

Financial Inclusion, Human Capital, and Wealth Accumulation: Evidence from the Freedman's Savings Bank

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This paper studies how access to financial services among a previously unbanked group affects human capital, labor market, and wealth outcomes. We use novel data from the Freedman's Savings Bank—created following the American Civil War to serve free Blacks—employing an instrumental variables strategy exploiting the staggered rollout of bank branches. Families with accounts are more likely to have children in school, be literate, work, and have higher occupational income, business ownership, and real estate wealth. Placebo effects are not present using planned but unbuilt branches, or for Whites, suggesting significant positive effects of financial inclusion. (*JEL* G21, G51, I22, N22, N31)

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“The mission of the Freedman’s Bank is to show our people the road to a share of the wealth and well being of the world.”

—Frederick Douglass

The ability to save allows agents to make investments when costs are lumpy and income streams vary over time. Access to financial services is a hallmark of developed societies, and research—primarily in developing countries—has found that financial inclusion promotes business and human capital investment among the poor (Dupas and Robinson 2013a; Karlan and Morduch 2009). Approximately one-quarter of the U.S. population is unbanked or underbanked, and there exist persistent racial and ethnic gaps in access to and utilization of financial services. Gaps in education and labor market outcomes in developed countries may be at least partially explained by these differences in the utilization of financial services.¹ This paper uses the creation of a bank designed to cater to recently freed slaves to explore the impacts of a large-scale increase in financial inclusion on a population that previously had no access to financial services.

The aim of this paper is to study how financial inclusion and the provision of financial services affect investment in human capital and labor market outcomes. Theoretical work has long considered that capital market imperfections may distort labor market outcomes (Banerjee and Newman 1993) and human capital investment (Anderson and Baland 2002).² Previous studies have largely focused on experiments in developing countries or marginal changes in financial inclusion, but larger-scale changes in financial inclusion may have different and possibly larger effects on investment if there are significant externalities and complementarities (Economides 1993). We study a change in access to financial services in which a significant share of the population quickly shifted from having essentially no access to banking services to having access to a large bank.

To explore this topic we use new data in a previously unexplored context—among African Americans in the nineteenth-century post-bellum American South. We exploit the creation of the Freedman’s Savings Bank, a financial institution set up to serve recently freed slaves following the American Civil War. To generate variation in access to financial services, we use the fact that, while the Freedman’s Savings

¹ Approximately 40% of the global population is unbanked, including 30% of the American poor. The gap in holding a bank account between different groups has long been noted by policymakers. Federal Deposit Insurance Corporation surveys in 2015 indicate that 18.2% of African Americans are unbanked, compared with 3.1% of Whites. The Equal Credit Opportunity Act of 1974 was explicitly motivated by a desire to address racial and ethnic gaps in access to consumer financial services.

² See Karlan and Morduch (2009) for a review of the literature on access to finance, largely focusing on the unbanked in developing countries.

Bank planned to build numerous bank branches across the South, some branch openings were delayed due to financial and political pressure, and the bank eventually collapsed following the Panic of 1873. We find that access to financial services had large effects on human capital, wealth, and labor market outcomes. Individuals in families with a bank account were more likely to attend school, had higher levels of literacy, were more likely to work, earned more, and had higher levels of real estate wealth.

The Freedman's Savings Bank was an early government-sponsored private enterprise that was created by Congress to provide financial services to formerly enslaved African Americans. Prior to the creation of the bank, very few African Americans had access to financial services, which led to concerns that Black Union Army veterans would be unable to receive and save their pay. The bank spread rapidly, and at one point had more interstate branches than any other U.S. financial institution, and approximately one in eight Blacks in the South lived in a family that held an account with the bank. The bank collapsed in 1874 due to losses on several large loans to railroads and quarries in the aftermath of the Panic of 1873. Historians have pointed to the collapse of the Freedman's Savings Bank as a reason that utilization of financial services is lower among African Americans.³

We obtain novel data on Freedman's Savings Bank account holders from 27 branches with surviving bank records. These 107,197 account records include names of main account holders and their family members, totaling 483,082 non-unique individuals, roughly 12% of the 1870 Black population in the American South. We match these records to a sample of the 1870 U.S. decennial census, from which we can observe information on schooling, literacy, employment, and wealth.

We first regress outcomes on whether an individual holds an account with the Freedman's Savings Bank. We find small but significant effects—individuals in households with accounts are approximately one percentage point more likely to attend school and be literate, while they are approximately two percentage points more likely to work, and have higher incomes. There are two significant concerns with this simple approach. First, our matching strategy is inexact: names are not unique, and they were neither enumerated nor digitized perfectly. We thus measure account-holding status with error, which may attenuate our results. Second, individuals who hold accounts may be unobservably

³ For example, Osthaus (1976) argues that losses on deposit accounts that many African Americans erroneously believed were guaranteed by the federal government led many Blacks to subsequently mistrust financial institutions and doubt government guarantees, and Baradaran (2017, 31) suggests that “if the government and the philanthropists’ purpose was to teach the freed slaves thrift and responsibility, the lesson they actually learned was to distrust the government and philanthropists.”

different from those who do not. For example, account holders may have higher levels of financial literacy or may live in different areas, which could also bias our results.

To overcome these concerns, we exploit the fact that some branches opened later, and some planned branches were never completed due to financial pressure and the bank's ultimate collapse. We employ a strategy similar in spirit to Huber (2018) and Giorcelli (2018), and compare outcomes in 1870 for individuals who lived near branches built prior to 1870 with those who lived near branches built or planned to be built after 1870. We instrument for holding an account with distance from a pre-1870 branch and an indicator of whether an individual lives in a county with such a branch, restricting the sample to individuals living within 50 miles of a branch or planned branch. By comparing individuals living near branches built prior to 1870, and individuals living near branches that remained unbuilt as of 1870, we alleviate the concern that individuals living closer to branches may be different from those living further away.

The results using the instrumental variables strategy generate estimates that are largely similar to the ordinary least squares results, albeit larger, suggesting that attenuation bias dominates using the simple strategy. We find that individuals in families that hold Freedman's Savings Bank accounts are more likely to attend school, are more likely to be literate, are more likely to work, and have higher income and real estate wealth.

We complement our primary empirical strategy with a second strategy, exploiting political partisanship. The growth of the bank was heavily linked to Reconstruction-era politics, and Republican administrations were generally more supportive of the bank's expansion. We use results from the 1868 congressional election, instrumenting for account status using two measures of county-level Republican support. We find results very similar to those using our main empirical strategy, further confirming that access to financial services had real effects on outcomes.

To assess the validity of our empirical strategy, we conduct several placebo exercises. While we find significant effects of proximity from branches on individual outcomes, we find no effect of distance from planned branches. We also do not find significant effects for Whites living near built branches relative to planned branches. The effects are also largely not present for individuals who opened accounts after outcomes are measured in 1870. We find these effects after a relatively short period of potential treatment—five years. Consistent with a financial inclusion channel, we find larger effects for individuals treated earlier. While the time period may seem short, large effects from access to financial services over short time periods are consistent with work in development and

household finance (Augsburg et al. 2015; Bruhn and Love 2013; Morse 2011). We also show that our main results survive a battery of robustness checks.

This paper joins a body of literature studying financial institutions in nineteenth-century America, and we examine a previously unstudied major institution, an early government-sponsored enterprise that provided financial services to Blacks.⁴ To our knowledge, this is the first paper to study how access to formal financial services through a bank affected individual depositor outcomes in the nineteenth century. While there exists significant work on banking in the nineteenth century (Benmelech and Moskowitz 2010; Calomiris and Carlson 2016, 2017; Frydman and Hilt 2015; Frydman, Hilt, and Zhou 2015) and work on household finance in the nineteenth century (Feigenbaum, Lee, and Mezzanotti 2018; Koudijs and Salisbury 2016, 2018), little of this work focuses on access to financial services among the poor or attempts to study the impact of financial inclusion on historical gaps between Blacks and Whites.

This paper also joins a literature on financial inclusion and the unbanked, which has largely focused on developing countries. Previous studies have largely focused on randomized control trials in developing countries, smaller populations, or marginal changes in financial inclusion and thus largely do not consider general equilibrium effects of large-scale changes in access to financial services (Ashraf, Karlan, and Yin 2006; Bruhn and Love 2013; De Mel, McIntosh, and Woodruff 1999; Dupas and Robinson 2013a,b). In contrast, this study focuses on a large-scale change in access to financial services, which affected 12% of Southern Blacks who lived in households that opened accounts.⁵

Much of the work in developing countries has found effects of access to savings accounts even in the absence of credit. For example, Schaner (2018) finds long-run effects on income and assets from savings accounts using a randomized controlled trial. Study participants who received the highest interest rate on their individual account were 28% more likely to be entrepreneurs and had substantially more business profit and capital at the end of the experiment. Dupas and Robinson (2013a) find that a simple safe place to save is enough to increase preventive health investment by at least 66%. Dupas and Robinson (2013b) find that after six months, daily private expenditures were about 37% higher for market women in the treatment group. Agarwal et al. (2017) study

⁴ In a contemporaneous paper, Traweck and Wardlaw (2018) use detailed data on a small sample of Freedman's Savings Bank account holders. They consider account opening and depositor behavior in the weeks around a bank failure that precipitated the Panic of 1873 in order to assess predictions of the classic Diamond and Dybvig (1983) model. See Gorton (1988) and Calomiris and Gorton (1991) for a more general discussion of financial panics.

⁵ See Karlan and Morduch (2009) for a review of the development literature.

a larger financial inclusion program in India and its effects on lending and loan outcomes.

In addition to much of the work in developing countries, Celerier and Matray (2019) focus on how financial inclusion affects wealth accumulation using branch deregulation. Their study finds large benefits from financial inclusion, with banked households accumulating higher levels of debt and durable assets.⁶ Appel and Nickerson (2016) and Aaronson, Hartley, and Mazumder (2019) focus on the practice of “red-lining” and denial of access to real estate loans to African Americans in certain areas. Brown, Cookson, and Heimer (2019) focus on residents of Native American reservations and study credit market outcomes. This paper contributes to the literature on the unbanked in two ways. First, we focus on a very large change, in which a population moves from having essentially no access to financial services to a significant share of the population utilizing banking services. Thus we may capture important general equilibrium effects, and speak to a channel that is important theoretically but difficult to study in many modern contexts. Second, while previous studies such as Celerier and Matray (2019) and Appel and Nickerson (2016) focus on wealth accumulation, ours explores human capital and entrepreneurship outcomes.

Finally, our paper contributes to a growing literature assessing the role of finance in speeding economic growth and recovery following conflicts and disasters. Following the displacement and capital destruction associated with these large, negative economic shocks, households and firms may be particularly likely to benefit from the availability of access to savings and lending products. Theoretical and empirical evidence suggests that banks can and do have positive effects, including after the U.S. Civil War (Feigenbaum, Lee, and Mezzanotti 2018), the Bosnian War (Demirgüç-Kunt, Klapper, and Panos 2009), and various natural disasters (Cortés 2014; Cortés and Strahan 2017; McDermott, Barry, and Tol 2013).⁷

1. Data and institutional background

1.1 Institutional background

1.1.1 Reconstruction-era South. Prior to the American Civil War, approximately one-third of the population of the American South was composed of African-American slaves. The economy of the South was

⁶ Improved bank efficiency and heightened competition associated with deregulation in the 1970s–1990s also increased income among the poor (Beck, Levine, and Levkov 2010) and African Americans (Levine, Rubinstein, and Levkov 2014.)

⁷ Research has also documented a positive role in post-conflict and disaster recovery for non-bank financial service providers, including payday lenders (Morse 2011), country stores (Feigenbaum, Lee, and Mezzanotti 2018), and microfinance institutions (e.g., Doyle 1998; Marino 2005).

largely agrarian and a cotton monoculture based on slave labor.⁸ The Civil War was fought between the North and the South from 1861 to 1865, and led to the defeat and occupation of the South by the North. The war led to the abolition of slavery in the United States. The Emancipation Proclamation of 1863 freed former slaves in Confederate-held areas, but slavery was not legally abolished everywhere in the United States until the passage of the Thirteenth Amendment in 1865.

Reconstruction is typically dated by historians between 1865 and 1877, and refers to the period following the Civil War during which the North reintegrated the South into the United States. Integrating freed slaves was a priority of the United States government during Reconstruction, and efforts were made to ensure that free Blacks were granted rights such as citizenship, the right to vote, and access to education and public services.

The South was under military occupation by the Union Army, and divided into five occupation zones. The Bureau of Refugees, Freedmen, and Abandoned Lands, or Freedmen's Bureau, was set up by the United States Department of War to assist freed slaves and protect Blacks' new status. Northern Republicans moved to the South en masse, and attempted to politically organize Blacks. Within the Republican Party, there was a split between the "radical Republicans" led by Charles Sumner and Thaddeus Stevens, who promoted a forceful approach to integrating Blacks and achieving equality between Blacks and Whites, and more cautious mainstream Republicans led by Presidents Lincoln and Johnson, who sought to reconcile the Union with Southern Whites. Southern Democrats opposed moves promoting the integration of Blacks. Southern Whites were resentful of both Northern occupation and the new status of freed slaves, which led to tensions. Physical destruction was widespread following the Civil War, and no attempts were made to compensate Southerners for property lost during the war (Feigenbaum, Lee, and Mezzanotti 2018).

The vast majority of freed slaves could not read or write. There was a near universal desire by freed slaves to acquire both property and education. Free Blacks also generally sought to enter occupations beyond agriculture, where the vast majority of Blacks worked (Foner 2015). Schools were segregated by race, set up throughout the South by the Freedmen's Bureau, as well as by local communities. Given the fact that the vast majority of adult Blacks could not read following emancipation, many adults attended schools.⁹

⁸ See Fogel and Engerman (1995) for a discussion of the antebellum Southern slave economy and Pritchett and Chamberlain (1993) for a discussion of the slave trade.

⁹ For this reason, we do not take age restrictions when examining effects on schooling outcomes. When we do restrict to children under the age of 18, we find larger effects on schooling.

While there were attempts to integrate Blacks during Reconstruction, and the Civil Rights Act of 1875 banned discrimination on the basis of race in public places, segregation was widespread. Black civil society was sparse during Reconstruction, with churches and the Freedmen's Bureau being the main black institutions in the South. Blacks did not have access to most white institutions, including banks. Many freed slaves saved through informal mechanisms via churches, or put their money into land (Osthaus 1976). Historians generally consider Reconstruction a failure in terms of integrating freed slaves, and the period of Reconstruction was followed by the Redemption, in which Southern Whites enforced racial segregation to effectively take away many of the new rights that Blacks won during Reconstruction (Foner 2015).

1.1.2 Freedman's Savings Bank. The origin of the Freedman's Savings Bank lies in African-American regiments that fought in the Union army during the Civil War. Very few Blacks had access to deposit institutions, and military authorities were concerned that black soldiers were unable to save their pay, and were unable to transfer funds to their families. Plans were drawn up by A. M. Sperry, an army paymaster and John W. Alvord, a military chaplain in Sherman's army, with the support of New York philanthropists and abolitionists. On February 13, 1865, a bill was introduced to incorporate the Freedman's Savings and Trust Company. The Freedman's Bank Act was passed on March 3, 1865, and signed into law by Abraham Lincoln (Fleming 2013). The objective of the new corporation was to receive deposits "by or in behalf of persons heretofore held in slavery in the United States, or their descendants." The Freedman's Savings and Trust Company was an early United States government-sponsored enterprise, more specifically a mutual savings bank established for the benefit of African Americans (Osthaus 1976). The bank was initially headquartered in New York, and the first branch was established in Washington, D.C.

The Freedman's Savings Bank affected education investment through at least three non-mutually exclusive channels. First, any interest unclaimed for two years after the death of a depositor was allocated to the education of black children (Fleming 2013). During this time period most schools catering to free Blacks either charged tuition or relied on voluntary community taxation. Second, the bank allowed free Blacks to save, pool resources, and create schoolhouses. Prior to the Civil War there were essentially no educational institutions for freed slaves, and indeed in many Southern states it was illegal to teach Blacks to read and write. With emancipation, groups of Blacks raised money to purchase land, build schoolhouses, and pay teachers' salaries (Foner

2015). Third, the bank allowed depositors to have access to funds when income streams varied over time.¹⁰

The bank made loans, including loans on real estate, and employees of the bank were prohibited from borrowing. Deposits of greater than five cents (worth approximately 75 cents in 2018 dollars) were accepted, with 6% annual interest paid on deposits of more than one dollar. A basic “thrift education” was given to depositors in the bank. The bank distributed pamphlets, but, given widespread illiteracy among freed slaves, much of the efforts to improve financial literacy were done through community meetings, word of mouth, and even songs that encouraged saving and thrift (Fleming 2013).

Our analysis uses the 1870 census, and this is only affected by the expansion of the Freedman’s Savings Bank. Following the time period we study, the bank collapsed following the Panic of 1873.¹¹ The bank invested heavily in real estate and made unsecured loans to railroads, quarries, and various firms that defaulted. The renowned African-American statesman and intellectual Frederick Douglass was briefly made the head of the bank in a move to instill confidence in depositors, but the move failed to save the bank. In June 1874, the Freedman’s Savings Bank was forced to suspend operations with only 50 cents to cover obligations per depositor.

The failure of a bank catering to former slaves, and the loss of their savings, led to general public concern and sympathy for the fate of depositors. Following a congressional investigation, Congress created a program to reimburse up to 62% of savings, but many depositors were never compensated (Fleming 2013). Due to the ambiguous effects of holding an account and the potential loss of savings following the collapse of the bank in 1874, we limit our analysis to outcomes from the 1870 census.

1.1.3 Mechanisms. There are at least four potential (non-mutually exclusive) mechanisms through which access to formal savings accounts might encourage investment in businesses and human capital (Dupas and Robinson 2013a). First, investment may be lumpy, and individuals may be unable to invest in their business until they have enough

¹⁰ Work in developing countries has found that access to savings increases schooling among children. For example, Anderson and Baland (2002) find that rotating savings and credit organizations in Kenyan slums lead to wives’ saving for children’s schooling, and that access to an informal savings institution increases women’s bargaining power to prevent men from engaging in short-term consumption.

¹¹ The Panic of 1873 was a global financial crisis, with several underlying causes debated by economic historians ranging from demonetization of silver in the United States and Germany, speculative investments in railroads, and the economic consequences of the Franco-Prussian War. In the United States several major banks and railroads failed, and the New York Stock Exchange closed for ten days in September.

funding for a large item such as a mule or plot of land. The second channel is that income may vary over time, and access to a deposit account may allow savers to make consistent recurring payments. This is especially true in an agricultural society such as the nineteenth-century American South, where streams of income may coincide with harvests, and payment sizes may depend on agricultural output that varies with weather patterns and other factors. The third is that it may be difficult to liquidate working capital when shocks occur, and individuals may need to save in the form of liquid assets outside of their business to insure against adverse events. Finally, increasing the costs of accessing funds by requiring individuals to withdraw deposits may reduce impulsive behavior and act as a commitment device (O'Donoghue and Rabin 1999).

