, we find evidence suggesting that impacts on both measures of violence were substantial during Bureau operations and increased after defunding (Appendix Table A3). These results are consistent with real impacts of the Bureau on violence that grew in the Bureau's absence.

mean.

To summarize the impacts on violence, we construct a standardized index that combines all 19th century measures. The index is based on standardizing each of the preceding four violence measures using the control county mean and standard deviation and constructing the average of the non-missing variables for each county. Panel E shows that we find that violence increases by 0.39 units of the index (0.67 standard deviations).

6.2 Long-Run Racial Violence

Next, we test whether the Freedmen's Bureau and the resulting backlash during Reconstruction had longer lasting effects. 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.

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. In 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 132% 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 1.5 executions during the 20th century, a 62% increase. These results suggest the increase in lynchings observed in treated counties in the late 1800s continued into the next 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 6.1 has been well-studied qualitatively, but the lack of historical data on their chapter locations precludes us from studying their role precisely. However, systematic data exist for later KKK waves, which allows us to investigate whether the historical presence of Bureau offices had persistent effects on the propagation of organized racial hostility. Specifically, if the Bureau contributed to lasting social antagonism—perhaps by fueling resentment—these effects might manifest in heightened later-wave Klan activity. 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. 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 92% increase over the control group mean. This estimate is significant at the 1% level and robust to different specifications. A natural alternative is to 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 17.2 percentage points, which is around 61% of the control group mean (Panel A of Appendix Table A4). While not directly comparable due to Ang (2023)'s focus on the entire U.S., this is roughly 26% of his main 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 0.5 more third wave KKK klaverns, a 77% increase over the control mean. Again, estimates are statistically significant and robust across specifications. We continue to see a positive effect when using a binary measure for the presence of any third wave KKK klavern (Panel B of Appendix Table A4). 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 1.4 monuments, almost doubling the number found in control counties. Panel F summarizes all of the impacts by using a standardized index that combines all of these long-run outcomes. Our preferred estimate indicates that the Bureau increased 20th century racial violence by 0.81 standard deviations.

6.3 Intergenerational Mobility

Finally, we study outcomes of young adults from the Opportunity Atlas (Chetty et al., 2018) to test whether the lasting legacy of the backlash still resonates in the economic and social outcomes of the present day. 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.[36] 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 0.4 percentiles in their later-life income rank.[37] Relative to the control group mean income rank (38.3 percentiles), this is roughly a 1% drop and amounts to around $1,000 of household income per year. Panel B shows a 0.4 percentage point increase

[36] 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.

[37] One benchmark for this 0.4 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.

in incarceration, roughly a 7% increase relative to the control mean.[38,39] 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.

7 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, 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.

7.1 Event-Study Tests

We begin by taking 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.[40] 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

[38]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. We find small, positive point estimates for this this outcome (indicating increases in teen fertility) although these are not statistically significant in our preferred specification (Appendix Table A5).

[39]The impacts on intergenerational mobility and incarceration are driven by effects on Black individuals (Appendix Table A6).

[40]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).

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 operation. 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 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).[41]

Our third event-study exercise focuses on the potential role of other confounding post-treatment shocks as measured by demographic or economic conditions. We follow standard

[41]The unconditional correlation of Bureau offices with the presence of a bank branch is modest at 0.05, as is the correlation with the total number of depositors at 0.09. After implementing the matching and weighting, these relationships are insignificant.

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.[42] Our main interest is examining whether the inclusion of these additional time-varying measures affects the Bureau event-study estimates.

We find reassuring results from these three exercises for electoral outcomes in Appendix Figure A7. The figure reproduces our main estimates (thick 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-varying county-level characteristics.

We provide similar sensitivity results for our analysis of the newspaper-based measure of lynchings in Appendix Table A7. 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.

Finally, we conclude this section by augmenting our event-study specification to include an expanded set of baseline controls. While equation (1) includes the seven core controls listed in Table 1, it is possible this does not adequately adjust for cross-county differences in the event-study estimates. Reassuringly, we obtain estimates that are similar when adjusting for *all* of the 1860 characteristics in Table 1 in several flexible specifications (Appendix

[42]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.

Figures A8 and A9).

7.2 Cross-Sectional Tests

We also conduct related robustness exercises for our cross-sectional estimates. First, we estimate several augmented models. To assess the importance of the U.S. Army presence, we add the troop measure as an additional control variable in the propensity score and regression adjustment steps. Next, we augment the model to include the Freedman's Bank branch indicator in a similar manner. We also use the full set of controls listed in both Panels A and B of Table 1. The results are quite similar from all of these augmented models, which provides further reassurance on the validity of our results (Appendix Table A8). Second, we show that our results are similar when dropping individual states from the analysis, which reduces the concern that some other policy overlaps with the Freedmen's Bureau in an unaccounted-for manner (Hoehn-Velasco et al., 2024) (Appendix Tables A9 and A10). Third, we find no evidence that Bureau field offices had spillover effects on adjacent counties (Appendix Table A11). Fourth, the differences in cross-sectional outcomes between counties with and without a Bureau field office are apparent throughout their distributions, which lowers the concern about outliers driving our results (Appendix Figures A11 and A12).

8 Mechanisms and Heterogeneity

In this section, we provide additional evidence related to the mechanisms that could underlie our results for the effects of the Freedmen's Bureau. We begin by using Census data to estimate impacts on additional outcomes that could have generated political or social responses in our setting. Next, we conduct heterogeneity analyses to shed light on additional channels that could drive the racial backlash that we document.

8.1 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.

Table 8 studies Black demographic shifts observed in Decennial Censuses.[43] 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 is 1.9 percentage points higher in treated counties in 1880 (6% of the control mean), before declining in magnitude for 1890 and 1900. This elevated Black population share may have motivated the 19th century increase in racial violence. However, by 1900 there is no longer evidence of a differential change in the Black population share in counties with a Bureau field office.[44] This finding suggests that backlash in the 20th century was not a response to a contemporaneous increase in the Black population share.

Another potential catalyst of White backlash is the prospect for improved economic outcomes and dignity signaled by improved schooling for Black children and literacy for adults. We examine these outcomes in light of the Bureau's efforts to develop and promote schools that served children, and in some cases adults, as recognized by historians (Du Bois,

[43] A caveat for our analysis of the Census outcomes measured in 1870 is that prior research suggests that this year suffered from underenumeration (Steckel, 1991), which could affect our results if underenumeration rates differed between counties with and without a Bureau field office.

[44] This finding holds in subsequent decades.

1903; Taylor, 1924; Du Bois, 1935; Foner, 2014).[45,46] For children, our dependent variables for school attendance and literacy are calculated for individuals aged 5–19, while adult literacy is calculated using all individuals over age 19. We see an increase in school attendance of 1.5 percentage points (6% of the control group mean) in counties with a Bureau field office in 1880. The effect is also apparent from 1900–1920. Evidence of positive impacts after 1870 is not surprising because many schools created by the Bureau were operated by charitable organizations or integrated in the public education system after its closure in 1872 (Vaughn, 2014). Impacts on literacy rates of Black children (column 3) are similar, while there are smaller-in-magnitude, but often imprecisely estimated, positive impacts for adults (column 4). A key takeaway is that the improved educational opportunities for the Black community could have contributed to the observed backlash in both the short and longer-terms.[47]

8.2 Determinants of Backlash and Racial Violence

Was backlash a response to *realized* Black or Republican political gains, or was it also present in areas that did not see these advances? Prior and contemporaneous work has shown that racial violence was heightened during this period in areas that elected Black officials (Logan, 2023) and Republican politicians (Testa and Williams, 2023). We turn to heterogeneity analysis to study this question.

In Table 9, we examine heterogeneity in both 19th and 20th century racial violence impacts in areas that did or did not elect any Black officials and in those that experienced more or less substantial declines in the Democratic vote share between 1860 and 1872, the last

[45] Du Bois (1935) notes that Black adult freedmen sought to learn to read by attending night schools. As demonstrated in Appendix Figure A13, the archival records indicate that buildings financed by the Bureau were often used as night schools.

[46] Our aim is to use our matched-sample event-study approach to build on the descriptive findings from Troost (2006) who previously examined the correlation between the presence of Bureau schools within a county and schooling outcomes using the 1870 Census.

[47] We also find evidence that Black individuals in counties with a Bureau field office were less likely to work as a farmer or farm laborer and more likely to work in domestic service in the medium run (Appendix Table A12).

year of the Bureau's operations. Columns 3 and 4 show heterogeneity comparing treatment effects in areas that elected any Black officials (278 counties, of which 179 were treated) and those that did not elect any Black officials (421 counties, of which 162 were treated). Columns 5 and 6 show heterogeneity by a median split in the decrease in the Democratic vote share.[48] An important caveat for these results is that we condition each analysis based on a post-treatment outcome.

We estimate impacts on the summary index measures presented previously to both increase power and reduce the number of hypotheses tested. Two sets of results emerge from this analysis. First, we see greater 19th and 20th century racial violence in areas with greater realized Black and Republican empowerment. For example, we find increases in the 19th century violence index of 0.87 and 0.21 standard deviations in areas that did vs. did not elect a Black official, respectively (calculated as the effects of 0.505 and 0.121 divided by the standard deviation of 0.581). Second, there remain large and significant increases in racial violence even in areas that did *not* have increases in realized Black and Republican progress. Areas that experienced, on average, an increase in the Democratic vote share still saw heightened racial violence in counties treated with a Bureau office (e.g., increases of 0.34 and 0.48 standard deviations in 19th and 20th century violence respectively). Overall, these results suggest that political progress was not a necessary condition for racial backlash, but such progress did trigger additional backlash.

A related question is whether the 20th century violence and hate that emerged in response to the Bureau was concentrated in the same counties that saw higher rates of violence in the 19th century. To explore this issue, columns 7 and 8 of Table 9 split the sample based on whether the 19th century violence and hate index was above or below the sample median. We find that the relationship between the Freedmen's Bureau and racial hate in the 20th

[48]The median change in the Democratic vote share is a 5.1 percentage point drop. Among counties with an above-median decrease (column 5), the Democratic vote share fell by 23.6 percentage points on average. Among the other half of counties, the Democratic vote share *rose* by 16.1 percentage points on average.

century is much stronger in counties with high levels of racial violence in the 19th century. This evidence suggests that the 19th century violent backlash to the Freedmen's Bureau might have triggered lasting changes in racial hate and the degree to which such hate is violently expressed in local areas.[49]

8.3 Intensity of Freedmen's Bureau Personnel

Field offices varied in the number of Freedmen's Bureau agents who were assigned to each location. This staffing variation has the potential to shed light on the degree to which the effects on political progress and racial backlash were driven by the presence of the Bureau versus the intensity of its operations in an area. For example, a single agent could trigger racial backlash if White individuals responded to the symbolic presence of the federal government in their local community. Alternatively, backlash could be muted if the number of Bureau agents was insufficient to support Black civic and legal rights. Ultimately, an analysis of how staffing levels correlate with Bureau impacts sheds light on questions of state capacity and efficiency.

There are several challenges associated with characterizing the level of support received by a field office. The Bureau was, in many senses, a public-private partnership, and we do not have a way to track the number of volunteers and staff from other organizations (e.g., the AMA and the Union League) who assisted in the mission of the Bureau. However, we can use records from Bureau state reports that detail office staff, including their full employment dates spanning all years of the Bureau's operations. These records provide a list of 513 staff which can be linked to a subset of the treated counties in our analysis.[50] Appendix C provides additional details on both the available data and challenges associated

[49] The p-values of the null hypothesis that effects are equal are all less than 0.01 for the 19th century violence and hate index. For the 20th century violence and hate index, the p-values associated with columns 3–4 and 7–8 are less than 0.01, while the p-value is 0.05 for columns 5–6.

[50] We count the number of Bureau employees in 1867 as it is a year that also has an Official Registers report (produced in odd years) and corresponds to the peak in the number of staff in the state reports.

with quantifying Bureau staffing using the archival records.

Table 10 presents heterogeneity analysis based on computing the Bureau office staffing per 1,000 Black residents and splitting the sample based on the median value (equal to 0.38). In Panel A, we examine impacts on Presidential Republican and Democratic vote shares in the two groupings of years shown in Table 2, with column 3 repeating the same specifications in the sample of treated counties for which we observe staffing records. The estimated coefficients are similar to those for the full sample in Table 2. Columns 4 and 5 show that there are somewhat larger effects on vote shares in areas with greater staff support. Altogether these results suggest that the Bureau had its largest political effects in areas with more official staffing support per capita.

Panel B of Table 10 examines the indices of our other primary outcomes.[51] Column 3 shows that areas with more staffing ended up with greater Black empowerment (0.78 vs. 0.60 standard deviations). We estimate a smaller increase in 19th century violence and hate in areas with more staff per capita (0.49 vs. 0.81 standard deviations). Despite this, the last row finds similar increases in 20th century racial violence.[52]

Overall, these results suggest that offices with more staffing support were more effective in carrying out their goals. This benefited Black residents both by increasing empowerment and limiting (but not eliminating) violence in the 19th century. At the same time, our finding that offices with fewer staff still had significant impacts highlights that the presence of the Bureau was sufficient to both advance Black political progress and trigger backlash. However, caution should be taken in interpreting these results, as staffing intensity was not randomly assigned.

[51] For Black Political Power, we standardize the outcomes of Table 3.

[52] The p-values of the null hypothesis that effects are equal exceed 0.10 for all rows.

9 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 and social impacts of the Freedmen's Bureau, an agency tasked with the broad mission of aiding and promoting the independence of former slaves. Our analysis shows that counties exposed to Bureau field offices saw substantially increased Black political participation and representation, as well as vote share gains for the abolitionist Republican party, in the years following the Civil War.

However, the counties treated with greater Bureau exposure exhibited a swift and severe 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 20th 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 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 institutions. Instead, the data for our analysis permits study of the dynamics of a significant reform to established

institutions. We find that reform efforts generated meaningful political and social progress 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 reform 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 short and 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. 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 by these changing evaluations based on qualitative approaches, our quantitative analysis 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.

