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Life Cycle and Intergenerational Effects of Income and Wealth

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in Economics

by

Xuanyu Fu

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ABSTRACT OF THE DISSERTATION

Life Cycle and Intergenerational Effects of Income and Wealth

by

Xuanyu Fu

Doctor of Philosophy in Economics

University of California, Los Angeles, 2021

Professor Dora Luisa Costa, Co-Chair

Professor Rodrigo Ribeiro Antunes Pinto, Co-Chair

This dissertation includes three essays in economic history and applied microeconomics. In the first chapter, I investigate the intergenerational effects of a negative shock in wealth for African Americans by examining the failure of the Freedman's Bank. I find that children of depositors were more likely to be literate than children of non-depositors after the bank failure. The positive literacy effect is explained by an increase in schooling and literacy for the depositors' children prior to the bank failure. I find that the bank was able to promote education for the depositors' children through its connection with a Christian educational organization, the American Missionary Association. While children from

families who lost a higher proportion of wealth were less likely to attend school after the bank failure, the human capital gains which occurred prior to the bank failure outweigh and outlast the adverse effect of wealth loss. In the second chapter, I test whether the failure of the Freedman's Bank contributed to the mistrust and underutilization of financial institutions by African Americans today using present day survey data. I find that African Americans are less likely to be banked if they reside in a county with higher exposure to knowledge of the bank failure. In addition, for unbanked households, those who reside in a county with higher exposure to knowledge of the bank failure are more likely to report "mistrust" in bank as the primary reason to be unbanked. The results suggest that the collapse of the Freedman's Bank can partly explain persistent gaps in the utilization of financial services by African Americans. Finally, in the third chapter, I explore whether low-income individuals with a guaranteed income had a higher likelihood of occupation turnover using from a randomized experiment that occurred in the 1970s, the Manitoba Basic Annual Income Experiment. I find that guaranteed income treatment increased the probability of an occupation switch, where most of the individuals who switched were above the age of 35. From survey results, I find that occupation turnovers resulted in non-pecuniary gains for all occupation switchers. Overall, these results suggest that when relieved of financial pressure, a subset of low-income individuals were more likely to switch occupations. Those in the treatment group accrued more non-pecuniary gain simply because they switched occupations more often.

The dissertation of Xuanyu Fu is approved.

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2021

I dedicate this dissertation to my parents for their love, support and sacrifice.

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Chapter 1

Intergenerational Effects of Wealth Loss: Evidence from the Freedman's Bank

Xuanyu Fu [1], UCLA

This paper investigates the intergenerational effects of negative shock in wealth for African Americans by examining an historical episode, the failure of the Freedman's Bank (1865-1874). The bank failure resulted in the loss of deposit wealth for approximately 100,000 depositors. I collect and match individual-level bank depositor records to the 1880 and 1900 Censuses to obtain information on their descendants' literacy and occupation outcomes. To estimate the causal effect of depositing, I employ an instrumental variable strategy which exploits county-level differences in the take-up rate in banking. Children of depositors were 17.6 percentage points more likely to be literate than children of non-depositors after the bank failure. However, there is no statistically

[1]I am especially grateful to Dora Costa, Rodrigo Pinto, Adriana Lleras-Muney, Michela Giorcelli for advice and encouragement. I also thank Felipe Goncalves, Moshe Buchinsky, Martha Bailey, Bernardo Silveira, Ricardo Perez-Truglia for their comments and suggestions. This project also benefited from comments by numerous participants at the UCLA Applied Microeconomics Proseminar, UCLA Economics History Proseminar, UC Berkeley Economic History Graduate Student Research Lunch, NBER Summer Institute DAE Workshop Poster Session, EHA Poster Session, LSE Graduate Economic History Seminar, All-UC Job Market Workshop and Florida State University Applied Workshop. Financial support from the UCLA's Center of Economic History and the Lewis L. Clark Graduate Fellowship are gratefully acknowledged. All errors are my own.

significant difference in labor force participation or occupation quality between children of depositors and non-depositors. The positive literacy effect is explained by an increase in school attendance and literacy for the depositors' children prior to the bank failure. I find that the bank was able to promote education for the depositors' children through its connection with a Christian educational organization, the American Missionary Association (AMA). While children from families who lost a higher proportion of wealth were less likely to attend school after the bank failure, the human capital gains which occurred prior to the bank failure outweigh and outlast the adverse effect of wealth loss.

1.1 Introduction

As the United States experienced dramatic increases in wealth concentration over the past decades, the role of parental wealth in shaping children's outcomes has become a central policy issue. Previous research has consistently shown strong correlation between parental wealth and children's earnings and educational attainment (Charles and Hurst 2003). However, it is unclear whether such correlations reflect true causal impact of financial resources or correlated underlying characteristics (e.g., ability, social connections...). Such underlying characteristics would allow children to achieve better outcomes regardless of their parents' direct investments in them.

Determining whether perturbations in wealth have intergenerational impacts is particularly pressing for the Black community as Black children face substantially lower rates of upward mobility and higher rates of downward mobility than white children (Chetty et al. 2020). Currently, little is known about the causal impact of parental wealth on the education and labor market outcomes on African American children, mostly due to lack of data[2].

[2]Throughout this paper, I will use the terms African Americans and Black interchangeably. The context

Aside from the decennial census, which has been documenting information regarding Black families since 1870, most data sources focused on African Americans with reliable sample sizes began only in the 1980s. Although recent survey data provide valuable information, they also preclude the evaluation of long-term or intergenerational outcomes.

In this paper, I study the intergenerational effect of a negative wealth shock on schooling, literacy and labor market outcomes using an historical event – the failure of the Freedman's Bank. The Freedman's Bank, formally known as The Freedman's Savings and Trust Company, was a nationally chartered private bank established by Congress at the end of the Civil War for African Americans[3]. The bank was a simple savings institution created primarily for former enslaved people and their descendants. During its years of operation, approximately 1 in 8 Black families who lived in the vicinity of the bank held an account. While the bank was established with good intentions, it was forced to close in 1874 due to the rampant abuse of banking reserves by the upper management. What began as a well-meaning attempt to encourage savings led to significant wealth loss for the families who entrusted their savings to the bank. The average balance lost during the bank failure was around $60 for Black depositor families, which represents approximately 1/3 of annual income for a farm laborer.

To investigate the impact of the wealth loss stemming from the bank failure, I collected approximately 100,000 individual-level records from the Freedman's Bank. I first identify depositor families in the full count 1870 Census, and then conduct a follow-up of the children's education and labor market outcomes through the 1880 and 1900 Censuses. I am able to track 23.8 percent of the children observed in the 1870 Census in my sample forward to the 1880 Census and 8.4 percent forward to the 1900 Census.

of my paper is set in the late 19th Century in the United States. In that time period, the vast majority of Blacks in the U.S. are African Americans.

[3]The Civil War ended in 1865.

In order to investigate the intergenerational effect of wealth loss on children's outcomes, I begin my analysis by proxying parental wealth loss with the depositor status of a child's parents. To estimate the causal effect of depositing on the children, I have to address endogeneity concerns. Since the choice to deposit in a bank is endogenous, simple comparisons between outcomes of children whose families became depositors with outcomes of children of non-depositors confound causal effects of depositing with selection effects. For example, those who deposited might have higher ambition or abilities, both of which could be passed down to their children, resulting in higher human capital accumulation and occupation quality. To address this identification problem, I employ an instrumental variable strategy which exploits county-level differences in the take-up rate of banking. Historical accounts (Davis 2003, Josiah 2004) suggests differences in take-up rates of banking was mostly driven by localized advertisement efforts. Bank cashiers at the local branch had a lot of discretion in the ways they advertised the bank. Cashiers who conducted more successful advertisements were able to increase the local demand for banking by lowering the cost of obtaining information about the bank, and in certain cases, explicitly lowering the eligibility requirements to open an account.

Using the outcomes from the 1880 Census, I show that children of depositors were 17.6 percentage points more likely to be literate compared to children of non-depositors, six years after the bank failure. This represents a 32.7 percent increase in literacy from a mean of 54 percent. As I follow these children into adulthood in 1900, I find that children of depositors remained 25.3 percentage points more literate than children of non-depositors, which is a 34.2 percent increase from a mean of 74 percent. However, I observe no statistically significant difference in labor force participation or occupation quality between children of depositors

and non-depositors. I supplement my analysis with a matching exercise which arrives at the same conclusion, showing that the results are robust to different identification assumptions.

Despite the financial setback experienced by depositor families of the Freedman's Bank due to its failure, my results show that their children were not "worse off" compared to children of non-depositors; in fact, they became much more literate, but did not have better labor market outcomes. Canonical intergenerational wealth transmission model (e.g., Becker and Tomes 1986) suggests that a loss of parental resources should dampen human capital investment in children[4]. This suggests that depositor status is potentially masking additional effects aside from wealth loss. To explore the exact forces that drives my result, I look closely at the experience of the depositors at the Freedman's Bank.

For the families of depositors, their experience at the Freedman's Bank can be decomposed into two components: 1) bank operations prior to its failure and 2) wealth loss from the bank failure. Each of these components could potentially explain my results. First, depositors could have invested in their children's human capital prior to the bank failure, which can result in children of depositors being more literate compared to children of non-depositors. Contemporary studies have shown that access to savings accounts can lead to household improvements, such as increase in spending on education (Prina 2015) and preventative health (Dupas and Robinson 2013). I investigate whether the Freedman's Bank provided similar benefits to its depositors as modern savings institutions. Alternatively, families who experienced wealth loss could have shifted their preferences away from investments in material possession and towards investments in human capital after the wealth shock. Recent evidence has described how preferences can change in response to economic shocks

[4]Since completed years of schooling were not measured the 1880 and 1900 Censuses, I use literacy to proxy for human capital accumulation.

(Giuliano and Spilimbergo 2014, Becker et al. 2020). Hence, I analyze each component of the banking experience separately to investigate which component is driving the positive literacy effect on the children of depositors.

To analyze the effect of opening a bank account prior to the bank failure, I exploit differences in the timing of account openings. Since the bank was in operation from 1865 to 1874, I observe families who have already opened an account and families who have yet to deposit through the 1870 linked sample. This allows me to document the evolution of literacy conditions and labor market decisions of Black household before and after opening a bank account. From this exercise, I find large human capital gains from opening a bank account, for both the depositors and their children. The literacy of depositors increased by 26.7 percent after opening a bank account, and the literacy rate for children of depositors increased by 30.6 percent. In the public health literature, studies has shown that community-based venues can be used to promote preventative health care [5]. Similarly, my findings show that the Freedman's Bank, a community-based venue for the Black community, was successful in promoting education. In particular, my findings show that the bank was able to promote schooling and literacy for the depositors' children through its connection with the American Missionary Association (AMA), a Christian educational organization. Many AMA teachers concurrently served as bank cashiers, hence they were able to inform depositors of schooling opportunities for their children and encouraged them to invest more in their children's human capital. Hence, for depositors at branches with AMA affiliation, opening an account is associated with an increase in social connections and information which influenced them to invest more in their children's education. Overall, the evidence suggestive that the

[5]Victor et al. (2018) shows that health promotion by barbers can result in reduction in blood pressure for Black men in Los Angeles. Bassett et al. (2018) shows that hair salons can be a promising venue for reaching young women in sub-Saharan Africa as risk of unintended pregnancy and HIV infection.

Freedman's Bank, while it was in operation, was very beneficial for the Black depositors and their children.

To quantify the effect of wealth loss on the depositor families, I exploit variation in the proportion of wealth loss relative to their total wealth prior to bank failure. By comparing households with the same wealth level in 1870 who allocated more/less of their wealth in the Freedman's bank, I find that children from families who allocated a higher proportion of their wealth towards the bank were less likely to attend school after the bank failure[6]. In addition, I find evidence that depositor families who lost a larger proportion of wealth made other adjustments to cope with the wealth loss. Specifically, other adult household members were more likely to enter the labor force, possibly earning additional income in an attempt to offset the wealth loss.

Overall, my results suggest that parental wealth loss from the failure of the Freedman's Bank lead to decreased schooling for the children. However, I find that the bank was very successful at promoting education and literacy among the depositors and their children prior to its failure. Combining the two offsetting effects together, the human capital gains from banking while it was in operation outweigh and outlast the adverse effect of wealth loss from its failure.

The contribution of this paper is threefold. First, this paper broadly relates to the literature on the intergenerational determinants of human capital and wages. This include a large body of papers studying how human capital (Black et al 2005, Oreopoulos et al 2006), welfare receipt (Dahl et al 2014), incarceration (Norris et al. 2020, Arteaga 2020) and family composition (Olivetti and Paserman 2015), among other variables, affect the next

[6]Wealth is defined as the sum of reported personal estate wealth and real estate wealth, both were measured in the 1870 Census.

generation. More specifically, my paper is directly related to the literature examining the effect of parental wealth on children's life trajectories. The empirical studies on the causal intergenerational effect of wealth have produced mixed evidence. On one hand, Bleakley and Ferrie (2016), Cesarini et al. (2016) and Ager et al. (2019) found large parental wealth shocks have no causal impact on the children's human capital accumulation. On the other hand, Miller (2020) and Chin (2005)'s work show that shocks in parental land and property wealth can have long-term impacts on children's literacy, income and occupation[7]. In addition to contributing to the existing debate on the role of parental wealth on children's outcomes, my work adds to the literature by focusing on the African American experiences. Among the economics literature that examines the causal effect of parental wealth on children's outcomes, Miller's paper is the only one in the existing literature whose focus centers on the Black community. This paper is, to the best of my knowledge, the first study to examine the intergenerational effect of a negative shock in wealth for Blacks.

Second, this paper contributes to the literature on the benefits of providing access to financial services. Random controls trials have shown that providing access to ordinary savings account can increase expenditure in education (Prina 2015) and preventative health (Dupas and Robinson 2013). The non-experimental literature shows similar results: increasing financial access to the low-income individuals can increase income and reduce poverty (Aportela 1999, Bruhn and Love 2009, Burgess and Pande 2005). Moreover, there is a strand of public health literature that discusses the beneficial roles that community-based venues can play in encouraging good practices (Victor et al. 2018, Bassett et al. 2018). My re-

[7]Using modern data, the work by Lovenheim and co-authors find that higher housing wealth increases college enrollment (Lovenheim 2011, Lovenheim and Reynolds 2013). In addition, Bulman et al. (2020) find that moderate increase in parental wealth has no effect on the children's college attendance and very large increase in parental wealth significantly increases college attendance.

search shows that banks can serve as a venue to promote education and literacy in the local community.

Finally, this paper relates to the literature documenting the African American experience immediately after the Civil War. Many scholars have examined schooling (Collins and Margo 2006), health (Ewban 1987, Ransom and Sutch 1977, Costa 2004), migration patterns (Logan 2009), wealth accumulation (Higgs 1982, Collins and Margo 2011, Collins 2011), intergenerational mobility (Collins and Wanamaker 2020) and patenting activities (Cook 2014) of African Americans in the historical setting. In particular, this paper is related to the literature that studies the depositors of the Freedman's Bank. Two other papers utilized the unique depositor dataset from the Freedman's Bank to examine the activities of the bank prior to its failure[8]. Traweek and Wardlaw (2018) studied the depositor behavior of the bank and found that events that impact institutional trust result in changes in banking participation. Stein and Yannelis (2019) examined the effect of financial inclusion on the depositors. They found the depositors were more likely to be literate, in the labor force, have higher occupation income and real estate wealth compared to non-depositors. Using a different identification strategy[9], my study confirms the schooling and literacy finding of Stein and Yennelis. My work is the first to examine the impact of the Freedman's Bank after its failure to understand its intergenerational effects.

The remainder of the paper proceeds as follows: Section 2 provides the historical background on the creation and failure of the Freedman's Bank. Section 3 describes the individual level depositor records from the Freedman's Bank and the techniques used to link depositor

[8]Contemporaneous to the writing of this paper, Hornbeck and Keniston are also working on a similar project using data from the Freedman's Bank.

[9]Stein and Yannelis (2019) compare outcomes for individuals who live near branches built prior to 1870 with those who lived near branches built/planned to be built after 1870. They instrument for depositing status with distance from a pre-1870 branch.

data to the censuses. Section 4 presents my instrumental variable strategy used to estimate the causal effect of depositing. Section 5 presents the results. I supplement my analysis with a matching strategy in Section 6. Section 7 discusses the mechanisms. Section 8 concludes.

1.2 Historical Background

Calls began in early 1865 to establish a savings bank mainly for the benefit of Black soldiers who fought for the Union during the Civil War. The efforts were spearheaded by John W. Alvord, a Northern Congregational minister and abolitionist, and Anson M. Sperry, an army paymaster. Many Black soldiers found themselves with no safe place to deposit their back pay and bounty payments for enlisting in the army. Even though the original intention stemmed from the needs of the soldiers, Alvord believed that a permanent savings bank was necessary if Blacks were to make a successful transition from slavery to economic freedom. The effort to establish such an institution was quickly introduced in Congress and soon the bill was presented to Abraham Lincoln and signed into law. In the hope that thrift would be encouraged, the Freedman's Bank was established with altruistic motives as "a simple savings institution created for former slaves and their descendants."[10]

On August 1, 1865, the first branch was opened in Washington, DC. Subsequent branch openings soon followed in Louisville, Richmond, Nashville, Wilmington, Huntsville, Memphis, Mobile and Vicksburg. Funds from military banks established during the Civil War were transferred and merged with the Freedman's Bank. The bank was very successful at attracting depositors. Alvord and Sperry both traveled throughout the Southern states organizing branch banks and soliciting deposits (Fleming 2018). Bank officials not only scattered circulars to broadcast advertising for the bank, they also organized meetings with community

[10]U.S., *Statutes at Large*, XIII, pg 511

leaders in local churches and other institutions to create and sustain interested in the bank (Josiah 2004). Bank officials often coordinated with officials from the Freedman's Bureau to conduct deposit drives to grow the assets of the Savings Bank (Davis 2003). However, bank officials also engaged in dubious and deceitful behaviors to attract deposits. They often wore union army uniforms to collect deposits, and exploited their association with the Freedman's Bureau. Hence, many Blacks entrusted their savings to the bank believing that the bank was backed by the federal government. The early efforts of the bank agents were considered successful: 37 branches were opened and the total number of depositors reached 70,000 with $57 million in deposits within a few years. (See Figure 1.1 for a map of bank locations, and see Table 1.7 in the appendix section for a list of bank locations).

Despite the good intentions of the founders of the bank, it became susceptible to mismanagement and corruption, especially after the bank amended its charter 1870[11]. After 1870, the bank began to involve itself in risky, speculative loans and investments. Loans not backed by any security were often issued. Moreover, members of the board of trustees began to treat the Freedmen's Bank as a dumping ground for the bad private claims of themselves and friends.

Due to mismanagement, the bank was unable to weather the Panic of 1873. The Freedmen's Bank officially closed in June 1874. Even though the bank was established by Congress, it was never in actuality a government institution. The government neither guaranteed nor insured the deposits of the Freedman's Bank. When it was clear that the depositors had been defrauded, petitions came from various Southern cities calling for the government to re-

[11]The original charter of the bank required at least two-thirds of the deposits be invested in United States securities, and one-third being held on deposit as an "available fund" for current needs. The Amendment of 1870 allowed one half of the portion of deposits formerly invested in United States securities to be invested in notes and bonds if they were secured by mortgages. In addition, the amendment allowed the bank to use the reserve funds for general banking usage.

imburse the depositors for their losses since they believed that they were giving their money to the government for safekeeping. In most instances, the initial deposit included funds that had been accumulated over a number of years. (Josiah 2004). The average balance across all Black depositors was around $60. To put it in context, "In 1869 the average wage for a farm laborer was $15.50 a month, board not included" (Osthaus 1976). Thus, the losses for Black depositor families represents 1/3 of the annual income for farm laborers in that period. $60 in 1870 could also buy several acres of land, 3 heads of cattle, or 10 hogs. Hence, the average balance that was lost during the bank failure represented enough capital to start a farm, or a substantial rainy-day fund. Congress, feeling somewhat responsible for the closure, established a commission to liquidate the assets of the bank and repay the depositors. Dividends were paid out to depositors who held a valid passbook. Depositors or heirs who made claims without passbooks had to complete a questionnaire responding to the same questions that were asked of them at the time they opened their accounts with the bank. In the end, historians estimated approximately half of the depositors sent in their pass books and received 3/5 of their original deposits, while the other half received nothing (Osthaus 1976).

1.3 Data

To track the educational and labor market outcomes of Black children before and after the closure of the Freedman's Bank, I assemble a dataset that combines newly available depositor records from the Freedman's Bank with the complete-count historical censuses. The dataset is compiled in two main steps. First, I link depositor records from the Freedman's Bank to the 1870 Census which allows me to identify depositor families within the general population.

For depositors who participated in the reimbursement process, I am able to observe the amount held in their accounts at the time of the failure from bank ledger sheets. Second, I identify a sample of Black sons in the 1870 Census, noting their parents' depositor status, and locate the sons[12] in the 1880 and 1900 Censuses to examine their early-life (1880) and mid-life (1900) outcomes.

1.3.1 Registers of the Signatures of Depositors

I collected the bank's account register records through a web-scrapping exercise using *Ancestry.com*. The surviving registers come from 26 out of 37 branches[13], which covers approximately 80 percent of the universe of depositors. The register forms were filled out by the bank cashier when a depositor opened a savings account. Each record contains the account number, name of depositor and date of account opening. Each depositor could also choose to report their age, residence, birth state and names of family members. The extent to which each depositor reported these additional information varies; information like birth year, birth state and names of family members facilitate the match to the 1870 Census[14]. The web-scrapping exercise yielded 109,937 entries. From these records, I dropped the accounts of various societies and organizations. In addition, there are cases where an individual opened several accounts. Hence, I identified the duplicated accounts and kept only the first occurrence[15]. After trimming the organization accounts and possible duplicates, there

[12]I focus on sons instead of both sons and daughters because it's difficult to track daughters through censuses as they typically change their names after marriage.