While the Freedman's Savings Bank made relatively few direct loans for real estate, access to savings allowed individuals to accumulate wealth to purchase property and invest in other forms of capital. For example, the Wilmington branch recorded that one-third of withdrawals were made to purchase homes, lots, horses, or capital equipment. Freed slaves would begin "to deposit usually with some special object in view. He wishes to buy a mule and cart, or a house, or a piece of land, or a shop" (Osthaus 1976). Anecdotal evidence from other branches also indicates that many Freedmen would save to purchase property or start small businesses (Osthaus 1976). John Alvord noted that "In a single day in our Charleston Savings Bank, I took the record of seventeen Freedmen who were drawing their money to pay for farms they had been buying, generally forty or fifty acres each." The Louisville branch estimated that of drafts of \$92,500 in 1867, \$35,000 was to purchase homes or land, \$42,500 was for mechanical and business improvements and seeds, and \$15,000 was for educational expenses (Osthaus 1976).

Another possible mechanism is that the observed effects are driven by the educational activities of the bank. The Freedman's Bank provided a basic thrift education to account holders, held community meetings, and distributed literature emphasizing the importance of savings and accumulating wealth. Unfortunately, we do not observe account balances or credit access, as these records have been lost for the vast majority of depositors. It is thus difficult to disentangle the effects of financial inclusion from the direct effects of education provided by bank employees. However, we can partially address this by examining a subset of individuals who were illiterate in 1870. The results are discussed in Section 4.2, and the large effects on outcomes for the illiterate population are largely consistent with effects being driven by financial inclusion.

1.1.4 Branch expansion. The expansion of Freedman's Savings Bank was characterized by a large early push, and then a long period

of stagnation post-1867 followed by further expansion after 1870. The initial expansion of the bank was closely tied to the work of John W. Alvord, who prior to his tenure at the bank was a Congregationalist minister and an inspector and superintendent of the schools for the Freedman's Bureau. He was able to take advantage of his status in order to attract customers and spread the bank's mission. Many of the early branch expansion decisions went through Alvord, an entrusted figure among Blacks due to his prior position.¹² The bank was initially headquartered in New York, Alvord's hometown, where the idea for the bank was first proposed in January 1865 at a meeting of businessmen and philanthropists, and rapid branch expansion followed (Fleming 2013).

Figure 1 shows the location of Freedman's Savings Bank branches. The red dots show branches that were built prior to 1870, generally in 1865 and 1866. States that allowed slavery prior to the Civil War are shown in gray, with states seceding from the Union shown in a lighter shade of gray. The bank expanded rapidly between 1865 and 1867.¹³ In 1867 political pressure during Reconstruction led to slower expansion until 1870 (Fleming 2013). The expansion of the bank was largely tied to the concerns about Union army veterans that led to the establishment of the bank (Osthaus 1976). Most of the branches were in former slave-holding states in the South (but not all), and in cities that also had a field office of the Freedmen's Bureau (Carrier and Walton-Raji 2014). There were also a handful of branches in Northern cities such as Cleveland and Philadelphia, which were near former slave-holding states and where many former slaves settled following emancipation.

Branches were established in Norfolk, Washington, and Richmond by October 1865. Branch expansion was tightly linked to the presence of black troops. For example, the Norfolk branch, distinguished as the inaugural branch, was formed to replace the deteriorating General Butler's military savings bank (Fleming 2013). General Butler's bank was set up with a similar ideology to the Freedman's Savings Bank, but given vast amounts of unclaimed deposits, it was not trusted by many black soldiers. During the Freedman's Savings Bank's early expansion, civilians joined former soldiers in establishing accounts at branches. In

¹² Alvord's close ties with the Freedman's Bureau, a trusted government enterprise, caused many Southern Blacks to incorrectly believe that the Freedman's Savings Bank was affiliated with the Bureau. In reality, during the bank's initial years, the bank and the Bureau were completely separate entities. Alvord took advantage of this, and was able to gain many customers through this false image. Perceived exploitation of this misunderstanding was later criticized by the Douglas Report (1876), which suggested that "the grossest deception was practiced upon the Negroes—they were told that it was a government institution and its solvency and safety guaranteed by the United States" (Fleming 2013).

¹³ Several temporary branches—including Chattanooga, Martinsburg and what may have been a mobile branch based out of Houston—are mentioned in some historical sources (Osthaus 1976) but do not appear to have remaining records.

the first two years discharged soldiers' funds made up the majority of accounts; however, after this early period, non-veterans quickly opened accounts and former military personnel came to represent a relatively small share of depositors (Osthaus 1976).

The second branch, which eventually became the bank headquarters, was located in Washington, D.C., and opened on July 11, 1865. The goal was to attract the large number of black troops in the area, along with the continuously increasing black population. A month after it opened, the branch's deposits totaled \$843.84 (Fleming 2013). The third branch was strategically placed in Richmond so that it could reach out to people in the local area that would potentially benefit from the branch's services. These three branches, built on the basis of proximity to black population centers, along with the mission to bring black personnel into positions of leadership, served as a blueprint for the opening of other branches throughout the country (Osthaus 1976).

Continued expansion was largely tied to the presence of black veterans, and as had been the case in Norfolk, several branches were opened to replace problematic military savings banks and to secure soldiers' unclaimed deposits. For example, a branch in Beaufort, South Carolina secured \$170,000 worth of soldiers' unclaimed deposits. In January 1866, the Freedman's Bank took over for the Free Labor Bank, located in New Orleans. Moreover, during a Southern roadshow, A. M. Sperry, Alvord's colleague, was granted permission by the War Department to access the army pay tables and supported the black soldiers in order to persuade them to deposit into Freedman's Bank. Along the Mexican border, he was able to obtain \$120,000 worth of deposits from black regiments of the 25th Army Corps (Fleming 2013).

The rapid expansion targeted not only cities with large black populations—such as Charleston, Baltimore, New Orleans, and Savannah—but also smaller ones with concentrations of black veterans, including Vicksburg (Osthaus 1976). While the growth of the bank offered a financial support system to the black population, the bank faced the perceived risk of over-expansion. In particular, there were operational issues due to undertrained staff and an apparent inability to communicate across branches, especially with headquarters in New York. In addition, the bank had scarce funds, which added an extra layer to the magnitude of its problems. For example, many branches in the Deep South had no communication with headquarters; without a governing body, and with an untrained workforce paid extremely low wages, proper administrative protocols were not consistently followed (Osthaus 1976).

The vast majority of account holders were black, although a few branches had a significant number of white depositors.¹⁴ In New Orleans, for example, approximately one-quarter of depositors were white. The Jacksonville and Beaufort branches also had some white businessmen with accounts. The New York branch—uniquely—catered to non-black European immigrants who were also largely excluded from many formal savings institutions and credit markets. Other than at these branches, the number of white depositors was very small, and they were typically missionaries or immigrants rather than native Southern Whites (Osthaus 1976).

Following the initial expansion of the bank, there was a long pause in branch expansion brought about by political and financial conditions. Starting in 1867, financial pressures and political opposition led to a general pause in the expansion of policies and organizations aimed at benefiting freed slaves (Foner 2015). For example, in Louisville, a local Democratic newspaper caused trouble for a new branch, and in Nashville, Whites denounced Blacks patronizing the bank. In January 1867, due to the fact that expenses exceeded income, the bank failed to pay interest for the first time (Osthaus 1976). There was a general lack of confidence in the bank to follow through with its mission. Austerity measures were in place by the end of 1867, including the suspension of new branch openings. The austerity measures eventually helped stimulate an increase in deposits that helped revitalize another expansion.

Financial pressure on the bank eased in 1868, and plans were made for further expansion. By 1870, expansion was back in full swing, with branches opening in a number of cities that had earlier been considered for a branch—including Atlanta, Louisville, Lexington, St. Louis, and Little Rock—but where building was delayed due to political and financial pressure, as well as staffing difficulties. For example, the Little Rock branch was opened more than three years after plans had been made to open a branch “as soon as possible” (Osthaus 1976). Planned openings in a number of other locations were never completed; for example, in Selma, Charlottesville, Charlotte, Andersonville, and Salisbury, where preliminary arrangements had been made in 1868 (Osthaus 1976). Expansion in the 1870s ended with the failure of the bank during the aftermath of the Panic of 1873. Before the bank’s collapse in the early 1870s, the bank had one of the largest interstate branch networks in the United States.

¹⁴ While all of our main analysis is restricted to Black account holders, we discuss in Section 4.4 the robustness of our results to exclusion of branches with more White accounts.

In Section 2, we provide an analysis of correlations between bank branch proximity and various demographic and other characteristics. The analysis indicates that, in general, individuals in locations with bank branches and those proximate to branches built before 1870 were generally similar to those with branches planned or built in or after 1870. This is largely consistent with the planned rollout of branches, and the fact that further branches were delayed due to external pressures after 1867 following the initial expansion.

1.2 Data and sample selection

1.2.1 Census data. Our principal outcome measures and control variables are drawn from the 1870 U.S. decennial census, the first in which many recently emancipated slaves appeared.¹⁵ This census was collected in person by paid enumerators, who submitted handwritten records to the Census Office. In addition to identifying information and basic demographics, the collected data included profession, value of owned real property, school attendance, and education. A sample census record is shown in Online Appendix Figure A1.

We rely on the digitized “1870 1% Sample with Black Oversample” compiled by IPUMS (Ruggles et al. 2015) and distributed through the North Atlantic Population Project (Minnesota Population Center 2017). This sample includes all members of each randomly sampled household, oversampling African Americans to include approximately 2% of the African-American population (and 1% of non-African Americans). Our regression analysis weights individuals using IPUMS-provided sampling weights (PERWT).

We geocode the approximate location of individuals in the census sample using their county of residence (STATEFIP and COUNTY), and county centroid latitudes/longitudes. We use these locations to measure the distance from the county of each of the built and planned Freedman’s Savings Bank branches, listed in Table 1. Our main analysis sample is restricted to individuals classified as Black (RACE=2) who live in the South (REGION ∈ {31,32,33}), and within 50 miles of a branch or planned branch. The census sample includes 34,187 such individuals, approximately two-thirds of whom live near a (pre-1870) branch, and one-third near a planned branch.

Although some of our analysis is conducted using only census data, our main analysis will rely on matching the census with Freedman’s Savings Bank data as described in Section 1.2.3. The construction of key outcome and control variables from the census data is described in Online Appendix Table A1, and summary statistics for these variables

¹⁵ In 1850 and 1860, slaves were counted on separate census “slaves schedules” that recorded information including age, skin color, and fugitive status, but did not collect names.

in our main analysis sample are reported in Table 2. Summary statistics by branch are reported in Online Appendix Table A2.

1.2.2 Freedman’s Savings Bank data. To identify Freedman’s Savings Bank account holders, we rely on the surviving account registers from 27 branches.¹⁶ The form and content of these registers varied over time and across branches, but they generally included basic demographic information on the main account holder, along with the names of various family members. In many cases, not all of the fields were filled out. A sample record is shown in Online Appendix Figure A2.

The original account registers were microfilmed by the National Archives and Records Administration (as Publication M816, 1970). A digitized version of the individual records is available in Progeny Family Explorer format on CD-ROM (FamilySearch 2000), from which we (imperfectly) extract a database of account holders and family members using the DBF Manager software (Astersoft 2016). Across all available branches, this sample includes 107,197 separate account records and 483,082 individuals.¹⁷

These records suffer from imperfect enumeration, digitization, and database extraction. For each record, we use string matching methods to attempt to identify the associated branch, first name, and last name. These fields are used to match to the census data as described here.

1.2.3 Matching Census and Freedman’s Savings Bank data.

Our main analysis sample relies on a match between the census data described in Section 1.2.1 and the Freedman’s Savings Bank records described in Section 1.2.2. We begin by excluding from the census sample 4,365 individuals who live within 50 miles of a branch for which Freedman’s Savings Bank account registers have not survived (indicated with an asterisk in Table 1). This reduces the size of our census subsample from 34,187 to 29,822.

In order to associate census records with the Freedman’s Savings Bank account records, we match using names that suffer from non-uniqueness, enumerator error, and digitization errors. To help mitigate the effects of these issues, we further restrict our sample to members of households with at least one member who has a “potentially matchable name,” defined as one with first and last names (the first space-delimited word of NAMEFRST, and NAMELAST) that (i) are at least two letters long, (ii) do not include question marks, and (iii) form a unique combination

¹⁶ Records from seven additional branches have not survived; these are indicated with an asterisk in Table 1.

¹⁷ These individuals are not necessarily unique, since many presumably appear on the records of multiple family members who opened separate accounts.

among all Blacks in the “1870 1% Sample with Black Oversample” census sample. This restriction further reduces our census subsample from 29,822 individuals to the 27,247 who compose our main analysis sample.

This sample includes 15,666 individuals who live within 50 miles of a (pre-1870) branch, whom we match to Freedman’s Savings Bank records in order to identify account holders. From each bank account record, we identify the names of potential account holders by forming every combination of first and last names that appear among the main account holder and family members.¹⁸ We then use this list of names to (imperfectly) identify account holders. For the subsample living within 50 miles of a (pre-1870) branch for which account records are available, we code individuals as having an account if any household member with a “potentially matchable name” matches the records of that nearest branch; otherwise we code them as not having an account. We code members of our sample who live more than 50 miles from a branch as not having an account.

Online Appendix Figure A3 shows the fraction of Blacks with accounts in the South, and provides validation for our matching strategy. In our sample 14.4% of individuals live in families that hold an account. Our numbers closely match historical sources, as does the geography of our matched account holders. Panel (a) shows the fraction of the Black population in each former slave state that had a branch of the Freedman’s Savings Bank. This figure is constructed by taking the number of aggregate deposits in each state reported in Osthaus (1976), and dividing by the number of Blacks in each state in the 1870 census. Panel (b) repeats the exercise, using the matched data. The broad geographic patterns look quite similar. Larger fractions of Blacks hold accounts in the Carolinas, Virginia, Tennessee, Mississippi, and Florida relative to other states. The fact that the patterns in the linked data match aggregate patterns helps validate our matching strategy.

2. Empirical strategy

2.1 Comparison of account holders to nonholders

We begin our analysis by comparing account holders with nonholders. We estimate the effect of holding an account using ordinary least squares (OLS) estimation of the following regression specification:

$$y_i = \alpha_t + \alpha_c + \beta^{OLS} A_i + \gamma_1 \mathbf{X}_i + \varepsilon_i, \quad (1)$$

¹⁸ We do this for three reasons. First, imperfect digitization and database extraction give very limited ability to distinguish which individual on an account record is the main account holder. Second, the inclusion of last names on account records is inconsistent; they are often reported only for some family members and implied for others. Finally, we expect that access to banking is likely to have effects not only on main account holders, but on their family members.

where y_i is an outcome of interest for individual i in 1870 (school attendance, literacy, labor force participation, occupational income, or value of real property). We regress these outcomes on A_i , an indicator of whether an individual has an account. We include fixed effects α_t for the date which a branch opened, and additionally α_c , for the metropolitan area classification in which an individual is located, if any. We include controls \mathbf{X}_i , which include a variety of individual demographic characteristics. The main coefficient of interest is β^{OLS} , which captures the effect of holding an account on the outcome y_i .

The estimates of β^{OLS} in Equation 1 suffer from two sources of bias. The first is measurement error. The census and Freedman’s Savings Bank records suffer from imperfect enumeration, digitization, and database extraction. The matching procedure described in Section 1.2 is also inexact. Thus our measure of holding an account, A_i , is measured with error, which may attenuate our estimates of β^{OLS} and bias them toward zero.¹⁹ The second is selection stemming from the fact that individuals who open accounts may be unobservably different from individuals who do not open accounts. For example, it is possible that those who open bank accounts are more organized or ambitious than individuals who do not open accounts. This could bias estimates of β^{OLS} upwards, and even lead to spurious effects driven by selection being detected. The fact that the two sources of bias potentially go in opposite directions means that the simple estimates of β^{OLS} may not be informative regarding the true effect of access to financial services. We therefore use an instrumental variables (IV) strategy to identify the effect of holding an account on outcomes.

2.2 Bank Branch Proximity

To identify the effect of holding an account on human capital and labor market outcomes, we exploit individuals’ proximity to the nearest branch. The natural concern with such an approach is that areas in which branches are located may differ from areas without branches.²⁰ For example, branches may be more likely to be located in areas closer to cores of metropolitan areas that have other services, and individuals living in these areas may have higher levels of ability and human capital accumulation. To alleviate this concern, we compare individuals who live near branches that were built before 1870 to those near branches

¹⁹ Consistent with the existence of measurement error, we show in Online Appendix Table A3 that the OLS coefficients are generally larger—though not necessarily statistically significantly so—for individuals that we cannot match to the 1880 census. These are precisely the individuals for whom our measure of account status is most likely measured with error.

²⁰ As Fleming (2013) notes, “Only those in the vicinity of the larger towns were directly affected by the bank.”

that were built or planned to be built in or after 1870. We thus do not need to assume that Blacks living near bank branches were similar to other Blacks in the 1870s South, but rather that Blacks living near branches were similar to Blacks who lived near branches that are built or scheduled to be built after 1870. Figure 2 illustrates the variation that we use. The top panel shows distance from branches built prior to 1870. The bottom panel shows distance from branches built or scheduled to be built post-1870.

We include individuals living within 50 miles of a built or planned branch (who satisfy the other sample restrictions described in Section 1.2), and instrument for holding an account A_i using (i) whether an individual is located in a county with a branch, B_i , and (ii) the distance to the nearest branch, M_i^B .²¹ This strategy is related to Huber (2018) and Giorcelli (2018),²² and yields the first-stage equation

$$A_i = \alpha_t + \alpha_c + \zeta_1 B_i + \zeta_2 M_i^B + \gamma_2 \mathbf{X}_i + \epsilon_i. \quad (2)$$

We include controls \mathbf{X}_i for whether an individual is in a metropolitan area, city population, sex, age, number of own children under age five in household, and fixed effects for the opening date of the nearest branch, relationship with household head, the number of married couples in the household, and occupation, and we weight using IPUMS sample weights.

Our instrumental variables approach uses individuals' likelihood of holding an account—predicted using the fitted values from estimation of Equation 2—to predict outcomes of interest:

$$y_i = \alpha_t + \alpha_c + \beta^{IV} \hat{A}_i + \gamma_3 \mathbf{X}_i + \eta_i. \quad (3)$$

There are two key identifying assumptions. The first is that the proximity instruments are correlated with holding an account, in other words $\mathbb{E}[A_i M_i^B | \mathbf{X}_i] \neq 0$ and $\mathbb{E}[A_i B_i | \mathbf{X}_i] \neq 0$. This assumption is testable and implies that the instruments are correlated with holding an account. Figure 3 provides suggestive evidence that this assumption holds, and that minimum distance from a branch is indeed correlated with account status. The figure shows the fraction of Southern Blacks with an account, by distance to the nearest branch. The fraction of individuals with an account increases with proximity to a branch, and increases sharply within 20 miles of a branch.

We test the excluded instruments' relevance more formally in Table 3, which reports the results of the OLS estimation of the first stage

²¹ Summary statistics for these instruments are reported in Table 2. In Section 4 we vary the 50-mile restriction, and show that results are robust.

²² Several studies such as Agarwal and Hauswald (2010), Mian (2006), Degryse and Ongena (2005), Huber (2018), and Petersen and Rajan (2002) have argued that proximity influences lending relationships.

(i.e., Equation 2). The estimated coefficients are of the expected sign, and both economically and statistically significant. For example, an individual living 10 miles closer to a branch is 0.4 to 0.7 percentage points more likely to have an account, and individuals in a county with a branch are 14 to 17 percentage points more likely to hold an account than those living outside a branch county. When we include both instruments, our F statistic is 88.9, which is large enough to rule out weak instrument concerns (Staiger and Stock 1997; Stock, Wright, and Yogo 2002).