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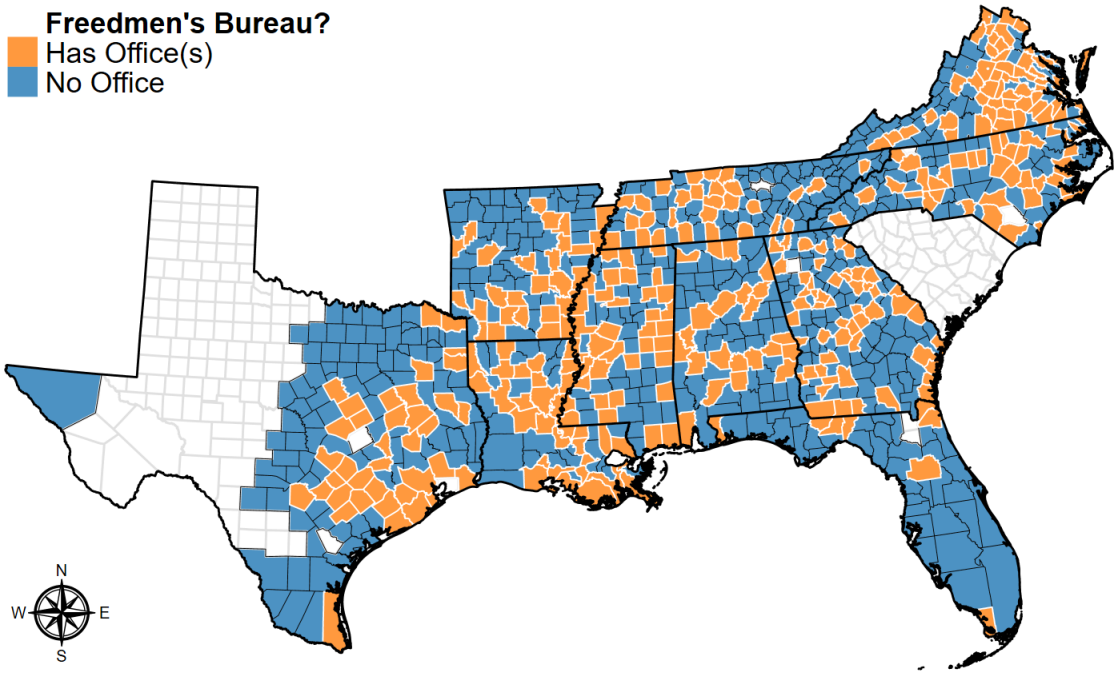
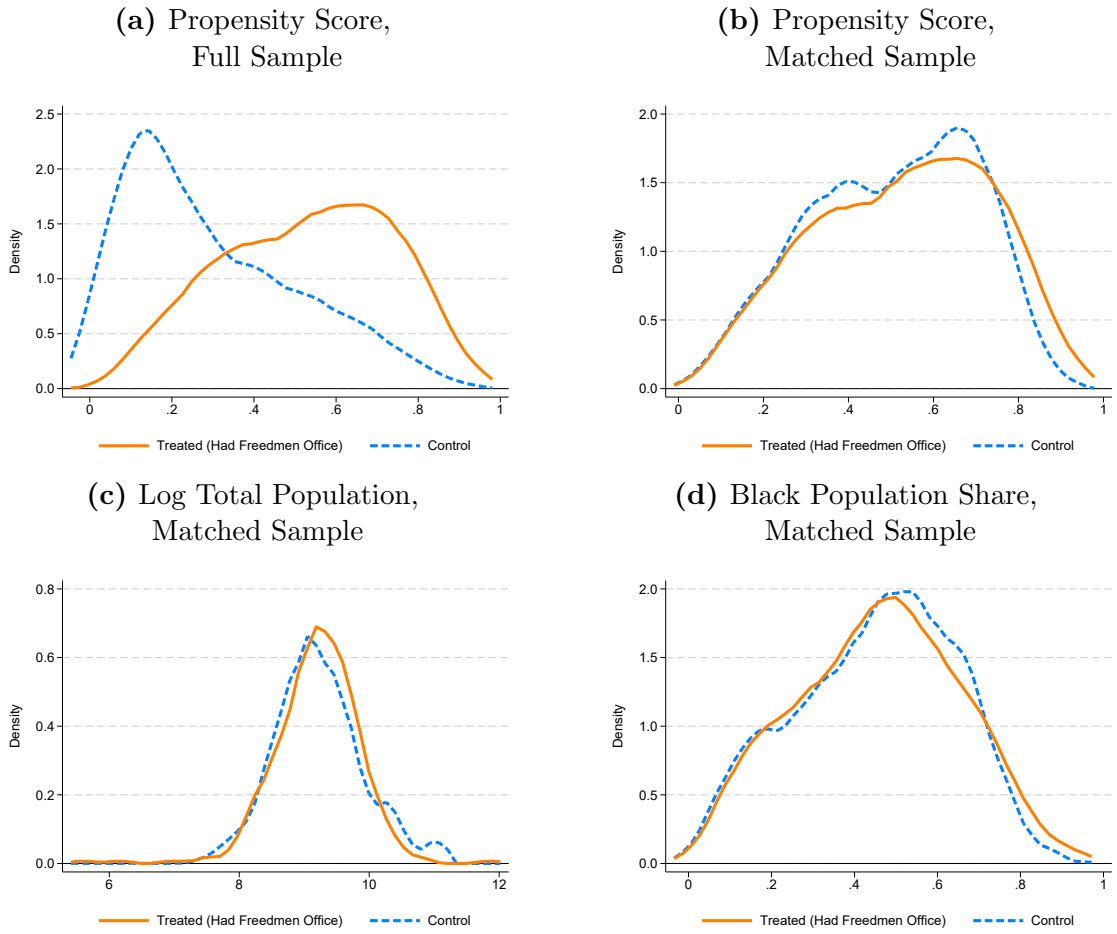
Figure 1: Map of Freedmen's Bureau Field Offices, Main Sample



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.

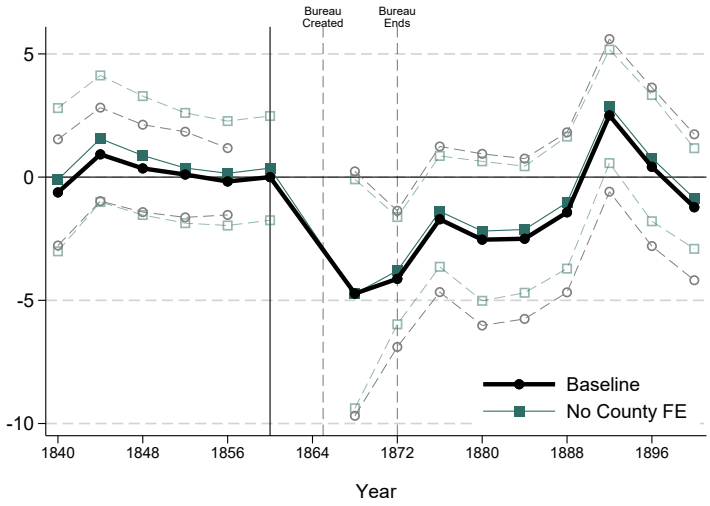
Figure 2: Density Plots of Propensity Scores and Key Characteristics



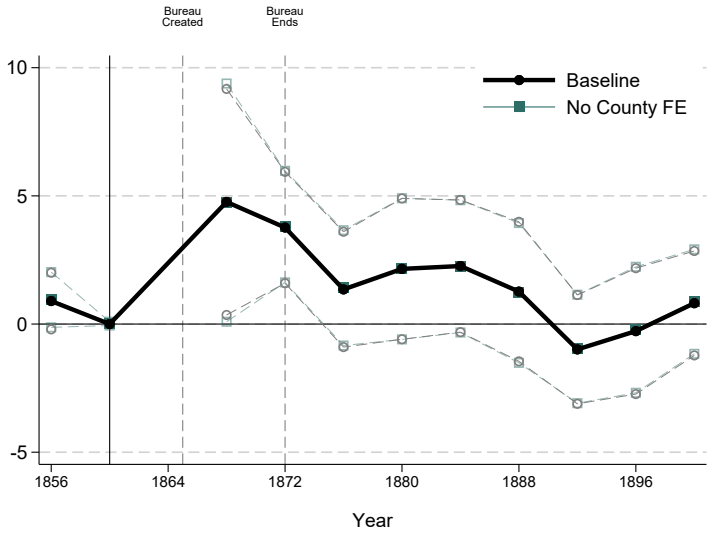
Notes: This figure presents kernel density plots of key variables for treated counties that had Freedmen Bureau field offices (in solid orange) and control counties that did not have an office (in dashed blue). Panels A and B present plots of the propensity scores separately for the raw and matched samples of counties, respectively. The propensity scores are estimated from a logit regression of the Bureau treatment indicator, FB_c, on the set of 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. Panels C and D present kernel density plots using the matched sample for the log of total population and Black population shares, respectively. These are weighted using 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.

Figure 3: Impacts on Presidential Vote Shares, Event Study Estimates

(a) Democratic Vote Share (1840–1900)

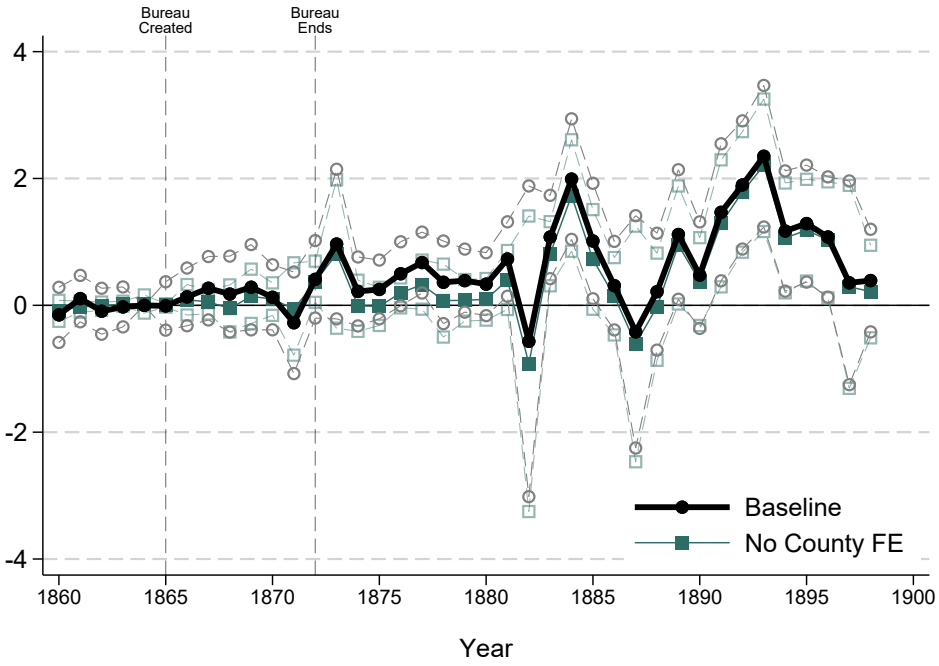


(b) Republican Vote Share (1856–1900)



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. The omitted reference year is 1860. The figure also shows results from a specification in which county fixed effects are excluded and there is no omitted reference year. 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. 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 lines surrounding the point estimates represent the 95% confidence interval based on standard errors clustered at the county level.

Figure 4: Impacts on Newspaper-Based Mentions of Lynchings



Notes: This figure reports event-study estimates 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). The baseline results are based on a regression that includes interactions between the county having any Freedmen's Bureau field office and year indicators, year indicators, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. The omitted reference year is 1864. The figure also shows results from a specification in which county fixed effects are excluded and there is no omitted reference year. 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 dashed 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.

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***	0.131***	-0.023
			(0.054)	(0.049)	(0.046)	(0.034)
Log Slave Population	8.225	6.891	1.334***	1.141***	0.592***	0.023
			(0.088)	(0.081)	(0.071)	(0.036)
Log Free Black Population	3.425	2.311	1.114***	0.728***	0.506***	0.002
			(0.150)	(0.095)	(0.098)	(0.050)
Black Population Share	0.456	0.284	0.173***	0.142***	0.092***	0.010
			(0.014)	(0.013)	(0.014)	(0.006)
Log Urban Population	1.129	0.323	0.806***	0.789***	0.665***	-0.175
			(0.170)	(0.181)	(0.193)	(0.157)
Has Railroad Line	0.581	0.258	0.323***	0.303***	0.245***	-0.011
			(0.033)	(0.032)	(0.035)	(0.013)
Distance to Nearest River (km)	18.850	35.035	-16.185***	-12.542***	-5.228**	0.855
			(2.803)	(2.467)	(2.349)	(0.867)
Panel B. Additional Characteristics						
Log Total Population	9.195	8.543	0.652***	0.552***	0.304***	0.009
			(0.053)	(0.048)	(0.043)	(0.031)
Log Black Population	8.279	6.933	1.346***	1.146***	0.606***	0.034
			(0.085)	(0.078)	(0.069)	(0.034)
Log Foreign Population	4.203	3.127	1.075***	1.093***	0.797***	0.157
			(0.108)	(0.101)	(0.106)	(0.113)
Slave Population Share	0.440	0.276	0.164***	0.137***	0.088***	0.009
			(0.014)	(0.013)	(0.013)	(0.007)
Free Black Population Share	0.017	0.008	0.009***	0.005***	0.004**	0.001
			(0.002)	(0.001)	(0.002)	(0.002)
Urban Population Share	0.055	0.012	0.043***	0.043***	0.040***	0.003
			(0.010)	(0.010)	(0.010)	(0.013)
Foreign Population Share	0.023	0.025	-0.001	0.004	0.009**	0.003
			(0.005)	(0.004)	(0.004)	(0.004)
Log Farm Value	14.337	13.354	0.982***	0.778***	0.353***	-0.032
			(0.115)	(0.098)	(0.093)	(0.081)
Log Number of Large Farms	1.422	0.730	0.692***	0.561***	0.410***	-0.012
			(0.071)	(0.067)	(0.073)	(0.065)
Log Agricultural Output	13.082	12.052	1.030***	0.826***	0.334***	-0.050
			(0.118)	(0.101)	(0.091)	(0.074)
Log Cotton Output	9.875	8.509	1.366***	1.785***	0.828***	0.344
			(0.333)	(0.233)	(0.233)	(0.263)
Log Manufacturing Output	10.288	8.900	1.387***	1.177***	0.636**	-0.342
			(0.275)	(0.261)	(0.280)	(0.269)
Manufacturing Output Share	0.207	0.172	0.035**	0.036**	0.031**	-0.011
			(0.015)	(0.014)	(0.015)	(0.017)
Churches per 1000 People	1.802	2.307	-0.505***	-0.483***	-0.444***	-0.081
			(0.079)	(0.076)	(0.077)	(0.061)
Democratic Vote Share	54.301	55.040	-0.739	-2.861***	-1.784*	0.241
			(1.349)	(0.971)	(1.009)	(1.047)
State Fixed Effects	–	–	No	Yes	Yes	Yes
Matched Sample	–	–	No	No	Yes	Yes
Reweighted	–	–	No	No	No	Yes
Counties	341	543	884	884	699	699

Notes: This table shows means and differences in means for 1860 characteristics of counties. 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 heteroskedasticity-robust standard errors 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-'72 × 1(Freedmen Office)	-4.012***	-3.189*	3.401***	2.262*
	(1.538)	(1.728)	(1.298)	(1.162)
Post '73-'00 × 1(Freedmen Office)	-0.724	-2.496	0.486	0.851
	(1.335)	(1.901)	(0.972)	(0.986)
Control Mean	61.40	59.60	28.29	22.96
R-squared	0.480	0.493	0.708	0.417
Observations	7,381	11,794	7,381	11,794
County FE	Yes	Yes	Yes	Yes