[13]The branches without register records are: Alexandria VA, Chattanooga TN, Columbia TN, Houston TX, Jacksonville FL, Lynchburg VA, Macon GA, Martinsburg WV, Montgomery AL, Raleigh NC and St Louis MO. Some registers records from the Lynchburg and Raleigh branches are available, but the number of records for each of these two branches amounts to less than 50 each; therefore, I classify these branches as locations without registers. For a list of the branch locations with available register data, please refer to Table 1.7 in the appendix.

[14]Please refer to Figure 1.12 in the appendix for a sample of the register record from the Freedman's Bank that underlie the digitized database available from *Ancestry.com*.

[15]I kept the record with the earliest account opening date.

remained 81,275 individual depositor records[16].

1.3.2 Ledger Sheets from the Freedman's Bank

The Freedman's Bank maintained a series of ledger sheets which were used to track reimbursement to the depositors during the liquidation process. After the failure of the Freedman's Bank, depositors from all over the South petitioned Congress to assume the bank's assets and pay up the losses. Depositors who were able to verify their identities and produce a valid passbook were eligible to receive the reimbursement[17], and the ledger sheets were created to track each round of dividend. The original records are stored in the National Archives and I digitized the records[18]. Each individual record contains the branch location, name of the depositor, account number, the total amount held in the bank at the time of the failure, and payment of dividends. The original records are currently in the National Archives; I digitized approximately 32,000 records from the surviving files which covers 19 out of 37 branches[19]. Approximately 60 percent of depositors claimed the first dividend, and approximately 30 percent had received the full reimbursement[20], which amounted to 3/5 of their losses (Gilbert 1972).

1.3.3 Matching Depositors to the 1870 Census

To link the Freedman's Bank records to the 1870 Census, I make use of the demographic information and names of family members from the register records to facilitate the match.

[16]Osthaus (1976) and Fleming (2018) estimated that 61,131 depositors remained at the time of the failure

[17]The Comptroller was in charge of the proceedings, and sent circulars to Black ministers, postmasters and newspapers in cities where branches had been located and asked them to inform depositors to send in their passbooks in order to receive reimbursements (Gilbert 1972).

[18]The original records, "Dividend Payment Record of the Freedman's Savings and Trust Company, 1882-1889", are available to view online at the National Archives Catalog website, with HMS entry number PC 45 (46-53) 73. Please refer to Figure 1.13 in the appendix for a sample of a ledger sheet record from the Freedman's Bank.

[19]For a full list of branches with ledger sheet data, please refer to Table 1.7 in the appendix.

[20]There were a total of 5 dividends.

I use an automated iterative matching procedure that approximately follows the algorithm used by Abramitzky, Boustan, and Eriksson (2012, 2014, 2019)[21]. I not only use names, state of birth and a window for birth year as matching criteria, but I also use the names of family members as additional matching criteria[22]. The matching procedure is outlined below[23].

I first focus on the sample where a family member's name (spouse, mother, father) is available. I then follow the steps outlined below[24]:

1. Look for records in the 1870 Census that exactly match on birth state[25], and approximately matches on the name of the depositor and the name of the family member[26].
2. Discard if the initials do not match.
3. If middle name or middle initial is reported, cases where the middle initials do not match are also discarded.
4. Discard cases where only first initial is provided.
5. For depositor records with non-empty birth year, follow the iterative matching procedure used by ABE that searches for matches within +/- 3 years of reported birth year.
6. For depositor records with empty birth year, consider the observation "matched" if it is a unique match.

[21] ABE algorithm from this point forward.
[22] The ABE algorithm uses name, state of birth and a window for birth year as matching criteria.
[23] For more details on the matching algorithm, please refer to the appendix Section B.
[24] This procedure is done 4 times in total: once each for spouse name, mother name, father name, and names of both mother and father.
[25] 60,413 out of 81,275 depositor records contain birth state. If birth state is not stated, then I do not block on birth state.
[26] This means Jaro-Winkler distance of both name of depositor and name of family member must be greater than 0.9.

I then compile all matches found using the steps outlined above. In the rare cases where I observe a duplicated census record matched to a single record from the Freedman's Bank, I assume that matches found using either one of both parents' names are incorrect if a match with spouse name is available. Similarly, I assume that matches found using one of the parents' names are incorrect if a match with both parent's names is available.

For the sample of depositor records within the Freedman's Bank where I fail to find a match along with a family member's name, or if names of family members were unavailable, I follow the steps below:

1. Search for records that match exactly on birth state, and approximately match on name[27].
2. Discard observations where the initials do not match.
3. If middle initial/middle names were reported, discard observations when the middle initial/name do not match.
4. Follow the ABE algorithm, allowing the iterative procedure to search up to +/- 3 years of reported birth year.

Using this procedure, I am able to match 17,620 out of 76,530[28] available records to the 1870 Census. The match rate, which is around 23 percent, is comparable to other studies that perform automated-record linkages using historical data in the nineteenth century (Costa 2010, Bleakley and Ferrie, 2016; Salisbury, 2017; Eli, Salisbury and Shertzer, 2018; Ager, et al. 2019).

[27]Jaro-Winkler distance greater than 0.9

[28]Among the 81,275 depositor records, I trim the sample to exclude depositors who indicated a non-Southern birth state.

1.3.4 Summary Statistics

Table 1.1 presents summary statistics based on the 1870 full count Census. In this table, I compared the characteristics of depositor families to the average family who resided in the South. Overall, the summary statistics show that Black depositor families were positively selected from the general Black population. Out of the 17,362 depositors who were successfully matched to the 1870 Census, 70.6 percent of the depositors were Black. Black families who chose to open an account at the Freedman's Bank held more wealth and were more literate than the average Black family. In particular, 10.4 percent of depositor families held real estate versus 5 percent in the general Black population. The rate of illiteracy for heads of depositor households was 70.5 percent while the rate of illiteracy among the heads of non-depositor households was 80.3 percent. In addition, Black depositor families were far more likely to live in close vicinity to the bank. While 15.6 percent of the general population lived in a county with banks, the linked sample shows that approximately 50 percent of the depositors resided in a county with a bank. Due to the fact that banks were located in urban counties and the majority of depositor families resided close to the bank, members from depositor families had more opportunities to enter occupations beyond agriculture: 46.5 percent of head of depositor families were farmers compared to 63.8 percent in the general Black population.

Table 1.1 also compares the characteristics of white depositor families to the average white family who resided in the South. White depositors were also slightly positively selected from the general population. For instance, 56.3 percent of white depositors held real estate versus 50.9 percent of white families in the South. However, white depositor families were not significantly more likely to reside in the vicinity of a bank. Only 17.9 percent of white

depositor families lived in a county with a bank. Because the majority of white depositor families lived in non-urban areas, 62.2 percent of heads of white depositor families were farmers compared to 57.7 percent in the overall white population.

The automated matching procedure I use to link the records from the Freedman's Bank to the 1870 Census relies heavily on using names of family members as a matching criteria. Depositors reported their demographic information and names of their family members to the bank cashier as they opened an account. In the register records, I observe large variation in the extent to which each depositor reported their personal information. Since I rely on a matching algorithm which uses names of co-residents as matching criteria, I am much more likely to locate depositors who accurately reported the names of his/her family members as they opened a bank account. Table 1.8 in the appendix shows the percentage of depositors who reported the names of their family members for the following two groups: those I was able to locate in the 1870 Census and for those that could not be located. Depositors who were successfully matched to the 1870 Census were 17 percentage point more likely to report the names of their spouse compare to their unmatched counterparts. Similarly, depositors who I can identify in the 1870 Census were more than 20 percentage points more likely to report the names of their parents.

Even though I am better able to link depositors who reported more personal information to the 1870 Census, it's unclear whether the linked depositors were positively selected compared to the unlinked sample. For the sample of depositors for which I can observe the amount held in their bank at the time of the failure, the depositors linked to the 1870 Census had 7 dollars less in their account compared to the unlinked depositors, with the difference being statistically significant (Table 1.9 in the appendix) . Some of the difference

could be explained by reporting habits. Table 1.9 in the appendix show that for depositors who reported the name of their spouse, those who were linked to the 1870 Census held 3 dollars less than their unlinked counterparts, with the difference being statistically insignificant. Across different types of reporting pattern for the depositors, depositors matched to the 1870 Census held less in their account than those not matched to the census[29]. However, this does not mean that depositors matched to the 1870 were comparatively less wealthy than the depositors not matched to the census. First, it's possible that depositors linked to the 1870 Census held less in their bank account, but have more wealth overall. In addition, depositors linked to the 1870 Census were more likely to report their age consistently across the bank records and the census, which can imply higher literacy rate. Overall, the depositors who were linked to the 1870 Census reported more information about themselves, but it's difficult to judge whether these depositors came from a higher socio-economic status than their unlinked counterparts.

1.3.5 Soldier Depositors

Since concern for the Black soldiers has first stimulated interest in a savings bank for African Americans, it is not surprising that many soldiers deposited their pay and bounty in the Freedman's Bank. Beginning in 1863, the United States Army set up regiments composed primarily of African Americans soldiers. The majority of recruits come from areas in the South which were reached by the Union Army in 1863-1864. Black soldiers were paid $10 a month, and were entitled to a bounty of $100 towards the close of the war (Ford 1933). Hence, soldiers who served in the Union Army were able to accumulate one or two

[29]Although Table 1.9 in the appendix shows that for many types of reporting pattern, the difference in the amount held in bank deposit between the matched sample and the unmatched sample is statistically insignificant.

hundred dollars, or even more at the close of the war[30] I examine the characteristics of Black soldiers, and Black soldier depositors in particular, using the United States Colored Troops (USCT) sample compiled by the Early Indicators Project[31]. The USCT sample was selected by extracting names from Regimental Books from randomly selected companies from the complete list of USCT regiments and companies. Hence, this is a representative sample of all 178,000 soldiers from served in the USCT. Based on characteristics taken from the 1870 Census, families of Black veterans were wealthier and more literate compared to the average Black family who resided in the South (Table 1.10 in the appendix). However, the family characteristics of veteran depositors were similar to that of the average depositor[32].

With higher disposable wealth and targeting from bank officials, I find that Black veteran families were almost twice as likely to open an account at the Freedman's Bank compared to other Black families (Table 1.11 in the appendix). I identify 5 percent of Black families who resided in a county with a Freedman's Bank branch to be depositors. For Black veteran families who resided in a county with a branch, I identified 7 percent of these families as depositors[33]. The overall pattern that exists the data points to the prevalence of soldiers among depositors, and this may explain some of the differences observed between depositor families and non-depositor families at baseline shown in Table 1.1. Since a portion of veterans were entitled to pension[34], I will include veteran status of one's father as one of the many family characteristics controls[35].

[30]Some Black veterans also received a pension after the war.

[31]The USCT sample include the Original USCT Sample (NIA P01 AG10120, PI: Fogel) and the Expanded USCT Sample(NIA P01 AG10120, PI: Costa). Both samples were linked to the 1870 Census.

[32]One notable difference is that veteran depositors were less likely to be illiterate compared to the average depositor.

[33]These numbers are likely to be lower bounds due to the the fact that I wa able to link 23 percent of the depositors to the 1870 Census.

[34]If veteran was deceased, their widow and children were entitled to pension

[35]This control is imperfect since the USCT sample from the Early Indicators Project is not a dataset that contains all records of those who served in the USCT.

1.3.6 Matching Sons to the 1880 and 1900 Censuses

My main linked sample is created by matching all the Black sons who resided in a county with surviving bank register data, to the censuses of 1880 and 1900. Using the 1870 full count Census, I identified 73,637 Black sons between the age of 0 and 11 who resided in the counties with banks in the U.S. south[36]. In the 1870 Census, I identified 3648 out of 73,637 children (5 percent) to have come from a family who deposited at the Freedman's Bank. To link the sons from the 1870 Census to the 1880 Census, I matched by name, age, state of birth and names of family members[37], using a similar revised ABE algorithm as described in the last section[38]. I matched 17,503 sons forward to the 1880 Census, and 1214 of them (7 percent) were sons of depositors. To locate the sons from the 1870 Census in the 1900 Census, I simply follow the ABE algorithm and conduct matches using first name, last name, age and state of birth[39]. I was able to match 6202 out of 73,637 sons to the 1900 Census, where I was able to locate 385 sons (6 percent) from depositor families.

My match rate from the 1870 Census to the 1880 Census is around 23.8 percent, and my match rate from the 1870 Census to the 1900 Census is around 8.4 percent. The 1870-1880 inter-census match rate of 23.8 percent is comparable to other studies that utilized census-based linking in the nineteenth century as mentioned in the last section. The 1870-1900 inter-census match rate, at 8.4 percent, is much lower than the standard, but is comparable to other studies (Collins and Wanamaker 2020) who conducted inter-census linkage in the

[36]Southern states are defined to include: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virgina and the District of Columbia.

[37]The children were aged 10-21 in 1880, and many were still residing in the same household as their parents.

[38]For more details on the matching algorithm, please refer to Section C of the appendix

[39]The children were aged 30-41 in 1900. To locate sons in the 1900 Census, I do not match using names of family members since they likely do not reside in the same households as they did in 1870. For additional details on the matching algorithm, please refer to Section D of the appendix

late 19th century[40]. In my linkage exercises[41], I was able to achieve the standard match rate of over 20 percent by supplementing the name-age-birth state combination linkage typically used by other studies with names of other household members (names of spouses and names of parents)[42]. There are several potential factors contributing to the lower than standard match rate observed in my linked 1870-1900 sample[43]. First, many African Americans changed names after emancipation as a way to forge a new identity (Litwack 1980, Costa and Kahn 2006). Inter-census linkage would fail if a son adopted either a new first or last name. Second, since my sample comprises of sons who resided in urban areas in the South, high mortality rates among Blacks, particularly in the cities, could cause the lower match rate (Costa 2004). Lastly, potential age misreporting is prevalent in my sample. The low literacy rate among African Americans in 1870 could have contributed to inaccurate age reporting (Hussey and Elo 2010, Hill et al. 1997) and age heaping (Mason and Cope 1987).

One concern with census linking is that unique matches are more likely to be made between two census points for sons with uncommon names or who were able to report accurate age. In addition, the matching algorithm that uses names of family members as an additional criterion is more likely to match individuals living with the same family members 10 years later. Sons with these characteristics may have higher socio-economic status than the general population. Table 1.1 shows that depositor families are positively selected, hence we are able to link a higher proportion of sons from depositor families. Table 1.12 in the appendix compares the 1870 Census family characteristics of all sons to the 1870 Census

[40]Collins and Wanamaker (2020) matched 9 percent of the African American population in 1880 forward to the 1900 Census.

[41]In particular, I am referring the two linkage exercises conducted in this paper: 1) Freedman's Bank register data linked to the 1870 Census and 2) 1870 Census linked to the 1880 Census.

[42]If I link the 1870 Census forward to the 1880 Census based solely on name, age and birth state, the match rate is around 14 percent.

[43]Most other studies conducting census-based linkage in the late nineteenth century focus on the white population, both native and immigrant.

family characteristics of sons who were successfully matched to the 1880 and 1900 Censuses. Sons in the linked sample were wealthier, more literate and less likely to be farmers in 1870 compared to sons whom I was unable to link. To improve external validity to the full population, the main results are re-weighted by these baseline characteristics. Table 1.13 in the appendix demonstrates that the re-weighting procedure substantially balances the matched sample with the unmatched segments of the population[44].

1.4 Identification Strategy

In order to investigate the intergenerational effect of wealth loss, I begin my analysis by proxying parental wealth loss with the depositor status of a child's parents. I compare the outcomes of children of depositors with children of non-depositors. To estimate the mean difference in these outcomes, I specify the following statistical model:

$$Y_{ibc} = \beta_1 + \beta_2 * Freedman_i + \beta_3 X_{ic} + \gamma_b + \epsilon_{ibc} \tag{1.1}$$

The indicator variable $Freedman_i = 1$ if the child is in a depositor family at the Freedman's Bank, and $= 0$ otherwise. In this specification, I compare the outcomes of children i, who resided in county c in 1870, within the same birth cohort b. In addition, I also control for a vector of observable family-level and county-level characteristics measured in 1870 denoted by X_{ic}. The vector of family-level characteristics include literacy of household head, dummies that control for the household head occupation category, veteran status, mix-raced status[45], family size dummies and wealth quartile dummies. I also control for county-level

[44]The weights used are based on the propensity of being matched $P_i(M_i = 1 \mid X_i)$, which is calculated from a probit of match status on covariates (e.g., literacy status, farm status). Observations are re-weighted by $(1 - P_i(M_i = 1 \mid X_i))/P_i(M_i = 1 \mid X_i) * q(1 - q)$, where q is the proportion of records linked.

[45]Throughout the paper, I will also utilize the term "mulatto" to refer to individuals who are mix-raced. Mix-raced or mulatto status serves as a proxy to control for the possibility that an individual was freed prior

characteristics, such as average farm output, average value of farm output and percent of the local black population who are farmers. Standard errors are clustered at the 1870 residential county level.

In 1880, when the children were of ages 10 to 21, the outcomes I am interested in are school attendance, literacy and labor force participation. A child is considered to have attended school if he enrolled in any type of school, excluding Sunday or evening schools, for the past year. A child is literate if he reported to be able to read or write.[46]. Finally, labor force is defined as reporting any gainful occupation. The historical definition for labor force participation[47] differs quite a bit from the modern definition[48]. In 1880, census enumerators reported occupations for each person ages 10 and above. Hence, children who earned money "regularly by labor, contributing to the family support, or appreciably assisting in mechanical or agricultural industry" were to be considered in the labor force and were to be reported with an occupation. In 1900, when the children were of ages 30 to 41, I am interested in observing their literacy, labor force participation and measurements of occupation prestige. Literacy and labor force participation measured in the 1900 Census are defined in the same manner as the 1880 Census. I define a new indicator variable to measure occupation prestige in 1900, which equals 1 if an individual is in an occupation that earns more than a "laborer". I construct this variable to measure occupation prestige because African Americans tend to concentrate in a few occupations in that time period. Hence, census reported variables that are commonly used to measure occupation prestige, such as occupation score, offers little

to emancipation.

[46]In 1880, census enumerators reported one's ability to read and write for all individuals ages 10 and above.

[47]The historical definition for labor force participation is valid for all censuses prior to 1940.

[48]For censuses taken after 1940, one is considered to be in the labor force if one is currently working, seeking work or has a job from which one is temporarily absent in a specific reference week.

variation.

The ordinary least-squares estimates of β_2 outlined by Equation (1) likely suffer from three sources of bias. The first is selection bias. The decision to become a depositor is likely endogenous – driven by unobservable information that could also influence a child's outcomes. For example, those who deposited might have higher ambition or abilities, both of which could be passed down to their children, biasing the estimates β_2 upwards. The second source of bias comes from the fact that the location of these bank branches was not chosen at random. Table 1.14 in the appendix shows that counties with a Freedman's Bank were more urban and had more Black residents than the typical Southern county based on 1870 county-level characteristics[49]. Since the majority of depositors live in close vicinity of a Freedman's Bank, the children of depositors likely had more schooling opportunities and were more able to enter a non-farming occupation compared to other Black children living in the South. This would also produce an upward bias in β_2. The third source of bias results from measurement error. Factors like the widespread use of first initials and the prevalence of common names on both census manuscripts and register data from the Freedman's Bank can lead to transcription errors in the digitization process. Hence, the depositor status of one's parents, $Freedman_i$, is measured with error. This will attenuate the estimate β_2 towards zero.

1.4.1 Instrumental Variable Approach

I propose an instrumental variable strategy in which I instrument for $Freedman_{ic}$ with the county level take-up rate in banking, denoted as $TakeUpRate_c$. The county-level take-up rate in banking is defined as half the total number of records within each branch register

[49] 1870 county-level characteristics are from the *Historical, Demographic, Economic, and Social Data: The United States, 1790-2002* (ICPSR 2896).

divided by the county Black population in 1870[50]. Since branches were chosen in cities that were comparatively far more urban, and had more Black residents than the average Southern city, I will limit my sample to counties with established banks in which register records are available, and exploit cross-county variation in the take-up rate.

$$TakeUpRate_c = \frac{\text{total number of register records within each branch * 0.5}}{\text{total county Black population in 1870}}$$

To formalize the instrumental variable approach, I would like to estimate it via two-stage least squares (2SLS) where the following first-stage equation is:

$$Freedman_{ic} = \alpha + \zeta_1 * TakeUpRate_c + \zeta_2 \mathbf{X_i} + \epsilon_{ic} \tag{1.2}$$

X_i includes family-level and county-level characteristics measured in 1870, including wealth quartile dummies, family head occupation dummies, mulatto status. family size, family head literacy status, percent of the black population who were farmers, average farm output and average value of farm output. Then, I use the family's likelihood of holding an account – predicted using the fitted value from estimation of Equation (2) – to predict outcomes of interest:

$$Y_{ibc} = \gamma + \beta_1 * \widehat{Freedman}_{ic} + \beta_2 \mathbf{X_i} + \delta_b + \eta_{ibc} \tag{1.3}$$

For the take-up rate to be a valid instrument, the first identification assumption requires the county-level take-up rate to strongly predict a family's propensity to bank, conditioned on other family-level observables. I see this pattern empirically in the first-stage results presented in Table 1.2: a 1 percentage point increase in the take-up rate increases a family's propensity to open an account by approximately 0.6-0.7 percentage points. The second identification assumption of using $TakeUpRate_c$ requires the exclusion restriction to hold,

[50]I multiply the total number of register records within each branch by 0.5 because approximately 50 percent of depositors live within the county boundary where the bank was located (see Table 1.1).

which states that the county-level take-up rate does not directly impact the outcomes of the children and the influence of the take-up rate on children's outcomes operate only through the decision of the family to open a bank account.