The second assumption is that distance from branches and whether a county has a built rather than a planned branch is uncorrelated with unobservable determinants of the outcomes studied, in other words $\mathbb{E}[\eta_i M_i^B | \mathbf{X}_i] = 0$ and $\mathbb{E}[\eta_i B_i | \mathbf{X}_i] = 0$. This assumption is not directly testable, but we can provide supportive evidence that it holds. First, in Section 4.3 we present the results of placebo tests that show that proximity to a branch does not predict similar outcome differences among (presumably untreated) Whites as among Blacks. Furthermore, we fail to find evidence that Blacks' outcomes differ systematically with proximity to a planned rather than a built branch. Second, we can exploit the fact that we have multiple instruments to conduct a Sargan (1958)–Hansen 1982 overidentification test. The resulting test statistic is 1.151, with a p -value of 0.2834, and we thus fail to reject the overidentifying restrictions.

A natural concern and potential threat to the exclusion restriction is that planned and built branches are somehow unobservably different. While this concern is fundamentally untestable, we can provide suggestive evidence that, at least on observables, individuals living near built and planned branches are similar. The top panel of Table 4 shows regression estimates of the relationship between branch distance and twelve predetermined demographic and other variables. The bottom panel shows differences in these variables between individuals living in counties with a built or a planned branch. Of the 24 regressions considered, only three have statistically significant relationships with an excluded instrument at the 10% level, with individuals living near or in counties with a planned branch being slightly older on average, and slightly more likely to be household heads.

2.3 1868 Election Results

We employ a second, complementary empirical strategy exploiting political conditions during the Reconstruction era, to which the Freedman's Savings Bank's expansion was heavily tied. The Republican-controlled U.S. Congress created the bank, and support for the institution was generally much stronger for the institution among

Republican politicians. Many bank cashiers had formerly held political office as Republicans (Osthaus 1976).

Local Republican administrations tended to be more favorable to the Freedman’s Bank and its expansion. For example, the Republican governor of North Carolina, W. W. Holden, was a trustee of the local branch (Osthaus 1976). At the time, Democratic administrations and institutions opposed the bank. For example, Fleming (2013) notes that “the disturbing force of Reconstruction politics is seen in the sudden checking of [Bank] expansion in 1867 and slow increase afterwards.” Osthaus (1976) also writes of “the extreme political partisanship of the Reconstruction era around all the latent hostility against black people and this black bank. Although the Bank had remained out of politics, it was in southern eyes associated with northern and the Republican efforts to alter their way of life... Southern Democratic papers were unanimous in their denunciation.” He continues, “Community prejudice dictated that no true Southerner could support a Negro bank backed by Republicans.”

To exploit this variation, we also estimate IV regressions using alternative instruments meant to proxy for local political conditions, and controlling for voter turnout. While the second stage remains the same as in Equation 3, we instead use fitted values from a first-stage equation analogous to Equation 2, estimating

$$A_i = \alpha_t + \alpha_c + \xi_1 S_i + \xi_2 V_i + \gamma_4 \mathbf{X}_i + \nu_i, \quad (4)$$

where S_i is the Republican vote share in the 1868 congressional election for the county where individual i resides, and V_i the total number of Republican votes received in the county.²³ The first assumption needed for validity is the instruments’ relevance, that is, that voting patterns are correlated with account holding. This is testable and is shown in Table 5, which shows the results of estimating the first stage. The F statistic including both instruments is 71.2, which—as with the branch proximity instruments—helps rule out weak instrument concerns (Staiger and Stock 1997; Stock, Wright, and Yogo 2002).

Table 5 shows that, consistent with the historical record, there is a strong relationship between political partisanship and the activities of the bank. The first column shows the relationship between Republican votes and having an account, while the second column shows the relationship between the Republican vote share and having an account.

²³ Election data for 1868 comes from Clubb, Flanigan, and Zingale (2006). All of our election results also control for the 1868 voter turnout rate (constructed as described in Online Appendix Table A1), and are restricted to the subsample of our main analysis sample for which the election variables are available. Summary statistics are reported in Table 2.

The third column includes both instruments, and Columns 4–6 repeat the analysis with demographic controls.²⁴

Since our strategy relying on Republican political support does not explicitly compare individuals near built and planned branches, we also consider estimates that relax the sample restriction to individuals within 50 miles of a planned or built branch. First-stage estimates using the election instruments in this expanded sample are reported in Online Appendix Table A4, and are similar. Online Appendix Figure A4 also presents a map of the variation used—the share of Republican votes in the 1868 congressional election.

The second assumption is that Republican vote share and total votes are uncorrelated with unobserved determinants of the studied outcomes. This assumption is untestable, but stronger than the assumption for our main branch proximity strategy given the political climate during Reconstruction. The main concern is that Republican administrations may have otherwise been more conducive to black education and economic development, through, for example, supporting educational expansion or allowing black businesses to operate in more geographic areas. This might affect both the ability of the Freedman’s Bank to expand, and lead to various outcomes for Blacks directly. While we are more cautious in interpreting the results from our second empirical strategy, we view the fact that estimates are similar to those from our main, proximity-based strategy as largely consistent with financial inclusion affecting human capital and labor market outcomes.

3. Main results

3.1 Human capital

We begin by considering the effect of access to finance on investments in and acquisition of human capital. In particular, we investigate two human capital-related outcomes: school attendance and literacy. In Table 6, we compare the school attendance and literacy of Freedman’s Savings Bank account holders with nonholders, reporting OLS estimates of Equation 1 as described in Section 2.1. Because the outcome variables are binary indicators, this can be interpreted as a linear probability model, where coefficients represent the marginal likelihood of school attendance or literacy associated with holding an account.

The positive coefficient estimates reported in Columns 1 and 2 show that account holders are indeed statistically significantly more likely than nonholders to be enrolled in school and to be literate. Among individuals in our main analysis sample, having an account is

²⁴ Online Appendix Figure A7 presents graphical evidence consistent with these results, illustrating in binned scatter plots the positive univariate relationship between account status and county-level Republican vote and share.

associated with an approximately 1.7 percentage point higher likelihood of attending school, and an approximately 5 percentage point higher likelihood of being literate.

Of course, these OLS estimates suffer from potential biases as described in Section 2.1, which precludes a causal interpretation. We therefore move to IV estimates as described in Section 2.2. Rather than considering the association of school attendance and literacy with account status, we instead consider the association with the account status predicted by the presence of and/or proximity to a pre-1870 branch (and other control variables).

In Table 7a, we report the results of this IV strategy, implemented using two-stage least squares estimation of Equation 3. The first-stage predictions of account status are reported in Table 3 and discussed in Section 2.2. The excluded instruments are the distance to the nearest pre-1870 branch, and an indicator for the presence of a pre-1870 branch in the county.

The estimated coefficient in Column 1 indicates that opening a Freedman’s Savings Bank account resulted in a large and statistically significant increase in the likelihood of school attendance. Including additional control variables (to help explain school attendance and account status using additional geographic and demographic attributes) in Column 2 leaves the estimated effect nearly unchanged: we estimate that an account increases school attendance likelihood by approximately 14 percentage points, with statistical significance at the 1% level.

We find analogous results for our other human capital outcome measure, reported in Columns 3–4: opening an account is associated with an increase in the likelihood of being literate by approximately 13–19 percentage points, which is both statistically significant and economically large. In contrast with school attendance, including additional control variables moderates the magnitude of the estimated effect on literacy and reduces statistical significance from the 1% to the 5% level, although larger standard errors mean the estimated effects with and without additional controls are not statistically significantly different from each other.

Figure 4 presents graphical evidence corroborating these regression results. The figures show estimates of the coefficients β_j (along with their 95% confidence intervals) from the following reduced-form specification:

$$y_i = \alpha + \sum_{j \in \{0, 10, 20, 30, 40\}} \beta_j \mathbb{1}[j \leq M_i^B < j + 10] + \varepsilon_i, \quad (5)$$

where M_i^B is the distance to the nearest branch. The coefficient estimates show the outcome gap between individuals living within a given 10-mile distance range from a branch, and a baseline group living

50–60 miles away. Those living near a branch have significantly higher school attendance and literacy, consistent with our IV estimates.²⁵

3.2 Labor market and wealth accumulation

Our second set of results considers the effect of access to finance on individuals' participation in the labor market and their accumulation of real property. Using the limited data available in the 1870 census, we analyze three outcomes: an extensive measure of employment, an intensive measure of occupational income, and the value of accumulated real property.

We begin by comparing these outcomes across Freedman's Savings Bank account holders and nonholders, reporting in Table 6 OLS estimates of Equation 1 as described in Section 2.1. Column 3 considers a binary outcome for whether an individual works, and the coefficient therefore represents the marginal likelihood of employment associated with holding an account implied by a linear probability model. We estimate a positive association, statistically significant at the 5% level: among individuals in our main analysis sample, account holders are approximately 2.6 percentage points more likely to work.

We then consider continuous measures of income and wealth, measured in logarithms so that estimated effects can be interpreted as (approximate) marginal effects in percentage terms. In Column 4, we consider the logarithm of occupational income, a measure of the salary associated with an individual's occupation.²⁶ The positive, statistically significant coefficient estimate shows that account holders hold occupations that earn approximately 2% more than nonholders. Finally, we consider the logarithm of the value of owned real property. Our OLS estimates, reported in Column 5, are economically small and statistically insignificant.

We now move to considering IV estimates as described in Section 2.2, which allow causal interpretation in the face of the measurement error and endogeneity problems that may bias our OLS results. As we did with human capital, we consider the association between our labor market and wealth accumulation outcomes and the account status predicted by a branch's presence and proximity. Table 7b reports the results of Two-Stage Least Squares estimation of Equation 3. The first-stage predictions

²⁵ Online Appendix Figure A5 presents further graphical evidence, comparing binned estimates of mean outcomes, along with the slope of the relationship between distance and outcomes, for built and planned branches. Consistent with the results presented in this section, we see a negative relationship between outcomes and distance from built branches. The relatively flat relationships between outcomes and planned branch distance is consistent with placebo tests discussed in Section 4.3.

²⁶ The 1870 census did not request information about individuals' actual incomes; Ruggles et al. (2015) impute this occupational income score (OCCSCORE) based on the median income of people with a given occupation in 1950.

of account status are reported in Table 3 and discussed in Section 2.2. As earlier, the excluded instruments are the distance to the nearest pre-1870 branch, and an indicator for the presence of a pre-1870 branch in the county.

The estimated coefficients in Column 1–2 indicate that opening a Freedman’s Savings Bank account resulted in a statistically significant increase in the likelihood of working by 3 to 6 percentage points. The magnitude is somewhat moderated with the inclusion of additional control variables, but remains larger in magnitude than the positive association we found using OLS.²⁷

For occupational income, we estimate effects that follow a similar pattern. IV estimates reported in Columns 3–4 indicate that holding an account increased income by approximately 4%. This effect is economically and statistically significant, and larger than the association suggested in the simple OLS comparison. Finally, we consider wealth accumulation, reporting regression results in Columns 5–6. Again, we find significant positive effects of access to finance on economic outcomes: holding an account increased the value of real property by approximately 2.5 to 2.6 percentage points. As with our human capital outcomes, Figure 4 and Online Appendix Figure A6 present graphical, reduced-form evidence consistent with our IV estimates: Individuals living near a branch have significantly higher employment, occupational income, and real property value.²⁸

3.3 Results using variation in 1868 election results

3.3.1 Human capital. We complement the estimates in Sections 3.1 and 3.2 with the results from our second strategy, instrumenting for account status using county-level 1868 Republican congressional vote and vote share as described in Section 2.3 (and restricting to the subsample of our main analysis sample for which the election variables are available). First, we study human capital-related outcomes: school attendance and literacy. The results are shown in the first four columns of Table 8, which again compare the school attendance and literacy of Freedman’s Savings Bank account holders with nonholders. Panel (a) is

²⁷ These larger IV magnitudes relative to OLS estimates are consistent with measurement error in the treatment. In the univariate case, the OLS estimator satisfies $\text{plim} \hat{\beta} = \beta \frac{\pi(q_1 - \hat{\pi})}{\hat{\pi}(1 - \hat{\pi})}$ where π is the (unknown) true rate of account ownership, $\hat{\pi}$ is the estimated account ownership rate, and q_1 is the (unknown) probability that we correctly classify someone as an account holder given that they actually have an account (Aigner 1973). If we let $\hat{\pi} = 0.14$, and assume $q_1 = 0.5$ and $\pi = 0.1$, then the IV estimates will be approximately 3.3 times as high as the OLS estimates.

²⁸ The effects of financial inclusion on these outcomes could, of course, operate in part through the human capital channels considered in Section 3.1. While we do not formally assess joint determination, we confirm in Table 12 that the IV estimates presented in Table 7b are not statistically significantly different for the subsample who remain illiterate in 1870.

restricted to the subsample of our main analysis sample for which the election variables are available; panel (b) relaxes the 50-mile distance restriction. The results are very similar to those from the preceding section using proximity to a pre-1870 branch.²⁹

Column 1 in each panel of Table 8 indicates that opening a Freedman's Savings Bank account leads to a large and statistically significant increase in school attendance. Column 2 includes controls, and the estimates are statistically indistinguishable from the estimates in Column 1. The estimates are also quite similar to the corresponding estimates using the branch proximity instruments in Table 7, albeit slightly smaller in the non-distance-restricted sample. Columns 3 and 4 repeat the analysis, changing the outcome to literacy. The estimates are again large and highly statistically significant. The estimates without demographic controls are somewhat larger than those using the branch proximity instruments but—as they did there—drop in magnitude when the controls are included.

3.3.2 Labor market and wealth accumulation. We next turn to labor market and wealth outcomes, using our second empirical strategy. Columns 5–10 of Table 8 examine the effect of access to a Freedman's Savings Bank account on individuals' participation in the labor market and their accumulation of real property. Again, the results are very similar to those in the preceding section, which rely on proximity to a pre-1870 branch as an instrument for account holder status.

The estimated coefficients in Columns 5 and 6 indicate significant effects of holding an account on whether an individual works. The estimates are similar albeit larger than those in Table 7b, with point estimates between 5 and 10 percentage points nearly twice as high. However, the coefficients are less precisely estimated, with larger standard errors than in the analogous columns of Table 7, consistent with the weaker first stage using the political empirical strategy. Columns 7 and 8 show results using occupational income, with somewhat smaller positive effects (1.3% to 2.7% in the distance-restricted sample, significant at at least the 10% level) than using the branch proximity instruments. Columns 9 and 10 show positive effects for real estate wealth (1.3% to 1.5% in the non-distance-restricted sample, significant at at least the 5% level).

The estimates in Tables 7 and 8 are broadly similar, in terms of both magnitudes and statistical significance. Using each strategy, we find positive effects on human capital, labor market, and wealth

²⁹ Online Appendix Figures A8 and A9 present further graphical evidence, comparing binned estimates of mean outcomes, along with the slope of the univariate relationship between the election instruments.

accumulation outcomes. The fact that estimates are similar using both strategies helps alleviate concerns that the observed effects may be driven by omitted factors.

3.4 Mechanisms: Entrepreneurship and Business Ownership

A natural question is how access to financial services might affect income. There are several potential mechanisms through which the ability to save might affect income. For example, access to funds may enable individuals to invest in human or physical capital. Earlier in this section, we saw direct effects on human capital outcomes. While the 1870 census unfortunately does not allow us to observe investments in physical capital, we can observe actions consistent with another possible mechanism: access to savings may allow individuals to accumulate the financial capital required to engage in entrepreneurship and open a business.³⁰

Table 9 presents results where the dependent variable is an indicator for business ownership inferred from census occupation descriptions using a classification described in Online Appendix Table A1. Panel (a) presents OLS estimates, while panel (b) presents IV estimates using the branch proximity instruments as described in Section 2.2. In each panel, Columns 1–2 present results using a stricter definition of business ownership, while Column 3 uses a slightly looser definition. We find statistically or marginally statistically significant relationships between holding a bank account and owning a business, and consistent with our previous results and the presence of measurement error, the magnitudes increase when we instrument for account ownership. Having an account increases the business ownership rate by approximately half a percentage point.

In panel (c) of Table 9, we report the results of estimates that instrument for account status using county-level 1868 Republican congressional vote and vote share as described in Section 2.3 (and restricting to the subsample of our main analysis sample for which these election variables are available). The results are similar to those in the earlier panel, and using both definitions of business ownership, we see significant positive effects of access to an account on business ownership.

4. Additional Results

4.1 Aggregated Estimates

Section 3 focused on the effects of the Freedman’s Savings Bank on individual outcomes. However, there may be important spillovers

³⁰ Savings-based channels may complement the positive effects of local financial development on business formation operating through credit availability, as in Guiso, Sapienza, and Zingales (2004).

from neighbors having bank accounts, leading to general equilibrium effects. For example, even if an individual does not have an account, a banked neighbor who opens a business may bring more money into the community or lead to greater employment opportunities. Greater access to education and literacy among neighbors with accounts might also affect those without accounts. We might even imagine that there are negative effects for individuals without bank accounts if, for example, the availability of banking caused price levels to rise.

We explore these general equilibrium effects in Table 10, which shows estimates aggregated at the county level. The top panel uses our main branch proximity instruments as described in Section 2.2, while the bottom panel uses the election instruments as described in Section 2.3. In all cases the dependent variables are county-level averages across individuals in our main analysis sample (i.e., Blacks in households where at least one member has a “potentially matchable name”), and the key explanatory variable is the number of account holders among such individuals.³¹

Both sets of estimates yield similar results, with positive effects of financial inclusion consistent with our individual analyses. The estimates imply that 1,000 additional account holders in a county is associated with a 0.3 to 0.5 percentage point increase in school attendance, and a 0.8 to 1.7 percentage point increase in literacy. The average county within 50 miles of a pre-1870 branch has approximately 2,700 account holders and 10,700 black residents in our sample. The estimated coefficients therefore suggest that if the entire aggregate affect was on account holders, opening an account would increase school attendance among these individuals by 4 to 5 percentage points, and literacy by 8 to 18 percentage points. Turning to labor market outcomes, the results suggest that an additional thousand accounts in a county is associated with a 2.0% to 3.5% increase in income, and an approximate 0.9 percentage point increase in the share of individuals working, which corresponds to a 9 to 10 percentage point increase among account holders if only they were affected. Turning to wealth outcomes, the results suggest an additional thousand accounts leads to an 11% to 12% increase in real estate wealth. Finally, Table 10 indicates a 0.02 to 0.04 percentage point increase in business ownership, or 0.2 to 0.4 percentage point increase in the propensity to own a business among account holders. These estimates are broadly in line with the estimates in Section 3, suggesting the general equilibrium effects are small.

³¹ There are 1,033 counties total in the South in the 1870 census, of which 923 have at least one black resident in the 2% IPUMS sample, and 888 at least one Black in a household with a potentially matchable name. Of these, 531 have the election variables available. Of the 888 counties, 171 are within 50 miles of a planned or built branch (112 with election variables).

4.2 Timing of Account Opening and Mechanisms

Our results suggest that the expansion of the Freedman's Bank led to large increases in human capital investment and improved labor market and wealth accumulation outcomes over a relatively short period of time—five years. A natural question that arises is whether effects are driven by the selection of branch location. It is possible that if locations with branches are fundamentally different from those without, and the effects we are picking up may be spurious. Another important question is whether the bank's effects were driven directly by access to financial services, or via other operations of the bank. For example, education and training of depositors provided by bank staff may have had direct effects on literacy and other outcomes.