Notes: This table reports estimates from 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 1872 and 1873 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)	0.177***	0.130***	0.129***
	(0.036)	(0.031)	(0.032)
Control Mean	0.763	0.774	0.774
R-squared	0.202	0.296	0.343
Observations	778	625	625
Panel B. Number of Black Officeholders			
1(Freedmen Office)	0.810***	0.838***	1.246***
	(0.203)	(0.212)	(0.307)
Control Mean	0.432	0.638	0.638
R-squared	0.344	0.352	0.453
Observations	884	699	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports estimates from regressions of Black political outcomes on an indicator for the county having any Freedmen's Bureau field office, state fixed effects, and the 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 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). Heteroskedasticity-robust standard errors 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)	0.610***	
	(0.161)	
Post '65-'82 × 1(Freedmen Office)		0.330**
		(0.149)
Post '83-'00 × 1(Freedmen Office)		0.911***
		(0.279)
County FE	Yes	Yes
Control Mean	1.385	1.385
R-squared	0.354	0.356
Observations	4,313	4,313

Notes: This table reports estimates from 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 1865–1882 and 1883–1900 in column 2), interactions between the 1865–1882 and 1883–1900 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. Heteroskedasticity-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 Crimes Against Freedmen (1865–1868)			
1(Freedmen Office)	1.244***	1.273***	1.350***
	(0.435)	(0.460)	(0.466)
Control Mean	1.229	1.683	1.683
R-squared	0.137	0.117	0.164
Observations	691	549	549
Panel B. Number of Black School Attacks (1865–1878)			
1(Freedmen Office)	0.468***	0.455***	0.670***
	(0.128)	(0.136)	(0.167)
Control Mean	0.297	0.375	0.375
R-squared	0.153	0.162	0.243
Observations	884	699	699
Panel C. Number of Black Executions (1865–1900)			
1(Freedmen Office)	0.484***	0.468***	0.534***
	(0.136)	(0.135)	(0.154)
Control Mean	0.607	0.755	0.755
R-squared	0.265	0.265	0.316
Observations	884	699	699
Panel D. Number of Black Lynchings Recorded (1882–1900)			
1(Freedmen Office)	0.302*	0.280	0.350*
	(0.173)	(0.177)	(0.183)
Control Mean	1.094	1.316	1.316
R-squared	0.246	0.240	0.264
Observations	884	699	699
Panel E. 19th Century Violence/Hate Index			
1(Freedmen Office)	0.321***	0.313***	0.391***
	(0.057)	(0.059)	(0.067)
Control SD	0.529	0.581	0.581
R-squared	0.339	0.326	0.460
Observations	884	699	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports estimates from regressions of anti-Black violence outcomes on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the 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 instances of violence or crimes against freedmen (officially recorded as "outrages") reported to the Freedmen's Bureau (1865–1868). The outcome in Panel B is the total number of attacks on Black schools (1865–1878). The outcome in Panel C is the number of executions of Black individuals performed under civil authority from 1865 to 1900. The outcome in Panel D is the total number of lynchings recorded between 1882 and 1900. The outcome in Panel E is a summary index that equals the average of all non-missing outcome variables after they have been standardized using the mean and standard deviation of the control group in the column 3 sample. Note that data on violence or crimes against freedmen reported to the Bureau is not available for all counties (see Appendix Section B.1 for details). Heteroskedasticity-robust standard errors 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)	0.944*	0.905*	0.903***
	(0.510)	(0.477)	(0.311)
Control Mean	0.633	0.685	0.685
R-squared	0.086	0.086	0.133
Panel B. Number of Black Executions (1900 to 2003)			
1(Freedmen Office)	1.466***	1.350***	1.482***
	(0.519)	(0.509)	(0.509)
Control Mean	1.971	2.386	2.386
R-squared	0.202	0.198	0.249
Panel C. Number of Second Wave KKK Klaverns			
1(Freedmen Office)	0.369***	0.359***	0.380***
	(0.075)	(0.074)	(0.070)
Control Mean	0.471	0.415	0.415
R-squared	0.185	0.205	0.243
Panel D. Number of Third Wave KKK Klaverns			
1(Freedmen Office)	0.399***	0.391***	0.465***
	(0.120)	(0.123)	(0.127)
Control Mean	0.501	0.602	0.602
R-squared	0.210	0.207	0.236
Panel E. Number of Confederate Monuments			
1(Freedmen Office)	1.173***	1.097***	1.424***
	(0.302)	(0.310)	(0.431)
Control Mean	1.409	1.654	1.654
R-squared	0.235	0.226	0.269
Panel F. 20th Century Violence/Hate Index			
1(Freedmen Office)	0.468***	0.447***	0.494***
	(0.125)	(0.118)	(0.092)
Control Mean	0.676	0.611	0.611
R-squared	0.224	0.213	0.291
Observations	884	699	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports estimates from regressions of twentieth century outcomes on an indicator for the county having any Freedmen's Bureau field office, state fixed effects, and the 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. The outcome in Panel F is a summary index that equals the average of all non-missing outcome variables after they have been standardized using the mean and standard deviation of the control group in the column 3 sample. Heteroskedasticity-robust standard errors 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)	-0.005***	-0.004**	-0.004**
	(0.002)	(0.002)	(0.002)
Control Mean	0.391	0.383	0.383
R-squared	0.524	0.454	0.462
Panel B. Incarceration Rate (1978–1983 Cohorts)			
1(Freedmen Office)	0.004***	0.004**	0.004**
	(0.001)	(0.001)	(0.002)
Control Mean	0.048	0.051	0.051
R-squared	0.298	0.255	0.273
Observations	884	699	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Matched Sample, Weighted	No	No	Yes

Notes: This table reports estimates from regressions of long-run outcomes (mobility and incarceration during adulthood, taken from the Opportunity Atlas) on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the 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. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Impacts on Black Population and Education Outcomes, Event-Study Results

	Black Population Share (1)	Black Child School Attendance Rate (2)	Black Child Literacy Rate (3)	Black Adult Literacy Rate (4)
1840 × 1(Freedmen Office)	0.003			
	(0.006)			
1850 × 1(Freedmen Office)	0.004	0.000	0.000	-0.005
	(0.006)	(0.007)	(0.006)	(0.009)
1870 × 1(Freedmen Office)	0.011*	0.007	0.004	-0.002
	(0.006)	(0.007)	(0.006)	(0.009)
1880 × 1(Freedmen Office)	0.019***	0.015**	0.017**	0.013
	(0.006)	(0.007)	(0.006)	(0.009)
1890 × 1(Freedmen Office)	0.016***			
	(0.006)			
1900 × 1(Freedmen Office)	0.012*	0.013**	0.020***	0.014
	(0.006)	(0.007)	(0.006)	(0.009)
1910 × 1(Freedmen Office)	0.009	0.014**	0.019***	0.015*
	(0.006)	(0.007)	(0.006)	(0.009)
1920 × 1(Freedmen Office)	0.003	0.023***	0.015**	0.011
	(0.006)	(0.007)	(0.006)	(0.009)
Observations	6,209	4,887	4,887	4,888
R-squared	0.930	0.941	0.930	0.914
Control Mean	0.337	0.232	0.224	0.298

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, literacy rate for Black adults ages 20+) 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 literacy rates 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 literacy rates. 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 9: Heterogeneity in Impacts by Initial Political Progress and Racial Violence

	Control		Black Officeholder Elected?		Decrease in Democratic Vote Share, 1860–1872		19th. Century Violence/Hate Index	
	SD	Baseline	Yes	No	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
19th Century	0.581	0.391***	0.505***	0.121*	0.532***	0.199***	0.479***	0.042**
Violence/Hate Index		(0.067)	(0.114)	(0.064)	(0.105)	(0.071)	(0.100)	(0.017)
R-squared		0.435	0.556	0.212	0.478	0.405	0.590	0.856
20th Century	0.611	0.494***	0.745***	0.215***	0.631***	0.296***	0.696***	0.151***
Violence/Hate Index		(0.092)	(0.166)	(0.073)	(0.142)	(0.094)	(0.147)	(0.052)
R-squared		0.276	0.377	0.153	0.300	0.447	0.347	0.257
Observations		699	278	421	347	348	348	351

Notes: This table reports estimates from regressions of two index outcomes on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the core control variables listed in Panel A of Table 1. The 19th century violence/hate index consists of the number of instances of violence or crimes reported to the Freedmen's Bureau from 1865–1868, the number of Black school attacks from 1865–1878, the number of Black executions from 1865–1900, and the number of Black lynchings recorded from 1882–1900. The 20th century violence/hate index consists of the number of mid-century Black killings, the number of Black executions from 1900 to 2003, the number of second-wave KKK klaverns, the number of third-wave KKK klaverns, and the number of Confederate monuments. We construct the summary indices by taking the average of all non-missing outcome variables after they have been standardized using the mean and standard deviation of the control group in the matched sample. All results also apply inverse propensity score weighting. Column 1 reports the standard deviation of the dependent variable for the control group of the estimation sample in column 2. Column 2 reports results from our baseline specification. Columns 3 and 4 split the sample based on whether the county elected a Black officeholder during Reconstruction (278 counties) or did not (421 counties). Columns 5 and 6 split the sample based on whether the decrease in the Democratic vote in Presidential elections between 1860 to 1872 was above or below the sample median of 5.1 percentage points. Columns 7 and 8 split the sample based on whether the 19th century violence/hate index was above or below the sample median. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 10: Heterogeneity in Impacts by Freedmen's Bureau Staff per Black Individual

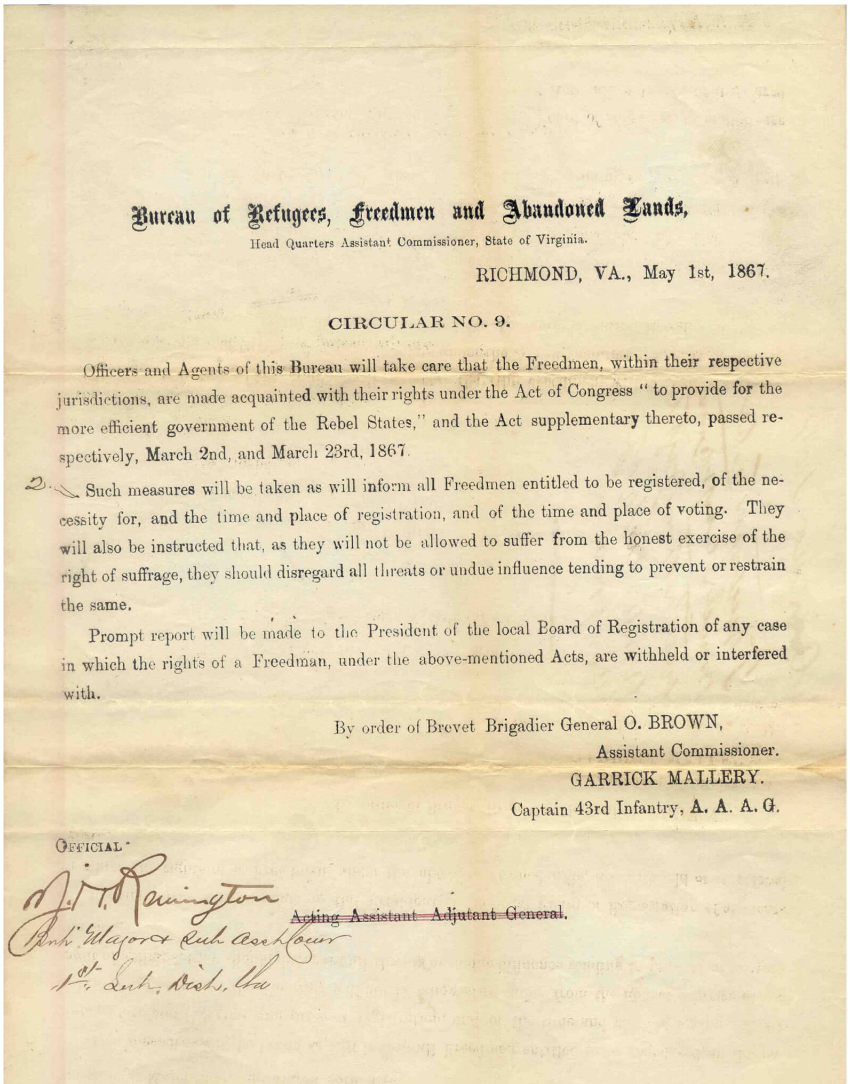
	Control Mean	Control SD		Freedmen's Bureau Staff Per Capita (Black)	
			Baseline	High	Low
	(1)	(2)	(3)	(4)	(5)
Panel A. Impacts on Presidential Vote Shares					
Dem Vote Share ('68-'72)	57.240	19.887	-4.373***	-4.342**	-3.805**
			(1.367)	(1.695)	(1.671)
R-squared			0.455	0.403	0.465
Rep Vote Share ('68-'72)	42.718	19.923	4.379***	4.385***	3.801**
			(1.367)	(1.696)	(1.671)
R-squared			0.454	0.402	0.464
Dem Vote Share ('73-'00)	63.345	13.872	-0.507	-2.093*	0.442
			(1.045)	(1.163)	(1.529)
R-squared			0.479	0.455	0.476
Rep Vote Share ('73-'00)	32.126	15.140	1.036	2.707**	-0.185
			(1.078)	(1.248)	(1.520)
R-squared			0.498	0.480	0.496
Panel B. Impacts on Summary Indices					
Black Political Power Index	-0.021	0.725	0.543***	0.563***	0.432***
			(0.106)	(0.141)	(0.151)
R-squared			0.429	0.370	0.438
19th Cen. Violence/Hate Index	0.002	0.581	0.404***	0.285***	0.470***
			(0.075)	(0.073)	(0.123)
R-squared			0.379	0.246	0.452
20th Cen. Violence/Hate Index	0.000	0.611	0.458***	0.443***	0.442***
			(0.075)	(0.104)	(0.098)
R-squared			0.357	0.265	0.340
Observations			607	482	483

Notes: This table reports estimates of the Presidential vote shares (Panel A) and summary index outcomes (Panel B) on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the core control variables listed in Panel A of Table 1. The Black Political Power index consists of the Black voter registration rate and the number of Black officeholders. The 19th century violence/hate index consists of the number of instances of violence or crimes reported to the Freedmen's Bureau from 1865–1868, the number of Black school attacks from 1865–1878, the number of Black executions from 1865–1900, and the number of Black lynchings recorded from 1882–1900. The 20th century violence/hate index consists of the number of mid-century Black killings, the number of Black executions from 1900 to 2003, the number of second-wave KKK klaverns, the number of third-wave KKK klaverns, and the number of Confederate monuments. We construct the summary indices by taking the average of all non-missing outcome variables after they have been standardized using the mean and standard deviation of the control group in the matched sample. All results also apply inverse propensity score weighting. Columns 1 and 2 report the mean and standard deviation of the dependent variable for the control group of the estimation sample of column 3. Column 3 reports results from our baseline specification when limited to the subset of treatment counties (249) for which we observe complete information on the number of Freedmen's Bureau personnel in the NARA records (and the full set of overlapping control counties for a total sample of 607 vs. 699 in Table 1). In column 4, we limit the set of treated counties to those with an above-median number of Freedmen's Bureau staff per Black resident of the county (as of 1860). In column 5, we repeat this exercise for treated counties at or below the median of Freedmen's Bureau staff per capita, which is 1 agent per 2,618 Black individuals. The control group remains constant across columns 3 to 5. Heteroskedasticity-robust standard errors 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.