The two identification assumptions stated above are reasonable in this context because qualitative evidence suggests that the variation mainly arises from the persuasiveness and advertisement ability of the local branch officers. Local bank cashiers, and members of the advisory boards were primarily in charge of growing the deposits of their branch. Cashiers would not only arrange for local residents to advertise their branches[51], but they also often held large public meetings at local churches to spread the virtues of the bank. Moreover, bank officers supplemented public meetings with pamphlets, circulars and advertisements in Black newspapers to publicize their activities. The cashier at the Washington, DC, branch, William J. Wilson, was said to have "increased his branch's business by personal persuasion and by making speeches and holding meetings in the Negro community. He designed circulars, secured the endorsement of prominent Negro leaders and instituted a ten cent and twenty-five cent savings program for those who could not open an account with the minimum five-dollar deposit" (Osthaus 1976). The promotional methods used by the this bank cashier are similar to modern marketing stretagies that have shown to be successful in encouraging individuals in developing nations to open a bank account (Dupas et al. 2016). Hence, branches that conducted more successful advertisement campaigns were able to increase the local demand for banking by lowering the cost of obtaining information about the branch, and in some cases, enacted policies that explicitly lowered the eligibility requirements to open a bank account.

[51]The bank officers would occasionally pay those who cooperated in advertisement a small honorarium.

One might assume counties that lack alternative banking institutions might exhibit a higher take-up rate. If such a correlation holds, the availability of banking institutions can positively impact a location's growth (Fulford 2015), posing a threat to the exclusion restriction. In Panel A of Figure 1.2, I show that the take-up rate is uncorrelated with the availability of other banks within the county. One potential reason for this seemingly counter-intuitive pattern might be due to discrimination against and legal obstacles faced by Blacks in using other banks throughout the South. One might assume that more prosperous and urban counties have a larger pool of residents with savings, hence a higher take-up rate. If such a correlation exists, the prosperity of a location would affect both the take-up rate of banking and children's outcomes, violating the exclusion restriction. In Panel A of Figure 1.2, I show that the take-up rate is uncorrelated with general demographic variables and measures of urban-ness[52]. In Panel B of Figure 1.2, I show the correlation between take-up rate and county-level characteristics specific to the Black population. While I find that the take-up rate is uncorrelated with the majority of variables, I find that the take-up rate is related to the percentage of the local black population who are farmers (Panel B of Figure 1.2). Counties where a larger percentage of the local black population are farmers have a lower take-up rate. In light of this fact, I will include county-level characteristics that relates to farming, including, percent of the local black population who are farmers, average farm output and average value of farm output, as control variables in the 2SLS estimation.

[52]In Table 1.15 in the appendix, I regress a full set of 1870 county-level characteristics against the take-up rate.

1.5 Results

I begin by comparing the 1880 outcomes of children from depositor families and children from non-depositor families. The children were between the age of 10 to 21 in 1880 and many of them still resided in the same household as their parents. In particular, I investigate the following outcomes: school attendance, literacy and labor force participation. In Panel A of Table 1.3, I report the OLS estimates of Equation (1). The results show that compared to children of non-depositors, sons of depositors were 3.8 percentage points more likely to attend school, 8.5 percentage points more likely to be literate and 5.6 percentage points less likely to enter the labor force.

I then compare the 1900 outcomes of children from depositor families and children from non-depositor families. By 1900, the children have advanced into their mid-life, aged 30 to 41. I consider the following outcomes in mid-life: literacy, labor force participation and occupation quality[53]. Panel B of Table 1.3 reports the OLS estimates; the results show no statistically significant difference in labor force participation and occupation quality between children of depositors and non-depositors. However, children of depositors were 4.9 percentage points more likely to be literate.

The OLS estimates suffer from potential biases as described previously, hence I report the two-stage least squares (2SLS) estimates in Panel A of Table 1.4 for outcomes in 1880 using the instrumental strategy described in Section 5.1. The estimated coefficient in columns one and two indicates that children of depositors of the Freedman's Bank were not more (or less) likely to attend school[54], or participate in the labor force compared to children of

[53]Occupation quality is a dummy variable that equals 1 if one is in an occupation with an occupation score greater than a laborer. I choose this measurement as opposed to occupation score because many Blacks held the same occupation. Hence, occupation score offers little variance.

[54]The estimated coefficient is positive, but not statistically significant.

non-depositors in 1880, six years after the bank failure. However, children of depositors were 17.6 percentage points more likely to be literate compared to children of non-depositors, with the difference being statistically significant. Considering that the 54 percent of the Black population between the ages of 10 and 21 in 1880 was literate, this effect is economically significant and represents a 32.6 percent increase in literacy for children of depositors.

Does the positive literacy effect for the children of depositors persist into mid-life? In Panel B of Table 1.4, I turn to outcomes measured in 1900. I observe no statistically significant differences in labor force participation or occupation quality between children of account holders and non-account holders. But for literacy, the children of depositors were 25.3 percentage points more likely to be literate compared to children of non-depositors. This represents a 34.2 percent increase, from a mean literacy rate of 74 percent.

In general, the IV point estimates imply larger effects than the OLS estimates. The household's depositor status at the Freedman's Bank is error-ridden. Measurement error would attenuate the OLS, but not the IV estimates. In my context, the issue of attenuation bias is severe enough that it outweighs the selection bias that in theory, would have bias the OLS estimates upwards. Hence, the overall results suggest that despite the financial setback experienced by families of depositors of the Freedman's Bank due to its failure, their children were not "worse off" compared to children of non-depositors; in fact, they were much more literate. The results appear to be counterintuitive; and indeed, the canonical intergenerational wealth transmission model (e.g., Becker and Tomes 1986) suggests that a loss of parental resources should dampen investment in children. In Section 7, I explore possible mechanisms which can explain my findings.

1.6 Propensity Score Matching

To complement my instrumental variable strategy which exploits cross-county-level variation in the take-up rate of banking, I also conduct a matching exercise that allows me to create a comparison group of households with similar pre-failure characteristics as the treatment group.

One way to address the selection bias and the location bias is to first model the family's decision to select into banking based on some observable characteristics, then choose a non-depositor comparison family who resided in the same city prior to the bank failure. But my dataset precludes this type of analysis. Since I was unable to match a large percentage[55] of depositors from the bank registrar data to the 1870 Census, it's very likely that the matching algorithm will choose a "control" family who has actually deposited. So to alleviate measurement error bias that could attenuate my results, I focus on choosing "control" families who resided in a county without a bank.

Therefore, my preferred empirical approach is to first identify counties that are reasonably similar to the banked counties prior to the closure. I exploit the fact that the bank had planned branch openings that never materialized due to financial pressure and its ultimate collapse (see Figure 1.6 in the appendix for location of planned banks). While the counties with planned bank openings were more urban, and had a higher African-American population than the average Southern county based on 1870 characteristics (see Table 1.16 in the appendix), they were less urban than counties with banks[56]. I trim the sample so that the set of counties with banks looks observationally similar to counties with planned bank

[55] I was able to match 23 percent of depositors to the Census.

[56] Less urban meaning lower population density, total assessed wealth, number of manufacturing plants, but similar in demographic characteristics like percent Black.

openings in 1870[57]. In addition, I show that within the trimmed sample, the counties with banks are on a similar growth path as counties with planned openings (see Table 1.17 in the appendix).

Within the trimmed banked sample, I conduct propensity score matching. I define my treatment group as families who selected into banking from the trimmed sample. Then I use propensity score matching to pair up each "treated" family with a family with very similar observable characteristics pre-bank failure who resided in a county with planned opening. The propensity score, $p(X) = P(B = 1 \mid X)$, is the probability of becoming a depositor conditional on X estimated from a logit model. I will describe the variables in X in the following section. Finally, I choose the control group families by applying the nearest neighbor matching method, which matches depositor families in the trimmed banked sample with families in counties with planned bank openings with the closest propensity scores.

1.6.1 Propensity Score Matching

To calculate the propensity score, the following variables are used as regressors in the logit model: family size, number of children under 5, number of children under 16, age of head of family, literacy of the head of family, family wealth quartile dummies, dummy for farmers and dummy for mulatto status. In addition, to ensure that control families are picked from counties that are observationally similar, I include county population density, and county percent Black as regressors. All variables are measured in 1870.

Table 1.18 in the appendix reports the estimation results of the logit model both in the

[57]Henceforth, I refer to this sample as the trimmed sample. Because the counties with banks had more Black residents in 1870 than counties with planned bank openings, I dropped counties where the number of African-American residents in 1870 is fewer than 5000 (these are banks with planned openings), and greater than 35000(these are counties with banks) to produce a more observationally similar set of counties. Lastly, since counties with banks still appear to be a bit more urban, I dropped counties where population density is greater than 100.

matched 1870-1880 sample and in the matched 1870-1900 sample. Although the magnitudes of the coefficients differ between the two samples, the sign and the significance level largely remain the same. The coefficient estimates suggest that family size and wealth both significantly increase a family's propensity to bank. In addition, the head of the household being illiterate, the head of household being a farmer and having more children in the household significantly decreases a family's propensity to open a bank account, indicating that more well-off Black families select into banking.

For each depositor family, I choose one (without replacement) family in a county with a planned bank whose fitted value in the logit model is the most similar to that of the depositor family. I would like the depositor families and the chosen control family to have characteristics that are as similar as possible prior to the bank failure. Table 1.19 in the appendix presents the results for the balance tests of matching covariates. The second and third columns report, respectively, the means of covariates for depositor families and the means for the corresponding non-depositor families based on the estimated logit model. Column four displays the difference (in percentage) between the two group means. The means of all covariates are very similar between the treatment group and the control group: the differences are less than 10 percent in most cases[58]. Column five reports the p-value from t-tests; the results indicate that the differences in the means of the treatment group and the control group are not statistically different from zero at the conventional significants levels. These results show that depositor families and the matched non-depositor families have very similar characteristics prior to the bank failure. Therefore, the differences that are observed in 1880 and 1900 are more likely due to the bank closure, rather than endogenous selection

[58]Family real estate value is the only covariate where the difference in means between treatment and control is at 12 percent in the 1870-1880 matched sample. Family personal estate and mulatto status are the covariates where the differences in means are larger than 10 percent.

biases.

1.6.2 Result from Propensity Score Matching

In the matching exercise, each child from a depositor family is paired with a child from a non-depositor family who resided in a county with a planned bank opening in 1870. I will focus on the same labor market and human capital outcomes used in the IV exercise.

First, I focus on outcomes measured in 1880 (Panel A of Table 1.20 in the appendix). I find that children from depositor families were 10 percentage points more likely to be literate, relative to children in the control group. However, I find no statistically significant difference in labor force participation or school attendance. Then, I focus on outcomes measured in 1900 (Panel B of Table 1.20 in the appendix). Similarly, I find children of depositors were 9 percentage points more likely to be literate relative to children of non-depositors in the control group. Again, there is no statistically significant difference in labor force participation or occupation standing between the two groups when the children are in their mid-life. Since the matching exercise provides an average treatment effect and the IV exercise provides a local average treatment effect, it's difficult to compare the magnitude of the findings. However, it's reassuring that the estimates from the matching exercise are consistent in signs and significance with the local average treatment effect.

1.7 Mechanism

Both the IV and the matching results show that children of depositors were no worse off than children of non-depositors. Moreover, the results seem to suggest that children of depositors "benefited" from the bank failure because it resulted in the improvement of literacy rate. In the post Civil War South, African Americans had very limited access

to credit; and we expect the effects of a negative wealth shock to be especially salient in a market with limited credit. Given the historical backdrop, the positive literacy outcome that I observe is the opposite outcome of what one would expect. This suggests that depositor status is masking other effects aside from wealth loss

In this section, I explore whether depositor status is masking other effects aside from wealth loss. To do so, I look closely at the experience of the depositors. For the account holders at the Freedman's Bank, the experience is a combination of years spent as a depositor at the bank and the wealth loss from bank failure. I will separately analyze the two aspects of the experience.

1.7.1 The Banking Experience

What changed for the depositors and their children following the opening of their bank accounts? Qualitative evidence suggests that the bank was beneficial for African Americans prior to its failure. The bank officers, aided by the distribution of pamphlets and circulars, were very successful at instructing and elevating the financial literacy of the depositors. Referring to the pamphlets published by the bank, Fleming (2018) wrote, "As a factor in Negro education there was then probably nothing better than this literature and the bank it represented and the effects were soon observed." There were more depositors in the bank than there were children in the school operated by the Freedman's Bureau. Many held the opinion that the thrift education given to the depositors was probably more useful than the kind of education frequently given to the children in the schools. Depositors at the bank appear to gain financial acumen and become more informed following the opening of accounts: "the Negroes who had bank books were less easily swindled by the multitude of sharpers who came to teach them the ways of freedman."

Estimation

To quantitatively measure the effect of bank openings on the depositors and their children, I exploit the differences in the timing of account openings for depositors and the staggered opening of the bank branches. From the register data, I am able to observe the date at which a depositor first opened an account. To estimate the effect of opening a bank account for the depositor and their children, I compare individuals within the same birth cohort whose families deposited in the same branch but at different time periods. Since the Freedman's Bank was in operation from 1865 to 1874, through the 1870 linked sample, I observe families who have already deposited and families who have not yet deposited[59]. For families who have deposited by 1870, I observe families who have been with the bank for less than one year to a maximum of four years. For families who have yet to deposit, some are less than a year from depositing while others are three years away from opening an account[60]. For this sample, I estimate:

$$Y_{ibs} = \alpha + \eta_b + \gamma_s + \sum_{m=-4}^{3} \beta_m \mathbb{1}(m_i = m) + \epsilon_{ibs} \tag{1.4}$$

for individual i from birth cohort b whose family deposited at bank branch s in 1870. The regression also controls for gender, mulatto status and distance to the county with a bank. Standard errors are clustered at the family level. The main right-hand side variable of interest, denoted $\mathbb{1}(m_i = m)$, are indicators for the difference in years between calendar year 1870 and the year the family first opened a bank account. The omitted coefficient

[59]Tradweek and Wardlaw (2018) suggests that the depositors who opened an account after the Panic of 1873 were different from the depositors who banked prior to the Panic. Therefore, my sample excludes those who deposited after September of 1873.

[60]I limit the sample to depositors from branches that had already opened by 1870. By 1870, 20 out of the 26 branches with registers had been established. See Table 1.7 in the appendix for a list of branches established prior to 1870.

$\mathbb{1}(m_i = -1)$ corresponds to 1 year prior to opening a bank account. This exercise allows me to document the evolution of household labor market decisions and literacy conditions before and after opening a bank account.

To interpret the results of this estimation as a causal effect of banking, the identification assumption requires that the timing of the bank deposit is orthogonal to a child's potential outcomes. It's likely that families who deposited as soon as the bank opened are different from the families who deposited three years after the bank opened. I justify the validity of this assumption in two ways. First, among the depositors in banks that opened prior to 1870, there exists no significant difference in characteristics (labor force participation, occupation quality, literacy) between depositors who were one year away from opening an account to depositors who were two or three years away from opening an account[61]. Second, I conduct a similar analysis on the sample of depositors in banks that opened in 1870[62]. Among the depositors in this sample, some depositors opened an account as soon as the branch opened (in 1870), while others opened the account a few years after the bank was in operation. I compare 1870 observed characteristics between depositors who opened an account immediately to depositors who opened accounts one, two or three years after the bank opened. Table 1.5 shows that family characteristics were very similar between depositors who chose to open an account at different time periods.

Main Results

Among the depositors, Panel A of Figure 1.3 shows no significant trend in labor force participation for the depositors prior to and after opening a bank account. For depositors in the labor force, I observe a trend towards switching to occupations that are of better

[61]This is shown by the fact that we observe no "pre-trend" in Figures 1.3 and 1.4.
[62]See Table 1.7 in the appendix for a list of branches that opened in 1870.

quality (either skilled blue-collar or white-collar), although the coefficients are statistically insignificant (Panel B of Figure 1.3), perhaps reflecting an improvement in the household's economic well-being. Panel C of Figure 1.3 shows an analogous plot for the depositor's literacy status. The graph exhibits no significant pre-trend in the literacy of the depositors. Following banking, depositors increase their literacy rate by 26.7 percent.

In Figure 1.4, I plot the coefficients from the same specification in a sample of the children of depositors. Panel A show the evolution for the children's literacy status before and after banking. The figure shows that children increased their literacy by 30.6 percent following banking. Similarly, the coefficients in Panel B show a broad positive trend in the children's school attendance following banking. Children of depositors increased school attendance by 10 percentage points from a mean of 25.6 percent, representing an increase of 39 percent. Finally, I observe no systematic change in labor force participation for the children of depositors after the depositor opened an account (Panel C of Figure 1.4).

I show that other adult members of the family did not respond in significant ways before or after banking in Figure 1.7 in the appendix. First, I observe no change in the adults' labor force participation following banking (Panel A). Panel B shows that the trend holds even when I limit the sample to female members of the household. Second, for the adults with an occupation, I observe no significant changes in their occupation quality, either before or after banking (Panel C). Third, Panel D show an upward trend in the literacy status following a family member's decision to bank, but the coefficients are not statistically significant in a systematic manner.

As a robustness check, I will exploit the staggered roll-out of bank branches and repeat the estimation for Equation (4) without imposing bank branch fixed effects. I restrict my

sample to depositor families who opened accounts as soon as a branch was established in their locality[63]. Hence, in other words, I compare the characteristics of depositor households who have been in the bank for different amounts of time due to the staggered nature of the branch roll-out. First, I show that the timing of branch establishment is uncorrelated with county-level variables. Table 1.21 in the appendix shows that county characteristics of bank branches that opened in 1866, 1868 and 1870 are not systematically different from counties with bank openings in 1865. Then, in Figure 1.8 in the appendix, I plot the trend for literacy rate of depositors and their children across households who opened accounts within a year of their local branch's establishment. In this specification, I still observe a broad positive trend in literacy for both the depositors and their children. In addition, the coefficients are not statistically different from the coefficients estimated using Equation (4).

Overall, my findings indicate that both the depositors and their children were able to significantly improve their literacy after opening a bank account. Stein and Yannelis (2019) conducted a similar analysis using an instrumental variable strategy. On the effect of banking on literacy and children's school attendance, they also concluded that families with accounts were more likely to be literate and have children in school. On the other hand, Stein and Yannelis find that depositors were more likely to be in the labor force compared to non-depositors, while I find no effect.

Freedman's Bank and the AMA

Since I observe a large increase in literacy rate for the depositors and their children following banking, I want to explore the possible factors that contribute to this trend. One possible channel in which the bank could promote literacy is through its ties with the Ameri-

[63]I restrict my sample to depositor families who opened accounts within the year of the branches' date of establishment.

can Missionary Association (AMA). AMA was a Christian educational organization founded in 1846. After the Civil War, AMA rose to be the strongest missionary association in the efforts to educate the freedman. The AMA established schools throughout the South for African Americans (Richardson 2009)[64]. The origin of the bank's connection with the AMA extended all the way back to one of its founders, John Alvord. Alvord was a Congregational minister and once served as AMA's corresponding secretary. On the local level, Alvord appointed AMA personnel as bank cashiers. At least 9 cashiers were AMA teachers, principals, or superintendents, while another 9 worked for the AMA in other capacities[65].

The close association between the AMA and the bank was instrumental in promoting literacy for the children of depositors. The cashiers with AMA connections served as spokesmen for the promotion of education. They informed depositors of schooling opportunities and encouraged them to send their children to school[66]. Among the depositors and their families, 59 percent were depositors at branches with at least one cashier affiliated with the AMA[67]. For depositors at branches with AMA connections, opening an account is associated with an increase in social connections and information which influenced them to invest more in their children's education. I repeat the estimation of Equation (4), comparing children from families who have deposited to children whose parents have yet to deposit in 1870,

[64]A few white students had attended early AMA schools, but most vanished after public schools were opened (Richardson and Jones 2015).

[65]The presence of bank cashiers with AMA connections did not significantly impact the take-up rate of banking. In Figure 1.9 in the appendix, I show that the average take-up rate for banks with AMA connections is not statistically different from banks without AMA connections.

[66]The presence of cashiers who were AMA teachers could also imply a higher supply of schools in the local area.

[67]59 percent of depositors and their families banked at a branch that was opened prior to 1870 with at least 1 cashier affiliated with the AMA. If we expand the sample to include branches opened in 1870, the number is 61 percent. Below are the branches with at least 1 cashier affiliated with the AMA (branches marked with an asterisk opened prior to 1870): Atlanta, Baltimore*, Beaufort*, Charleston*, Chattanooga, Columbus, Jacksonville, Lexington, Macon, Memphis*, New Bern*, Norfolk*, Savannah*, Tallahassee*, Vicksburg*, Washington DC*, Wilmington*.

limiting the sample to depositor families at branches with AMA connections. Within this sample, I find that the children's literacy increased by 29.2 percent following the parents' decision to open an account. Similar, I observe that school attendance increased by 31.3 percent. On the other hand, I do not observe a statistically significant increase in literacy or school attendance among children whose parents banked at branches without AMA connections (See Figure 1.5). The results suggest that the increase in literacy among children is mostly driven by bank cashiers with AMA affiliations.

Although my results suggest that a branch's connection with the AMA can explain the increase in literacy for children, the AMA connection does not seem to be a channel that explains the increase in literacy among the depositors themselves. For the depositors, I observe a general increase in literacy (although statistically insignificant) following banking both in branches with AMA connections and in branches without AMA connections (see Panel A of Figure 1.10 in the appendix). One potential explanation for the increase in literacy is that depositors from all branches have a shared desire to fully participate in the banking process. This desire could have led many depositors to seek out evening schools or Sunday schools to learn how to read and write, in order to understand the content written in their individual passbooks.