We explore these possibilities by studying the timing of account opening. Table 11 repeats the analysis in Table 6, allowing heterogeneous effects across whether an individual opened an account before or after 1870, the year in which we observe outcomes.³² For all outcomes, the coefficients are larger in magnitude for accounts opened prior to 1870. The effect of having an account pre-1870 is significant at the 5% level or higher for literacy, school attendance, working, and occupation income. The effect of opening an account in or after 1870 is significant at conventional levels only for one outcome—school attendance—and the magnitudes are small (although we can not generally reject equality of the pre- and post-1870 coefficients). We thus conclude that the effects are largely driven by individuals who opened an account prior to 1870, which is consistent with the main driver being the Freedman's Bank, rather than the location of branches.

We also consider heterogeneous effects across branch timing rather than account timing, comparing the effects among those who live near the earliest pre-1870 branches (those built in 1865 and 1866) with those near branches built later in the 1860s. The observed patterns are consistent with our earlier results that timing of account opening matters, since earlier branches allowed earlier accounts, and therefore more time for potential treatment before we observe outcomes in 1870. In Online Appendix Table A5, we present estimates from separate IV regressions that use either the earlier or the later pre-1870 branches as “treatments.” While there are no statistically significant differences for three outcomes (literacy, working, and occupational income), we generally find effects larger in magnitude for the earlier branches, where individuals had up to five years of potential treatment before outcomes are measured, rather than two, consistent with financial inclusion affecting outcomes. This is evident visually: the estimated effects for

³² Note that the sample size is smaller than in the main analysis due to the fact that many records are missing information on when an account was established.

each outcome (and 95% confidence intervals) for the earlier and later pre-1870 branches are shown in Online Appendix Figure A10.

We further explore whether observed effects are driven directly by access to finance, or by an educational channel, by exploring outcomes for illiterate individuals. If the effects are driven by educational expansion, we would expect the labor market and wealth accumulation effects to be less pronounced for illiterate individuals, who presumably benefited less from the bank's educational activities. Table 12 shows IV estimates for these outcomes on the subsample of individuals who were illiterate in 1870. We find strong effects on work, income, and real estate wealth for this illiterate sample, consistent with a financial inclusion channel, rather than an educational expansion channel. While this test is imperfect and it is certainly possible that individuals benefited from the educational activities of the bank without achieving literacy, the results are more consistent with a financial inclusion than an education channel.

While large effects over a relatively short five-year time horizon may be surprising, this is consistent with much of the development and household finance literatures. For example, Augsburg et al. (2015) study a randomized evaluation of a microcredit program in Bosnia, and find that borrowers started and expanded small-scale businesses. Borrowers with access to the microcredit program were 6 percentage points more likely to have self-employment income 14 months after treatment. There were also substantial increases in the labor supply of children aged 16–19, with 20 additional hours worked weekly, and a reduction in their school attendance of 9 percentage points over 14 months.

Schaner (2018) examines the effects of a field experiment that affected temporary incentives to save. Men who received an interest rate subsidy had significantly more total income and assets 2.5 years after the experiment, with effects driven by increased rates of entrepreneurship. Dupas and Robinson (2013b) randomized expanded access to bank accounts, which allowed individuals to save but not borrow. They found that market women significantly increased business investment, with a 38%–56% increase after four to six months.

In a natural experiment, Bruhn and Love (2013) exploit the opening of Banco Azteca in Mexico, which served low-income unbanked individuals. They find a 7% increase in income levels after two years. Ashraf, Karlan, and Yin (2006) randomized a commitment savings product. After 12 months, average savings balances increased by 81%. There is also evidence of short-term effects of financial inclusion in the household finance literature, such as the finding in Morse (2011) that access to credit mitigates foreclosures following natural disasters.

4.3 Placebo analysis

The primary concern with the results presented in Section 3 is that the results may be driven by a violation of the exclusion restriction rather than effects related to the Freedman’s Savings Bank. For example, one may be concerned that effects are driven by proximity to urban areas rather than holding an account with the bank. Another potential concern is that our effects may be driven by branch location. It is possible that Alvord and the bank leadership opened branches in areas with greater demand for financial services. To assuage these concerns, we conduct several placebo tests.

First, we consider the relationship between outcome variables and proximity to a Freedman’s Savings Bank branch for a population we expect not to have been affected by the bank: Whites. In particular, we evaluate whether Whites’ outcomes vary systematically with (i) whether an individual is located in a county with a branch, B_i , and (ii) the distance to the nearest branch, M_i^B , using OLS to estimate

$$y_i = \alpha_t + \alpha_c + \eta_1 B_i + \eta_2 M_i^B + \gamma_4 \mathbf{X}_i + \varepsilon_i. \quad (6)$$

We use the IPUMS data described in Section 1.2 to create an analogue to our main analysis sample containing Whites living in the South within 50 miles of a branch or planned branch. The results of estimating Equation 6 on this sample are reported in Table 13a. We fail to find evidence that branch proximity is associated with Whites’ school attendance, literacy, likelihood of working, or occupational income. Whites who live in branch counties do have higher real property (perhaps because of differences in real estate costs), but otherwise the effects of distance go in exactly the opposite direction than we might expect if our main IV results were driven by an exclusion restriction violation: Whites who live further from branches own more valuable real property. Taken together, these results for Whites suggest that the effect of the Freedman’s Savings Bank on Blacks’ outcomes are not driven by better economic opportunities near branches.

We now consider whether our main IV results may be driven by differences associated with proximity to the types of places where the bank considered building branches, rather than where it actually built them. To this end, we estimate an analogue to Equation 6 that uses proximity to a planned rather than a built branch:

$$y_i = \alpha_t + \alpha_c + \eta_1 P_i + \eta_2 M_i^P + \gamma_4 \mathbf{X}_i + \varepsilon_i, \quad (7)$$

where P_i is an indicator for whether an individual is located in a county with a planned branch, and M_i^P is the distance to the nearest planned branch. The results of estimating Equation 7 using OLS in our main analysis sample are reported in Table 13b. We fail to find evidence that proximity to a planned branch is associated with literacy, likelihood

of working, occupational income, or real property. Those living in a planned branch county are somewhat more likely to attend school, but outside these counties, the effect of planned branch distance goes against that predicted by the natural exclusion restriction violation: Blacks who live further from planned branches are more likely to attend school. Taken together, these results using planned branch distance suggest that the effect of the Freedman’s Savings Bank are not driven simply by better economic opportunities near locations where branches were planned (whether or not they were actually built).

As a final test, we attempt to assess whether individual outcomes have an association with proximity to a Freedman’s Savings Bank branch (consistent with a causal relationship of access to finance) distinct from any noncausal association with proximity to a planned branch. To do so, we estimate the effects of proximity to the nearest branch or planned branch, allowing these effects to differ based on whether a branch or planned branch is closer, estimating

$$y_i = \alpha_t + \alpha_c + \zeta_1 BP_i + \zeta_2 NB_i + \zeta_3 M_i^{BP} + \zeta_4 NB_i \times M_i^{BP} + \gamma_4 \mathbf{X}_i + \nu_i, \quad (8)$$

where BP_i is an indicator for whether an individual is located in a county with a branch or planned branch, NB_i is an indicator for whether the individual lives nearer to a branch than a planned branch, and M_i^{BP} is the distance from the nearest branch or planned branch.

Estimates of Equation 8 compare the effect of proximity to a branch (for those who live near a branch) with the effect of proximity to a planned branch (for those who live near a planned branch). If our main IV results are driven by factors other than access to finance, we might expect to see similar effects of proximity to branches and planned branches. The results, shown in Table 14, suggest that this is not the case.

There are four key explanatory variables (along with our standard control variables, included in the even-numbered columns). The coefficient on BP_i measures the outcome difference between those who live in the county itself versus those who live within 50 miles but outside it. The second explanatory variable, NB_i , is an indicator for whether an individual lives within 50 miles of a branch rather than a planned branch; positive coefficient estimates here are consistent with the Freedman’s Savings Bank having a positive effect on outcomes (but could in theory be driven by systematic differences between the locations where branches were opened versus planned).

The key explanatory variables for our test are the last two. The estimated coefficients on M_i^{BP} are mixed in sign and never statistically significant. Consistent with the results of the placebo test reported in Table 13b, these results suggest that outcome differences are not driven by distance from a planned branch. In contrast, our estimated

coefficients on the interaction term, $NB_i \times M_i^{BP}$, are consistently negative, and often statistically significant (in seven of ten specifications, they are significantly negative at the 10% level). These estimates show that distance from a branch has a more negative association with outcomes than distance from a planned branch, consistent with causal effects of access to finance.³³

4.4 Robustness tests

We also consider a variety of estimates that help demonstrate that the main IV results discussed in Section 3 are robust to the use of alternative estimation specifications and samples. Results using alternative specifications are presented in Online Appendix Table A6, and those replicating our baseline IV estimator on alternative samples are presented in Online Appendix Table A7. Each cell reports the estimated coefficient on *Has Account* derived from a separate IV regression, with the outcome variable for each regression indicated in its column header.

The first row of Table A6 shows our baseline specification including all controls, reproducing the results from the even-numbered columns of Table 7. These results weight observations using IPUMS-provided census sampling weights; the second row shows unweighted results. Perhaps unsurprisingly given that the IPUMS census sample attempts to oversample African Americans at (approximately) equal rates, the results change little in terms of magnitudes and significance.

The third and fourth rows of Online Appendix Table A6 show results using only one of our two excluded branch proximity instruments rather than both: either the continuous branch distance measure, or the discrete indicator for the presence of a branch in the county. The first-stage regression results were included in Columns 4–5 of Table 3. When using only the discrete instrument, the IV results remain statistically significant at the 5% level or higher. The results are underpowered using the minimum distance instrument alone, and only two of the five estimates remain statistically significant. For all columns (even for the insignificant, negative estimated coefficient on *Works*), 95% confidence intervals would include our baseline estimates.

Rows 5–7 of Online Appendix Table A6 include as a control variable the distance to the nearest large city, using either IPUMS-designated metropolitan area central/principal cities, or cities with populations of at least 25K or 10K (per IPUMS CITYPOP variable). All estimates

³³ Online Appendix Figures A5 and A6 present graphical evidence consistent with these results (and with the previous set of placebo tests). We observe a negative relationship between outcomes and distance from built branches, but a relatively flat relationship between outcomes and distance from planned branches.

remain positive and all but one are statistically significant, and we cannot reject equality with our baseline IV estimates.

In Online Appendix Table A7, we consider robustness to the use of alternative estimation samples. The first two rows consider subsamples likely to contain fewer black military veterans in order to confirm that veterans are not driving our main results. As noted in Section 1.1, serving veterans was an important impetus for the establishment of the bank, though they ultimately represented a relatively small share of depositors. In the first row we exclude all households containing one or more males who would have been of military age during the U.S. Civil War (aged 23–35 in 1870).³⁴ In the second row we exclude the three Southern states that enrolled the largest number of soldiers in the United States Colored Troops, which together represented approximately 67% of Southern USCT enrollees (Louisiana, Mississippi, and Tennessee, per Gladstone 1996).³⁵ The results remain similar to our baseline specifications, though two of the ten are no longer statistically significant.

The third and fourth rows of Online Appendix Table A7 vary the distance restriction. In the main results, we only consider individuals within 50 miles of a branch or planned branch; here we consider restriction to 40 or 60 miles instead. The results remain quite similar to the baseline results in terms of magnitude and statistical significance. The exception is whether an individual works, where point estimates are quite similar to those from the main sample but are no longer statistically significant.

Rows 5–9 of Online Appendix Table A7 explore other alternative geographic restrictions. The fifth row is limited to branches that were ever built (whether in or before 1870), while the sixth row excludes branches built in 1870, as they may not be a suitable control if, for example, they may have had some effect on 1870 outcomes. The seventh row excludes the Southern border states of Maryland, Delaware, Kentucky, and West Virginia, which allowed slavery but did not secede during the American Civil War. It is conceivable that these former slaves who did not join the Confederacy are very different from those living in former Confederate states under military occupation. Online Appendix Table A7 indicates that this is not the case.

³⁴ Black soldiers served in the Civil War under the United States Colored Troops regiments from May 1863 until they were disbanded at the conclusion of the war in October 1865. At their peak, black soldiers constituted roughly one-tenth of Union Army manpower. Smaller black regiments were later formed in the United States Regular Army.

³⁵ These areas came under Union occupation relatively early in the war; hence, the Union Army could recruit freed slaves. Other areas of the South—such as Virginia and the Carolinas—were not occupied until late in the war, so many Blacks there remained enslaved.

An additional potential concern is that if Freedman's Bank branches were more likely actually to be built in locations with Freedman's Bureau field offices, our estimates may be picking up their effects rather than the bank branches'. In fact, almost all branches were planned or built in locations that also had a Bureau office. In the eighth row, we exclude individuals living near the five branches and planned branches in cities that did not also have a field office of the Freedman's Bureau (Baltimore, Little Rock, Andersonville, Salisbury, and Sherman, per Carrier and Walton-Raji 2014). We also consider, in the ninth row, estimates excluding two branches that had a significant number of non-black account holders according to Osthaus (1976): New Orleans and Beaufort (along with New York and Jacksonville, which are already excluded from our analysis). In all these rows, the results are again quite similar, and we cannot reject equality with the baseline point estimates, although the effect on whether an individual works sometimes loses statistical significance at conventional levels.

Finally, we also consider estimating effects using a specification more similar to that used in Huber (2018). In particular, we relax the sample-inclusion requirement that individuals live within 50 miles of a branch or planned branch and include linear controls for the distance from each branch. The results are presented in Online Appendix Table A8. Point estimates are quite similar to those from our main specification (Table 7), although the estimated effects on literacy are statistically insignificant at conventional levels. However, we cannot reject equality of these point estimates with those from our main specification.

5. Concluding Remarks

Access to financial services has long been considered a hallmark of developed societies, and large changes in financial inclusion may have important effects. In this paper we demonstrate that in a population that had little to no access to banking services, a large increase in access to financial services had large effects on human capital and labor market outcomes. We find that the creation and expansion of the Freedman's Savings Bank led to increases in literacy, schooling, real estate wealth, work, and income for account holders, who were predominantly freed slaves in the nineteenth-century Southern United States.

The new data in this paper may be used to explore many other questions in the future. The experience of Freedman's Savings Bank may have had other important effects on the development of African Americans in the United States. In particular, after 1870 the collapse of the bank and loss of deposits may have had adverse effects on African Americans, and potentially important intergenerational effects. Historians, notably Osthaus (1976), have long noted that the collapse

of Freedman's Savings Bank—which many African Americans thought was fully backed by the federal government—and loss of savings led to a lack of trust in financial institutions by African Americans, and at least in part explains persistent gaps in utilization of financial services.

The FDIC National Survey of Unbanked and Underbanked Households concludes that African-American households are considerably more likely to be unbanked: 2015 survey results indicate that 18.2% of African-American households were unbanked, compared with 3.1% of white households. Almost one-third of households indicate a lack of trust in banks as the primary reason that they did not have bank accounts, with this explanation more common among African Americans. In Online Appendix Table A9 we show that African Americans in the present day who live in counties that once had a Freedman's Savings Bank branch are more likely to list mistrust of financial institutions as a reason for being unbanked; this association is not present for Whites. Personal experiences have been shown to have effects on household financial decision making (Kuchler and Zafar 2019; Malmendier and Nagel 2011), and these experiences may have intergenerational effects. Further work should disentangle whether this historical experience can at least partly explain persistent gaps in the utilization of financial services, but the possibility that the collapse of the Freedman's Savings Bank had measurable effects more than a century later is consistent with its having played a significant role in its customers' lives, and therefore with the large, positive short-run effects of financial inclusion that we estimate in this paper.

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Table 1
List of Freedman's Savings Bank branch/planned branch locations

Branches (opened pre-1870)				Planned branches (including 1870)					
Location		Population	Status	Location		Population	Status		
Huntsville	AL	<15,000	Opened	1865	Little Rock	AR	<15,000	Opened	1870
Washington	DC	109,119	Opened	1865	Atlanta	GA	21,789	Opened	1870
Natchez	MS	<15,000	Opened	1865	Lexington	KY	<15,000	Opened	1870
Vicksburg	MS	<15,000	Opened	1865	Louisville	KY	100,753	Opened	1870
Memphis	TN	40,226	Opened	1865	Saint Louis [†]	MO	310,864	Opened	1870
Lynchburg	VA	<15,000	Opened	1865	Columbus	MS	31,274	Opened	1870
Norfolk	VA	19,229	Opened	1865	Philadelphia ^{*†}	PA	674,022	Opened	1870
Richmond	VA	51,038	Opened	1865	Nashville	TN	25,865	Opened	1870
Mobile	AL	32,034	Opened	1866	Selma	AL	<15,000	Planned	
Tallahassee	FL	<15,000	Opened	1866	Andersonville	GA	<15,000	Planned	
Savannah	GA	28,235	Opened	1866	Columbus	GA	<15,000	Planned	
Augusta	GA	15,389	Opened	1866	Albany	GA	<15,000	Planned	
New Orleans	LA	191,418	Opened	1866	New Madrid [†]	MO	<15,000	Planned	
Baltimore	MD	267,354	Opened	1866	Jackson	MS	<15,000	Planned	
New Bern	NC	<15,000	Opened	1866	Charlotte	NC	<15,000	Planned	
New York [†]	NY	942,292	Opened	1866	Salisbury	NC	<15,000	Planned	
Beaufort	SC	<15,000	Opened	1866	Cincinnati [†]	OH	216,239	Planned	
Charleston	SC	48,956	Opened	1866	Harrisburg [†]	PA	23,104	Planned	
Shreveport	LA	<15,000	Opened	1868	Galveston	TX	<15,000	Planned	
Wilmington	NC	30,841	Opened	1868	Sherman	TX	<15,000	Planned	
Raleigh [*]	NC	<15,000	Opened	1868	Lexington	VA	<15,000	Planned	
Montgomery [*]	AL	<15,000	Opened		Charlottesville	VA	<15,000	Planned	
Columbia [*]	TN	<15,000	Opened						
Alexandria [*]	VA	<15,000	Opened						
Jacksonville [*]	FL	<15,000	Opened						
Macon [*]	GA	<15,000	Opened						

^{*}Excluded from main analysis sample: Missing Freedman's Savings Bank account records

[†]Excluded from main analysis sample: Outside South

Table 2
Summary statistics

	Mean	Std. Dev.	p_{50}	p_{25}	p_{75}
Has Account (%)	14.4				
Branch distance	79.9	96.3	43.0	0.0	110.0
Branch in county (%)	29.9				
Republican vote (K)	3.0	3.7	1.2	0.4	4.2
Republican share (%)	47.8	25.9	51.3	30.9	64.3
Attended school (%)	3.9				
Literate (%)	15.8				
Works (%)	39.4				
Income (\$100 in 1950)	5.3	6.8	0	0	9
Real property (\$)	9.7	307.0	0	0	0
Business owner (stricter, %)	0.1				
Business owner (looser, %)	0.2				
In metro area (%)	12.2				
City population (1000)	19.6	54.5	0	0	0
Male (%)	48.7				
Age	22.3	17.7	18	8	33
Relationship to household head					
Self (head)	19.3				
Spouse	13.9				
Child	41.7				
Roomer, boarder, lodger	13.5				
Other	11.6				
Number in household					
Married couples	0.9	0.5	1	1	1
Own children age <5	0.3	0.6	0	0	0
Voter turnout rate (%)	68.7	26.0	71.1	47.5	95.4
Observations	27,247				

This table reports distributional summary statistics for key outcomes, instruments, and control variables in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). Summary statistics for the election variables *Republican vote*, *Republican share*, and *Voter turnout rate* are calculated over the subset for which all three are available ($n = 17,762$). Variables and their construction are described in Online Appendix Table A1. For each variable we report the mean and—for non-indicator variables—the standard deviation, median (p_{50}), first quartile (p_{25}), and third quartile (p_{75}).