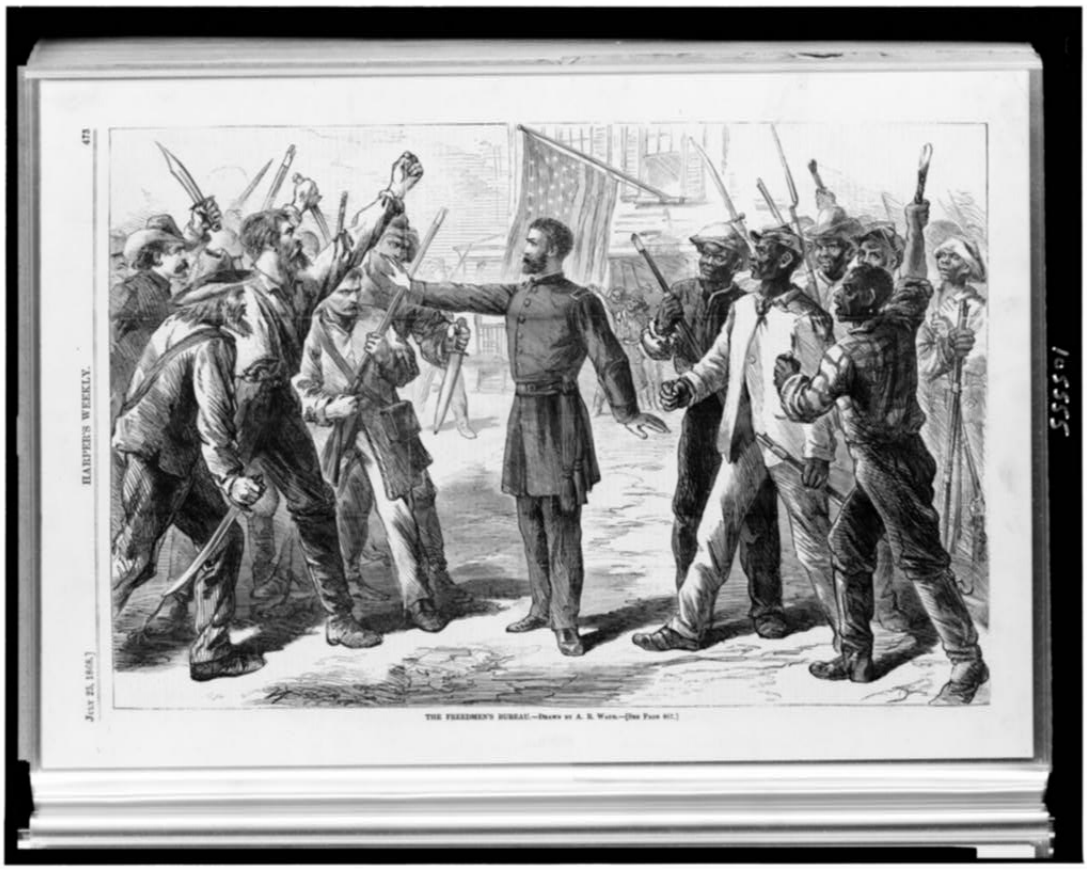
By order of Brevet Brigadier General O. BROWN,
Assistant Commissioner.
GARRICK MALLERY.
Captain 43rd Infantry, A. A. A. G.

OFFICIAL:

~~Acting Assistant Adjutant General.~~

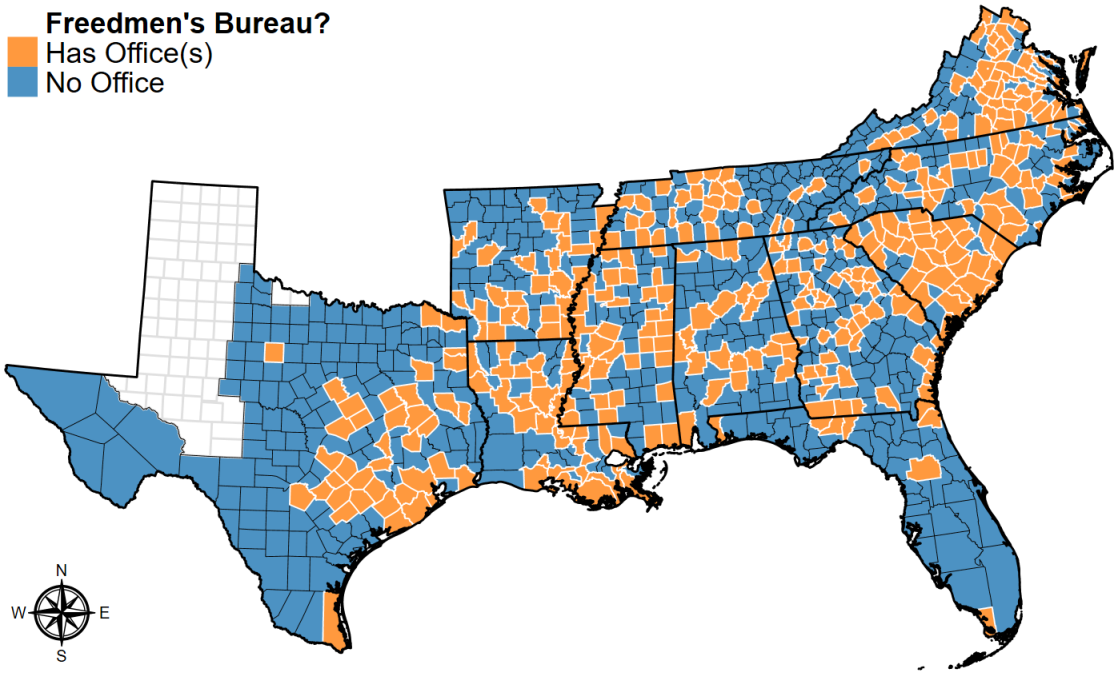
Notes: This figure shows official Freedmen's Bureau orders made on May 1, 1867 regarding voting rights. Congress passed the First and Second Reconstruction Acts in March 1867. This legislation enfranchised Black men for the election of delegates to state constitutional conventions. Congress charged the 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)



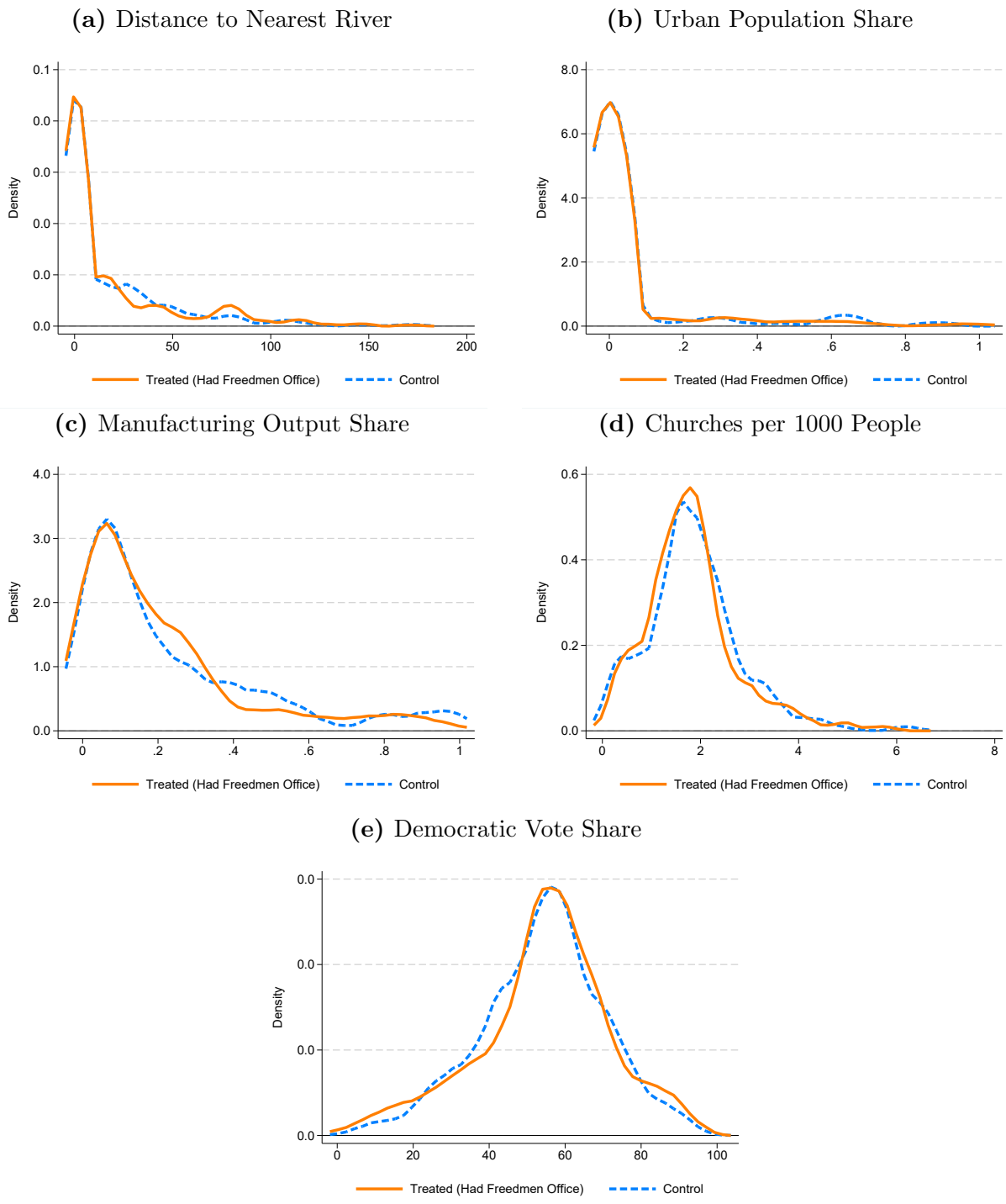
Notes: This figure reproduces an image of an agent of the Freedmen's Bureau standing between armed groups of White and Black Americans. Illustration created by A.R. Waud and appeared in the periodical Harper's Weekly, July 25, 1868 (Library of Congress).

Figure A3: Map of Freedmen's Bureau Field Offices, Including South Carolina



Notes: This map shows historical (1900) counties in the 11 Southern states that formed the Confederate States of America. 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. Unlike Figure 1, this figure includes areas without baseline (1860) elections data including South Carolina and several counties in western Texas. Some portions of Texas are omitted because they were not established as counties during the period of study.

Figure A4: Distribution of Additional Baseline Characteristics in Counties With and Without Bureau Offices



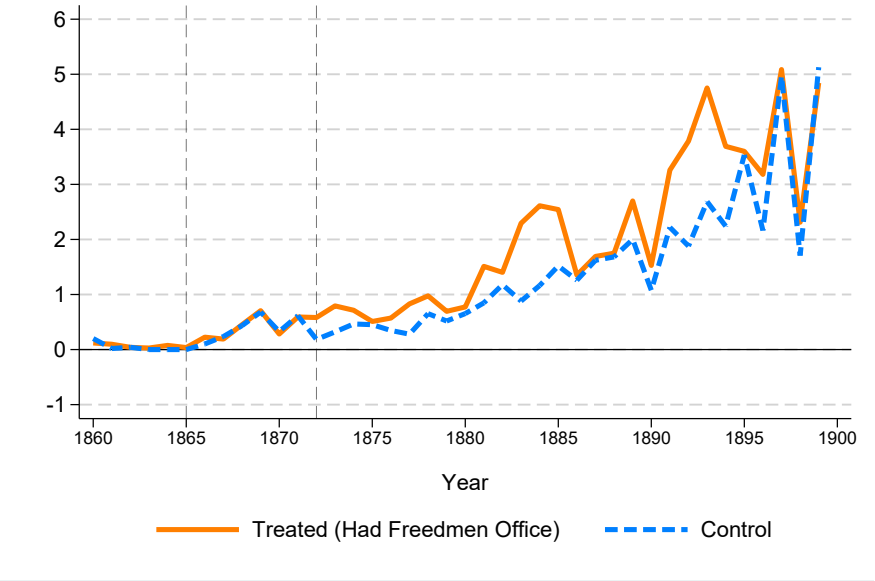
Notes: This figure presents kernel density plots of additional 1860 variables in the matched sample of treated counties for treated counties that had Freedmen Bureau field offices (in solid orange) and control counties that did not have an office (in dashed blue). Kernel density estimates are weighted using 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.

Figure A5: Presidential Vote Shares in Counties With and Without Bureau Offices

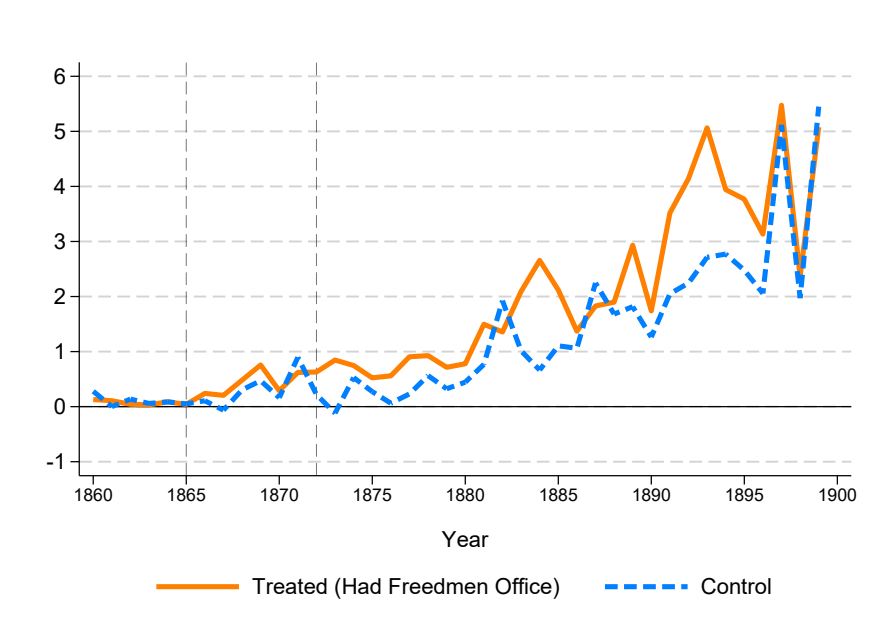
Notes: Panels A and B report the raw averages for the treated counties (which had at least one Freedmen's Bureau field office) and the ATT-reweighted averages for the matched set of control counties of the Presidential Democratic and Republican Party vote shares, respectively. Panels C and D report analogous results that are consistent with our baseline event-study estimates. In particular, we plot the raw averages for the treatment counties and the regression-implied counterfactual (no treatment) mean, where the latter is calculated by subtracting the event-study coefficient from the raw average for treatment counties.