In addition, I show that the economic conditions for households who opened accounts at AMA affiliated branch were not systematically different from households who banked at non-AMA affiliated branches. This provides further proof that depositors at AMA-connected branches were not investing more in their children's education because they became more affluent, but because they became more aware of education opportunities by connecting with educators at the bank. In Panels B and C of Figure 1.10 in the appendix, I compare the

depositors' labor force participation and occupation quality separately for those who banked at AMA and non-AMA connected branches. The trend in depositors' economic conditions are similar across branches with or without AMA connection.

1.7.2 Wealth Loss

In this section, I focus on the wealth loss aspect of the banking experience. Specifically, I look at the intensive margin, comparing households with the same wealth level in 1870 who allocated more/less of their wealth in the Freedman's bank. To conduct this analysis, I limit my sample to a subset of depositors in which I am able to observe the amount in their bank account at the time of the bank failure. These depositors appear in the ledger sheets maintained by the Freedman's Bank. All individuals in this sample were depositors who sent in a valid passbook during the bank's liquidation period.

Depositors in Ledger Sheets

Since the analysis is limited to depositors who sent in a valid passbook after the bank failure, one might be concerned that this is not a representative sample of all depositors. Historians believed that the depositors who sent in passbooks were wealthier than the rest. For some depositors and their families, the cost of claiming a small dividend might be prohibitive (Osthaus 1976)[68]. From the sample of depositors linked to the 1870 Census, I was able to locate 59 percent of depositors in the ledger sheet[69]. Table 1.22 in the appendix compares depositors linked to the ledger sheet to those who cannot be located in the ledger

[68]In the event of a depositor's death, family members had to appear before the justice of the peace to obtain an affidavit swearing that they were the deceased's legal heirs, and they had to communicate with the comptroller. Depositors who changed their names would have had to go through a similar procedure to receive reimbursement.

[69]From the sample of all depositors, I was able to link 48 percent to the ledger sheets. But many depositors came from banks without available ledger sheets. If I limit the sample to depositors from banks with available ledger sheet, 59 percent were linked to the ledger sheets.

sheet. Contrary to the general claims of historians, I find that depositors matched to the ledger sheets were less well-off than depositors who cannot be matched to the ledger sheets. Based on characteristics recorded in the 1870 Census, linked depositors held less wealth than their unlinked counterparts.

To explain away the discrepancy between the claims of historians and the patterns shown in the data, one has to look into the identity of the depositors who did not appear in the ledger sheets. These depositors can be categorized into two types: those who closed their accounts prior to the bank failure, and those who did not hand in their passbooks despite suffering losses. Most depositors who closed their accounts prior to the bank failure did so during the Panic of 1873. The Panic of 1873 generated runs on several branches of the Freedman's Bank[70], and many wealthier depositors withdrew their savings during the Panic. Since I find that depositors who did not appear in the ledger sheets were wealthier compared to depositors linked to the ledger sheet, this suggests that the majority of the depositors that cannot be located in the ledger sheets closed their accounts prior to the bank failure.

However, the claims of the historians were not inaccurate. The data suggest that depositors successfully linked to ledger sheets were wealthier compared to the depositors who failed to hand in their passbooks. The Panic of 1873 had little effect upon the Charleston branch. "It is somewhat singular that the colored element in the community seemed to be entirely free from all panicky feelings and during the entire day there were no more than the usual number of applicants for money."[71] Within the Charleston branch, where we know that very few depositors closed their accounts prior to the bank failure, Table 1.22 in the appendix shows that depositors who did not appear in the ledger sheets were less wealthy compared

[70]The bank announced that a sixty days' notice would be required to withdraw money during the Panic of 1873.

[71]This quote was in the reporting of the Charleston *News and Courier* on September 24, 1873.

to depositors linked to the ledger sheet.

Estimation

My goal is to compare households with the same wealth level in 1870 who allocated more/less of their wealth in the Freedman's Bank. In order to do so, I track the families of depositors who appear in the ledger sheet forward to the 1880 Census and estimate:

$$Y_{ibs} = \alpha + \gamma_s + \phi_b + \beta ProportionLost_i + \zeta \mathbf{X_i} + \epsilon_{ibs} \tag{1.5}$$

for individual i, whose family deposited at branch s in 1870, in birth cohort b. As before, the outcome variables Y_{ibs} of interest include school attendance, literacy, labor force participation and occupation quality for sons in 1880. The main right-hand side variable of interest, $ProportionLost_i$, is defined as currency held in a bank account divided by total wealth as measured in 1870 (the sum of personal estate wealth and real estate wealth)[72]. X_i includes a vector of controls that include 1870 wealth quartile dummies, mulatto status, gender, literacy status of family head, family head occupation category dummies and distance to county with a bank.

To accurately measure the proportion of wealth loss, it's important to accurately measure household wealth prior to the bank failure. The 1870 Census was the last census to ask all household heads to report total dollar value of real estate and personal wealth. But unfortunately, the 1870 Census is notoriously unreliable in its measurement of wealth. Ager et al. (2019) and Steckel (1994) both noted that the 1870 Census exhibits a large extent of non-reporting of wealth variables. Census enumerators in 1870 were instructed to only

[72]The 1870 Census was the last census to measure wealth. Personal estate reports the dollar value of all stocks, bonds, mortgages, notes, livestocks, plate, jewels and furniture. Real estate wealth reports the dollar value of any real estate owned by the respondent. The full value of the real estate was to be reported, even if the property was encumbered by a lien, mortgage, or other debt.

report wealth if the assessed/reported value was more than 100 dollars[73]. The concern is that we cannot correctly distinguished between households with low levels of wealth and non-reporting households[74]. For cases where I observe the amount in the Freedman's Bank account exceeded reported wealth, I set $ProportionLost_i$ as 1. Due to the prevalence of zeros/non-reporting wealth among Black households in 1870, 80 percent of depositors in the sample has $ProportionLost_i$ set as 1. To alleviate the concern regarding the accuracy of reported household wealth in 1870, I will estimate Equation (5) only on a sample of households who reported positive wealth as a robustness check[75].

Main Results

Table 1.6 shows that families of depositors who loss a higher proportion of wealth were more adversely affected six years after the bank failure. I find that children in families who experienced higher proportion of wealth loss were less likely to attend school in 1880, six years after the bank failure (Panel A of Table 1.6). A 10 percentage point increase in the proportion of wealth loss for one's family decreases the likelihood of attending school by 2.9 percentage points. In Panel B of Table 1.6, I examine how wealth loss affects the labor market decisions of other adult members in the family. First, I find that depositors who lost a higher proportion of wealth from the bank failure were more likely to enter the labor force. However, those in the labor force were less likely to be in a white-collar or skilled-blue-collar occupation. Depositors who lost a higher proportion of wealth might have been less likely to advance into higher-paying careers that require starting capital (e.g., storekeeper...). Lastly, I

[73]There are many instances where reported personal estate wealth or real estate wealth is less than 100 dollars.

[74]There is a large discrepancy in the extent of non-reporting based on race. Within Southern households in 1870, I observe 70.6 percent of white households to report wealth and 20.9 percent of Black households to report wealth (see Table 1.1). Even among the Black depositor households in which I observed the deposit amount exceeded 100 dollars, only 24.5 percent reported their wealth in the 1870 Census

[75]Only 4 percent of households who reported positive wealth in 1870 had $ProportionLost_i$ set as 1.

compare the labor force behavior of other adult family members between households who lost more/less proportion of wealth. Panel C of Table 1.6 shows that adult family members who lost a higher proportion of wealth were more likely to be in the labor force. In particular, the increase in labor force participation was mostly driven by female family members. Female family members from households who experienced a 10 percentage point increase in the proportion of wealth loss were 3.2 percentage points more likely to enter the labor force[76]. The result suggests that female members of the family were increasing their labor market earnings to cope with the wealth loss following the bank failure.

In the appendix, I present several robustness checks using different samples and alternative specifications. First, I estimate Equation (5) on a sample of depositor families who reported positive wealth in 1870. The results, reported in Table 1.23 in the appendix, remain very similar to the baseline, although the effect on the depositors' labor force participation and occupation quality loses statistical significance at conventional levels. Second, I present an alternative specification of Equation (5) which utilizes the logarithm of proportion of wealth loss instead of proportion of wealth loss in percentage points. Table 1.24 in the appendix shows that the results remain very similar to the baseline specification in terms of magnitude and significance. Finally, I use the dollar amount of bank deposit loss as the main explanatory variable instead of proportion of wealth loss. Comparing the outcomes of children from the same wealth quartile in 1870, I show that children from families who lost 100 dollars more bank deposit wealth were 2 percentage points less likely to attend school in Table 1.25 in the appendix. However, condition on pre-failure wealth quartile, the amount of wealth loss loss has no effect on the labor force participation of the depositors, or other

[76]In the 1880 Census, women were counted to be in the labor force if they were receiving wages and salary for their services.

adult family members.

Allowing for Heterogeneous Effect

In this section, I explore heterogeneous effects of wealth loss on depositors' children by family size, age of the child and local economic conditions. First, I investigate whether proportion of wealth loss affects children differently based on family size before the bank failure[77]. I split the sample into families with a fewer than 5 members, and those with 5 or more members and compared the effect of wealth loss for children in these two groups. Table 1.26 in the appendix shows that the adverse effect of wealth loss on children's school attendance is driven by those in larger families. This suggests that bigger families were more constrained after the bank failure and had fewer resources to send their children to school.

Second, I examine heterogeneous effect of wealth loss on children's school attendance based on the child's age. The marginal effect of proportion of wealth loss on school attendance was negative across all ages, but only statistically significant for children aged 8 to 10 in 1880 (Figure 1.11 in the appendix). For children who were 10 in 1880, those from families who lost 10 percentage points more of their proportional wealth were 5 percentage points less likely to attend school in 1880. In my sample, school attendance peaks for children aged 11 to 13. 32 percent of 8 year olds reported school attendance while 54 percent of 11 year olds went to school. The findings indicate that families who suffered from wealth loss delayed enrolling their children in school[78]. For these families, perhaps they were still recuperating from the wealth loss and were waiting until they had enough savings to send their children to school.

[77]Family size is measured in the 1870 Census

[78]Although some states had passed compulsory schooling laws by 1880, most Southern states did not have any form compulsory schooling laws until 1905.

Lastly, I investigate whether local economic conditions play a role in the way wealth loss affects children's human capital accumulation. There exist large variation in the degree of economic growth[79] between cities with banks from 1870 to 1880. I split the sample into counties above and below the median economic growth rate. The results show that the effect of wealth loss on the children's school attendance is significant only for counties with lower growth rates (Table 1.27 in the appendix). The results suggest that families who resided in a location with economic growth were able to recover their losses faster. Hence, their children's school attendance was unaffected. It also suggests that worsening local economic conditions could exacerbate the deleterious effect of wealth loss.

1.7.3 Other potential channels

Although my results suggest that the positive literacy effect on children from depositing is largely driven by the activities of the bank prior to its failure, I cannot pinpoint the true mechanism with certainty. There are several alternative channels that can also explain my results.

The first explanation for the positive literacy effect is that depositor families are more well-connected than non-depositor families. It's likely that families with more pre-disposed social connections are more susceptible to local advertisement efforts, hence are more likely to open a bank account. In additions, the Freedman's Bank serves as a community venue where depositors can build more social connections as they conduct their banking business. Depositor families can their social connections to find schooling opportunities, or they can use their connections as an insurance mechanism against tough times (Ager et al. 2019). It's difficult to test this hypothesis because I cannot empirically trace out social networks.

[79] I define economic growth rate as the percent change in population density from 1870 to 1880.

However, both of these explanations are consistent with my findings, resulting in children of depositors being more literate compared to children of non-depositors.

A second possibility is that depositor families had fewer children after the failure of the Freedman's Bank. Fewer offspring could then have enabled higher investment in each child's human capital. By 1880, depositor households had 0.1 fewer children after the bank failure[80]. However, Table 1.1 show that Black depositor households had more children prior to the bank failure: depositor households had 0.24 more children compared to non-depositor households. The higher fertility prior to the bank failure and lower fertility after offset each other. Hence, it's unlikely that differential fertility is driving my result.

A third potential explanation for the positive literacy effect I observe among sons is that families are potentially sacrificing their daughters to educate their sons after the failure of the Freedman's Bank. By including an interaction term to Equation (5), I proceed to test whether the relationship between proportion of wealth loss and school attendance is different for sons and daughters. In Table 1.28 in the appendix, I show that families who suffered from higher proportion of wealth loss lowered human investment for both sons and daughters[81] (see Table 1.28 in the appendix). Hence, both sons and daughters are adversely affected by wealth loss, and families are not sacrificing their daughters to increase human capital investment in their sons.

Summing up, my results show that children of depositors were more likely to be literate compared to children of non-depositors because their literacy increased prior to the failure of the Freedman's Bank. In particular, the AMA played an instrumental role in promoting

[80] On average, depositor families had 1.31 children born after 1874. Non-depositor families had 1.41 children born after 1874. These statistics are computed based on 1880 Census.

[81] Although proportion of wealth loss has no relationship with sons' labor force participation, daughters from families who lost a higher proportion of wealth are more likely to enter the labor force.

children's education through the bank. Although I am unable to rule out social connections as a potential driver behind my results, I have performed several checks whose results speak against alternative explanations, such as differential fertility and preferential treatment for sons. Overall, my results show that the depositor families benefited from the bank prior to its failure, and were adversely affected thereafter.

1.8 Conclusion

In this paper, I leverage a historical event – the failure of the Freedman's Bank – to study the intergenerational effect of a negative wealth loss for the Black community. The failure of the Freedman's Bank in 1874 resulted in a significant loss of wealth for tens of thousands of account holders, who were largely newly emancipated freedmen in the South. I show that children from families who lost a higher proportion of wealth were less likely to attend school. Specifically, the effect of wealth loss on children's human capital accumulation is worse for children in larger families, and for those who resided in locations with slow economic growth.

However, the experience of a Freedman's Bank depositor family can be decomposed into two aspects: 1) years spent as a depositor while the bank was in operation and 2) wealth loss from the bank failure. While analyzing the effect of financial inclusion while the bank was in operation, I find that the children of depositors experienced significant human capital gains. The human capital gains were so large that it offset the adverse effect of the wealth loss experienced by these families. Hence, overall, I find that children of depositors were more likely to be literate than children of non-depositors.

While conditions today differ significantly from the historical setting of the Freedman's Bank, this research can still provide policy implications. Globally, about 1.7 billion adults

remain unbanked (World Bank Findex Report 2017)[82]. A bank branch can become a community center, where depositors can interact with others from all backgrounds. As suggested by previous literature, opening a bank account will help depositors escape from poverty. My research suggest that depositors might reap additional and unexpected benefits from banking by connecting with the right people. Hence, policies in developing nations that open branches in bank desserts and encourage universal financial inclusion might generate unintended positive effects, such as human capital gains for the future generations. My work also highlights the importance of creating institutions which can safeguard the savings of depositors. Wealth loss caused by mismanagement or embezzlement within banks will result in lower human capital accumulation for future generations, potentially wiping out the benefits gained as a result of financial inclusion.

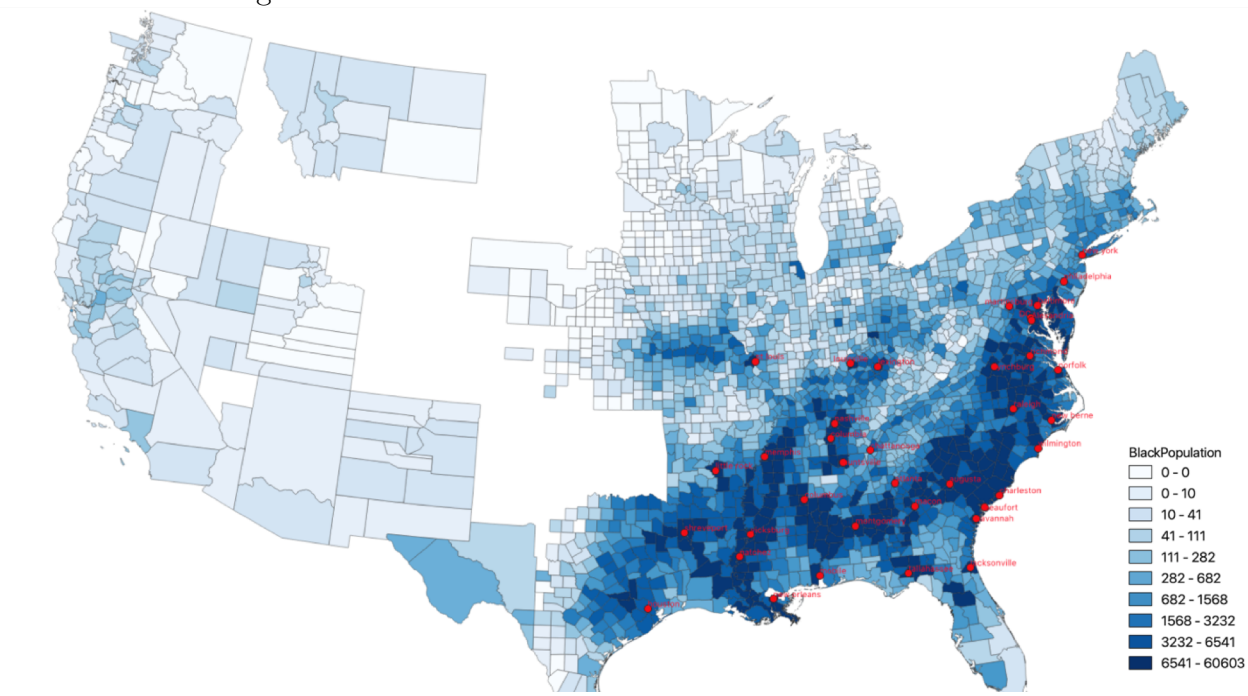
Even though my results show that parental wealth loss adversely affects children's schooling and literacy, it's unclear whether such effects persist for the grandchildren and future generations. Acemoglu and Robinson (2008) outlined a theory which predicts elite persistence despite changes in economic relation and political institution. Many empirical studies have shown support for the theory of elite persistence – wealthy families were able to recover from large wealth losses in the long-run. This has been shown true in many different contexts: Southern slaveholders following the Civil War (Ager et al. 2019) and Chinese and Russian elites following the Communist Revolutions (Alesina et al. 2020, Pakulski et al. 1996). Studies have pointed to cultural transmission within families and social connections as possible drivers of recovery. Pinpointing the factors that suppress/accelerate recovery from wealth loss is an important avenue for future research.

[82]Because account ownership is near universal in high-income countries, virtually all unbanked adults live in developing economies.

The Freedman's Bank and the legacy that it created prompts another particular pressing question: whether or not the failure of the Freedman's Bank contributed to the mistrust and underutilization of financial institutions by African Americans today. Historians (Osthaus 1976) have long hypothesized that the collapse of the bank can be linked to a cultural legacy of distrust towards the banking system within the Black community. Future work should use the rich datasets of the Freedman's Bank to test this hypothesis empirically.