Table 3
First-stage estimates: Branch proximity instruments

	(1)	(2)	(3)	(4)	(5)	(6)
	Has Account	Has Account	Has Account	Has Account	Has Account	Has Account
Branch Distance	-0.000698*** (0.000132)		-0.000398*** (0.0000810)	-0.000700*** (0.000138)		-0.000405*** (0.0000797)
Branch in County		0.170*** (0.0190)	0.141*** (0.0198)		0.171*** (0.0186)	0.142*** (0.0194)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrl.				✓	✓	✓
Observations	27,247	27,247	27,247	27,247	27,247	27,247

This table reports OLS estimates (i.e., a linear probability model) of the likelihood that an individual in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”) has a Freedman’s Savings Bank account. The reported predictors are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 4
Control variables and branch proximity

(a) Distance to nearest pre-1870 branch

	(1)	(2)	(3)	(4)	(5)	(6) Relationship to Household Head					(10)	(11) Number in Household	
	Metro area	City pop (K)	Male	Age	Farm	Self	Spouse	Child	Roomer etc.	Other	Married couples	Own children <5	
Branch Distance	-0.000432 (0.000548)	-0.113 (0.0759)	0.0000686 (0.0000562)	-0.00787*** (0.00273)	-0.000157 (0.000222)	-0.000100* (0.0000526)	-0.0000555 (0.0000342)	0.0000737 (0.000136)	0.000111 (0.000196)	-0.0000292 (0.0000420)	0.0000829 (0.000192)	0.0000797 (0.0000825)	
Observations	27,247	27,247	27,247	27,247	27,247	27,247	27,247	27,247	27,247	27,247	27,247	27,247	

(b) Presence of pre-1870 branch in county

	(1)	(2)	(3)	(4)	(5)	(6) Relationship to Household Head					(10)	(11) Number in Household	
	Metro area	City pop (K)	Male	Age	Farm	Self	Spouse	Child	Roomer etc.	Other	Married couples	Own children <5	
Branch in County	0.0967 (0.246)	36.79 (27.95)	-0.0191 (0.0162)	1.403*** (0.500)	0.0907 (0.0633)	0.00722 (0.0162)	0.00112 (0.0102)	-0.0286 (0.0238)	0.0196 (0.0419)	0.000734 (0.0170)	-0.0271 (0.0514)	-0.00509 (0.0178)	
Observations	9,999	9,999	9,999	9,999	9,999	9,999	9,999	9,999	9,999	9,999	9,999	9,999	

This table reports OLS estimates of the association between the instruments and various control variables described in Section 2.2. Panel (a) is estimated on our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name"), where the dependent variable is indicated in the column header and the independent variable is the distance to the nearest pre-1870 branch. Panel (b) is restricted to the subsample who live in a county with a branch, and the independent variable is an indicator for the presence of a pre-1870 branch in the county. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 5
First-stage estimates: Election instruments

	(1)	(2)	(3)	(4)	(5)	(6)
	Has Account	Has Account	Has Account	Has Account	Has Account	Has Account
Rep. Vote (1868)	0.0000262*** (0.00000214)		0.0000259*** (0.00000293)	0.0000258*** (0.00000190)		0.0000251*** (0.00000295)
Rep. Share (1868)		0.218*** (0.0545)	0.00810 (0.0599)		0.208*** (0.0523)	0.0177 (0.0595)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrl.				✓	✓	✓
Observations	17,762	17,762	17,762	17,762	17,762	17,762

This table reports OLS estimates (i.e., a linear probability model) of the likelihood that an individual in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”) has a Freedman’s Savings Bank account. The reported predictors are the county-level 1868 Republican congressional vote and vote share. All columns include the county-level 1868 voter turnout rate as an additional control, or state-level turnout where county-level turnout is not available. Estimates are restricted to the subsample for which the electoral variables are available. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 6
Account holding and census outcomes

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Has Account	0.0166*** (0.00316)	0.0504*** (0.0106)	0.0255** (0.0105)	0.0195*** (0.00513)	0.000769 (0.00140)
Observations	27,247	27,247	27,247	27,247	27,247

This table reports OLS estimates of the association between having a Freedman's Savings Bank account and various outcomes in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name"). Column 4 includes as a control an indicator variable for nonzero income; Column 5 includes as a control an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 7
Account holding and census outcomes exploiting branch proximity

(a) Human capital outcomes

	(1) Attended School	(2) Attended School	(3) Literate	(4) Literate
Has Account	0.143*** (0.0156)	0.139*** (0.0152)	0.194*** (0.0662)	0.134** (0.0628)
Fixed Effects	✓	✓	✓	✓
Demographic Ctrls.		✓		✓
Observations	27,247	27,247	27,247	27,247

(b) Labor market and wealth accumulation outcomes

	(1) Works	(2) Works	(3) Income	(4) Income	(5) Real Property	(6) Real Property
Has Account	0.0586*** (0.0198)	0.0285* (0.0160)	0.0400*** (0.0100)	0.0385*** (0.00917)	0.0245** (0.0100)	0.0258*** (0.00968)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrls.		✓		✓		✓
Observations	27,247	27,247	27,247	27,247	27,247	27,247

This table reports IV estimates of the effect of having a Freedman’s Savings Bank account on various outcomes in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). The excluded instruments are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county, as in Columns 3 and 6 of Table 3. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions (panel b, Columns 3–4) also include an indicator variable for nonzero income. Real property regressions (panel b, Columns 5–6) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 8
Account holding and census outcomes exploiting 1868 election results

(a) Estimates (with 50-mile distance restriction)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Attended School	Attended School	Literate	Literate	Works	Works	Income	Income	Real Property	Real Property
Has Account	0.152*** (0.0147)	0.136*** (0.0122)	0.293*** (0.0539)	0.165** (0.0682)	0.0973*** (0.0264)	0.0455** (0.0199)	0.0131* (0.00718)	0.0265*** (0.00849)	0.00538 (0.00454)	0.00458 (0.00493)
Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Ctrls.		✓		✓		✓		✓		✓
Observations	17,762	17,762	17,762	17,762	17,762	17,762	17,762	17,762	17,762	17,762

(b) Estimates (without 50-mile distance restriction)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Attended School	Attended School	Literate	Literate	Works	Works	Income	Income	Real Property	Real Property
Has Account	0.109*** (0.00907)	0.0981*** (0.0115)	0.242*** (0.0418)	0.129*** (0.0464)	0.0909*** (0.0321)	0.0545* (0.0324)	0.00398 (0.00546)	0.0157** (0.00705)	0.0146** (0.00601)	0.0128*** (0.00433)
Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic Ctrls.		✓		✓		✓		✓		✓
Observations	48,405	48,405	48,405	48,405	48,405	48,405	48,405	48,405	48,405	48,405

This table reports IV estimates of the effect of having a Freedman’s Savings Bank account on various outcomes in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). The excluded instruments are the county-level 1868 Republican congressional vote and vote share, as in Columns 3 and 6 of Table 5. Panel (b) expands the sample by relaxing the 50-mile distance restriction. All columns include the county-level 1868 voter turnout rate as an additional control, or state-level turnout where county-level turnout is not available. Estimates are restricted to the subsample for which the electoral variables are available. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions (Columns 7–8) also include an indicator variable for nonzero income. Real property regressions (Columns 9–10) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 9
Mechanisms: Business ownership

(a) Ordinary least squares estimates

	(1) Stricter	(2) Stricter	(3) Looser
Has Account	0.000841* (0.000446)	0.000873* (0.000460)	0.00143** (0.000643)
Fixed Effects	✓	✓	✓
Demographic Ctrls.		✓	✓
Observations	27,247	27,247	27,247

(b) Instrumental variables estimates: Branch proximity instruments

	(1) Stricter	(2) Stricter	(3) Looser
Has Account	0.00514*** (0.000811)	0.00515*** (0.000863)	0.00634*** (0.00109)
Fixed Effects	✓	✓	✓
Demographic Ctrls.		✓	✓
Observations	27,247	27,247	27,247

(c) Instrumental variables estimates: Election instruments

	(1) Stricter	(2) Stricter	(3) Looser
Has Account	0.00762*** (0.00135)	0.00898*** (0.00133)	0.00861*** (0.00128)
Fixed Effects	✓	✓	✓
Demographic Ctrls.		✓	✓
Observations	17,762	17,762	17,762

This table reports OLS and IV estimates of the effect of having a Freedman’s Savings Bank account on the likelihood that an individual in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”) has a business. Columns 1–2 in each panel rely on a stricter definition of business ownership, while Column 3 uses a looser definition. Panel (a) presents OLS estimates, while panels (b) and (c) present IV estimates. The excluded instruments in Panel (b) are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county, as in Columns 3 and 6 of Table 3. The excluded instruments in panel (c) are the 1868 Republican congressional vote and vote share, as in Columns 3 and 6 of Table 5; these results include the 1868 voter turnout rate as an additional control (or state-level turnout where county-level turnout is not available), and are restricted to the subsample for which the electoral variables are available. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 10
County-level aggregates

(a) Branch proximity instruments						
	(1)	(2)	(3)	(4)	(5)	(6)
	Attended School	Literate	Works	Income	Real Property	Business Ownership
Account Holders (K)	0.00346*** (0.000492)	0.00754** (0.00373)	0.00871*** (0.00203)	0.0353*** (0.00440)	0.123*** (0.0219)	0.000362*** (0.0000109)
Observations	171	171	171	171	171	171

(b) Election instruments						
	(1)	(2)	(3)	(4)	(5)	(6)
	Attended School	Literate	Works	Income	Real Property	Business Ownership
Account Holders (K)	0.00469*** (0.000855)	0.0170* (0.00927)	0.00910*** (0.00333)	0.0195*** (0.00673)	0.108*** (0.0289)	0.000213*** (0.0000121)
Observations	112	112	112	112	112	112

This table reports IV estimates of the prevalence of Freedman's Savings Bank account holders on various outcomes at the aggregated county level. The key explanatory variable is the county-level number of account holders in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name") scaled up to thousands using the 2% IPUMS sampling frequency. Business ownership is defined using the "stricter" definition. In panel (a), the excluded instruments are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county. In panel (b), the excluded instruments are the county-level 1868 Republican congressional vote and vote share; county-level 1868 voter turnout rate (or state-level, where county-level is unavailable) is included as an additional control; and the sample is restricted to counties where the electoral variables are available. Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 11
Early versus late account opening

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Has Account (early)	0.0199*** (0.00389)	0.0389*** (0.0125)	0.0237** (0.00962)	0.0163** (0.00639)	0.000106 (0.00233)
Has Account (late)	0.00817** (0.00375)	0.0234 (0.0151)	0.0134 (0.0118)	0.00377 (0.00636)	0.0000476 (0.00178)
Observations	21,030	21,030	21,030	21,030	21,030
p -val: $\beta_{\text{Early}} = \beta_{\text{Late}}$	0.0227	0.443	0.401	0.114	0.983

This table reports OLS estimates of the association between having a Freedman's Savings Bank account and various outcomes in our main analysis sample as described in Section 1.2, restricted to those who live near a built branch (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch, in households where at least one member has a "potentially matchable name"). Accounts are split based on whether the account opening year is before or after 1870. As in Table 6, Column 4 includes as a control an indicator variable for nonzero income; Column 5 includes as a control an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 12
Illiterate subsample: Labor market and wealth accumulation outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Works	Works	Income	Income	Real Property	Real Property
Has Account	0.0642*** (0.0225)	0.0439** (0.0188)	0.0448*** (0.00997)	0.0442*** (0.00933)	0.0211** (0.00877)	0.0227*** (0.00878)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrls.		✓		✓		✓
Observations	22,939	22,939	22,939	22,939	22,939	22,939

This table replicates the analysis in panel (b) of Table 7, restricted to the subsample who are not literate in 1870. That is, it reports IV estimates of the effect of having a Freedman’s Savings Bank account on various outcomes for illiterate individuals in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). The excluded instruments are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county, as in Columns 3 and 6 of Table 3. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions (Columns 3–4) also include an indicator variable for nonzero income. Real property regressions (Columns 5–6) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 13
Placebo tests

(a) Placebo tests using Whites					
	(1)	(2)	(3)	(4)	(5)
	Attended School	Literate	Works	Income	Real Property
Branch Distance	0.0000218 (0.0000446)	0.0000789 (0.0000630)	0.00000186 (0.0000120)	0.00000245 (0.00000649)	0.000170*** (0.0000538)
Branch in County	0.00331 (0.00747)	0.00645 (0.0110)	0.00139 (0.00164)	-0.000318 (0.00119)	0.0298*** (0.00899)
Controls	✓	✓	✓	✓	✓
Observations	26,389	26,389	26,389	26,389	26,389

(b) Placebo tests using proximity to planned branches					
	(1)	(2)	(3)	(4)	(5)
	Attended School	Literate	Works	Income	Real Property
Planned Branch Distance	0.000115** (0.0000447)	0.0000282 (0.000108)	-0.0000239 (0.0000261)	-0.00000895 (0.0000141)	0.0000191 (0.0000222)
Planned Branch in County	0.0228*** (0.00576)	-0.0185 (0.0159)	0.00487 (0.00396)	-0.00179 (0.00215)	0.000675 (0.00182)
Controls	✓	✓	✓	✓	✓
Observations	27,247	27,247	27,247	27,247	27,247

In panel (a), this table reports OLS estimates of the association between various outcomes and proximity to a Freedman's Savings Bank branch for Whites living in the South within 50 miles of a Freedman's Savings Bank branch or planned branch; the reported predictors are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county. Panel (b) reports OLS estimates of the association between various outcomes and proximity to a planned Freedman's Savings Bank branch in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name"); the reported predictors are the distance to the nearest planned branch (including 1870) and an indicator for the presence of a planned branch in the county. Controls are metropolitan area status, city population, age, sex, number of own children under age five in household, and fixed effects for the opening date of the nearest branch, relationship with household head, number of married couples in the household, and occupation. Columns 4 and 5 also include indicator variables for nonzero income and real property, respectively. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Table 14
Placebo tests using proximity to branches or planned branches

(a) Human capital outcomes

	(1) Attended School	(2) Attended School	(3) Literate	(4) Literate
Branch or Planned in County	0.0422*** (0.00883)	0.0224*** (0.00737)	0.00720 (0.0637)	-0.0182 (0.0466)
Near Branch	0.00644*** (0.00145)	0.0107*** (0.00241)	0.0851*** (0.00871)	0.0413** (0.0166)
Branch or Planned Distance	0.000277 (0.000232)	-0.0000219 (0.000208)	0.000148 (0.00157)	0.000245 (0.00115)
Near Branch×Branch or Planned Distance	-0.000295** (0.000127)	-0.0000687 (0.000105)	-0.00305*** (0.000527)	-0.00166*** (0.000522)
Controls		✓		✓
Observations	27,247	27,247	27,247	27,247

(b) Labor market and wealth accumulation outcomes

	(1) Works	(2) Works	(3) Income	(4) Income	(5) Real Property	(6) Real Property
Branch or Planned in County	0.00109 (0.0368)	0.0127 (0.0116)	0.0109 (0.0300)	0.00102 (0.00966)	-0.00296 (0.00561)	-0.00178 (0.00476)
Near Branch	0.0412*** (0.00560)	-0.000491 (0.00491)	0.0478*** (0.00465)	0.0101*** (0.00232)	0.00407*** (0.000751)	0.00568*** (0.00153)
Branch or Planned Distance	-0.000721 (0.000987)	0.000118 (0.000297)	-0.000364 (0.000756)	0.0000136 (0.000247)	-0.000106 (0.000140)	-0.0000248 (0.000123)
Near Branch×Branch or Planned Distance	-0.000698 (0.000470)	-0.0000396 (0.000152)	-0.000546* (0.000307)	-0.000152* (0.0000842)	-0.000183*** (0.0000529)	-0.000170*** (0.0000578)
Controls		✓		✓		✓
Observations	27,247	27,247	27,247	27,247	27,247	27,247

This table reports OLS estimates of the association between various outcomes and proximity to a Freedman’s Savings Bank branch or planned branch in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). The reported predictors are an indicator for the presence of a branch or planned branch in the county, an indicator for being within 50 miles of a branch, the distance to the nearest branch or planned branch, and an interaction effect. Even-numbered columns also control for metropolitan area status, city population, age, sex, number of own children under age five in household, and fixed effects for the opening date of the nearest branch, relationship with household head, number of married couples in the household, and occupation. Income regressions (panel b, Columns 3–4) also include an indicator variable for nonzero income. Real property regressions (panel b, Columns 5–6) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

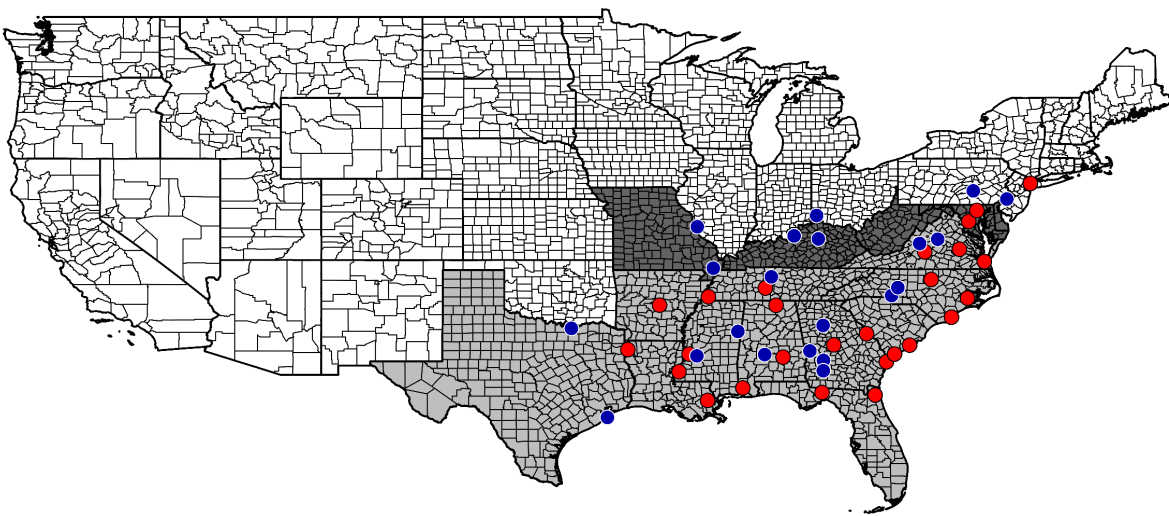
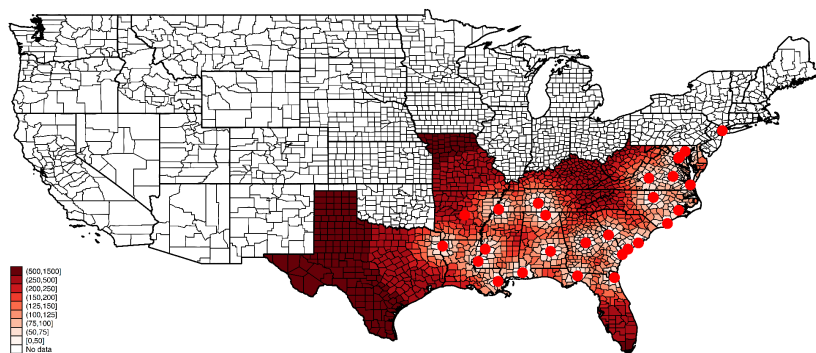


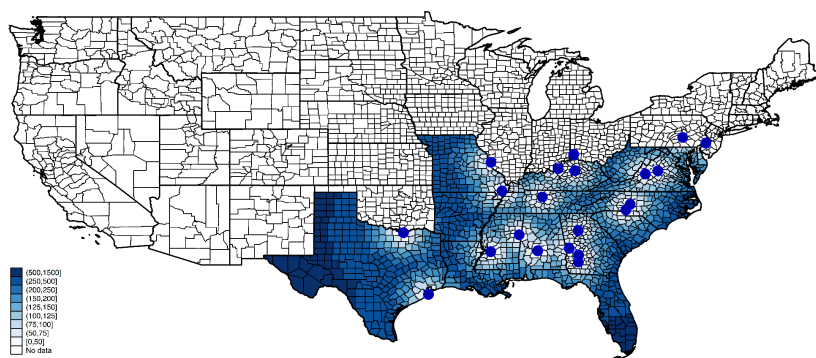
Figure 1

Freedman's Savings Bank branch locations

This map presents the location of proposed and implemented Freedman's Savings Bank branches. The red dots indicate pre-1870 branches, while the blue dots indicate planned branches (including those opened in 1870). Southern slave states that seceded during the American Civil War are shaded light gray, while border states that allowed slavery prior to 1865 but did not secede from the Union are shaded dark gray.