Figure A6: Newspaper-Based Mentions of Lynchings in Counties With and Without Bureau Offices

(a) Treated vs. Reweighted Control



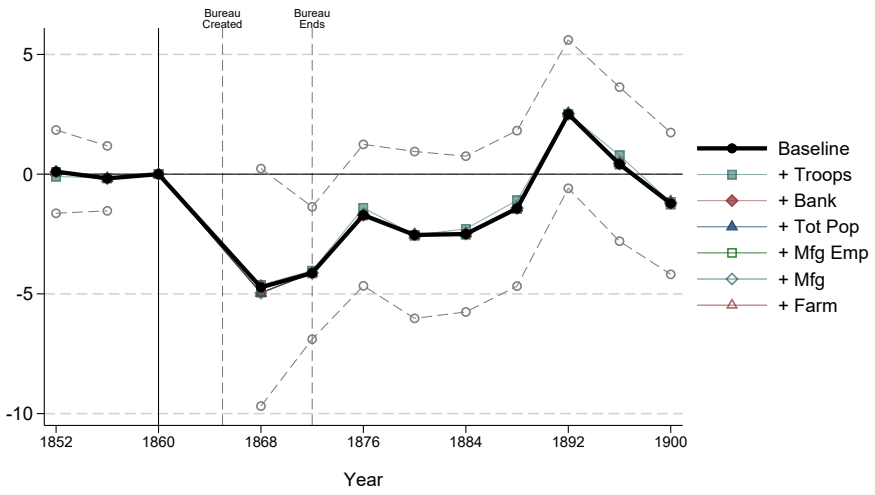
(b) Treated vs. Counterfactual



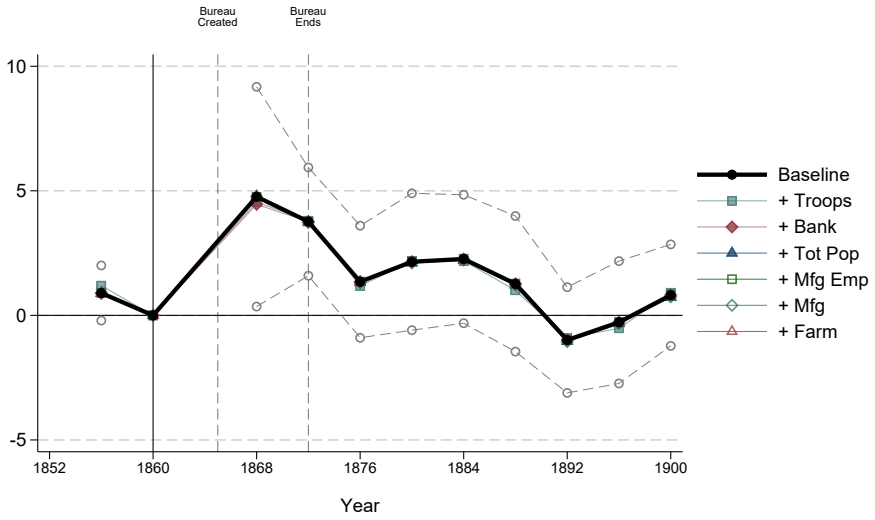
Notes: Panel A reports the raw average for the treated counties (which had at least one Freedmen's Bureau field office) and the ATT-reweighted average for the matched set of control counties of the rate of newspaper reports of lynchings. Panel B reports analogous results that are consistent with our baseline event-study estimates. In particular, we plot the raw average for the treatment counties and the regression-implied counterfactual (no treatment) mean, where the latter is calculated by subtracting the event-study coefficient from the raw average for treatment counties.

Figure A7: Impacts on Presidential Vote Shares, Event Study Robustness to Time-Varying Controls

(a) Democratic Vote Share (1852–1900)



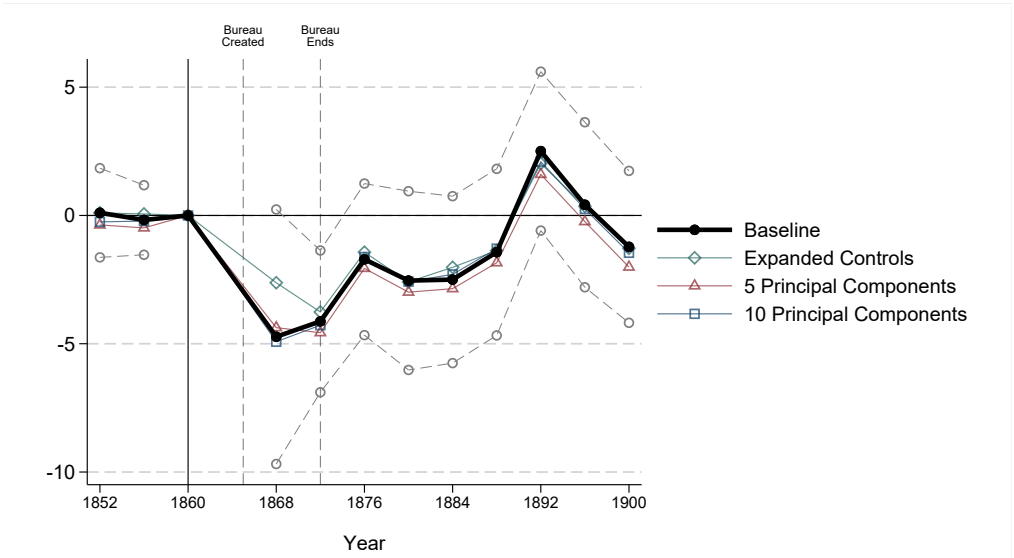
(b) Republican Vote Share (1856–1900)



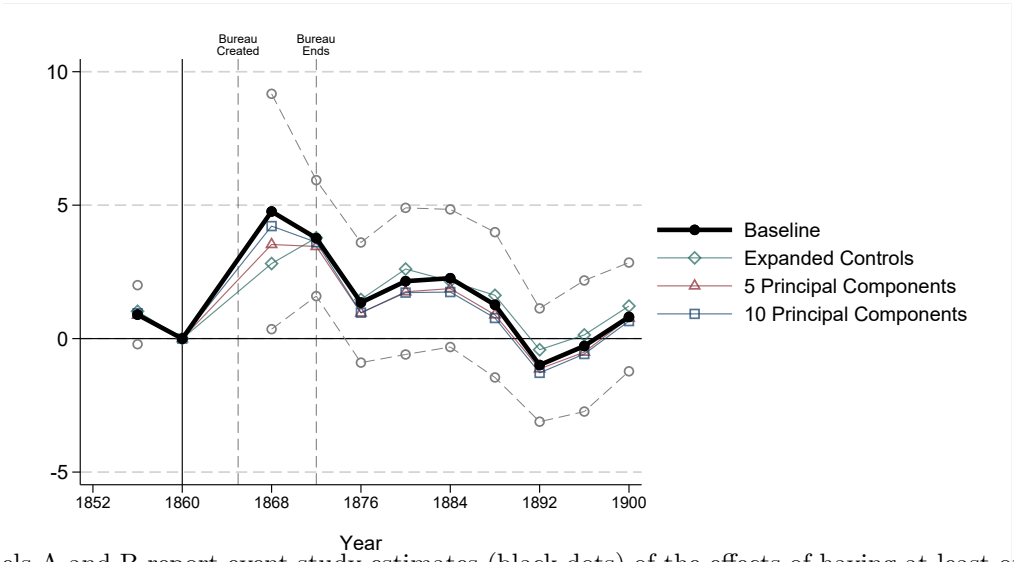
Notes: Panels A and B report event-study estimates (black dots) of the effects of having at least one Freedmen's Bureau field office on county-level Presidential Democratic and Republican Party vote shares, respectively. The baseline results are based on a regression that includes interactions between 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 7 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 3.

Figure A8: Impacts on Presidential Vote Shares, Event Study Robustness to Baseline Controls

(a) Democratic Vote Share (1852–1900)

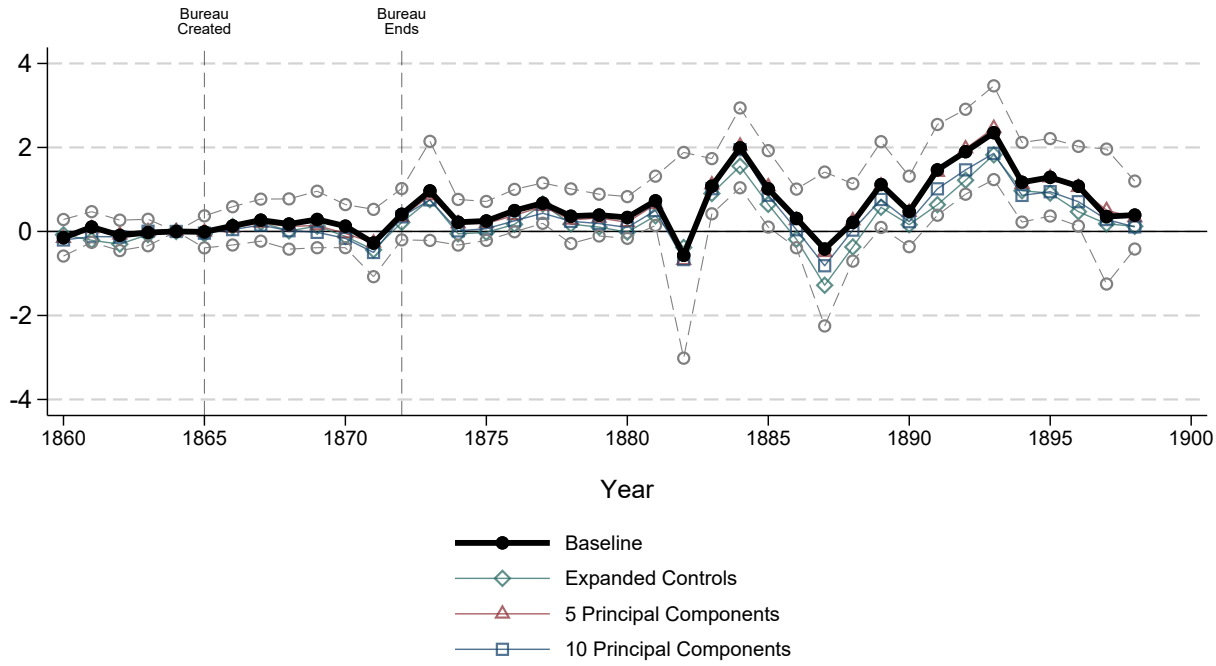


(b) Republican Vote Share (1856–1900)



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 second specification (hollow diamonds) instead includes interactions between year indicators and the expanded controls, which are all variables listed in Table 1. The third and fourth specifications modify the baseline specification by replacing the core controls with the top 5 (hollow triangles) or the top 10 (hollow squares) principal components of the expanded controls. 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. The dashed surrounding the point estimates represent the 95% confidence interval based on standard errors clustered at the county level. For additional notes, see Figure 3.

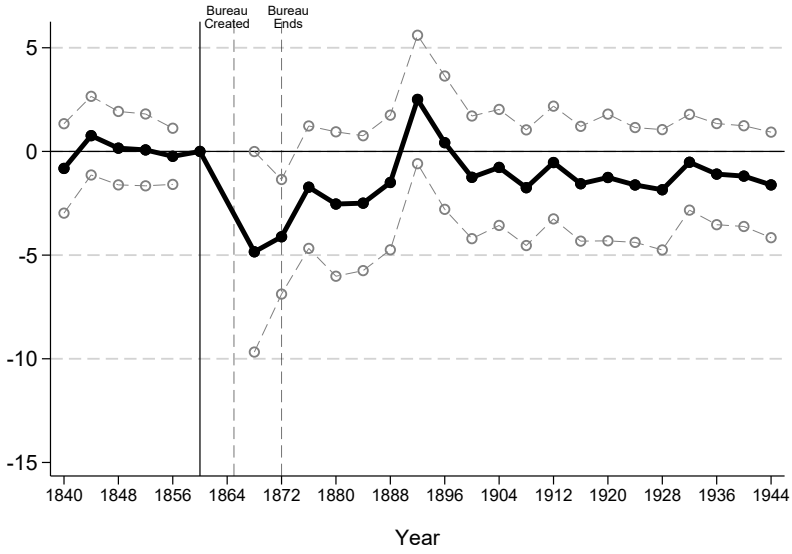
Figure A9: Impacts on Newspaper-Based Mentions of Lynchings, Robustness to Baseline Controls



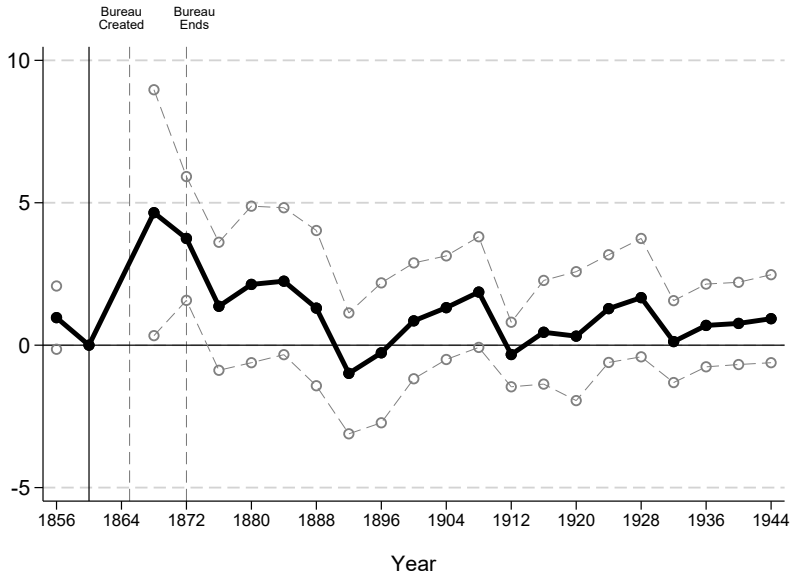
Notes: This figure reports event-study estimates of the impacts of having at least one Freedmen's Bureau field office on county-level rates of newspaper mentions of lynching (100 times the number of pages on which the term lynching is found divided by the total number of newspaper pages). The baseline results are based on a regression that includes interactions between the county having any Freedmen's Bureau field office and year indicators, year indicators, interactions between year indicators and the core controls listed in Panel A of Table 1, and county fixed effects. The second specification (hollow squares) instead includes interactions between year indicators and the expanded controls, which are all variables listed in Table 1. The third and fourth specifications modify the baseline specification by replacing the core controls with the top 5 (hollow triangles) or the top 10 (hollow squares) principal components of the expanded controls. 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 1864. The dashed 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 A10: Impacts on Presidential Vote Shares, Evidence from Extended Time Period