1.9 Figures and Tables

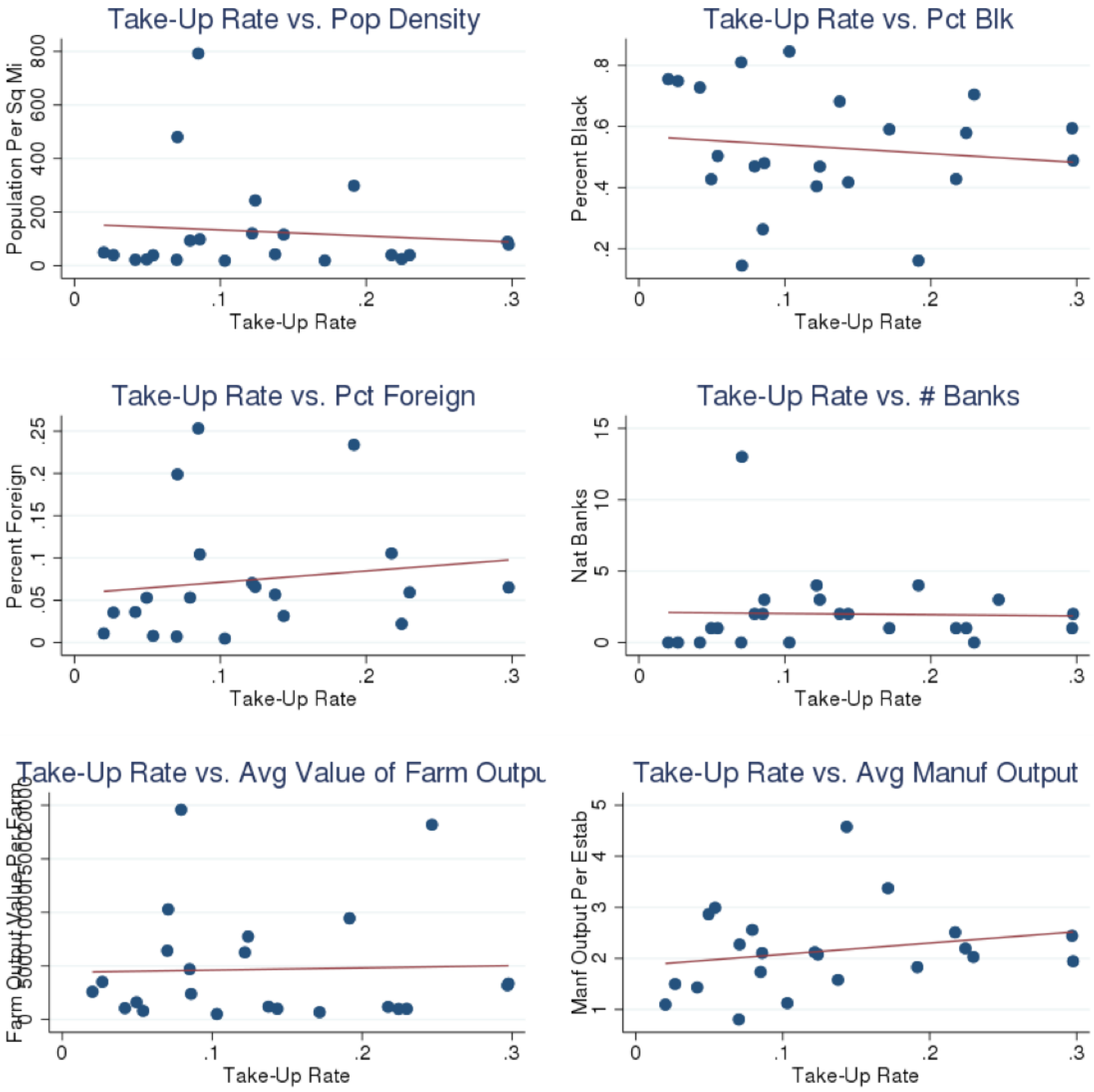
Figure 1.1: Locations of the branches of Freedman's Bank



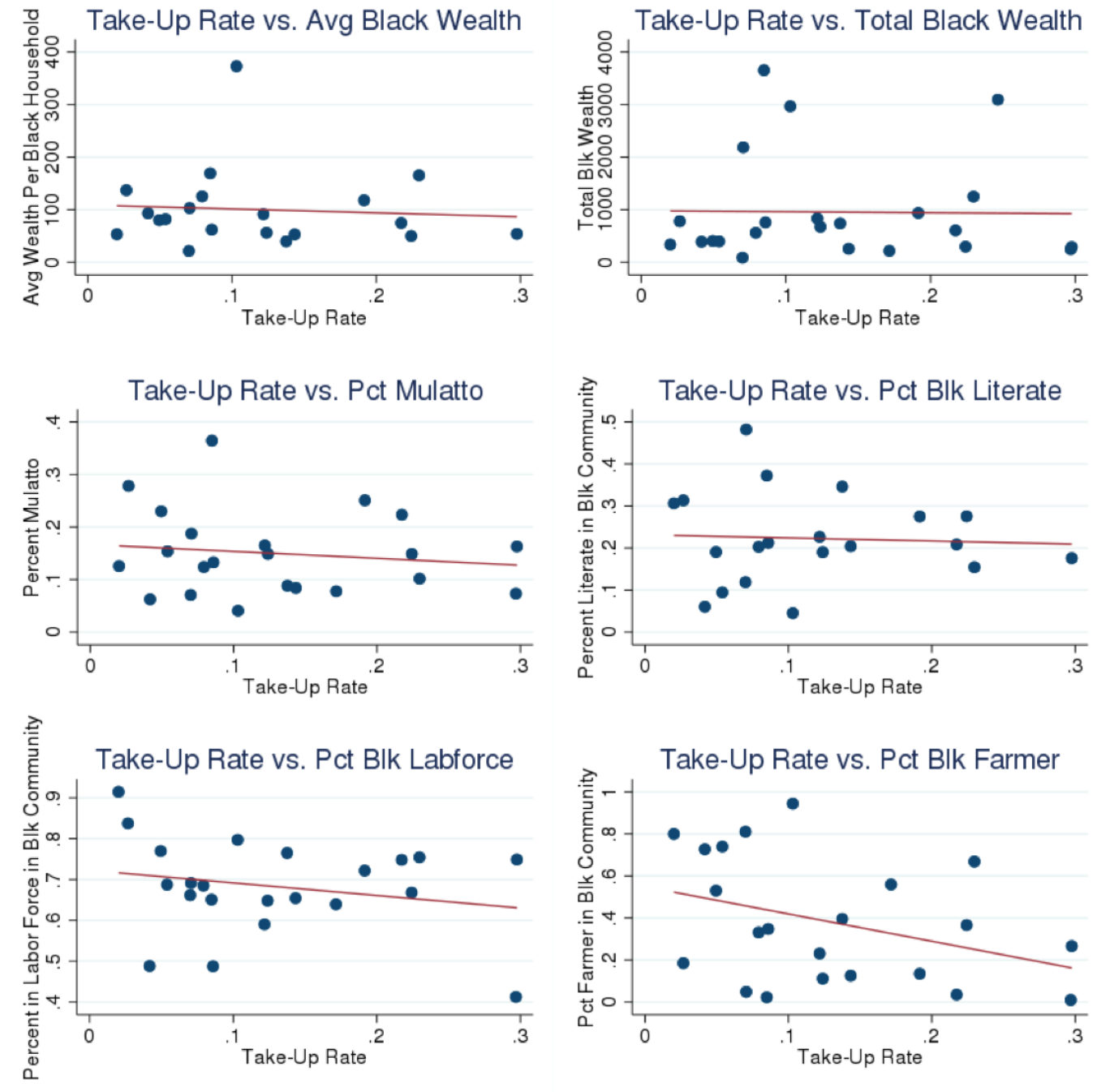
Notes: This figure shows the location of Freedman's Bank. The map is delineated by county in 1870. The shades represent the number of Black residents within a county in 1870. Darker shades represent a larger number of Black residents.

Figure 1.2: Correlation between county take-up rate of banking and county characteristics

Panel A:



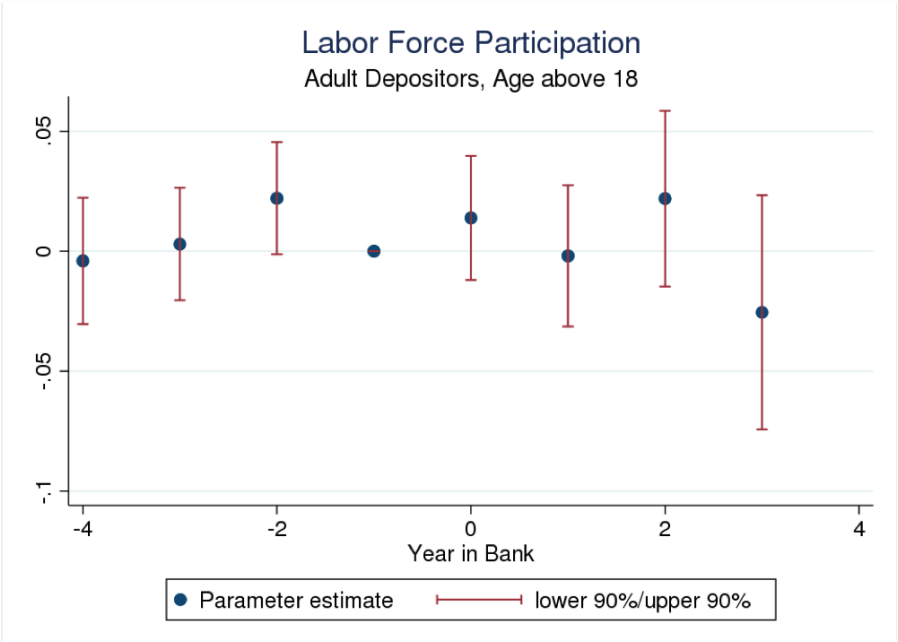
Panel B:



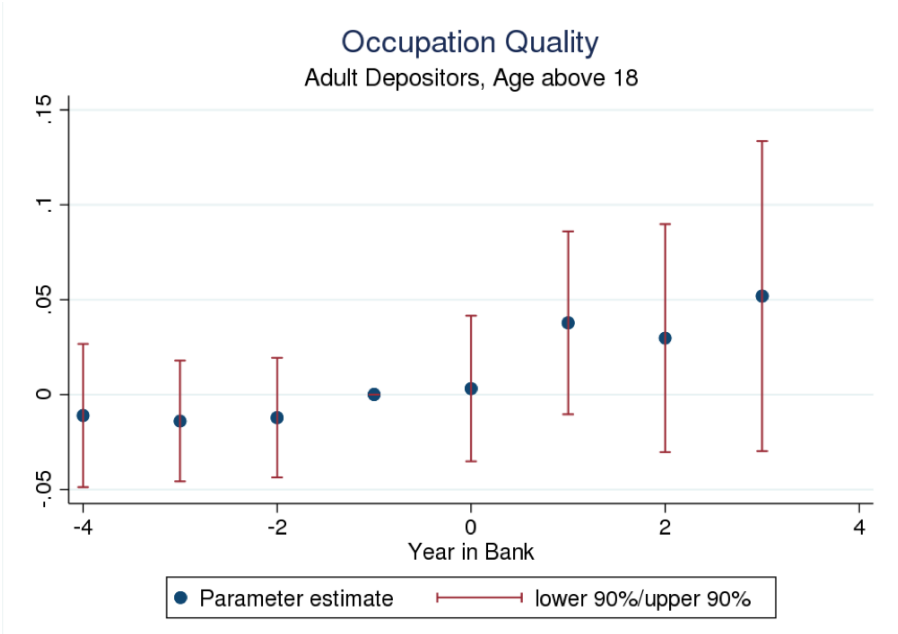
Notes: This figure plots the relationship between the county take-up rate of banking and county characteristics in 1870. The county characteristics plotted in this graph are: population density, percent Black, percent employed in manufacturing, avg value of manufacturing output, number of manufacturing establishments and avg value of farm output,. The red line plots the best fitted line between the county take-up rate and county characteristics. The 1870 county level characteristics are taken from Haines(2010).

Figure 1.3: Changes in depositors' outcomes around the time of bank account opening

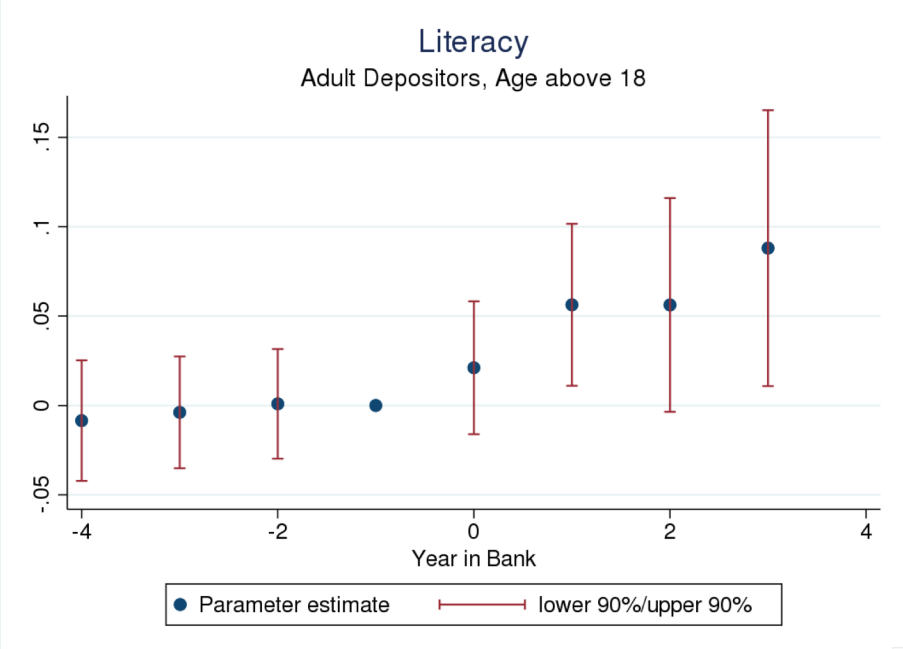
Panel A:



Panel B:



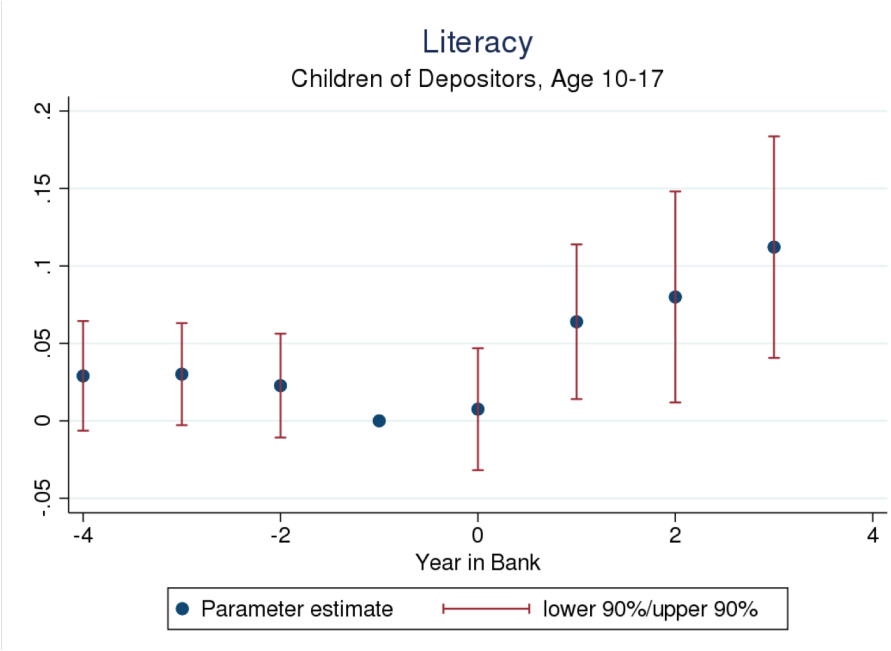
Panel C:



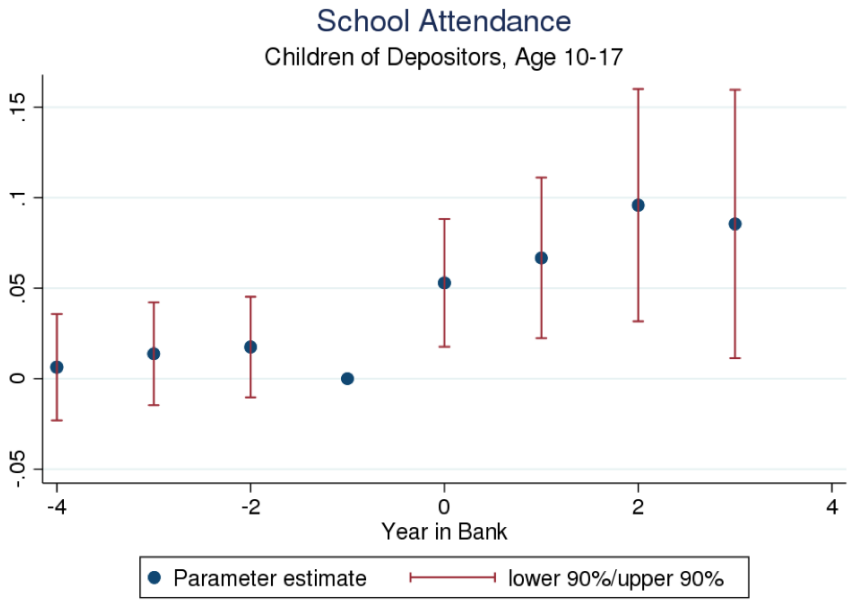
Notes: Panel A shows coefficients from a regression of labor force participation on a vector of leads and lags of years until bank account opening. Panel B shows coefficients from a regression of an indicator variable that equals 1 if in an occupation that earns more than a laborer on a vector of leads and lags of years until bank account opening. Panel A shows coefficients from a regression of literacy on a vector of leads and lags of years until bank account opening. The model includes state fixed effects, birth-cohort fixed effects, mulatto status dummy, dummies that control for distance to the bank and gender dummy (see Equation (4) for details). All variables are measured in 1870. Depositors are aged 18 and above in 1870. Standard errors are clustered on the family level.

Figure 1.4: Changes in children's outcomes around the time of parents' account opening

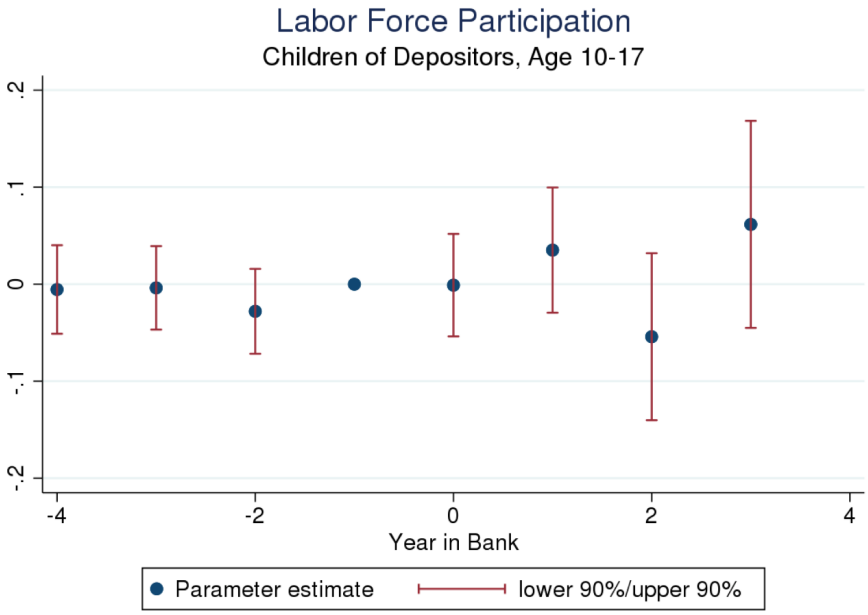
Panel A:



Panel B:



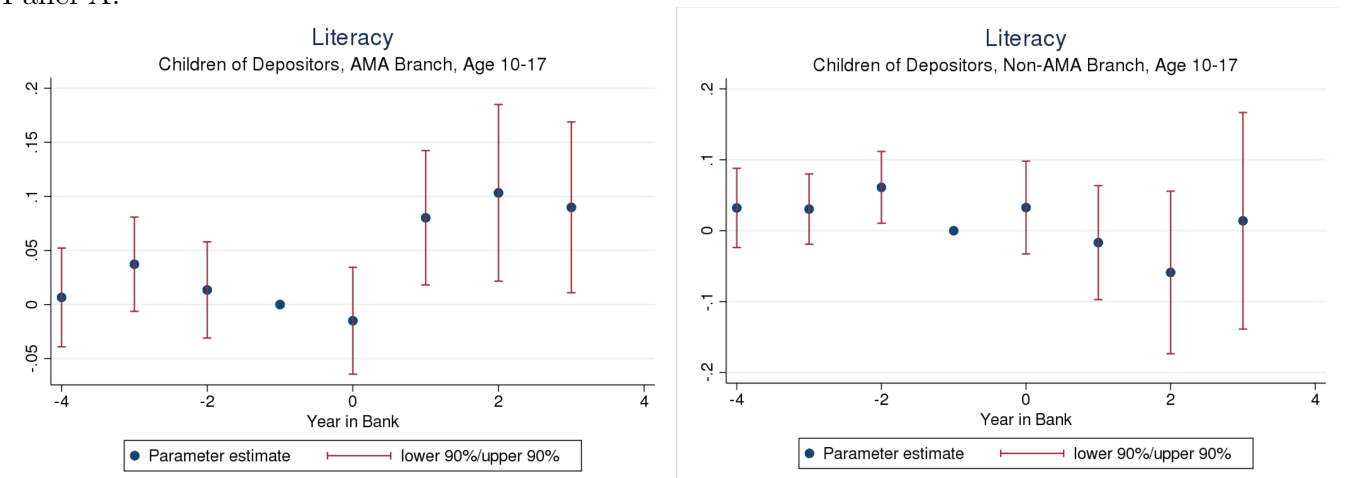
Panel C:



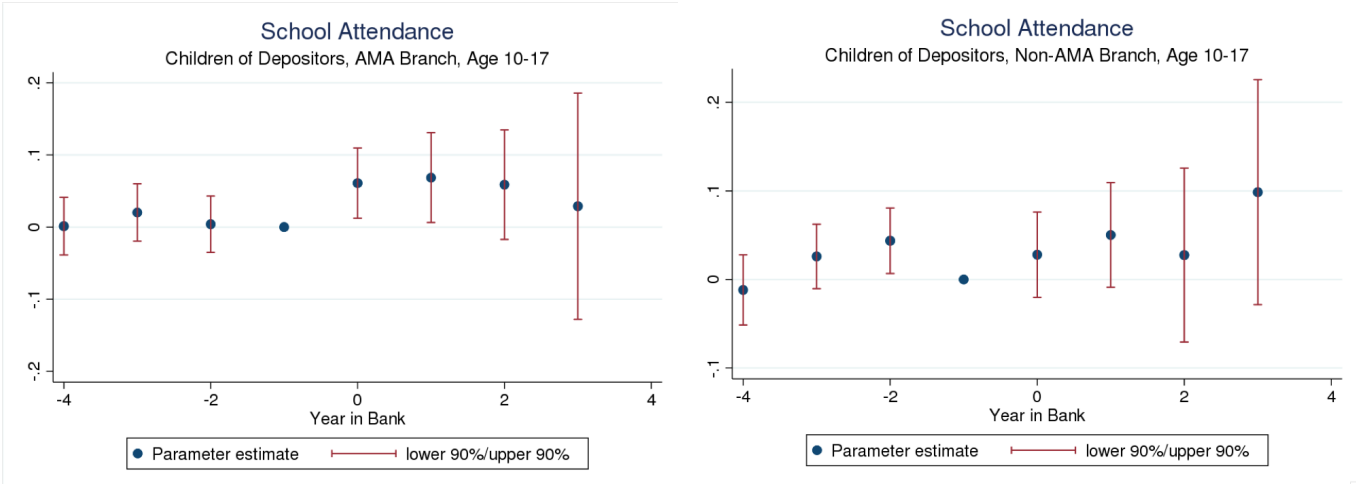
Notes: Panel A shows coefficients from a regression of indicator variable that equals 1 if one is literate on a vector of leads and lags of years until bank account opening. Panel B shows coefficients from a regression of of school attendance on a vector of leads and lags of years until bank account opening. Panel C shows coefficients from a regression of labor force participation on a vector of leads and lags of years until bank account opening. The model includes state fixed effects, birth-cohort fixed effects, dummies that control for distance to the bank and gender dummies (see Equation (4) for details). All variables are measured in 1870. Children are aged 10 to 17 in 1870 when analyzing literacy, school attendance and labor force participation. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Figure 1.5: Changes in children's literacy around the time of parents' account opening for AMA branches vs. non-AMA branches