(a) Branches (opened pre-1870)



(b) Planned branches (including 1870)

Figure 2

Distance from Freedman's Savings Bank branches

This map presents the location of proposed and implemented Freedman's Savings Bank branches, and the minimum distance from planned and implemented branches. Distance is measured from the geographic centroid of each county. The red dots in panel (a) indicate pre-1870 branches, while the blue dots in panel (b) indicate planned branches (including those opened in 1870). Counties are colored using distance in miles to the nearest branch/planned branch.

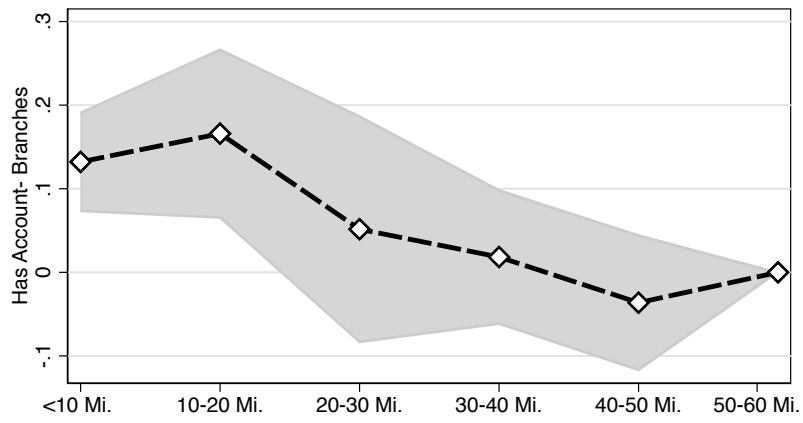


Figure 3

Freedman's Savings Bank account status by branch distance

This figure shows the fraction of Blacks in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name") with a Freedman's Savings Bank account, by distance from the nearest branch, relative to a baseline group (50–60 miles from a branch). The solid line shows the fraction, while the dashed line shows a 95% confidence band around the mean.

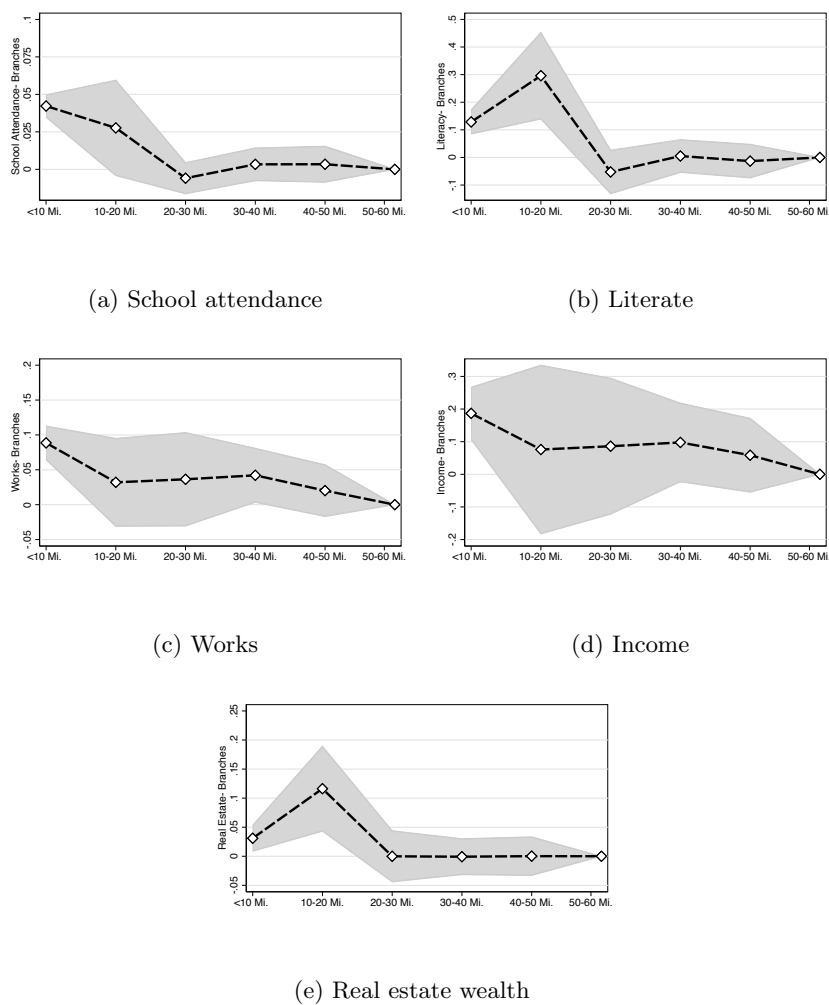


Figure 4

Outcomes by branch distance

This figure plots estimates of the coefficients β_j from the following specification $y_i = \alpha + \sum_{j \in \{0,10,20,30,40\}} \beta_j \mathbb{1}[j \leq M_i^B \leq j+10] + \varepsilon_i$. The baseline (50–60 miles from a branch) is normalized to zero. Standard errors are clustered by distance to the nearest branch. The gray shaded area depicts a 95% confidence interval.

Online Appendix for Financial Inclusion, Human Capital, and Wealth Accumulation: Evidence from the Freedman’s Savings Bank

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Table A1: Main variable descriptions

This table describes the variables used in our analysis and explains their construction. References in SMALL CAPITAL LETTERS are to IPUMS variable names. Distances are calculated using county centroid latitude/longitudes (i.e., of county of residence and branch or planned branch county).

Variable	Description	Calculation
Outcome variables		
Attended School	Attended school in last year	Includes individuals who “attended school within the year.” Indicator for SCHOOL = 2.
Literate	Able to read or write	Includes individuals except those who “cannot read” and “cannot write,” or are under age 10. Indicator for LIT ∈ {2, 3, 4}.
Works	In labor force	Classified by IPUMS based on “profession, occupation, or trade of each person” from census. Indicator for LABFORCE = 2.
Income	Log occupational income	Natural logarithm of IPUMS-assigned median income of people with a given occupation in 1950 (in hundreds of 1950 dollars). $\ln(1 + OCCSCORE)$.
Real Property	Log real property value	Natural logarithm of reported value of owned real estate. $\ln(1 + REALPROP)$.
Business Owner (Stricter)	Owns a business (stricter definition)	Includes individuals whose occupation is: Clothing Dealer, Club House, Coal Dealer, Confectionary, Cotton Fct, Drinking Saloon, Fish Dealer, Frm & Producer, Fruit Business, Fruit Shop, Furnish Rooms, Furnished Rooms (landlord), Furniture Mkr, Grocer, Grocery Merchant, Grocery Store, Keeps A Bar, Keeps Boarder, Keeps Boarding House, Keeps Eating House, Keeps A Grocery, Keeping Sch, Keeps Hous, Keeps Private Sch, Keepsh, Marketer, Meat Dealer, Merchant, Negro Trader, Restaurant Keeper, Restaurant Kpr, Restaurant, Retail Grocer, Ret Grocer, Sell in Market, Sells in Market, Boarding House, Boarding House Keeper, Boarding Master, Butcher, Beef Butcher, Bar Kpr, Barbar, Barkeeper, Billiard Saloon, Billiards Saloon Keeper, Cake Shop, Cake Vender, Candy Baker, Planter, Keeping Saw Mill, or Builder & Contractor.

(continued)

Table A1: Main Variable Descriptions (Cont.)

Variable	Description	Calculation
Business Owner (Looser)	Owens a business (looser definition)	In addition to individuals classified as business owners under the stricter definition, also includes individuals whose occupation is Clothier, Cigar Mkr, Cigar-makwer Cigars Mkr, Frm Leaser, Fish-monger, Drugist, Drsssmaker, Dealer in Housing, Dressmkr, Dressmaking, Hair Dresser, Hair Picker, Hairdresser, Master Barber, Printing Ofc, S-ping House, Barber, or Hair Merchant.
Key explanatory variables		
Has Account (A_i)	Household member matches account records	Indicator for individuals who live within 50 miles of a branch and have a household member whose first and last names match an account record from the nearest branch as described in Section 1.2.
Branch Distance (M_i^B)	Distance to nearest branch	Distance to nearest Freedman's Savings Bank branch (pre-1870).
Branch in County (B_i)	Lives in branch county	Indicator for individuals living in a branch county (i.e., $M_i^B = 0$).
Planned Branch Distance (M_i^P)	Distance to nearest planned branch	Distance to nearest planned Freedman's Savings Bank branch (including 1870).
Planned Branch in County (P_i)	Lives in planned branch county	Indicator for individuals living in a planned branch county (i.e., $M_i^P = 0$).
Branch or Planned Distance (M_i^{BP})	Distance to nearest branch or planned branch	Distance to nearest built or planned Freedman's Savings Bank branch.
Branch or Planned in County (BP_i)	Lives in branch or planned branch county	Indicator for individuals living in a branch or planned branch county (i.e., $M_i^{BP} = 0$).
Near Branch (NB_i)	Lives within 50 miles of branch	Indicator for individuals who live within 50 miles of a branch (i.e., $M_i^B \leq 50$).
Republican Vote (1868) (V_i)	1868 Republican congressional vote	County-level number of votes for Republican congressional candidate in 1868 per Clubb, Flanigan, and Zingale (2006).
Republican Share (1868) (S_i)	1868 Republican congressional vote share	County-level fraction of votes for Republican congressional candidate in 1868 per Clubb, Flanigan, and Zingale (2006).

(continued)

Table A1: Main Variable Descriptions (Cont.)

Variable	Description	Calculation
Other control variables		
Metro Area	Metropolitan area status	Classified by IPUMS as “not in metro area” (LABFORCE = 1), “in metro area, central/principal city” (LABFORCE = 2), “in metro area, outside central/principal city” (LABFORCE = 3).
City Population	City population	IPUMS-calculated population of city, if any. CITYPOP.
Male	Sex	Indicator for SEX = 1.
Age	Age at last birthday	AGE.
Relationship	Relationship to HH head	Classified by IPUMS. RELATED.
Married Couples	Married couples in HH	Total number of married couples living in household as classified by IPUMS. NCOUPLES.
Own Children <5	Own children <5 in HH	Number of individual’s own children under age five living in household as classified by IPUMS. NCHLT5.
Voter turnout rate (1868)	1868 voter turnout rate	County-level voter turnout rate in 1868 per Clubb, Flanigan, and Zingale (2006); where county-level turnout is unavailable, state-level turnout constructed by aggregating all available counties in state.

Table A2: Summary statistics by branch

This table reports mean values of key control variables by nearest branch for individuals in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”). Variables and their construction are described in Online Appendix Table A1.

	Metro area (%)	City pop (K)	Male (%)	Age	Farm (%)	Relationship to Household Head (%)					Number in Household	
						Self	Spouse	Child	Roomer etc.	Other	Married couples	Own children <5
Opened Pre-1870												
Huntsville	—	0.3	48.0	21.2	50.8	18.2	13.8	43.6	9.8	14.6	0.8	0.3
Mobile	—	18.1	48.7	23.3	2.4	17.8	10.7	35.8	24.4	11.3	0.8	0.2
Washington	88.8	87.7	43.6	24.3	3.6	18.5	11.9	34.7	24.2	10.7	0.9	0.2
Tallahassee	—	0.1	48.1	21.5	3.4	21.8	15.5	47.0	6.4	9.3	0.8	0.3
Savannah	—	11.3	46.6	24.0	23.1	20.2	15.9	37.1	17.4	9.5	1.0	0.2
Augusta	—	1.8	49.0	21.7	8.1	20.6	15.0	44.7	10.1	9.6	0.9	0.3
Shreveport	—	0.4	51.3	20.3	64.2	20.5	16.1	45.2	6.2	12.1	0.9	0.3
New Orleans	87.9	168.3	45.7	26.3	1.5	19.9	13.0	30.1	24.6	12.4	0.9	0.2
Baltimore	55.2	121.5	46.6	24.5	8.4	15.8	11.9	34.5	25.9	11.9	0.9	0.2
Natchez	—	0.7	48.4	22.5	7.9	22.3	13.2	36.9	8.4	19.2	0.8	0.2
Vicksburg	—	0.9	50.2	23.3	38.4	21.5	15.6	40.7	9.6	12.6	0.9	0.2
New Bern	—	1.2	49.5	24.2	39.9	23.2	15.5	41.2	8.4	11.8	0.8	0.3
Wilmington	—	4.7	52.4	22.5	9.9	18.2	14.1	42.5	14.1	11.2	0.9	0.2
Beaufort	—	—	49.8	23.8	73.7	24.3	17.4	44.7	1.6	12.0	0.8	0.3
Charleston	—	20.4	44.0	24.2	15.2	21.4	14.6	38.5	12.7	12.8	0.9	0.2
Columbia	—	—	49.7	20.3	47.9	16.3	14.6	43.8	13.2	12.2	1.0	0.4
Memphis	—	7.9	48.9	21.5	49.3	18.6	13.7	39.7	14.1	13.9	1.0	0.3
Lynchburg	—	0.7	50.5	21.9	16.1	18.3	13.8	45.2	12.1	10.6	0.8	0.3
Richmond	40.4	15.0	46.9	24.2	17.4	17.6	12.8	37.5	20.2	11.8	0.9	0.2
Norfolk	—	—	48.9	22.1	35.1	19.0	14.4	44.3	12.1	10.3	0.9	0.2
Opened 1870												
Little Rock	—	4.9	52.2	22.6	13.1	23.3	16.7	35.5	11.8	12.7	0.8	0.2
Atlanta	—	9.0	48.4	21.4	15.5	18.8	13.7	49.9	10.3	7.4	0.9	0.3
Lexington	—	2.4	47.4	21.5	21.1	13.8	10.5	35.4	26.8	13.4	1.0	0.2
Louisville	62.6	46.8	46.2	22.4	22.3	14.3	10.6	36.0	30.5	8.5	1.0	0.2
Columbus	—	0.2	50.2	21.1	11.4	21.5	14.8	47.6	4.7	11.4	0.8	0.3
Nashville	—	4.1	49.1	21.9	15.9	17.7	13.6	41.6	12.9	14.2	0.9	0.3
Planned												
Selma	—	0.3	50.7	21.6	19.3	21.4	15.3	46.5	4.5	12.3	0.9	0.3
Albany	—	0.1	52.3	20.9	3.5	23.5	17.7	47.2	4.9	6.7	0.9	0.3
Columbus	—	0.5	49.3	20.7	11.7	19.3	15.0	47.9	9.4	8.5	0.9	0.3
Andersonville	—	0.1	48.9	20.8	7.8	22.5	17.2	49.6	3.4	7.4	0.9	0.3
Jackson	—	—	51.1	19.5	44.5	17.0	12.0	42.9	11.0	17.0	0.8	0.3
New Madrid	—	0.3	68.0	21.9	2.0	14.0	10.0	36.0	28.0	12.0	0.7	0.2
Charlotte	—	0.1	54.8	19.9	34.0	16.7	13.4	46.4	10.0	13.4	0.9	0.3
Salisbury	—	—	51.9	20.1	24.8	16.3	11.2	48.4	13.8	10.3	0.9	0.3
Cincinnati	50.0	11.1	40.0	23.6	7.1	18.6	10.0	27.1	34.3	10.0	0.8	0.3
Galveston	—	11.8	53.6	23.8	7.2	18.8	14.5	31.9	20.3	14.5	1.0	0.4
Sherman	—	—	52.6	16.9	21.1	19.3	10.5	40.4	15.8	14.0	0.5	0.2
Charlottesville	—	0.2	49.5	21.5	7.9	19.6	13.6	49.1	10.0	7.7	0.8	0.3
Lexington	—	0.4	48.7	20.9	11.9	13.9	10.9	49.7	18.9	6.6	1.0	0.3

Table A3: OLS estimates with 1880 census match interaction

This table reports OLS estimates (as in Table 6) of the association between having a Freedman’s Savings Bank account and various outcomes in our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman’s Savings Bank branch or planned branch, in households where at least one member has a “potentially matchable name”), allowing a differential impact for individuals who are not matched to the 1880 census. In particular, we interact our key “Has Account” variable with an indicator variable for whether there is no individual classified Black or Mulatto with the same first and last name (the first space-delimited word of NAMEFRST, and NAMELAST) in the 100% 1880 census sample compiled by IPUMS (Ruggles et al. 2015). Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Has Account	0.0231*** (0.00390)	0.0520*** (0.0101)	0.0264** (0.0112)	0.0205*** (0.00671)	0.00244 (0.00170)
No 1880 match	0.00507* (0.00293)	0.00693 (0.00471)	0.00171 (0.00629)	0.00948** (0.00430)	0.0000764 (0.00116)
Has Account×No 1880 match	-0.0150*** (0.00376)	-0.000930 (0.0141)	-0.00150 (0.0112)	0.00212 (0.00784)	-0.00446* (0.00250)
Observations	27,247	27,247	27,247	27,247	27,247

Table A4: First-stage estimates: Election instruments (without 50-mile distance restriction)

This table reports OLS estimates (i.e., a linear probability model) of the likelihood that an individual in our main analysis sample as described in Section 1.2 excluding the 50-mile distance restriction (i.e., Blacks, in the South, in households where at least one member has a “potentially matchable name”) has a Freedman’s Savings Bank account. The reported predictors are the county-level 1868 Republican congressional vote and vote share. All columns include the county-level 1868 voter turnout rate as an additional control, or state-level turnout where county-level turnout is not available. Estimates are restricted to the subsample for which the electoral variables are available. “Fixed effects” are for metropolitan area status, the opening date of the nearest branch, and occupation. “Demographic controls” are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Has Account	Has Account	Has Account	Has Account	Has Account	Has Account
Rep. Vote (1868)	0.0000347*** (0.00000261)		0.0000381*** (0.00000221)	0.0000320*** (0.00000359)		0.0000353*** (0.00000314)
Rep. Share (1868)		0.107 (0.0699)	-0.0701*** (0.0230)		0.0922 (0.0597)	-0.0589** (0.0229)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrl.				✓	✓	✓
Observations	48,405	48,405	48,405	48,405	48,405	48,405