(a) Democratic Vote Share (1852–1944)

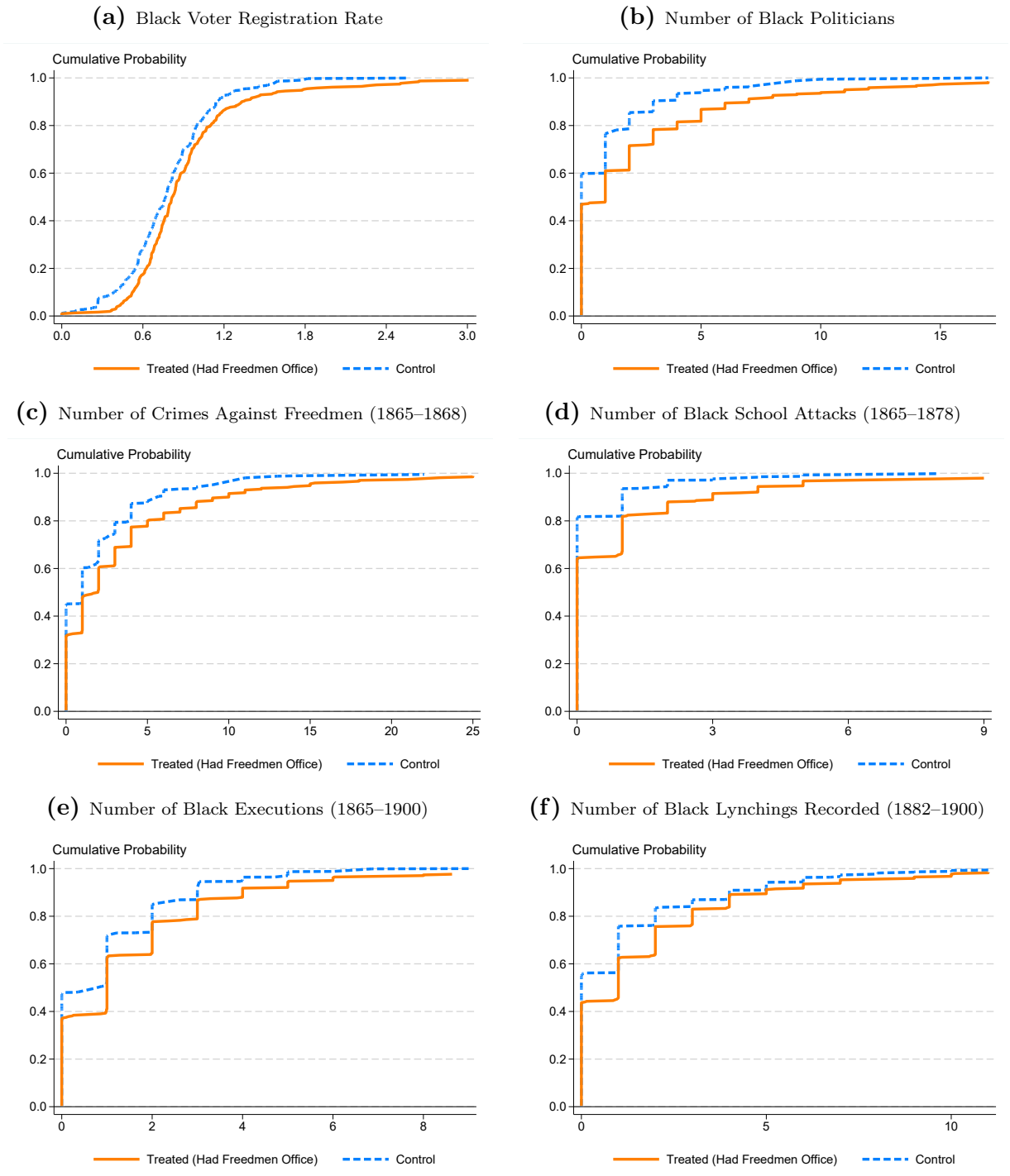


(b) Republican Vote Share (1856–1944)



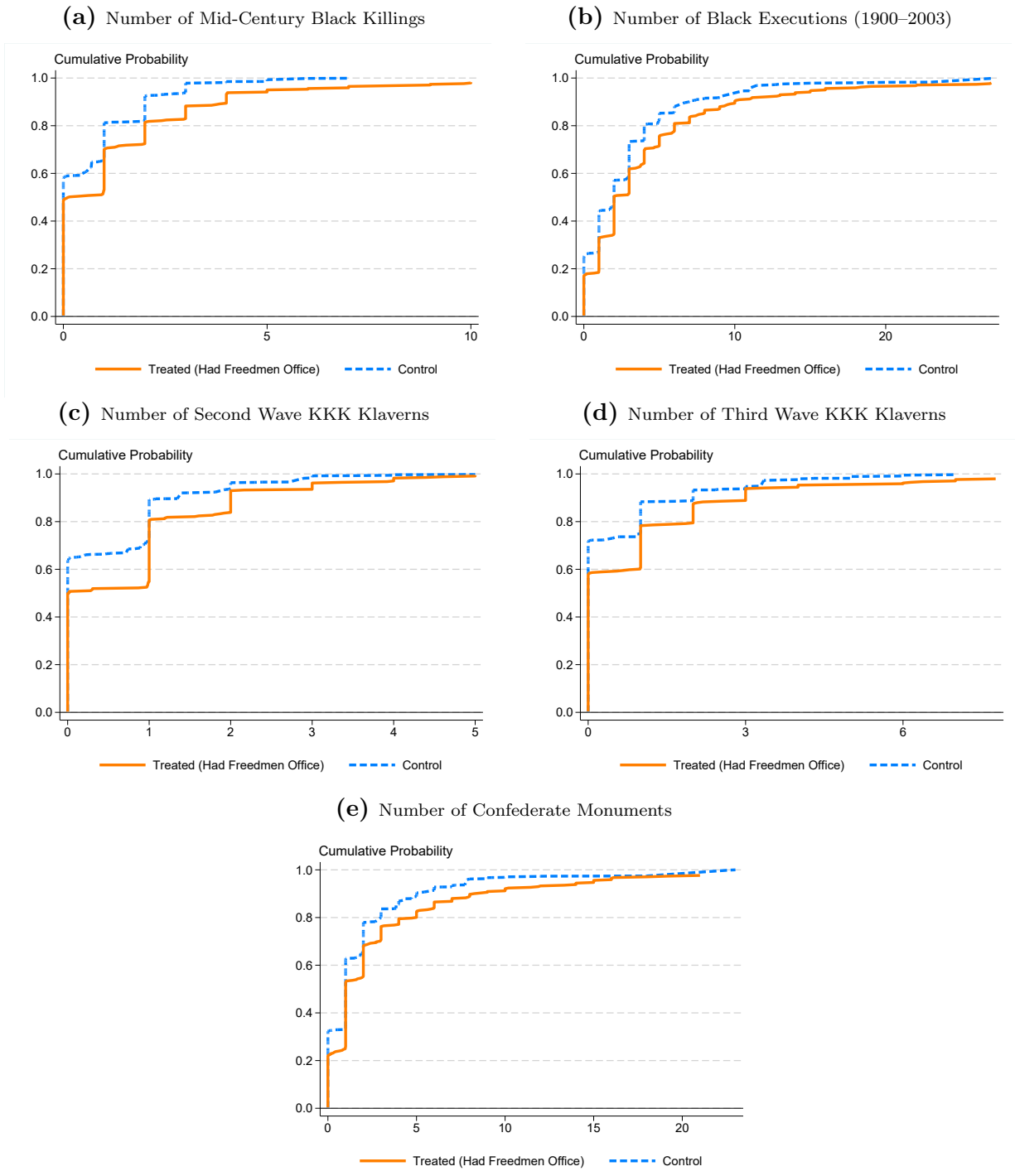
Notes: Panels A and B report event-study estimates (black dots) of the effects of having at least one Freedmen's Bureau field office on county-level Presidential Democratic and Republican Party vote shares, respectively, over an extended time horizon. The results are based on a regression that includes 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. 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 3.

Figure A11: Distributions of Political and Racial Violence Outcomes



Notes: This figure plots CDFs of political and racial violence outcomes using the matched sample of counties used in our main analysis. CDFs are shown separately for the treated counties that did have Bureau field offices and control counties that did not. We use ATT weights to compute the CDFs using the matched sample of control counties. CDFs are winsorized at the 99th percentile. The voter registration rate in Panel A is the number of registered Black voters in 1867–1869 divided by an estimate of the voting eligible population, i.e., one-forth of the 1860 Black population.

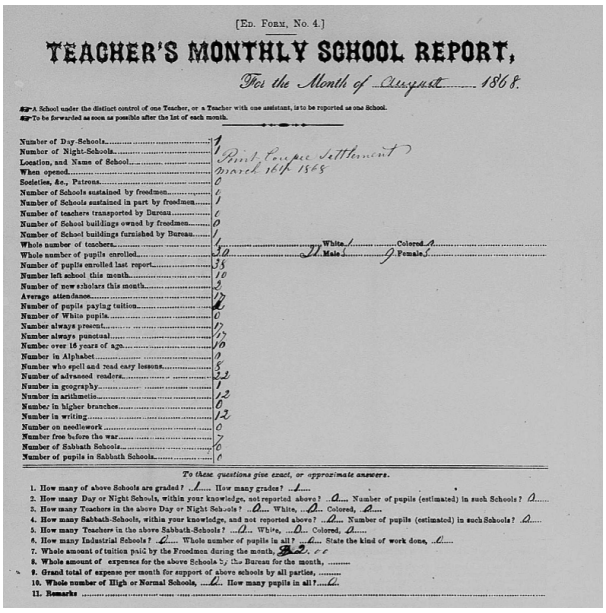
Figure A12: Distributions of Twentieth Century Racial Violence and Monument Outcomes



Notes: This figure plots CDFs of twentieth century outcomes using the matched sample of counties used in our main analysis. CDFs are shown separately for the treated counties that did have Bureau field offices and control counties that did not. We use ATT weights to compute the CDFs using the matched sample of control counties. CDFs are winsorized at the 99th percentile.

Figure A13: Bureau Schools and Night Schools Example

(a) Full Teacher Report



[Ed. Form, No. 4.]

TEACHER'S MONTHLY SCHOOL REPORT,

For the Month of August 1868.

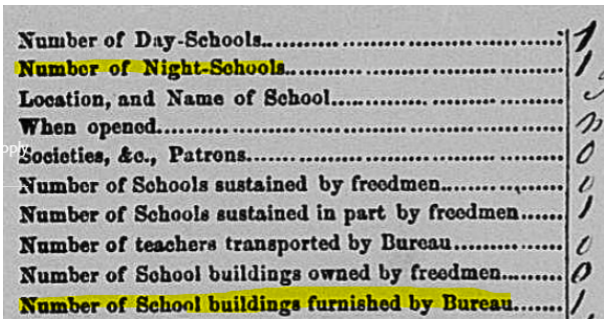
☞A School under the distinct control of one Teacher, or a Teacher with one assistant, is to be reported as one School.
☞To be forwarded as soon as possible after the 1st of each month.

Number of Day-Schools	1
Number of Night-Schools	
Location, and Name of School	Point Coupee Settlement
When opened	March 16th 1868
Societies, &c., Patrons	
Number of Schools sustained by freedmen	1
Number of Schools sustained in part by freedmen	1
Number of teachers transported by Bureau	0
Number of School buildings owned by freedmen	0
Number of School buildings furnished by Bureau	1
Whole number of teachers	1 (White 1, Colored 0)
Whole number of pupils enrolled	50 (Male 1, Female 1)
Number of pupils enrolled last report	35
Number left school this month	10
Number of new scholars this month	3
Average attendance	17
Number of pupils paying tuition	2
Number of White pupils	0
Number always present	12
Number always punctual	17
Number over 16 years of age	10
Number in Alphabet	0
Number who spell and read easy lessons	5
Number of advanced readers	32
Number in geography	1
Number in arithmetic	12
Number in higher branches	0
Number in writing	12
Number on needlework	0
Number free before the war	7
Number of Sabbath Schools	0
Number of pupils in Sabbath Schools	0

To these questions give exact, or approximate answers.

1. How many of above Schools are graded? 1 How many grades? 1
2. How many Day or Night Schools, within your knowledge, not reported above? 0 Number of pupils (estimated) in such Schools? 0
3. How many Teachers in the above Day or Night Schools? 0 White, 0 Colored, 0
4. How many Sabbath-Schools, within your knowledge, and not reported above? 0 Number of pupils (estimated) in such Schools? 0
5. How many Teachers in the above Sabbath-Schools? 0 White, 0 Colored, 0
6. How many Industrial Schools? 0 Whole number of pupils in all? 0 State the kind of work done, 0
7. Whole amount of tuition paid by the Freedmen during the month, $2.00
8. Whole amount of expenses for the above Schools by the Bureau for the month,
9. Grand total of expense per month for support of above schools by all parties,
10. Whole number of High or Normal Schools, 0 How many pupils in all? 0
11. Remarks

(b) Night School and Bureau Buildings Lines



Number of Day-Schools	1
Number of Night-Schools	1
Location, and Name of School	Point Coupee Settlement
When opened	March 16th 1868
Societies, &c., Patrons	0
Number of Schools sustained by freedmen	0
Number of Schools sustained in part by freedmen	1
Number of teachers transported by Bureau	0
Number of School buildings owned by freedmen	0
Number of School buildings furnished by Bureau	1

Notes: This figure shows an official Bureau monthly school report from from August 1868. Panel A displays the entire report, while Panel B shows the selected portion that highlights information on the school and its number of night schools and the number of buildings furnished by the Bureau (Records of Superintendent of Education for the state of Louisiana, 1864-1869, microfilm publication M1026, Roll 6).