Panel A:



Panel B:



Notes: Each figure shows coefficients from a regression of the outcome variable on a vector of leads and lags of years until bank account opening. The model includes state fixed effects, birth-cohort fixed effects, dummies that control for distance to the bank and gender dummies (see Equation (4) for details). The top two figures restrict the sample to depositors at AMA affiliated branches. The bottom two figures restrict the sample to depositors at non-AMA affiliated branches. All variables are measured in 1870. Children are aged 10 to 17 in 1870 when analyzing literacy, school attendance and labor force participation. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Table 1.1: Characteristics of account holders at Freedman's Bank vs. general population residing in the South, based on 1870 Census

	Depositor			All		
	Total	Black	White	Total	Black	White
Positive Wealth	43.33	26.08	76.54	52.81	20.94	70.60
	(49.55)	(43.91)	(42.38)	(49.92)	(40.69)	(45.56)
Wealth	1403.84	184.41	3498.37	1760.96	111.67	2581.42
	(15354.79)	(977.44)	(26287.11)	(36124.39)	(3202.47)	(45538.03)
Positive Personal Estate	38.61	21.18	73.07	49.97	19.33	67.06
	(48.69)	(40.86)	(44.36)	(50.00)	(39.49)	(47.00)
Personal Estate	485.99	66.68	1197.02	598.61	54.61	877.37
	(9070.59)	(494.46)	(15603.61)	(18935.28)	(1114.90)	(24096.13)
Positive Real Estate	26.73	10.44	56.34	34.57	4.96	50.86
	(44.26)	(30.58)	(49.60)	(47.56)	(21.71)	(50.00)
Real Estate	917.85	117.74	2301.35	1162.36	57.06	1704.04
	(11061.18)	(772.44)	(19026.95)	(22105.60)	(2650.04)	(27457.39)
Family Size	6.09	5.58	6.86	5.37	4.92	5.53
	(2.72)	(2.61)	(2.58)	(2.83)	(2.75)	(2.78)
Number of Children	3.11	2.83	3.51	2.73	2.59	2.77
	(2.12)	(2.05)	(2.13)	(2.16)	(2.11)	(2.16)
Illiteracy	52.67	70.48	18.87	41.51	80.28	19.13
	(49.93)	(45.61)	(39.13)	(49.27)	(39.79)	(39.33)
Farmer	52.25	46.49	62.20	60.08	63.77	57.68
	(49.95)	(49.88)	(48.49)	(48.97)	(48.07)	(49.41)
Reside in County with Bank	39.41	49.46	17.88	15.17	15.60	14.77
	(48.87)	(49.98)	(38.33)	(35.88)	(36.29)	(35.48)
Depositor N	17362	12258	5104			
Depositor Family N	76230	51270	24960	11590951	4494250	7096701

Notes: Summary statistics based on full count population taken from the following states: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. Standard deviations are reported in parenthesis.

Table 1.2: First stage

	(1)	(2)	(3)	(4)	(5)	(6)
	Second-stage dependent variable					
	A. 1880 Outcomes			B. 1900 Outcomes		
	Literate	School	Labforce	Literate	Labforce	BetterOcc
VARIABLES	Freedman	Freedman	Freedman	Freedman	Freedman	Freedman
TakeUp	0.663***	0.748***	0.665***	0.625***	0.633***	0.625***
	(0.178)	(0.193)	(0.167)	(0.059)	(0.062)	(0.060)
F-statistic	13.91	15.00	21.81	13.81	14.19	16.32
N	14,360	14,020	14,286	5,139	5,009	4,920
R-squared	0.081	0.087	0.081	0.052	0.052	0.052

Notes: Each column reports the first-stage coefficients from a separate two-stage least squares regression. The dependent variable in the first stage is an indicator = 1 if the son's parent was a depositor at the Freedman's Bank. Different second-stage outcome variables have different samples, thus different first-stage results. For the outcomes taken from the 1880 Census, literacy outcomes are available for ages $\geq$ 10 in 1880. School attendance is evaluated for ages $\geq$ 10 in 1880. Labor force is defined for ages $\geq$ 10 in 1880, and is set as missing if no occupation is recorded. For the outcomes taken from the 1900 Census, literacy variables and labor force is set as missing if no answer was provided in the Census. The outcome *BetterOcc* in 1900 is an indicator = 1 if the son was in an occupation that has an occupation score greater than 20, which is the occupation score for a laborer. *BetterOcc* is set as missing if one is unemployed or no occupation is recorded. All specification include birth cohort fixed effects and 1870 family level fixed effects: wealth quartile, family head occupation, mulatto, family size, veteran status, family head literacy status. The following 1870 residential county characteristics are also used as controls: percent Black as farmers, average farm output, average value of farm output. Standard errors are clustered at the county level.

Table 1.3: OLS estimates on early-life outcomes and mid-life outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	A. 1880 Outcomes			B. 1900 Outcomes		
VARIABLES	Literate	School	Labforce	Literate	Labforce	BetterOcc
Freedman	0.085***	0.038**	-0.056***	0.049***	-0.010	0.016
	(0.017)	(0.014)	(0.014)	(0.015)	(0.009)	(0.02)
Mean	0.539	0.316	0.724	0.740	0.982	0.239
N	14,360	14,020	13,084	5,139	5,009	4,920
R-squared	0.095	0.161	0.293	0.060	0.015	0.039

Notes: The sample consists of Black sons who resided in a Southern county with surviving bank register records in 1870. The children were ages 10-21 in the 1880 Census and ages 30-41 in the 1900 Census. *Freedman* denotes an indicator = 1 if the son's parent was a depositor at the Freedman's Bank. Labor force is set as missing if no occupation is recorded. The outcome *BetterOcc* is an indicator = 1 if the son was in an occupation that has an occupation score greater than 20, which is the occupation score for a laborer. *BetterOcc* is set as missing if one is unemployed or no occupation is recorded. All specification include birth cohort fixed effects and 1870 family level fixed effects: wealth quartile, family head occupation, mulatto, family size, veteran status, family head literacy status. The following 1870 residential county characteristics are also used as controls: percent Black as farmers, average farm output, average value of farm output. Standard errors are clustered at the county level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.4: 2SLS on early-life outcomes and mid-life outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	A. 1880 Outcomes			B. 1900 Outcomes		
VARIABLES	Literate	School	Labforce	Literate	Labforce	BetterOcc
Freedman	0.176**	0.107	0.102	0.253**	0.040	-0.031
	(0.056)	(0.100)	(0.091)	(0.099)	(0.043)	(0.215)
Mean	0.539	0.316	0.724	0.740	0.982	0.239
N	14,360	14,020	13,084	5,139	5,009	4,920
R-squared	0.093	0.159	0.282	0.037	0.002	0.028

Notes:The sample consists of Black sons who resided in a southern county with surviving bank register records in 1870. The children were ages 10-21 in the 1880 Census and ages 30-41 in the 1900 Census. *Freedman* denotes an indicator = 1 if the son's parent was a depositor at the Freedman's Bank. Labor force is set as missing if no occupation is recorded. The outcome *BetterOcc* is an indicator = 1 if the son was in an occupation that has an occupation score greater than 20, which is the occupation score for a laborer. *BetterOcc* is set as missing if one is unemployed or no occupation is recorded. All specification include birth cohort fixed effects and 1870 family level fixed effects: wealth quartile, family head occupation, mulatto, family size, veteran status, family head literacy status. The following 1870 residential county characteristics are also used as controls: percent Black as farmers, average farm output, average value of farm output. Standard errors are clustered at the county level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.5: Differences in Characteristics between depositors who banked in different years

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Literacy	LabForce	BetterOcc	Family Size	Personal Estate	Real Estate
1(deposit year=1871)	0.028	-0.019	-0.059	0.314	7.753	-14.237
	(0.043)	(0.030)	(0.045)	(0.256)	(11.191)	(25.449)
1(deposit year=1872)	0.052	-0.036	-0.053	0.077	17.602	32.403
	(0.044)	(0.031)	(0.044)	(0.256)	(11.844)	(27.958)
1(deposit year=1873)	0.025	-0.010	-0.071	0.158	16.422	4.849
	(0.048)	(0.033)	(0.045)	(0.268)	(20.755)	(30.361)
N	1160	1141	869	1160	1160	1160
R-squared	0.117	0.482	0.238	0.130	0.111	0.208

Notes: The sample consists of Black depositors matched to the 1870 Census who deposited at a branch that opened in 1870. This regression compares the characteristics of depositors who opened an account in 1871, 1872 and 1873 to depositors who opened an account in 1870. Robust standard errors are in parenthesis. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.6: Effects of proportion of wealth loss on depositor families in 1880

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Children		B. Depositor		C. Adult Family		
VARIABLES	School	Labforce	Labforce	BetterLab	Labforce	BetterLab	Fem Labforce
PropLoss	-0.029**	0.021	0.023**	-0.052***	0.020**	-0.012	0.032*
	(0.014)	(0.015)	(0.011)	(0.019)	(0.009)	(0.014)	(0.017)
Mean	0.343	0.434	0.867	0.245	0.711	0.213	0.711
N	583	551	503	419	1011	709	677
R-squared	0.235	0.337	0.379	0.241	0.294	0.201	0.147

Notes: The sample consists of Black depositors and their families who appeared in ledger sheers and are linked to the 1880 Census. All regressions include a vector of controls that include bank branch dummies, wealth quartile dummies, mulatto status, gender dummies, family head occupation category dummies, literacy status of household head and distance to bank. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

1.10 Appendix

1.10.1 Details on Algorithms Used to Match Register Records to the 1870 Census

I describe the steps I use to match the register records from the Freedman's Bank to the 1870 Census in detail below:

1. Matching with family member criteria, birth state present – spouse, mother, father, parents (60413 with birth state out of 81275)
   (a) Block by birth state
   (b) Clean names in both datasets to remove non-alphabetic characters and account for common misspellings and nicknames
   (c) Jaro-Winkler distance string match on both depositor's name and family member's name
   (d) Keep if name distance > 0.9 and family member name distance > 0.9

(e) Last name distance > 0.9

(f) Initials should be the same for name and family member

(g) Middle name/middle initial should be the same for depositor and family member if present

(h) Iteratively match by windows of birth year if birth year is available, up to +/- 3 years window following ABE algorithm

i. If unique match on birth year, a pair is matched

ii. If there are multiple potential matches in the Census with same year of birth, the observation is discarded

iii. If there are no matches by exact year of birth, the algorithm searches for matches with +/- 1 year of reported birth using the steps outlined above. If there is a unique match, then I consider a pair to be matched. If this is unsuccessful, I expand the window and repeat this step iteratively until +/- 3 years.

(i) If none of the attempts produces a unique match for a depositor record where birth year is present, the observation is discarded.

(j) For records with no birth year recorded, the algorithm searches for unique matches

2. Match with no family name criterion, birth state and birth year available (done for all individuals)

(a) Block by birth state

(b) Jaro-Winkler distance string match on full name

(c) Keep if name distance > 0.9

(d) Keep if last name distance > 0.9

(e) Keep if initials are the same

(f) Middle name/middle initial should be the same if present

(g) Iteratively match by windows of birth year if birth year is available, up to +/- 3 years window following ABE algorithm

(h) Drop matches if only first initial is present

3. Matching with family member criteria, birth state not present – spouse, mother, father, parents

(a) Block by initials

(b) Clean names in datasets A and B to remove non-alphabetic characters and account for common misspellings and nicknames

(c) Jaro-Winkler distance string match on both name and family member's name

(d) Keep if name distance > 0.9 and family member name distance > 0.9

(e) Last name distance > 0.9

(f) Middle name/middle initial should be the same for depositor and family member if present

(g) If unique, then consider the linkage a match

4. I then compile the matched datasets based on the steps described above (compiling 9 datasets, where 4 datasets include matching with spouse name, mother name, father name, and both parents' names while birth state is present, another 4 include matching with family states while birth state is not present, and the last dataset is matching with no family name criterion) in the following way:

(a) I first compile the datasets where family members' names were used in the matching criteria. In the rare cases where I observe one depositor observation matched to many different Census records, matches with spouse name take priority. If the spouse name is not available, matches with both parents' names take priority (this step was done twice, once for the datasets with birth state, once for the datasets without birth state).

(b) For the depositors with birth state available but not matched along with a family member, I search for them in the matched dataset found in step 2, where individuals were matched based on birth state and birth year. If found, I use that linkage and consider it a match and combine with the dataset compiled in the last step.

1.10.2 Details on Algorithms Used For Inter-Census Matching: From the 1870 to 1880 Census

Using the linked register record–1870 Census dataset as a baseline, I describe the steps I use to match the individuals from the 1870 to the 1880 Census in detail below:

1. Select the sample in 1870 to match to the 1880 Census

   (a) I first identify all Black families who were resided in counties with banks. From the 1870 Census – register data linked sample, I can note the depositor status of each family. In addition, for each family, I note the following characteristics: personal estate wealth, real estate wealth, family size, number of children under the age of 5, literacy status of the household head and occupation of the household head. These family level characteristics in 1870 are used as controls in my analysis. With the family characteristics and depositor status in mind, I tried to match all Black sons, ages 0 – 11 in 1870, who resided in counties with banks forward to the 1880 Census. There are 80,685 individuals in the sample that I attempt to link to the 1880 Census.

2. Matching with family member criteria – (mother, father, parents)

   (a) Block by birth state

   (b) Clean names in both datasets to remove non-alphabetic characters and account for common misspellings and nicknames

(c) Jaro-Winkler distance string match on both depositor's name and family member's name

(d) Keep if name distance > 0.9 and family member name distance > 0.9

(e) Last name distance > 0.9

(f) Initials should be the same for name and family member

(g) Middle name/middle initial should be the same for depositor and family member if present

(h) Iteratively match by windows of birth year if birth year is available, up to +/- 2 years window following ABE algorithm

i. If unique match on birth year, a pair is matched

ii. If there are multiple potential matches in the Census with same year of birth, the observation is discarded

iii. If there are no matches by exact year of birth, the algorithm searches for matches with +/- 1 year of reported birth using the steps outlined above. If there is a unique match, then we considered a pair to be matched. If this is unsuccessful, we expand the window and repeat this step iteratively until +/- 2 years.

iv. If none of the attempts produce a unique match for a depositor record where birth year is present, the observation is discarded.

3. Match with no family name criterion, birth state and birth year available

(a) Block by birth state

(b) Jaro-Winkler distance string match on full name

(c) Keep if name distance > 0.9

(d) Keep if last name distance > 0.9

(e) Keep if initials are the same

(f) Middle name/middle initial should be the same if present

(g) Iteratively match by windows of birth year if birth year is available, up to +/- 3 years window following ABE algorithm

(h) Drop matches if only first initial is present

4. I then compile the matched datasets based on the steps described above. Here I am compiling 4 datasets in total. 3 datasets are the matches found using mother name, father name, and both parents' names as additional match criteria. The last dataset is the matched result that didn't use names of family members as match criteria.

   (a) I first compile the datasets where family members' names were used in the matching criteria. In the rare cases where I observe one depositor observation matched to many different Census records, matches with names of both mother and father take priority.

   (b) If a child cannot be located in the 1880 Census with name of a family member as a matching criterion, I search for them in the dataset that only matched on name, birth state and birth year (dataset found using Step 3). If found, I use that linkage and consider it a match and combine with the dataset compiled in the last step.

1.10.3 Details on Algorithms Used For Inter-Census Matching: From the 1870 to 1900 Census

Using the linked register record–1870 Census dataset as a baseline, I describe the steps I use to match the individuals from the 1870 to the 1900 Census in detail below:

1. Select the sample in 1870 to match to the 1900 Census

   (a) This is the same sample of Black sons that I attempted to link to the 1880 Census. There are 80,685 individuals in the sample that I attempt to link to the 1900 Census.

2. Match with name, birth state and birth year

   (a) Block by birth state

   (b) Jaro-Winkler distance string match on full name

   (c) Keep if name distance > 0.9

   (d) Keep if last name distance > 0.9

   (e) Keep if initials are the same

   (f) Middle name/middle initial should be the same if present

   (g) Iteratively match by windows of birth year if birth year is available, up to +/- 3 years window following ABE algorithm

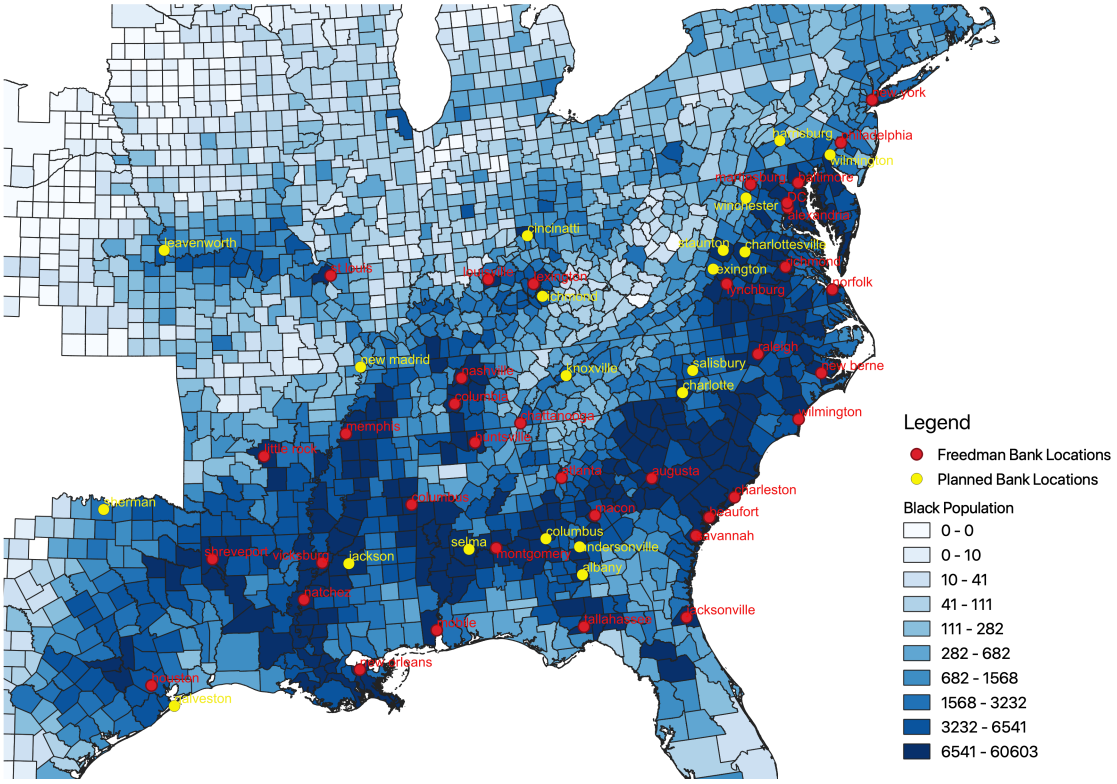
   (h) Drop matches if only first initial is present

1.10.4 Creating the Sample Used in Matching Exercise

The sample used in the matching exercise in Section 6 is created via inter-census linkage. The steps used to identify matches in the 1880 and 1900 are completely the same as the steps used in appendix Section B and appendix Section C. The only difference is that I select a different sample in 1870 to match forward to 1880 and 1900. I first identify all Black families who resided in counties in the trimmed banked sample (refer to Section 6) in 1870. The trimmed banked sample include a selected set of counties with banks, and a selected set of counties with planned bank openings. From the 1870 Census–register record data linked sample, I can note the depositor status and their family level characteristics for each child. With the family characteristics and depositor status in mind, I tried to match all Black sons, ages 0 to 11 in 1870, who resided in counties with banks forward to the 1880 Census. There are 82,891 individuals in the sample that I attempt to link to the 1880 and 1900 Censuses. I was able to link 19,582 individuals to the 1880 Census (match rate: 23 percent) and 8909 individuals to the 1900 Census (match rate: 11 percent).