Table A5: Heterogeneous effects by branch opening date

This table reports a variety of IV estimates of the effect of having a Freedman's Savings Bank account on various outcomes. Each cell reports the estimated coefficient on *Has Account* derived from a separate regression; the outcome variable for each regression is indicated in its column header. The sample for each regression are the subset of individuals in our main analysis sample as described in Section 1.2 who live within 50 miles of a Freedman's Savings Bank branch as indicated in the row title, or a planned branch. "Earlier Pre-1870 Branches" opened 1865–66, and "Later" after 1866. All regressions include fixed effects for metropolitan area status, the opening date of the nearest branch, and occupation; panel (b) regressions also include controls for city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions (Column 4) also include an indicator variable for nonzero income. Real property regressions (Column 5) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

(a) Without demographic controls

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Earlier Pre-1870 Branches (and Planned)	0.163*** (0.0176)	0.205*** (0.0736)	0.0634*** (0.0223)	0.0436*** (0.0115)	0.0206** (0.00913)
Later Pre-1870 Branches (and Planned)	0.00625 (0.0574)	0.106 (0.0936)	-0.00772 (0.0583)	0.0178 (0.0137)	0.0701*** (0.0216)
<i>p</i> -value: Equality of $\beta_{\text{Has account}}$	0.006***	0.301	0.288	0.110	0.011**

(b) With demographic controls

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Earlier Pre-1870 Branches (and Planned)	0.159*** (0.0171)	0.142** (0.0699)	0.0331* (0.0182)	0.0421*** (0.0105)	0.0222** (0.00912)
Later Pre-1870 Branches (and Planned)	-0.0493 (0.0550)	0.0611 (0.0936)	-0.0268 (0.0492)	0.0272** (0.0134)	0.0542*** (0.0206)
<i>p</i> -value: Equality of $\beta_{\text{Has account}}$	0.000***	0.389	0.279	0.327	0.099*

Table A6: Robustness to alternative estimation specifications

This table reports a variety of alternative IV estimates of the effect of having a Freedman's Savings Bank account on various outcomes. Each cell reports the estimated coefficient on *Has Account* derived from a separate regression; the outcome variable for each regression is indicated in its column header. "Baseline IV" replicates the results with fixed effects effects and demographic controls from Table 7. "Unweighted" weights observations equally, rather than using IPUMS sample weights. "Alternative Excluded Instruments" instrument *Has Account* using either *Branch Distance* or *Branch in County* (but not both, as in our baseline). "Control for Distance to Nearest" include as a control variable distance to the nearest large city, using either IPUMS-designated metropolitan area central/principal cities, or cities with populations of at least 25K or 10K (per IPUMS CITYPOP variable). All regressions include controls for metropolitan area status, city population, age, sex, number of own children under age five in household, and fixed effects for the opening date of the nearest branch, relationship with household head, number of married couples in the household, and occupation; and are conducted on our main analysis sample as described in Section 1.2 (i.e., Blacks, in the South, within 50 miles of a Freedman's Savings Bank branch or planned branch, in households where at least one member has a "potentially matchable name"), with $n = 27,247$. Income regressions (Column 4) also include an indicator variable for nonzero income. Real property regressions (Column 5) also include an indicator variable for nonzero real property. Except in the "unweighted" regressions, observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
Baseline IV	0.139*** (0.0152)	0.134** (0.0628)	0.0285* (0.0160)	0.0385*** (0.00917)	0.0258*** (0.00968)
Unweighted	0.142*** (0.0163)	0.125** (0.0622)	0.0281* (0.0166)	0.0361*** (0.00900)	0.0244** (0.00956)
<i>Alternative Excluded Instruments</i>					
Branch Distance only	0.119*** (0.0314)	0.0856 (0.119)	-0.0255 (0.0278)	0.0443*** (0.0162)	0.00695 (0.0232)
Branch in County only	0.147*** (0.0177)	0.154** (0.0686)	0.0504** (0.0232)	0.0362*** (0.0123)	0.0335*** (0.00949)
<i>Control for Distance to Nearest</i>					
Metro. Area Central/Principal City	0.166*** (0.0175)	0.213*** (0.0677)	0.0337* (0.0203)	0.0503*** (0.00989)	0.0273** (0.0107)
City with population $\geq 25K$	0.144*** (0.0208)	0.135 (0.0854)	0.0507** (0.0219)	0.0529*** (0.0139)	0.0350*** (0.0121)
City with population $\geq 10K$	0.145*** (0.0233)	0.179* (0.0945)	0.0544** (0.0261)	0.0507*** (0.0162)	0.0331*** (0.0126)

Table A7: Robustness to alternative sample definitions

This table reports a variety of alternative IV estimates of the effect of having a Freedman's Savings Bank account on various outcomes. Each cell reports the estimated coefficient on *Has Account* derived from a separate regression; the outcome variable for each regression is indicated in its column header. "Alternative Samples to Minimize Veterans" exclude all households containing a male age 23–35 in 1870, or exclude states from which many Union Army Colored Troops were enrolled (Louisiana, Mississippi, and Tennessee), respectively. "Alternative Maximum Distance from Branch/Planned Branch" limits or extends the sample from 50 miles. "Other Alternative Geographic Samples" exclude branches that were never built, branches built in 1870, southern border states (Delaware, Kentucky, Maryland, and West Virginia), branches in locations without a Freedmen's Bureau Field Office (Baltimore, Little Rock, Andersonville, Salisbury, and Sherman), or branches with a larger number of non-Black account holders (New Orleans and Beaufort; New York and Jacksonville are already excluded from the main sample), respectively. The excluded instruments in all regressions are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county, and all regressions include controls for metropolitan area status, city population, age, sex, number of own children under age five in household, and fixed effects for the opening date of the nearest branch, relationship with household head, number of married couples in the household, and occupation. Income regressions (Column 4) also include an indicator variable for nonzero income. Real property regressions (Column 5) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1) Attended School	(2) Literate	(3) Works	(4) Income	(5) Real Property
<i>Alternative Samples to Minimize Veterans</i>					
Exclude HH with age 23–35 male <i>n</i> = 16,081	0.146*** (0.0234)	0.198** (0.0777)	0.0197 (0.0218)	0.0247*** (0.00957)	0.0288** (0.0132)
Exclude LA, MS, TN <i>n</i> = 19,098	0.109*** (0.0142)	0.0822 (0.0703)	0.0285* (0.0164)	0.0157** (0.00730)	0.0259*** (0.00892)
<i>Alternative Maximum Distance from Branch/Planned Branch</i>					
40 Miles <i>n</i> = 20,553	0.158*** (0.0201)	0.155** (0.0767)	0.0262 (0.0228)	0.0481*** (0.0107)	0.0401*** (0.0129)
60 Miles <i>n</i> = 32,821	0.141*** (0.0188)	0.123** (0.0584)	0.0185 (0.0142)	0.0414*** (0.00788)	0.0294** (0.0125)
<i>Other Alternative Geographic Samples</i>					
Built branches only <i>n</i> = 21,030	0.220*** (0.0425)	0.245** (0.114)	0.0828*** (0.0311)	0.0417* (0.0214)	0.0413** (0.0171)
Exclude 1870 Planned Branches <i>n</i> = 21,883	0.105*** (0.0107)	0.0759 (0.0572)	0.0216 (0.0156)	0.0453*** (0.00672)	0.0238*** (0.00829)
Exclude Border States <i>n</i> = 23,913	0.113*** (0.0128)	0.180*** (0.0659)	0.000382 (0.0164)	0.0522*** (0.00834)	0.0297*** (0.00739)
Exclude branches w/o Freedmen's Bureau FO <i>n</i> = 24,456	0.142*** (0.0152)	0.206*** (0.0625)	0.0261 (0.0168)	0.0495*** (0.00933)	0.0209** (0.00988)
Exclude branches w. more non-Black accountholders <i>n</i> = 25,702	0.137*** (0.0144)	0.157** (0.0640)	0.0323** (0.0151)	0.0429*** (0.00917)	0.0172 (0.0107)

Table A8: Instrumental variables estimates controlling for branch distances

This table reports IV estimates of the effect of having a Freedman's Savings Bank account on various outcomes. In contrast with the estimates presented in Table 7, we extend our main analysis sample (described in Section 1.2) by eliminating the 50-mile distance requirement. These specifications also control for the linear distance from each built branch. The excluded instruments are the distance to the nearest pre-1870 branch and an indicator for the presence of a pre-1870 branch in the county, as in Columns 3 and 6 of Table 3. "Fixed effects" are for metropolitan area status, the opening date of the nearest branch, and occupation. "Demographic controls" are city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions (panel b, Columns 3–4) also include an indicator variable for nonzero income. Real property regressions (panel b, Columns 5–6) also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by distance to the nearest branch or planned branch are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

(a) Human capital outcomes

	(1)	(2)	(3)	(4)
	Attended School	Attended School	Literate	Literate
Has Account	0.0577** (0.0269)	0.0446* (0.0244)	0.102 (0.0664)	0.0518 (0.0704)
Fixed Effects	✓	✓	✓	✓
Demographic Ctrl.		✓		✓
Observations	76,229	76,229	76,229	76,229

(b) Labor market and wealth accumulation outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Works	Works	Income	Income	Real Property	Real Property
Has Account	0.0729*** (0.0231)	0.0304* (0.0184)	0.0367*** (0.0115)	0.0470*** (0.0138)	0.0215** (0.0106)	0.0148 (0.0113)
Fixed Effects	✓	✓	✓	✓	✓	✓
Demographic Ctrl.		✓		✓		✓
Observations	76,229	76,229	76,229	76,229	76,229	76,229

Table A9: Modern trust in financial institutions and the Freedman's Savings Bank

This table reports OLS estimates of the association between the presence of a Freedman's Savings Bank branch in a county (ever) and the fraction of individuals stating that a lack of trust in financial institutions is the reason that their household is unbanked in the 2017 FDIC National Survey of Unbanked and Underbanked Households. The first column restricts the sample to Blacks, and the second column to Whites. Standard errors clustered by state are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1) Blacks	(2) Whites
Branch in County	.074* (.041)	.021 (.11)
Observations	635	1,267

Inquiries numbered 7, 16, and 17 are not to be asked in respect to infants. Inquiries numbered 11, 12, 16, 17, 18, and 20 are to be answered (if at all) merely by an affirmative mark, as /.

1870

SCHEDULE 1.—Inhabitants in Rutland Township, in the County of Montgomery, State of Kansas, enumerated by me on the 13 day of August, 1870.
 Post Office: Montgomery City Sam Thompson, Ass't Marshal. 636

1	2	3	4			7	8		10	11		13	14	15	16	17	18	19	20
			4	5	6		8	9		11	12								
		Davis C. A.	27	M	W	Keeping house													
		M. D.	10	M	W														
		E. M.	5	M	W														
		R. M.	3	M	W														
87	74	Frederic B. D.	27	M	W	Farmer		250											
		M. B.	2	M	W	Keeping house													
		H. M.	3	M	W														
		R. E.	1	M	W														
84	58	Brandy Johnson	47	M	W	Farmer		400											
		M. B.	30	M	W	Keeping house													
		Mary	11	M	W	at home													
		L. B.	4	M	W														
		Jacob	2	M	W														
55	30	James L.	43	M	W	Farmer		400											
		Margaret	39	M	W	Keeping house													
		S. J.	17	M	W	working farm													
		M. M.	16	M	W	at home													
		D. S.	13	M	W	at home													
		A. R.	11	M	W	at home													
		A. E.	10	M	W	at home													
		D. O.	8	M	W														
		L.	6	M	W														
		W. O.	4	M	W														
		A.	2	M	W														
		M. A.	2	M	W														

Figure A1: Sample 1870 U.S. decennial census record

Handwritten census records such as these underlie IPUMS's digitized sample (Ruggles et al. 2015).

Record for *Eli Jones*

Date and No. of Application, *Jan'y 30th 1867*

Name of Master, *Mrs Clark Adams*

Name of Mistress,

Plantation, *Parish of Sterville La*

Height and Complexion,

Father or Mother? Married? *not*

Name of Children,

Regiment and Company, *Stomachy in U. S. Navy*

Place of Birth, *Parish of Sterville La*

Residence, *Come Melpomene & Magnolia*

Occupation, *Dayman at Shakespeare & Foundry*

REMARKS,

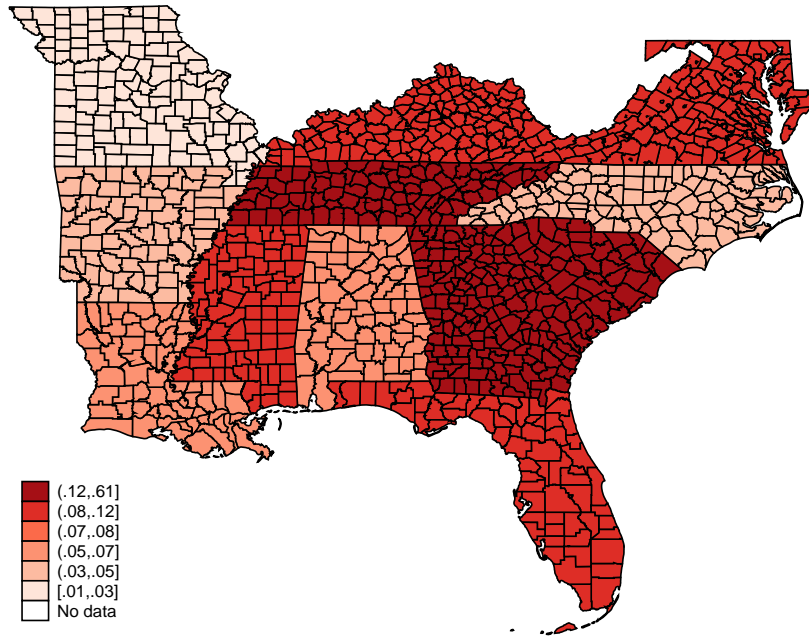
*Prother's name Caroline Washington, in city
Sister & do Susan Jones, Mathilda Washington*

No 459.

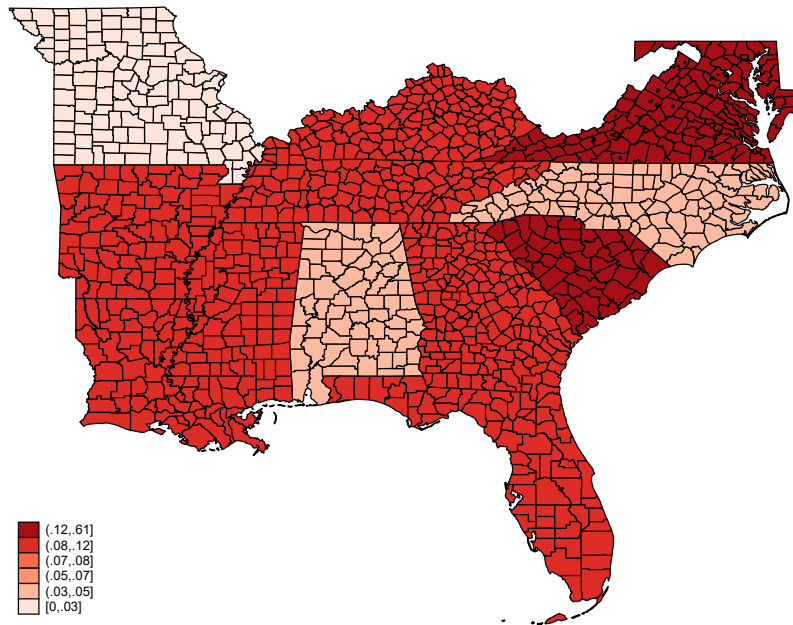
Signature, *Eli Jones*

Figure A2: Sample Freedman's Savings Bank account record

Handwritten account registers such as these underlie the digitized database available from FamilySearch (2000).



(a) Freedman's Savings Bank deposits



(b) Freedman's Savings Bank deposits: Linked data

Figure A3: Freedman's Savings Bank deposits

These maps present the share of the Black population in each state that held an account with the Freedman's Savings Bank. Panel (a) shows the number of aggregate deposits in each state reported in Osthaus (1976) divided by the number of Blacks in each state in the 1870 census. Panel (b) instead uses the fraction of account holders calculated using Census records matched to Freedman's Savings Bank records. The maps are restricted to Southern slave states that seceded during the American Civil War and border states that allowed slavery prior to 1865 but did not secede, and only include states which had a Freedman's Savings Bank branch.

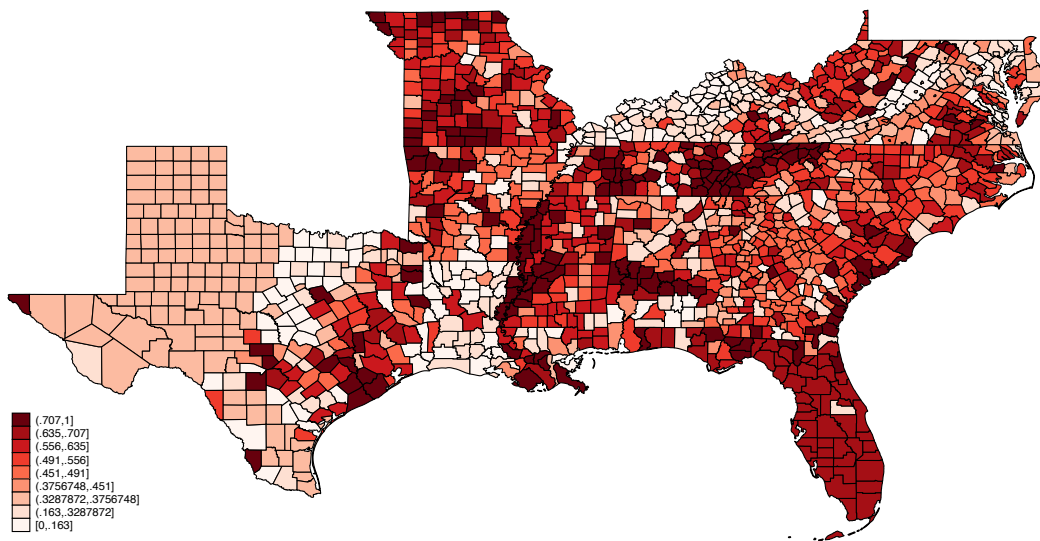


Figure A4: Republican vote share

This map shows the county-level fraction of votes for Republican congressional candidate in 1868 in the South from Clubb, Flanigan, and Zingale (2006).