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	-0.019	0.024
			(0.050)	(0.041)	(0.034)	(0.032)
Change in Log Black Population 1850–60	0.435	0.497	-0.063	0.043	-0.008	-0.001
			(0.055)	(0.044)	(0.037)	(0.033)
Change in Black Population Share 1850–60	0.024	0.010	0.015***	0.016***	0.007	-0.003
			(0.005)	(0.005)	(0.005)	(0.004)
Change in Log Total Population 1850–60	0.379	0.513	-0.134***	-0.031	-0.002	0.020
			(0.052)	(0.042)	(0.035)	(0.032)
Change in Log Manufacturing Output 1850–60	1.207	1.085	0.122	0.246	0.036	-0.076
			(0.293)	(0.297)	(0.326)	(0.341)
Change in Log Farm Value 1850–60	1.003	1.095	-0.092	0.019	-0.027	-0.019
			(0.110)	(0.091)	(0.084)	(0.084)
Change in Churches per 1000 People 1850–1860	0.186	0.256	-0.072	-0.085	-0.063	0.020
			(0.095)	(0.088)	(0.090)	(0.076)
State Fixed Effects	–	–	No	Yes	Yes	Yes
Matched Sample	–	–	No	No	Yes	Yes
Reweighted	–	–	No	No	No	Yes
Counties	341	543	884	884	699	699

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 heteroskedasticity-robust standard errors 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 Governor Elections, Difference-in-Difference Estimates

	(1)	(2)
	Democratic	Republican
Post '67-'72 × 1(Freedmen Office)	-1.598	2.475**
	(1.425)	(1.196)
Post '73-'00 × 1(Freedmen Office)	0.671	0.617
	(1.402)	(0.892)
Control Mean	54.91	22.52
R-Squared	0.806	0.872
Observations	6136	6136
County FE	Yes	Yes

Notes: This table reports estimates of a difference-in-difference specification in which gubernatorial election 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 1867 to 1872 and 1873 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. The pre-period encompasses elections held from 1855 to 1866. 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. We restrict the sample to counties observed in all of their state-specific set of elections from 1840 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 A3: Violence During vs. After Bureau's Defunding

	Mean of DV	1865 to 1878	'65 to '69	'70 to '78
	(1)	(2)	(3)	(4)
Black School Attacks	0.375	0.670***	0.265***	0.405***
		(0.167)	(0.074)	(0.146)
R-squared		0.243	0.225	0.199
Black Executions	0.136	0.135**	0.049*	0.086
		(0.060)	(0.028)	(0.054)
R-squared		0.161	0.188	0.094
Observations		699	699	699

Notes: This table reports effects on violence (attacks on Black schools and the number of executions of Black individuals) during and immediately after the Bureau's defunding at the end of 1869. The first row of column 2 reports the same results as Panel B of Table 5, while columns 3 and 4 restrict to Black school attacks between 1865 to the end of 1869 and those between 1870 and the end of 1878 respectively. Column 2 of the second row reports the outcome of Panel C of Table 5; however, it first limits to the same time period as Black school attacks (i.e. 1865 to 1878), and then restricts to the same split as the first row in columns 3 and 4. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: 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)	0.168***	0.160***	0.172***
	(0.035)	(0.036)	(0.036)
Control Mean	0.301	0.283	0.283
R-squared	0.165	0.183	0.208
Panel B. Third Wave KKK Klavern in 1960s			
1(Freedmen Office)	0.115***	0.106***	0.120***
	(0.035)	(0.035)	(0.040)
Control Mean	0.254	0.309	0.309
R-squared	0.286	0.278	0.263
Observations	884	699	699
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 C 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 D of Table 6). Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A5: Impacts on Long-Run Opportunity as Measured by Teenage Pregnancy, OLS and Matched Sample Estimates

	(1)	(2)	(3)
1(Freedmen Office)	0.007*	0.007*	0.007
	(0.004)	(0.004)	(0.005)
Control Mean	0.345	0.357	0.357
R-squared	0.477	0.486	0.490
Observations	884	699	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	No	Yes	Yes
Reweighted	No	No	Yes

Notes: This table reports estimates from 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 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. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A6: Impacts on Long-Run Opportunity, By Race

Group	Pooled (1)	Black (2)	White (3)
Panel A. Upward Mobility (1978–1983 Cohorts)			
1(Freedmen Office)	-0.004**	-0.003*	0.001
	(0.002)	(0.002)	(0.002)
Control Mean	0.383	0.324	0.429
R-squared	0.627	0.857	0.947
Observations	699	675	699
Panel B. Incarceration Rate (1978–1983 Cohorts)			
1(Freedmen Office)	0.004**	0.006*	0.000
	(0.002)	(0.003)	(0.001)
Control Mean	0.051	0.100	0.033
R-squared	0.998	0.675	0.951
Observations	699	649	698
Panel C. Teenage Pregnancy (1978–1983 Cohorts)			
1(Freedmen Office)	0.007	0.006	-0.003
	(0.005)	(0.005)	(0.004)
Control Mean	0.357	0.452	0.285
R-squared	0.979	0.722	0.932
Observations	699	656	699
State FE and Core Controls	Yes	Yes	Yes
Matched Sample	Yes	Yes	Yes
Matched Sample, Weighted	Yes	Yes	Yes

Notes: This table reports estimates from regressions of long-run outcomes (mobility and incarceration during adulthood and teenage pregnancy, taken from the Opportunity Atlas) on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the core control variables listed in Panel A of Table 1. Column 1 reports results for all individuals, while columns 2 and 3 report results for Black and White individuals. All columns are limited to a sample of similar counties matched on these core variables and apply 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. The outcome in Panel C 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). Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A7: 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-'82 × 1(Freedmen Office)	0.330**	0.316**	0.325**	0.307*	0.394**	0.395**	0.287*
	(0.149)	(0.150)	(0.146)	(0.165)	(0.174)	(0.158)	(0.155)
Post '83-'00 × 1(Freedmen Office)	0.911***	0.932***	0.904***	0.787***	0.899***	0.945***	0.877***
	(0.279)	(0.291)	(0.279)	(0.289)	(0.289)	(0.290)	(0.278)
Control Mean	1.385	1.385	1.385	1.385	1.385	1.385	1.385
R-squared	0.356	0.357	0.356	0.373	0.361	0.357	0.361
Observations	4313	4313	4313	4313	4313	4313	4313
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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 A8: Impacts on Reconstruction Era and Long-Run Outcomes, Robustness to Different Controls

Dependent variable	(1) DV Mean	(2) Baseline	(3) + Troops	(4) + Bank	(5) Expanded Controls
Black voter registration rate	0.774	0.129***	0.162***	0.133***	0.129***
		(0.032)	(0.036)	(0.032)	(0.032)
Black officeholders	0.638	1.246***	0.982***	1.251***	1.154***
		(0.307)	(0.317)	(0.306)	(0.320)
Crimes against Freedmen	1.683	1.350***	1.204**	1.377***	1.370***
		(0.466)	(0.513)	(0.462)	(0.508)
Black school attacks	0.375	0.670***	0.712***	0.677***	0.736***
		(0.167)	(0.163)	(0.166)	(0.162)
Black executions (1865 to 1900)	0.755	0.534***	0.432***	0.537***	0.549***
		(0.154)	(0.157)	(0.154)	(0.145)
Black lynchings	1.316	0.350*	0.382**	0.356*	0.241
		(0.183)	(0.186)	(0.182)	(0.183)
Black executions (1900 to 2003)	2.386	1.482***	1.497***	1.454***	1.327***
		(0.509)	(0.507)	(0.505)	(0.510)
Mid-century killings	0.685	0.903***	0.831***	0.906***	0.862***
		(0.311)	(0.310)	(0.311)	(0.310)
2nd wave KKK	0.415	0.380***	0.378***	0.382***	0.347***
		(0.070)	(0.070)	(0.069)	(0.068)
3rd wave KKK	0.602	0.465***	0.439***	0.466***	0.434***
		(0.127)	(0.130)	(0.127)	(0.130)
Confederate monuments	1.654	1.424***	1.362***	1.430***	1.448***
		(0.431)	(0.469)	(0.428)	(0.409)
Income mobility	0.383	-0.004**	-0.006***	-0.004**	-0.004**
		(0.002)	(0.002)	(0.002)	(0.002)
Incarceration	0.051	0.004**	0.003**	0.004**	0.003**
		(0.002)	(0.002)	(0.002)	(0.001)

Notes: This table reports results from alternative specifications intended to assess the robustness of our matching approach. Column 1 reports the mean of each dependent variable for control counties. 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 expanded set of controls in Panels A and B of Table 1. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A9: Impacts on Presidential Election Shares, Robustness to Excluding Different States

	(1) Baseline	(2) Drop AL	(3) Drop AR	(4) Drop FL	(5) Drop GA	(6) Drop LA	(7) Drop MS	(8) Drop NC	(9) Drop TN	(10) Drop TX	(11) Drop VA
Panel A. Democratic Presidential Vote Share											
Post '68–'72 × 1(Freedmen Office)	-4.012***	-4.420***	-4.486***	-3.955**	-1.262	-3.654**	-3.637**	-5.919***	-4.424***	-4.114**	-3.915**
	(1.538)	(1.611)	(1.601)	(1.560)	(1.579)	(1.548)	(1.642)	(1.615)	(1.619)	(1.633)	(1.727)
Post '73–'00 × 1(Freedmen Office)	-0.724	-0.361	-0.516	-0.624	0.933	-1.598	-0.714	-2.253*	-1.019	-0.619	-0.313
	(1.335)	(1.393)	(1.426)	(1.354)	(1.455)	(1.294)	(1.420)	(1.323)	(1.419)	(1.438)	(1.495)
R-squared	0.480	0.493	0.479	0.486	0.497	0.493	0.449	0.474	0.467	0.492	0.467
Panel B. Republican Presidential Vote Share											
Post '68–'72 × 1(Freedmen Office)	3.401***	3.986***	3.546***	3.392**	1.770	3.084**	2.950**	4.454***	3.685***	3.259**	3.856***
	(1.298)	(1.357)	(1.350)	(1.318)	(1.290)	(1.321)	(1.383)	(1.445)	(1.315)	(1.375)	(1.435)
Post '73–'00 × 1(Freedmen Office)	0.486	0.338	-0.097	0.442	-0.173	1.426	0.310	1.183	0.758	0.056	0.783
	(0.972)	(1.030)	(1.028)	(0.990)	(1.008)	(0.911)	(1.031)	(1.032)	(1.016)	(1.032)	(1.120)
R-squared	0.708	0.716	0.706	0.714	0.717	0.735	0.706	0.711	0.700	0.704	0.671
Observations	7381	6877	6765	7182	6084	6735	6654	6448	6647	6585	6452

Notes: This table reports estimates from a difference-in-difference specification in which Presidential 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 1872 and 1873 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. Column 1 reports our baseline results. Columns 2–11 report results in which individual states are dropped from the analysis. 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 A10: Impacts on Reconstruction Era and Long-Run Outcomes, Robustness to Excluding Different States

Dependent variable	(1) DV Mean	(2) Baseline	(3) Add SC	(4) Drop AL	(5) Drop AR	(6) Drop FL	(7) Drop GA	(8) Drop LA	(9) Drop MS	(10) Drop NC	(11) Drop TN	(12) Drop TX	(13) Drop VA
Black voter registration rate	0.774	0.129***	0.116***	0.136***	0.134***	0.137***	0.126***	0.140***	0.140***	0.142***	0.128***	0.095***	0.106***
		(0.032)	(0.030)	(0.033)	(0.032)	(0.033)	(0.037)	(0.034)	(0.035)	(0.036)	(0.032)	(0.030)	(0.035)
Black officeholders	0.638	1.246***	1.692***	1.150***	1.233***	1.244***	1.318***	1.013***	0.965***	1.283***	1.342***	1.433***	1.624***
		(0.307)	(0.353)	(0.310)	(0.330)	(0.312)	(0.357)	(0.295)	(0.316)	(0.293)	(0.325)	(0.333)	(0.346)
Crimes against Freedmen	1.683	1.350***	1.412***	1.401***	1.268***	1.309***	0.966*	1.341***	1.329***	1.070**	1.212**	1.655***	1.661***
		(0.466)	(0.464)	(0.499)	(0.478)	(0.473)	(0.502)	(0.465)	(0.469)	(0.516)	(0.488)	(0.473)	(0.583)
Black school attacks	0.375	0.670***	0.738***	0.638***	0.758***	0.703***	0.785***	0.632***	0.379***	0.788***	0.710***	0.740***	0.769***
		(0.167)	(0.170)	(0.168)	(0.179)	(0.171)	(0.194)	(0.173)	(0.116)	(0.183)	(0.177)	(0.183)	(0.199)
Black executions (1865 to 1900)	0.755	0.534***	0.567***	0.468***	0.485***	0.524***	0.474***	0.602***	0.543***	0.501***	0.525***	0.524***	0.744***
		(0.154)	(0.163)	(0.151)	(0.157)	(0.159)	(0.174)	(0.159)	(0.166)	(0.165)	(0.164)	(0.169)	(0.168)
Black lynchings	1.316	0.350*	0.203	0.258	0.470**	0.410**	0.317	0.343*	0.195	0.325	0.403**	0.403**	0.443**
		(0.183)	(0.211)	(0.183)	(0.188)	(0.183)	(0.213)	(0.180)	(0.177)	(0.206)	(0.190)	(0.188)	(0.218)
Black executions (1900 to 2003)	2.386	1.482***	1.585***	1.235**	1.455***	1.491***	1.255**	1.525***	1.452***	1.468***	1.392***	1.318***	2.494***
		(0.509)	(0.486)	(0.482)	(0.548)	(0.508)	(0.557)	(0.541)	(0.562)	(0.557)	(0.529)	(0.476)	(0.564)
Mid-century killings	0.685	0.903***	0.831***	0.563***	0.948***	0.921***	0.824**	0.853**	0.953***	0.990***	0.946***	0.985***	1.085***
		(0.311)	(0.286)	(0.150)	(0.337)	(0.318)	(0.359)	(0.336)	(0.342)	(0.348)	(0.336)	(0.349)	(0.373)
2nd wave KKK	0.415	0.380***	0.373***	0.354***	0.389***	0.400***	0.395***	0.305***	0.379***	0.416***	0.339***	0.382***	0.517***
		(0.070)	(0.066)	(0.070)	(0.073)	(0.070)	(0.077)	(0.069)	(0.076)	(0.076)	(0.071)	(0.073)	(0.077)
3rd wave KKK	0.602	0.465***	0.446***	0.498***	0.478***	0.435***	0.524***	0.415***	0.431***	0.348***	0.539***	0.476***	0.619***
		(0.127)	(0.122)	(0.132)	(0.136)	(0.126)	(0.144)	(0.130)	(0.132)	(0.110)	(0.134)	(0.141)	(0.146)
Confederate monuments	1.654	1.424***	1.468***	1.340***	1.486***	1.419***	1.588***	1.460***	1.451***	1.355***	1.428***	1.412***	1.667***
		(0.431)	(0.428)	(0.429)	(0.471)	(0.439)	(0.473)	(0.447)	(0.458)	(0.467)	(0.443)	(0.455)	(0.371)
Income mobility	0.383	-0.004**	-0.003*	-0.004*	-0.003	-0.004**	-0.003	-0.005**	-0.004*	-0.005**	-0.005**	-0.005**	-0.006***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Incarceration	0.051	0.004**	0.003**	0.003*	0.003**	0.004**	0.004**	0.003**	0.003**	0.004**	0.003**	0.004**	0.006***
		(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)