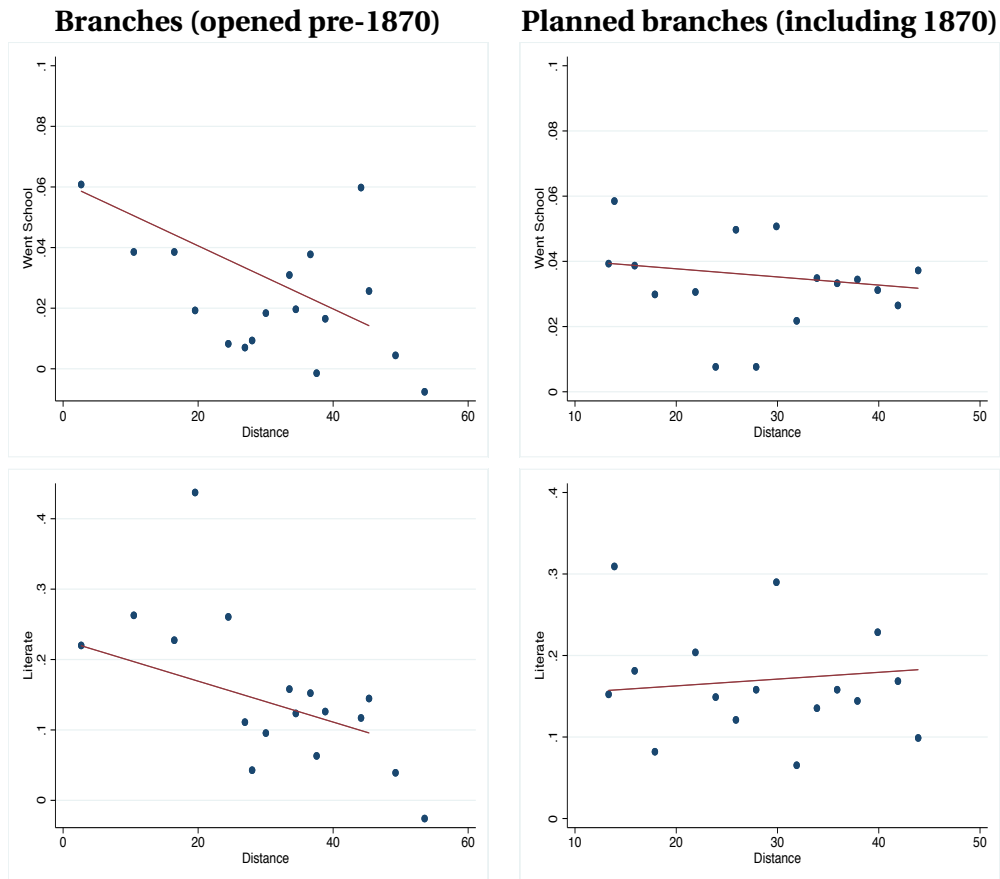


Figure A5: Human capital outcomes by branch/planned branch distance

This figure shows binned scatter plots of mean values of various outcome variables in our main analysis sample. The left-hand column plots means by distance from a Freedman’s Savings Bank branch (opened pre-1870), while the right-hand column plots means by distance from a planned branch (including those built in 1870).

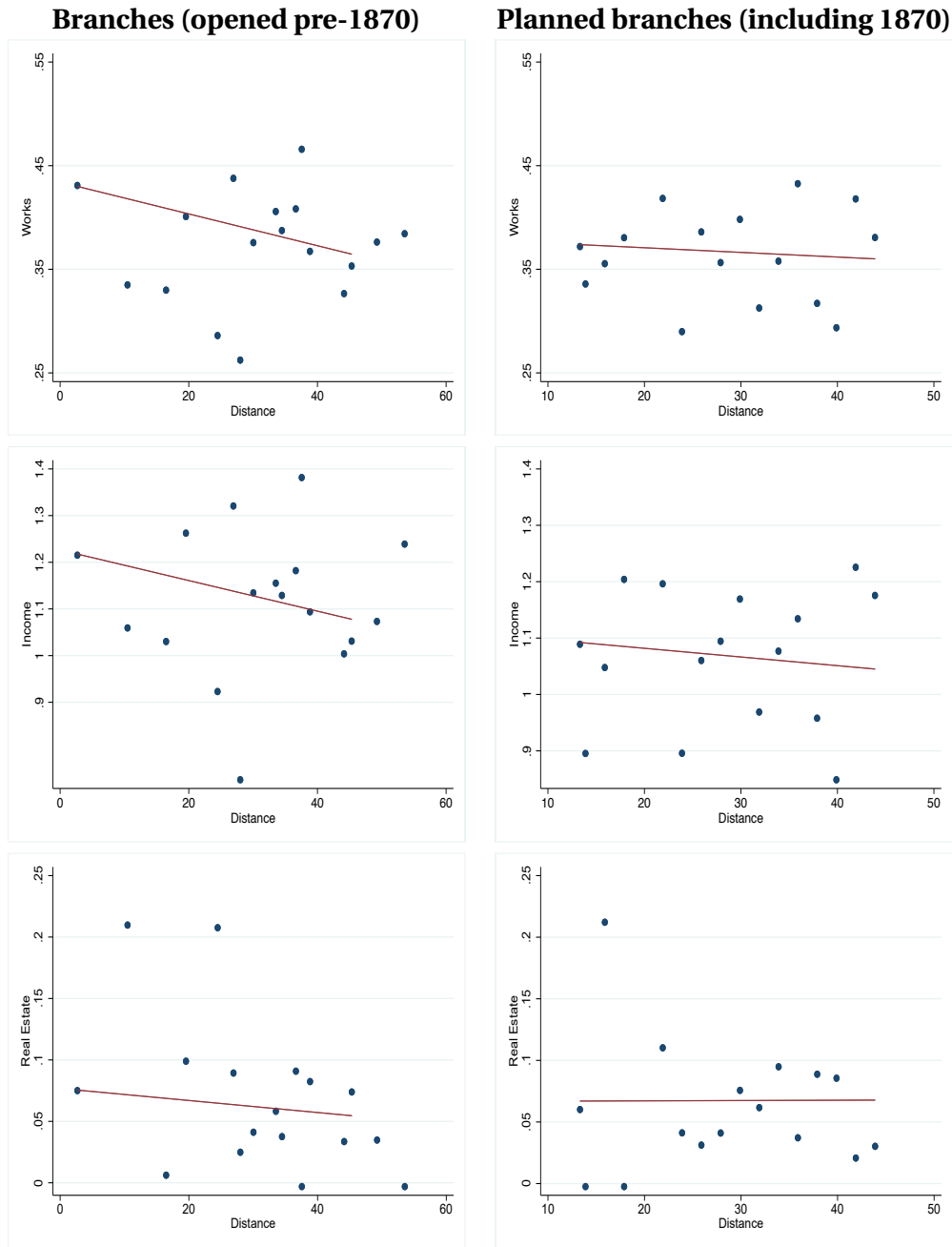


Figure A6: Labor market and wealth accumulation outcomes by branch/planned branch distance

This figure shows binned scatter plots of mean values of various outcome variables in our main analysis sample. The left-hand column plots means by distance from a Freedman's Savings Bank branch (opened pre-1870), while the right-hand column plots means by distance from a planned branch (including those built in 1870).

Republican congressional vote (1868) Republican congressional share (1868)

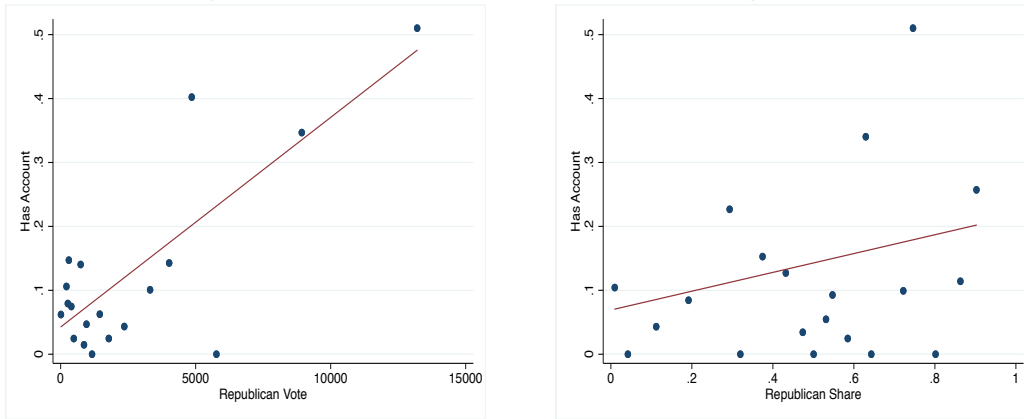


Figure A7: Freedman's Savings Bank account status by election instruments

This figure shows binned scatter plots of the fraction of Blacks in our main analysis sample (restricted to the subsample for which election variables are available) with a Freedman's Savings Bank account. The left-hand column plots means by the county-level 1868 Republican congressional vote, while the right-hand column plots means by 1868 Republican congressional vote share.

Republican congressional vote (1868) Republican congressional share (1868)

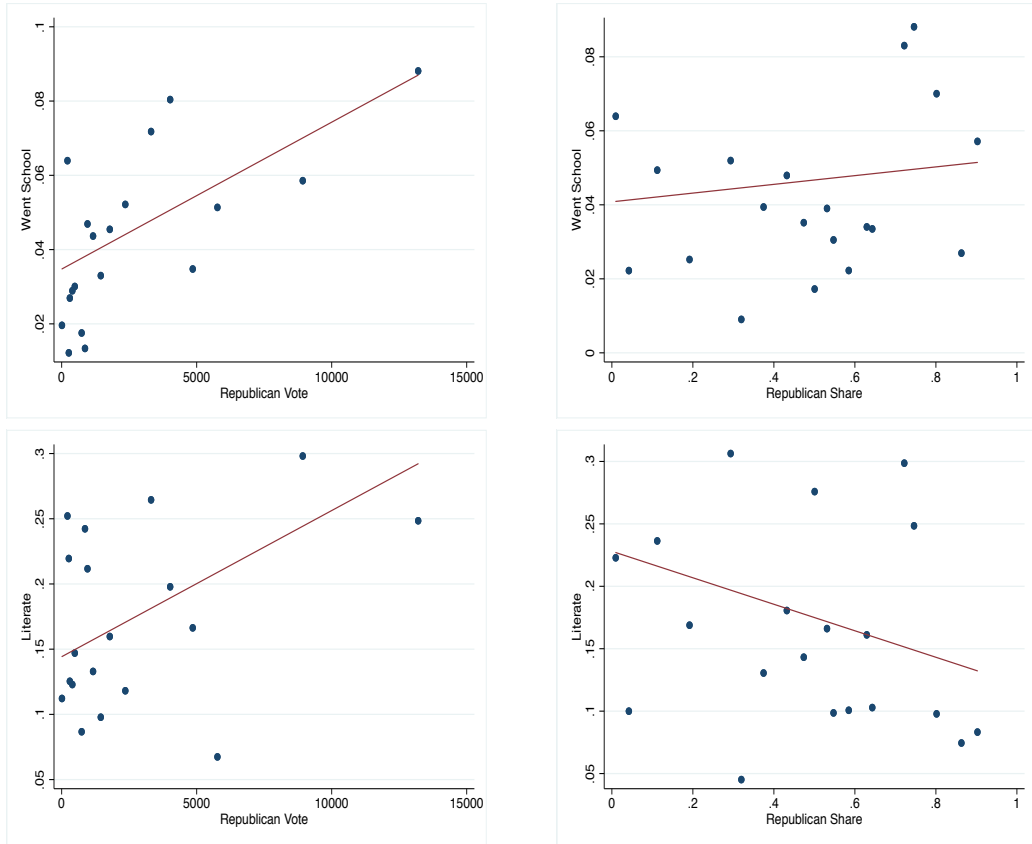


Figure A8: Human capital outcomes by election instruments

This figure shows binned scatter plots of mean values of various outcome variables in our main analysis sample (restricted to the subsample for which election variables are available). The left-hand column plots means by the county-level 1868 Republican congressional vote, while the right-hand column plots means by 1868 Republican congressional vote share.

Republican congressional vote (1868)

Republican congressional share (1868)

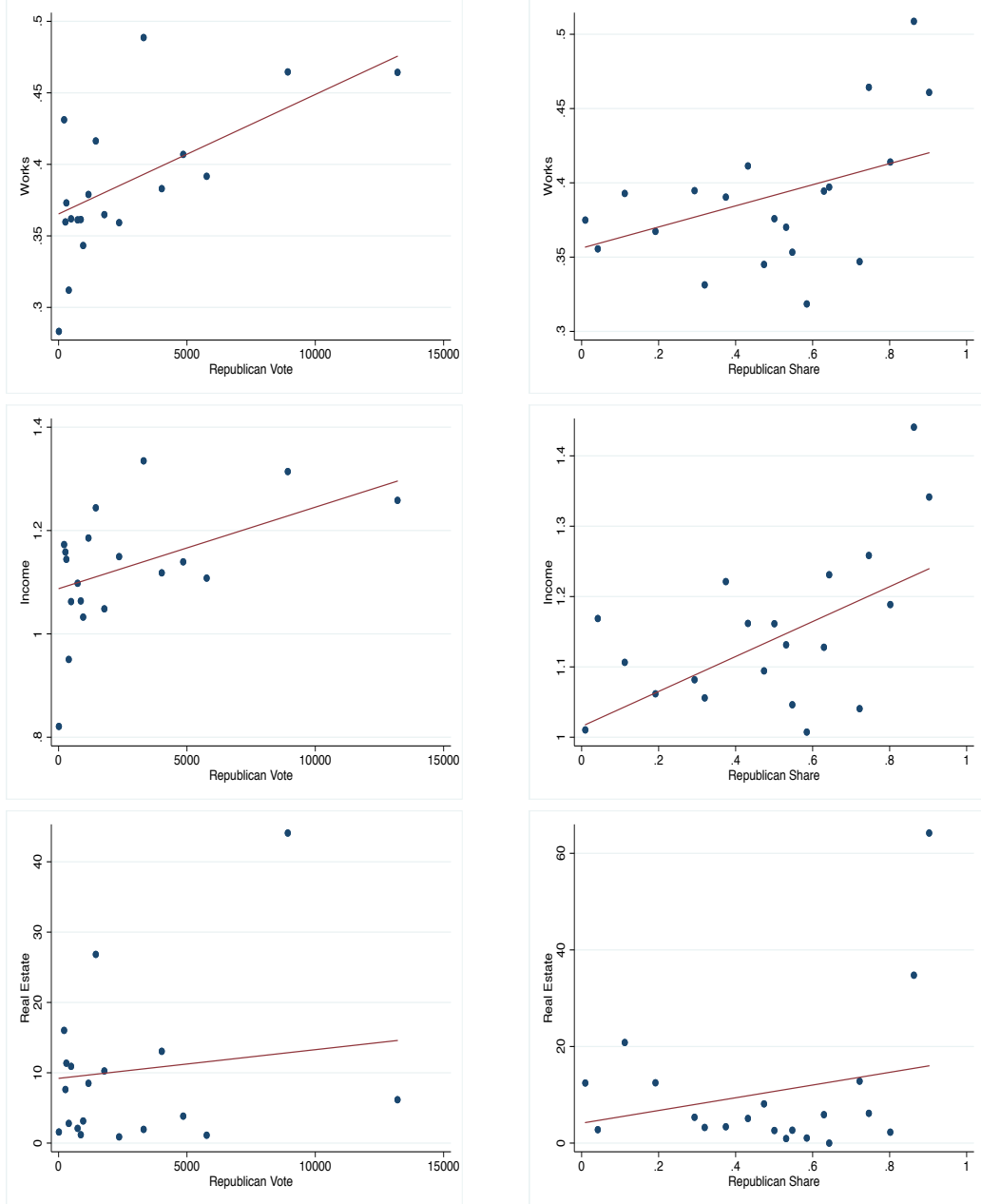
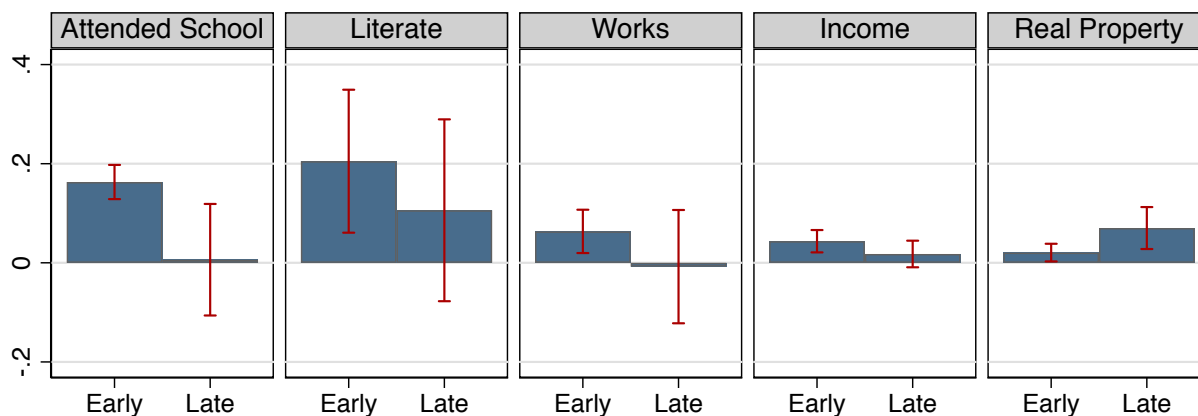
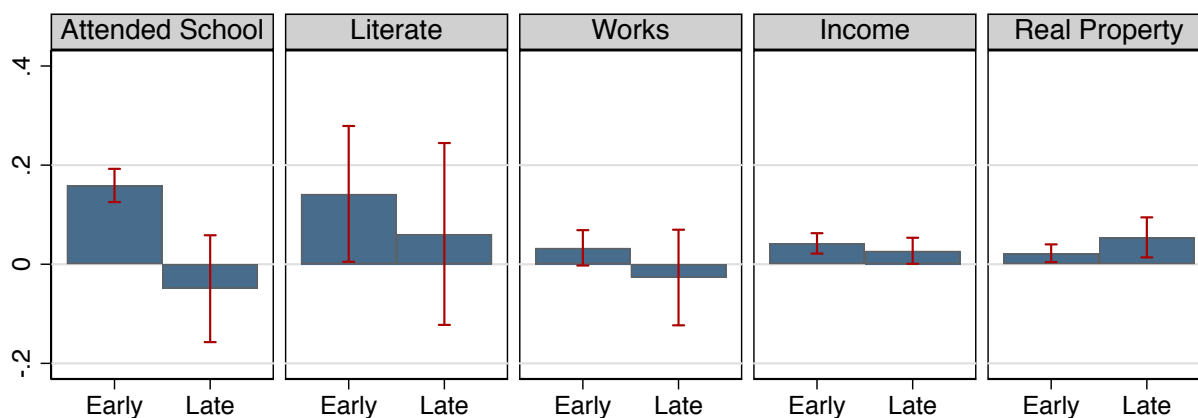


Figure A9: Labor market and wealth accumulation outcomes by election instruments

This figure shows binned scatter plots of mean values of various outcome variables in our main analysis sample (restricted to the subsample for which election variables are available). The left-hand column plots means by the county-level 1868 Republican congressional vote, while the right-hand column plots means by 1868 Republican congressional vote share.



(a) Without demographic controls



(b) With demographic controls

Figure A10: Heterogeneous effects by branch opening date

These figures illustrate the coefficients and 95% confidence interval from the estimates in Online Appendix Table A5 of the effect of having a Freedman’s Savings Bank account on various outcomes. The sample for each regression are the subset of individuals in our main analysis sample as described in Section 1.2 who live within 50 miles of a Freedman’s Savings Bank branch (“Earlier” branches opened 1865–66 or “Later” after 1866), or a planned branch. All regressions include fixed effects for metropolitan area status, the opening date of the nearest branch, and occupation; panel (b) regressions also include controls for city population, age, sex, number of own children under age five in household, and fixed effects for relationship with household head and the number of married couples in the household. Income regressions also include an indicator variable for nonzero income. Real property regressions also include an indicator variable for nonzero real property. Observations are weighted using IPUMS sample weights (PERWT).