Notes: This table reports results from alternative samples. Column 1 reports the mean of each dependent variable for control counties. Column 2 reports our baseline specification. Column 3 reports results when adding South Carolina to the sample. Columns 4–13 report results in which individual states are dropped from the analysis. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A11: Assessing Spillover Effects on Nearby Counties

Treatment variable:	(1) 1(Freedmen Office)	(2) 1(Freedmen Office within 23 miles)
Black political power index	0.444***	0.034
	(0.083)	(0.079)
R-squared	0.392	0.241
19th century violence/hate index	0.313***	-0.088
	(0.059)	(0.060)
R-squared	0.326	0.227
20th century violence/hate index	0.447***	-0.107*
	(0.118)	(0.056)
R-squared	0.213	0.147
Observations	699	358
Treatment variable mean	0.488	0.492

Notes: This table reports estimates from regressions of summary indices on either an indicator for the county having any Freedmen's Bureau field office (column 1) or an indicator for the county have any Freedmen's Bureau field office within 23 miles (column 2), which is the distance that splits control counties into two equally-sized groups. The column 1 sample contains the matched set of counties with and without field offices, while the sample in column 2 is limited to these control counties. All regressions include state fixed effects and the core control variables listed in Panel A of Table 1. The estimates in column 1 differ slightly from those reported in the main text because the estimates in this table are not constructed using propensity score weights. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A12: Impacts on Black Occupation Outcomes, Matching Results

	1870		1900		1920	
	Mean (1)	Estimates (2)	Mean (3)	Estimates (4)	Mean (5)	Estimates (6)
Share in farmer occupation	0.100	0.005	0.181	-0.015***	0.196	-0.019***
		(0.007)		(0.005)		(0.006)
Share in farmer occupation (owner-occupant)	0.014	-0.000	0.057	-0.003	0.057	-0.002
		(0.001)		(0.003)		(0.003)
Share in farmer occupation (renter)	0.086	0.005	0.123	-0.011**	0.139	-0.018***
		(0.007)		(0.005)		(0.006)
Share in farm laborer occupation	0.356	-0.023	0.193	-0.024***	0.169	-0.017**
		(0.015)		(0.006)		(0.007)
Share in domestic service occupation	0.058	0.002	0.042	0.014***	0.071	0.015***
		(0.004)		(0.002)		(0.003)

Notes: This table reports estimates from regressions of share of Black individuals ages 16–64 with the indicated occupation on an indicator for the county having any Freedmen's Bureau office, state fixed effects, and the core control variables listed in Panel A of Table 1. Columns 1 and 2 are based on outcomes from the 1870 Census, columns 3 and 4 are based on the 1900 Census, and columns 5 and 6 are based on the 1920 Census. All results are based on our baseline regression adjustment and propensity score weighting approach. Heteroskedasticity-robust standard errors are reported in parentheses. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B Data Appendix

B.1 Sample Details

The starting 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.[53]

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 778 counties (88% of the starting sample of 884 counties). Most counties from our main analysis sample that lack Black voter registration data are in Tennessee (90%). Second, the newspaper data for annual lynching reports is available for 381 counties (43% of the starting 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 starting sample).[54] 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 9 (with a matching score above 0.97/1) 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)

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

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

using 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 1,757 cases. We prioritize the Census Place Project in the cases of exact matches (and slight misspellings) but use the latter approach for the remaining 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*."[55] 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).[56] 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,

[55] 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 1,797 newspapers (across 381 counties in all years and 373 counties after 1865) overlap with our Freedmen's Bureau data sample.

[56] 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.[57] 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

[57] 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.[58] 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 1,646 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.

[58]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

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

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

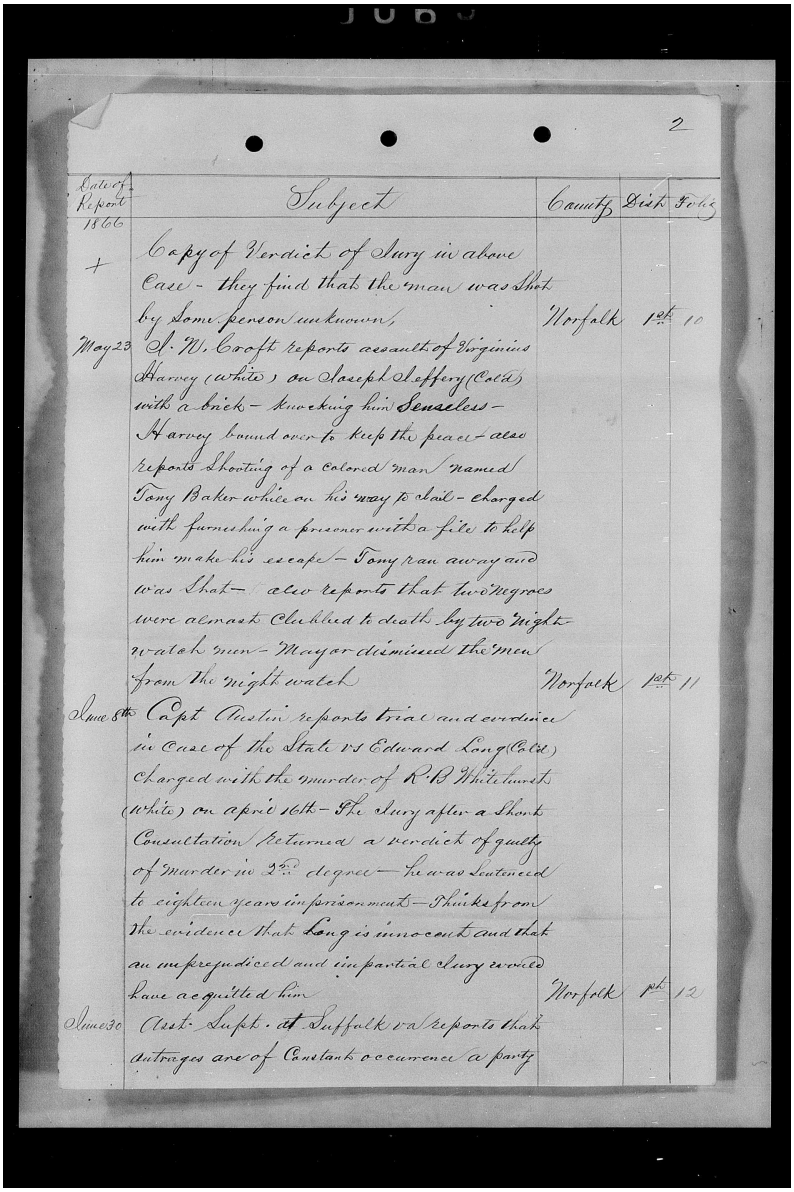
SOLDIERS OUT ON BOND.

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)

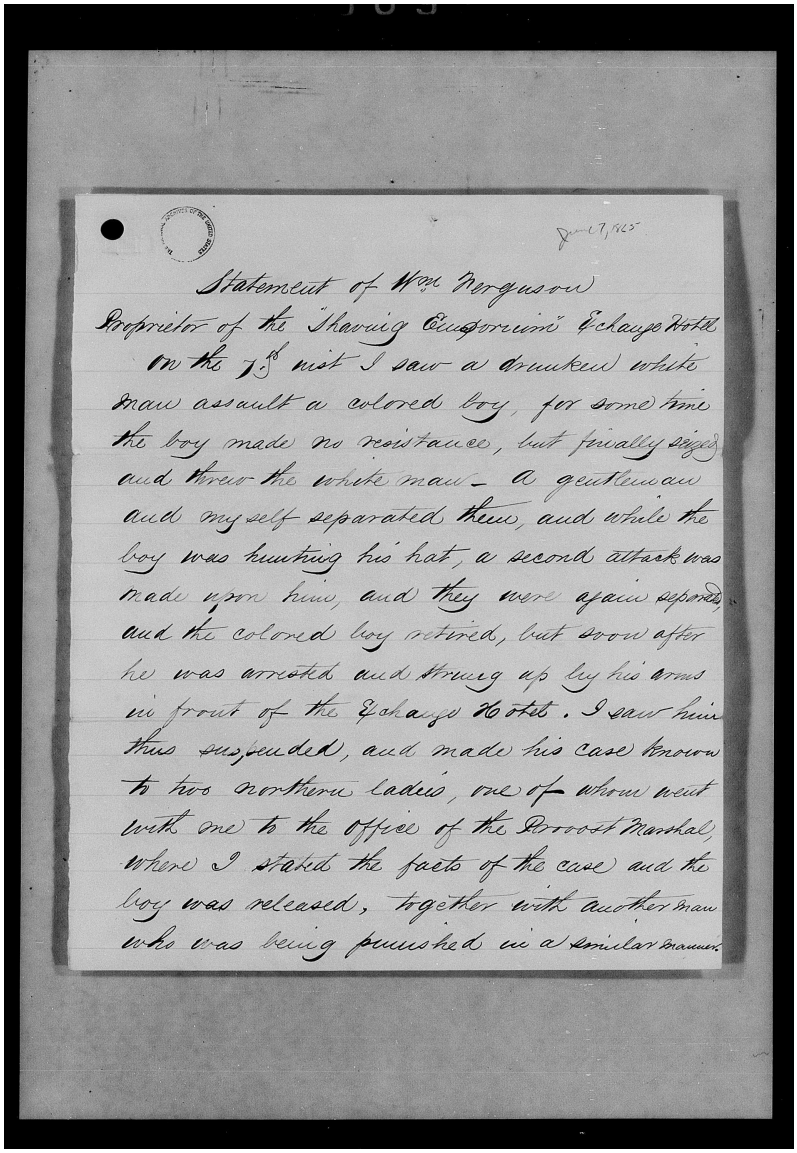


2

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	L. 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 occurence 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 rose
and threw the white man - a gentleman
and myself 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.[59] 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.[60] 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.[61]

Notwithstanding these complications, information on total spending by the War Department is available from General Oliver O. Howard in the Secretary of War's Annual Reports to the President. Appendix 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.[62]

Appendix Table C1 shows that spending on Bureau staff declined from the fiscal year prior to October 24, 1868 from an amount of $864,743.91 down to just $45,809.72 by October 20, 1871. Note that we do not have detailed information on staff spending in the first year and a half of operations and need to use an estimate of spending in 1866.[63] While the annual

[59]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), 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 [emphasis added]." 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 considered, the Bureau reopened the courts to continue protecting freedmen's rights.

[60]The report notes: "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."

[61]The 1868 Tennessee Report writes: "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."

[62]For example, the 1867 report covers the period of 11 months ending August 31, 1867.

[63]The 1866 spending noted in the relevant table of the Report of 1866 is incongruous with the Commissioner's overall summary discussed in the text. The latter noted that average monthly expenditures were

reports do not note the number of agents in each year, the 1866 report does provide a breakdown of the (salaried) agents by state.

Appendix Table C2 reports 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 302 and 112 respectively, for a total salaried staff of 414. As our sample is composed of 341 counties with Bureau offices, this amounts to roughly 1.2 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.

Appendix Table C3 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 state where they worked (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.

While the Official Registers provide rich data (including birth state and compensation) on U.S. government personnel in these odd years, it is unclear how complete these are, above and beyond the issue of unpaid staff/volunteers noted in the beginning of this section. One hint of the incomplete nature of the records 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 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). The final column of Appendix Table C3 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 is 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. Moreover, none of the staff have their position listed as "clerk" (most staff are listed as Agent, Commissioner, Subassistant Commissioner, Assistant Subassistant Commissioner, or Superintendent). While the historical data provide useful context on the Bureau, we view them as sufficiently limited to prevent the use of

about $350,000. This would amount to total annual spending of $4.2 million, which is more in line with other estimates in our Table C1. Higher expenditures from 1865–1866 would not be surprising because the Bureau was particularly active in providing food and medical care during those years.

staffing as our main measure of the treatment.

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

Period of Reporting	Salaries of Agents/Commissioners	Salaries of Clerks	Total Expenditures
Jan 1, 1865 - Oct 31, 1865			$478,363.17*
Nov 1, 1865 - Sep 30, 1866			$3,850,000.00**
Oct 1, 1866 - Aug 31, 1867	$184,336.19	$337,085.25	$3,279,497.43
Sep 1, 1867 - Aug 31, 1868	$455,473.06	$409,270.85	$3,814,425.19
Sep 1, 1868 - Aug 31, 1869	$278,691.07	$329,049.05	$2,170,211.52
Sep 1, 1869 - Aug 31, 1870	$65,809.34	$91,805.37	$1,535,747.91
Sep 1, 1870 - Aug 31, 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 716), 1867 (page 656), 1868 (page 1022), 1869 (page 522), 1870 (page 325), and 1871 (page 453); see https://catalog.hathitrust.org/Record/000078451. In the first row, we used expenditures from the Report of the Commissioner of the Bureau of Refugees, Freedmen, and Abandoned Lands (page 16), which reports expenditures from January 1, 1865 to October 31, 1865 (10 months), though the Act which formed the Bureau officially was passed in March of 1865. For 1865 (*), we do not have breakdown of salaries by employee type, but the report does note "Labor (by freedmen and other employees)" totaled "$237,697.62." For 1866 (**), the table of expenditures analogous to other years (page 715) provides abnormally low expenditure totals which reflect a small part of spending due to accounting shifts. In lieu of these partial numbers, we impute annual expenditure using a summary below this source table (on page 716) which notes, "...average expenditure per month is about three hundred and fifty thousand dollars, ($350,000)." We multiply this average by 11 months (the period covered in the table) to arrive at our estimate of $3,850,000. For all subsequent years, reports appear to cover 12 months but just note the end dates (so as not to generate overlap with 1866, the third row covers just 11 months). 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."

Table C2: Bureau Personnel as of November 1, 1866 Annual Report

State	Individuals under pay		
	Officers (1)	Civilians (2)	Total (3)
Alabama	20	14	34
Arkansas	25	4	29
Florida	20	5	25
Georgia	24	18	42
Louisiana	48	19	67
Mississippi	24	7	31
North Carolina	35	13	48
Tennessee	15	8	23
Texas	25	2	27
Virginia	66	22	88
All	302	112	414

Notes: The information in this table comes from page 753 of the 1866 Annual Report of the Secretary of War to the President.

Table C3: 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	77
All	493	211	213	$570,292	$1,161	10	19	513

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). 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).