1.10.5 Additional Figures and Tables

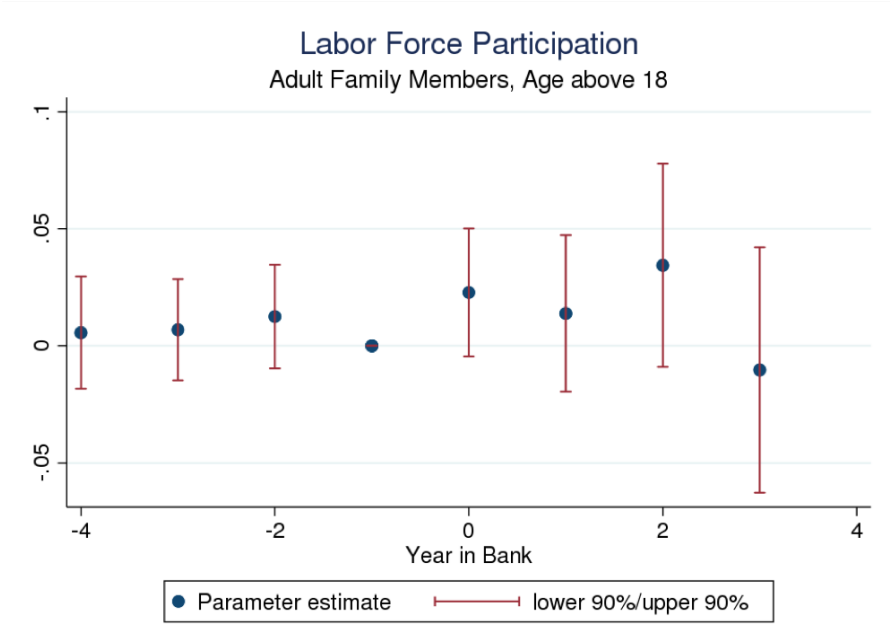
Figure 1.6: Locations of planned Freedman's Bank openings



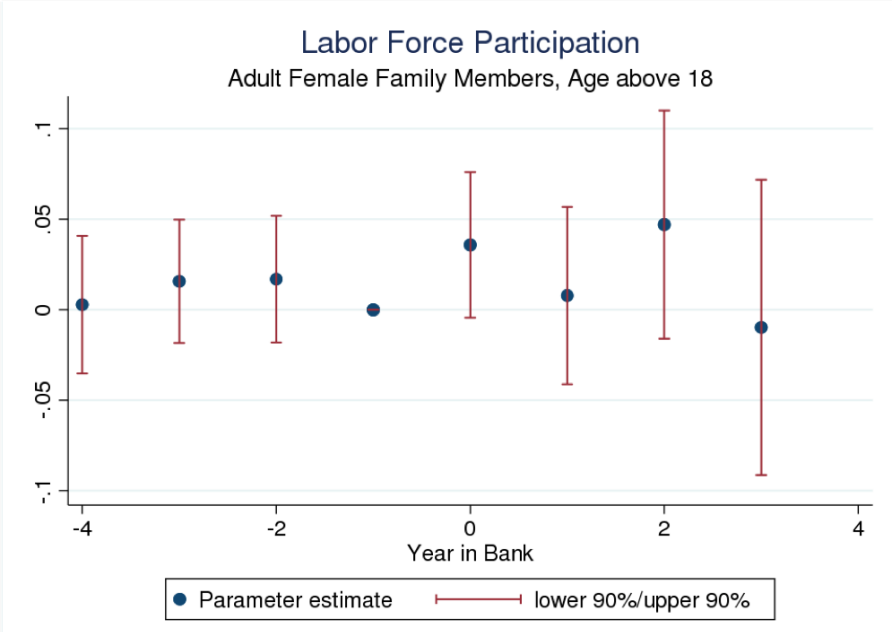
Notes: This figure shows the locations of Freedman's Bank. The map is delineated by county in 1870. The shades represent the number of Black residents within a county in 1870. Darker shades represent a larger number of Black residents.

Figure 1.7: Changes in adult family members around the time of account opening

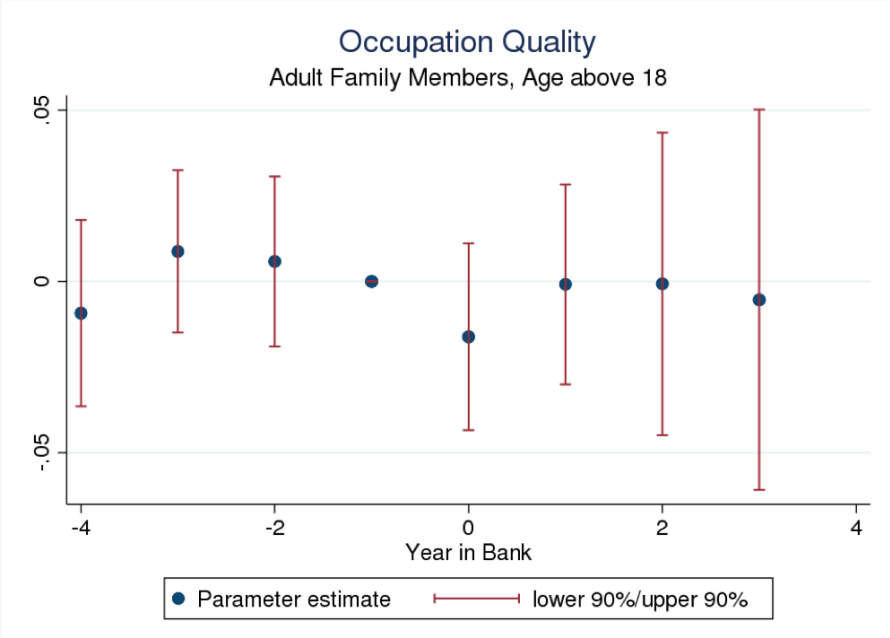
Panel A:



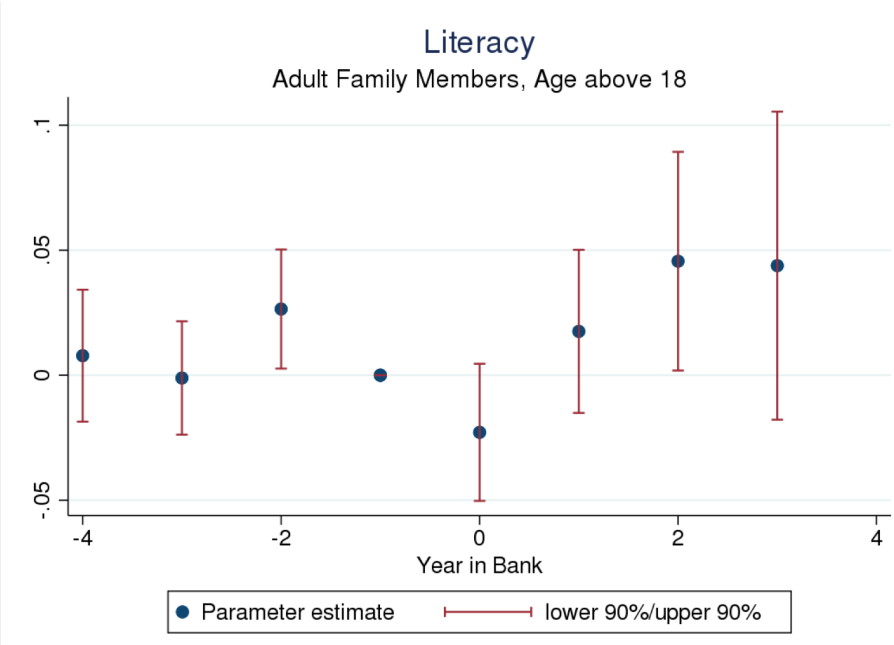
Panel B:



Panel C:



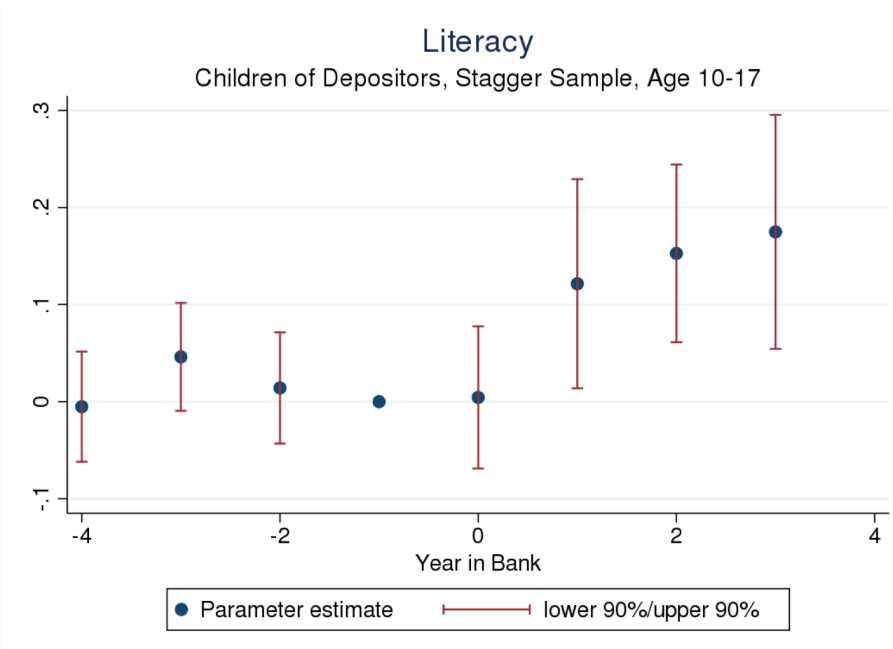
Panel D:



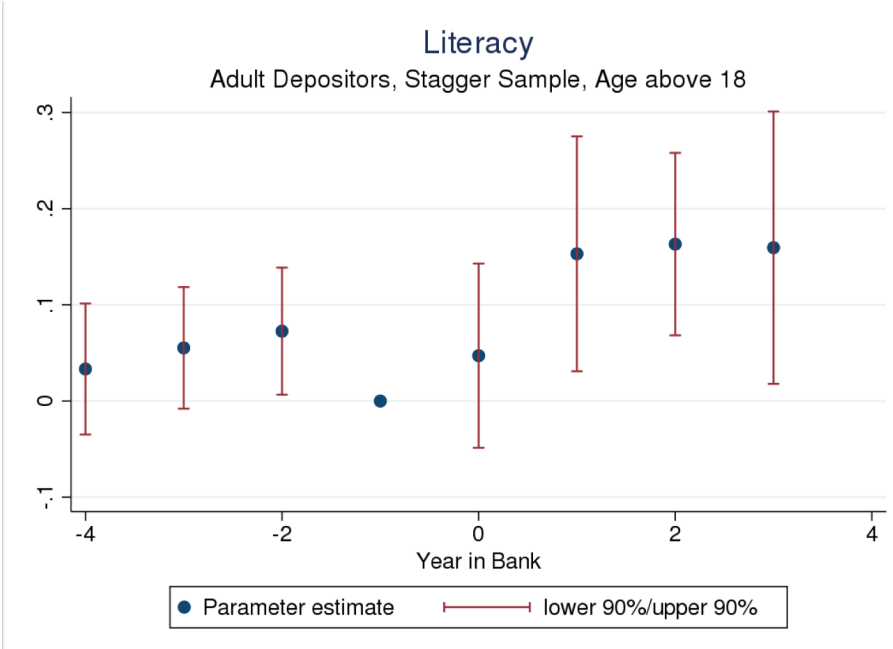
Notes: Panel A shows coefficients from a regression of labor force participation on a vector of leads and lags of years until bank account opening. Panel B shows coefficients from a regression of labor force participation on a vector of leads and lags of years until bank account opening on a sample of female household members. Panel C shows coefficients from a regression of an indicator variable that equals 1 if the person is in an occupation that earns more than a laborer on a vector of leads and lags of years until bank account opening. Panel D shows coefficients from a regression of literacy on a vector of leads and lags of years until bank account opening. The model includes state fixed effects, birth-cohort fixed effects, dummies that control for distance to the bank and gender dummies (see Equation (4) for details). All variables are measured in 1870. All adult family members are aged 18 and above in 1870. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Figure 1.8: Changes in literacy around the time of account opening for staggered sample

Panel A:



Panel B:



Notes: This figure shows coefficients from a regression of Equation (5) on a sample of children and depositors from depositor families who opened an account within the year of bank establishment. The children were ages 7-18 in 1880. The model includes bank site fixed effects, distance to the bank, gender dummies, mulatto status. All control variables are measured in 1870. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Figure 1.9: Average take-up rate by AMA connection

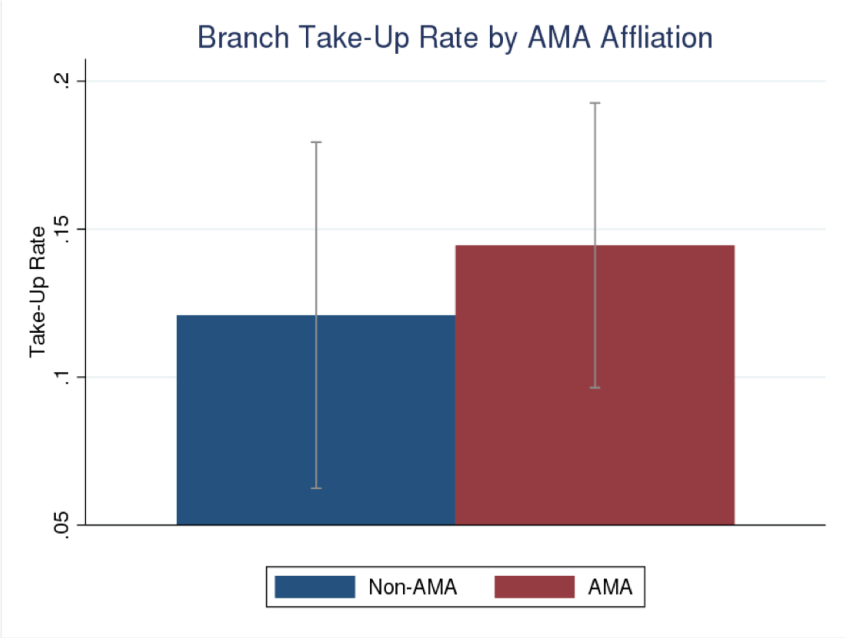
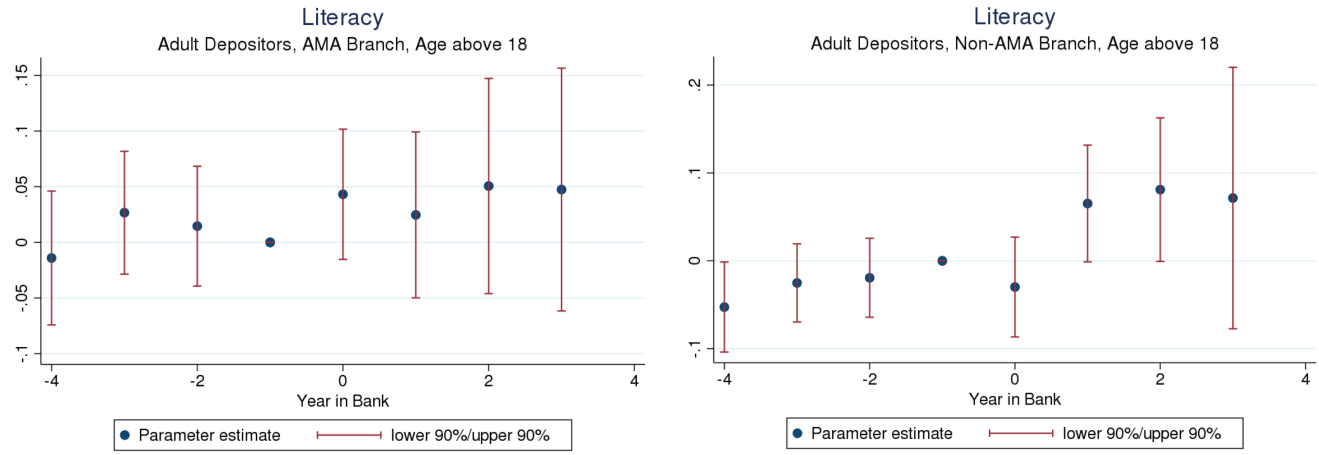
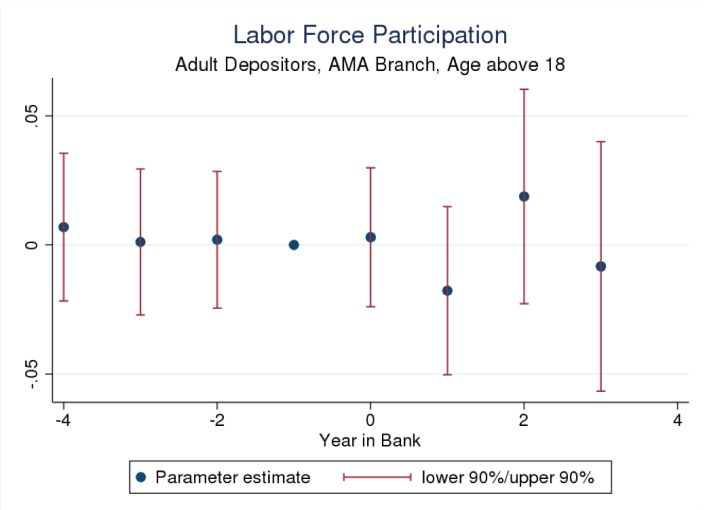


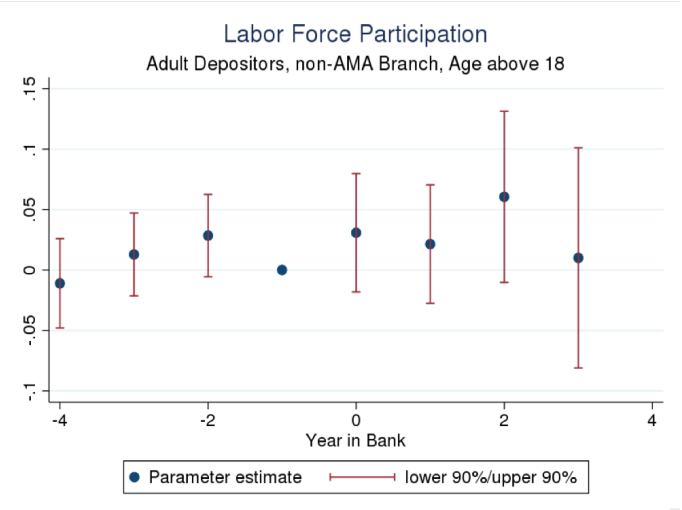
Figure 1.10: Changes in depositors' outcomes around the time of account opening for AMA branches vs. non-AMA branches

Panel A:

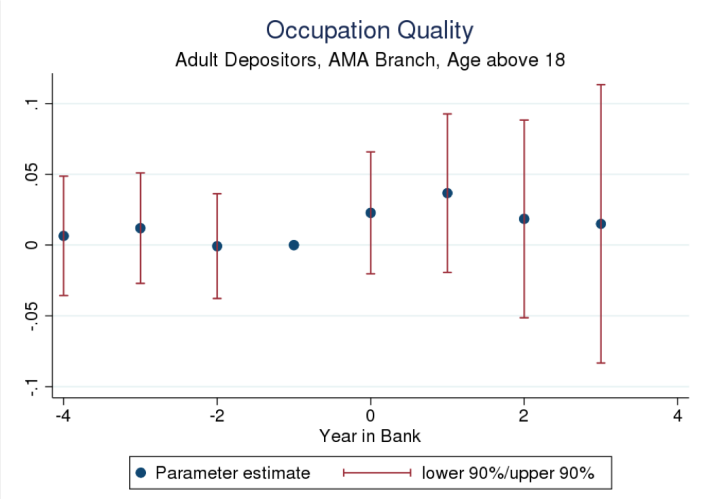


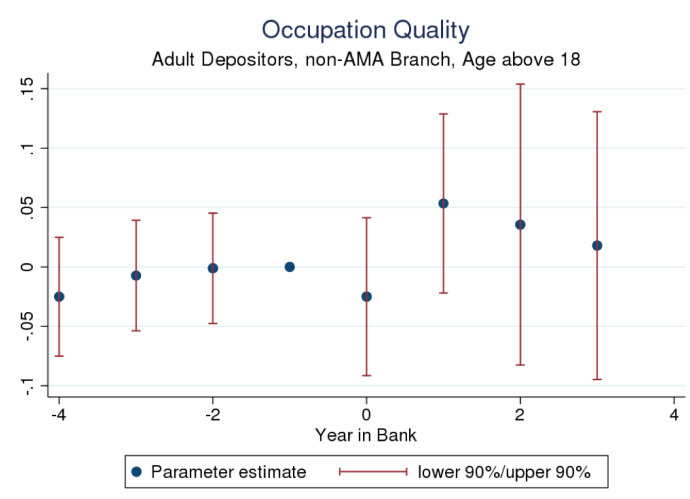
Panel B:





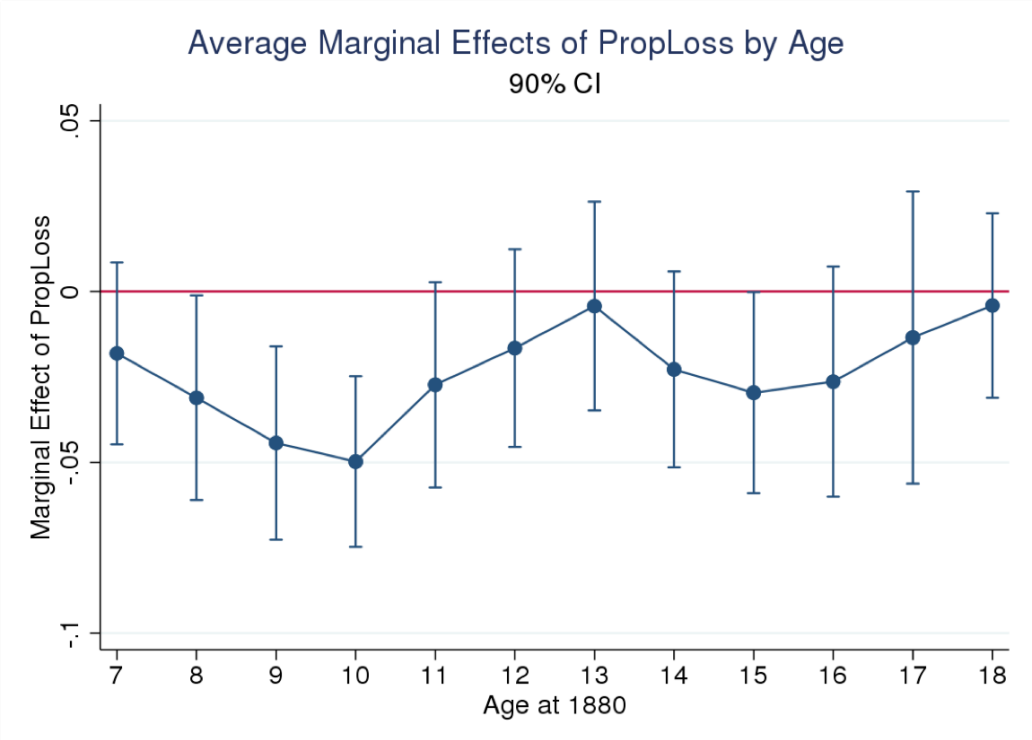
Panel C:





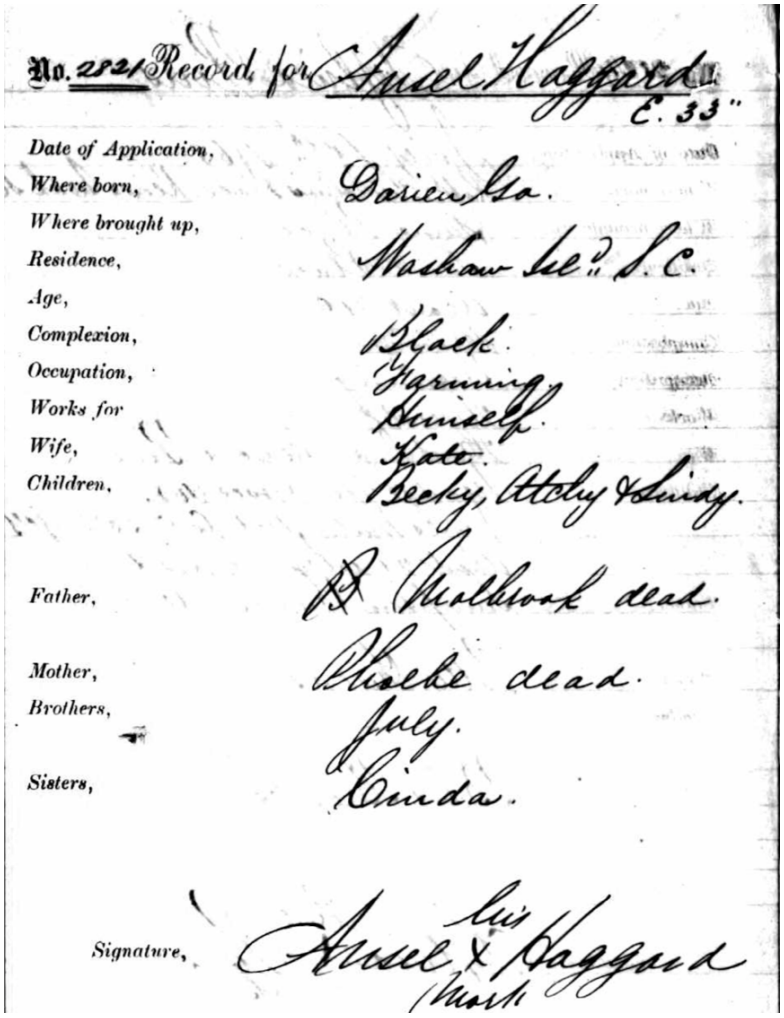
Notes: Each figure shows coefficients from a regression of the outcome variable on a vector of leads and lags of years until bank account opening. The model includes state fixed effects, birth-cohort fixed effects, dummies that control for distance to the bank and gender dummies (see Equation (4) for details). The top two figures restrict the sample to depositors at AMA affiliated branches. The bottom two figures restrict the sample to depositors at non-AMA affiliated branches. All variables are measured in 1870. Depositors are aged 18 and above in 1870. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Figure 1.11: Heterogeneous effect of proportion of wealth loss on children's school attendance by age



Notes: This figure shows coefficients from a regression of Equation (5) on a sample of children from depositor families of different ages. The children were ages 7-18 in 1880. The model includes bank site fixed effects, distance to the bank, gender dummies, mulatto status and family wealth quantile dummies and family head occupation dummies. All control variables are measured in 1870. Error bars are 90 percent confidence intervals of the estimate. Standard errors are clustered on the family level.

Figure 1.12: Sample of register records from the Freedman's Bank



No. 2921 Record for Ansel Haggard
C. 33"

Date of Application,
Where born, Darien Ga.
Where brought up,
Residence, Washaw Lee Co. S.C.
Age,
Complexion, Black.
Occupation, Farming
Works for Himself.
Wife, Kate.
Children, Becky, Atoly & Lindy.

Father, R Mallbrook dead.
Mother, Phoebe dead.
Brothers, July.
Sisters, Linda.

Signature, his Ansel X Haggard mark

Figure 1.13: Sample of ledger records from the Freedman's Bank

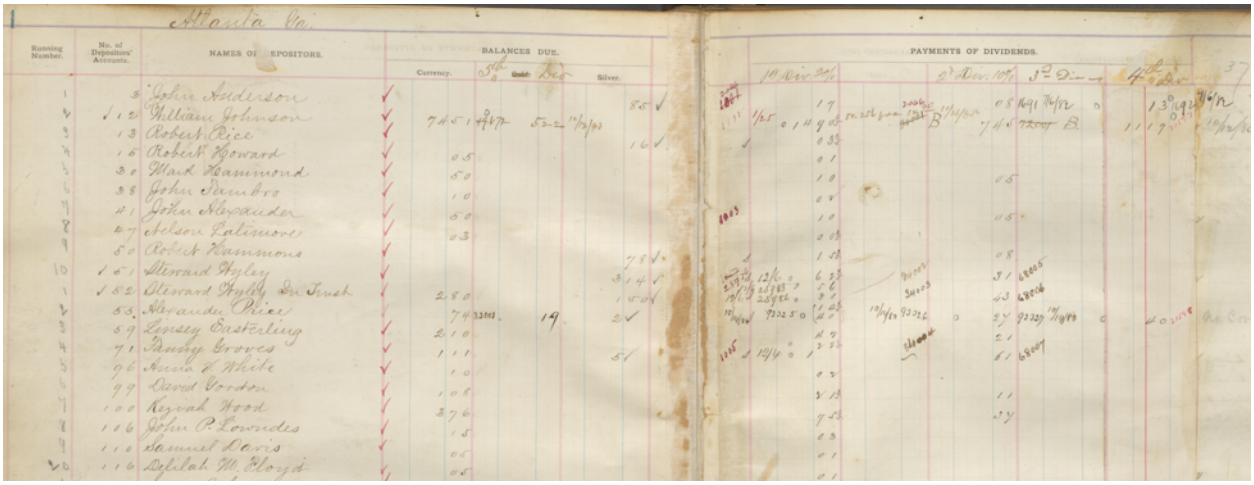


Table 1.7: List of bank branches

Bank Location		# Accounts	Register	Ledger	Open Year
Alexandria	VA	415	no	yes	N/A
Atlanta	GA	4518	yes	yes	1870
Augusta	GA	6701	yes	yes	1866
Baltimore	MD	6768	yes	yes	1866
Beaufort	SC	5989	yes	no	1866
Charleston	SC	16695	yes	yes	1866
Chattanooga	TN	N/A	no	no	1869
Columbia	TN	N/A	no	no	1870
Columbus	MS	927	yes	no	1870
Houston	TX	N/A	no	no	N/A
Huntsville	AL	1698	yes	no	1865
Jacksonville	FL	7215	no	yes	1866
Lexington	KY	1975	yes	yes	1870
Little Rock	AR	1358	yes	no	1870
Louisville	KY	7336	yes	no	1865
Lynchburg	VA	910	no	yes	1871
Macon	GA	3084	no	yes	1868
Martinsburg	WV	N/A	no	no	N/A
Memphis	TN	6298	yes	no	1865
Mobile	AL	9173	yes	no	1866
Montgomery	AL	N/A	no	no	1870
Nashville	TN	6189	yes	yes	1870
Natchez	MS	707	yes	yes	1865
New Bern	NC	4157	yes	yes	1866
New Orleans	LA	8569	yes	no	1866
New York	NY*	6943	yes	no	1866
Norfolk	VA	5424	yes	yes	1865
Philadelphia	PA*	3004	yes	no	1870
Raleigh	NC	N/A	no	no	1868
Richmond	VA	7691	yes	yes	1865
Savannah	GA	14558	yes	yes	1866
St Louis	MO*	N/A	no	no	1870
Shreveport	LA	1320	yes	yes	1868
Tallahassee	FL	1730	yes	yes	1866
Vicksburg	MS	8662	yes	yes	1865
Washington	DC	21401	yes	no	1865
Wilmington	NC	7266	yes	no	1868

Notes: * Bank branched located outside of the South, therefore excluded from analysis.

Table 1.8: Percent of depositors who reported personal information in the sample of depositors matched to the 1870 Census and their unmatched counterparts

	Unmatched	Matched	t-test
Percent Reported Spouse Name	37.72	54.17	38.55
	(48.47)	(49.83)	
Percent Reported Mother Name	54.65	82.17	77.71
	(49.78)	(38.28)	
Percent Reported Father Name	50.04	77.46	72.77
	(50.00)	(41.79)	
Percent Reported Children Names	28.73	39.62	26.27
	(48.91)	(48.91)	
Percent Reported Residence	23.85	28.79	12.80
	(42.62)	(45.28)	
Percent Reported Birth State	66.63	95.97	120.95
	(47.15)	(19.66)	
Percent Reported Age	33.28	56.24	54.30
	(47.12)	(49.61)	
N	60902	17290	

Notes: Summary statistics based on depositors in the register records from the Freedman's Bank. The sample exclude depositors who reported a non-Southern state of birth. The matched column indicates depositors who were matched to the 1870 Census. The unmatched column indicates depositors not matched to the 1870 Census. For the t-test columns, the null hypothesis of the t-test is that the matched group and the unmatched group have the same sample means.

Table 1.9: Difference in bank deposit amounts between depositors matched to the 1870 Census and their unmatched counterparts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Amount	Amount	Amount	Amount	Amount	Amount	Amount	Amount	Amount
Matched	-7.428***	-3.003	-4.398**	-2.868	-2.770	-6.952*	-2.560	-1.398	-0.417
	(2.320)	(3.484)	(2.152)	(2.480)	(2.269)	(3.775)	(2.408)	(2.920)	(0.600)
Sample	All	Spouse	Mother	Father	Parents	Children	Age	Birth State	Residence
N	11,625	5,555	8,060	7,484	8,383	4,526	9,069	5,643	3,055
R-squared	0.011	0.011	0.006	0.005	0.009	0.009	0.007	0.008	0.014

Notes: The sample is restricted to depositors in the register records, excluding those who reported a non-Southern state of birth, and who also appeared in the ledger records. The "matched" variable is an indicator variable that = 1 if a depositor was matched to the 1870 Census. Each column reports the difference in bank deposits for the matched sample and unmatched sample for different subsamples. For example, the spouse sample reports the difference in bank deposits for those who reported the name of their spouse in the register record. Robust standard errors are shown in parenthesis. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.10: Characteristics of Black population vs. characteristics of Black veteran population, based on 1870 Census

	Black		USCT Veterans	
	Overall	Depositor	Overall	Depositor
Pct Positive Wealth	20.94	26.08	31.25	27.76
	(40.69)	(43.91)	(46.35)	(44.83)
Wealth	111.67	184.41	181.49	214.97
	(3202.47)	(977.44)	(1383.63)	(1253.75)
Pct Positive Personal Estate	19.33	21.18	27.55	16.51
	(39.49)	(40.86)	(44.68)	(37. 17)
Personal Estate	54.61	66.68	110.15	104.53
	(1114.90)	(494.46)	(1048.23)	(832.22)
Pct Positive Real Estate	4.96	10.44	8.17	16.28
	(21.71)	(30.58)	(27.39)	(36.96)
Real Estate	57.06	117.74	71.33	110.44
	(2650.04)	(772.44)	(741.67)	(455.68)
Family Size	4.92	5.58	5.12	4.83
	(2.75)	(2.61)	(2.67)	(2.89)
Number of Children	2.59	2.83	2.45	2.24
	(2.11)	(2.05)	(1.97)	(1.97)
Pct Illiteracy	80.28	70.48	71.47	60.32
	(39.79)	(45.61)	(45.16)	(48.98)
Pct Farmer	63.77	46.49	61.60	40.14
	(48.07)	(49.88)	(48.64)	(49.07)
Pct Reside in County with Bank	15.60	49.46	31.98	66.06
	(36.29)	(49.98)	(46.64)	(47.41)
N	4494250	12258	14382	436

Notes: Summary statistics based on full count population taken from the following states: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. The veteran/USCT sample is compiled by the Early Indicators Project which were linked to the 1870 Census. The USCT sample include the Original USCT Sample (NIA P01 AG10120, PI: Fogel) and the Expanded USCT Sample(NIA P01 AG10120, PI: Costa). Standard deviations are reported in parenthesis.

Table 1.11: Percent of local Black population linked to depositor records

	Counties with Bank	Counties w/o Bank
Black	4.95	0.69
	(20.12)	(8.31)
Black Vet	7.02	1.51
	(25.55)	(12.21)
Black with Wealth	6.70	0.85
	(25.00)	(9.17)
Black Vet with Wealth	7.40	1.60
	(26.18)	(12.54)
Literate Black	5.64	0.78
	(23.07)	(8.78)
Literate Black Vet	8.34	2.25
	(27.66)	(14.82)

Notes: Summary statistics based on full count population taken from the following states: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. The veteran/USCT sample is compiled by the Early Indicators Project which were linked to the 1870 Census. The USCT sample include the Original USCT Sample (NIA P01 AG10120, PI: Fogel) and the Expanded USCT Sample(NIA P01 AG10120, PI: Costa). Standard deviations are reported in parenthesis.

Table 1.12: Summary statistics for sons in 1870 sample, linked 1870-1880 sample, and linked 1870-1900 sample

	1870 Sample		1870-1880 Sample		1870-1900 Sample	
	Non-Depositor	Depositor	Non-Depositor	Depositor	Non-Depositor	Depositor
Pct Positive Wealth	18.43	26.64	20.98	29.65	20.25	31.17
	(38.77)	(44.22)	(40.72)	(45.69)	(40.19)	(46.38)
Wealth	126.48	225.28	147.33	236.06	162.88	284.03
	(1006.79)	(978.96)	(1083.25)	(734.26)	(1330.34)	(935.32)
Pct Positive Personal Estate	14.74	17.76	16.99	20.09	16.97	21.56
	(35.45)	(38.23)	(37.56)	(40.09)	(37.54)	(41.18)
Personal Estate	51.13	63.56	59.38	66.86	64.28	64.56
	(489.60)	(381.07)	(512.26)	(205.31)	(669.43)	(180.70)
Pct Positive Real Estate	6.53	14.34	7.26	16.47	6.77	18.18
	(24.70)	(35.05)	(25.89)	(37.11)	(25.13)	(38.62)
Real Estate	75.36	161.72	87.95	169.21	98.60	219.47
	(723.63)	(856.95)	(837.67)	(643.99)	(961.13)	(848.44)
Family Size	5.35	5.78	5.49	5.88	5.38	5.90
	(2.06)	(2.05)	(2.05)	(2.05)	(2.10)	(2.10)
Children Under 5	1.25	1.23	0.89	0.93	1.26	1.30
	(0.91)	(0.93)	(1.33)	(1.27)	(0.91)	(0.97)
Age of Family Head	36.27	37.43	36.44	37.60	36.25	37.44
	(10.51)	(10.02)	(10.08)	(9.47)	(10.30)	(9.54)
Pct Illiterate	73.02	62.75	72.51	60.87	70.01	60.52
	(44.39)	(48.35)	(44.65)	(48.83)	(45.83)	(48.94)
Pct Farmer	39.27	22.18	46.32	23.79	35.57	18.70
	(48.84)	(41.55)	(49.87)	(42.60)	(47.88)	(39.04)
N	69989	3648	16289	1214	5817	385

Notes: Summary statistics based on matched samples. The sample includes all Black sons, aged 0-11 in 1870, who resided in a county with a bank with surviving registers from the following states: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. Percent illiterate denotes the percent of the head of households who were illiterate in 1870. Percent farmer denotes the percent of the head of households who reported "farmer" or "farm laborer" as their occupation. Standard deviations are reported in parenthesis.

Table 1.13: Comparing the linked sample to the unlinked population in the 1870 Census

	1870-1880		1870-1900	
	Unweighted	Weighted	Unweighted	Weighted
Positive Wealth	0.025	0.001	0.022	-0.002
	(0.003)	(0.003)	(0.005)	(0.005)
Wealth	23.294	4.089	43.266	9.732
	(8.435)	(7.885)	(16.519)	(14.290)
Positive PE	0.021	0.001	0.025	0.000
	(0.003)	(0.003)	(0.005)	(0.005)
Personal Estate	6.931	-0.958	14.313	2.524
	(3.890)	(3.432)	(8.156)	(6.894)
Positive RE	0.010	0.001	0.006	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)
Real Estate	16.363	5.047	28.953	7.207
	(6.522)	(6.237)	(12.068)	(10.350)
Family Size	0.213	0.019	-0.114	0.031
	(0.017)	(0.017)	(0.030)	(0.028)
Children Under 5	0.114	0.007	-0.033	-0.002
	(0.007)	(0.008)	(0.014)	(0.012)
Age of Family Head	0.350	0.053	-0.115	-0.014
	(0.084)	(0.087)	(0.162)	(0.134)
Illiteracy	-0.010	0.002	-0.034	0.003
	(0.004)	(0.004)	(0.006)	(0.006)
Farmer	0.024	0.003	-0.048	-0.009
	(0.004)	(0.004)	(0.006)	(0.008)

Notes: Sample include all Black sons resided in a county with a bank with surviving registers from the following states: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. There are approximately 18,000 cases that matched forward to 1880, and approximately 7000 cases that matched forward to 1900. Each row reports coefficients from a regression of a 1870 family level characteristic on an indicator for being in the matched sample. The unweighted columns show unweighted results and the weighted columns weight by the propensity of being matched $P(M_i = 1 \mid X_i)$, which is calculated from a probit of match status on the covariates above(X_i) and first name frequency, last name frequency and county dummies. Observations are re-weighted by $(1 - P_i(M_i = 1 \mid X_i))/P_i(M_i = 1 \mid X_i) * q(1 - q)$, where q is the proportion of records linked.

Table 1.14: 1870 county characteristics for counties with bank registers vs counties without bank

	Counties With Bank	Counties Without Bank
Pop Density	203.83	26.35
	(439.93)	(15.13)
Pct Black	52.02	40.49
	(18.68)	(21.50)
Pct Foreign	6.99	0.96
	(6.88)	(1.82)
Pct Emp Manuf	3.35	1.38
	(3.00)	(1.87)
# of Manuf Est	365.84	42.41
	(578.07)	(51.11)
Avg Manuf Output	13.17	6.42
(in thousands)	(7.80)	(6.35)
# of Farms	1510.64	874.98
	(1258.07)	(631.85)
Avg Farm Output	1.46	1.26
(in thousands)	(0.83)	(1.50)
Total Wealth	50.86	3.66
(in millions)	(85.77)	(3.96)
N	25	439

Notes: Summary statistics based on county level statistics in 1870 compiled by Haines (2010) . Only counties from Southern states are included: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. Counties with banks are defined as the 25 counties in the South with surviving register data. Counties without banks are defined as Southern counties that never had a Freedman's Bank branch. Standard deviations are reported in parenthesis.

Table 1.15: Correlation between 1870 county characteristics and county-level take-up rate of banking

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Pop Density	Pct Blk	Pct Foreign	Pct Emp Manuf	Avg Manuf Output	# Manufacturer	Avg Farm Output	Total Wealth	Banks
TakeUp	1241.65	-0.37	0.16	0.06	2.00	-301.54	-1776.40	667.97	0.04
	(1431.50)	(0.40)	(0.12)	(0.05)	(1.30)	(1169.36)	(1928.82)	(819.88)	(0.03)
N	25	25	25	25	25	25	25	25	25
R-squared	0.06	0.03	0.04	0.03	0.04	0.00	0.02	0.04	0.11

Notes: The sample is restricted to counties in the South with surviving register data. Southern states include: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. Take up rate of banking is defined as the total number of register records within each branch * 0.5 divided by total county Black population in 1870. The majority of 1870 county characteristics were taken from Haines(2010). County-level bank data were taken from Fulford(2015). Robust standard errors are shown in parenthesis. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.16: 1870 county characteristics for counties with bank registers vs. counties with planned bank openings

	Overall	Bank	Bank-Trim	Plan	Plan-Trim
Pop Density	26.35	203.83	43.05	38.26	39.94
	(15.13)	(439.93)	(25.26)	(13.23)	(12.72)
Pct Black	40.49	52.02	61.18	43.32	54.52
	(21.50)	(18.68)	(14.22)	(23.55)	(19.62)
Pct Foreign	0.96	6.99	3.87	2.88	1.34
	(1.82)	(6.88)	(3.24)	(6.13)	(0.87)
Pct Emp Manuf	1.38	3.35	2.21	1.88	1.98
	(1.87)	(3.00)	(1.52)	(1.84)	(2.13)
# of Manuf Est	42.41	365.84	106.33	95.38	99.91
	(51.11)	(578.07)	(72.57)	(62.90)	(72.36)
Avg Manuf Output	6.42	13.17	11.46	7.45	7.21
(in thousands)	(6.35)	(7.80)	(8.31)	(3.69)	(3.68)
# of Farms	874.98	1510.64	1410.80	1075.56	1115.36
	(631.85)	(1258.07)	(1280.04)	(659.33)	(594.34)
Avg Farm Output	1.26	1.46	1.55	1.67	2.05
(in thousands)	(1.50)	(0.83)	(1.12)	(1.55)	(1.75)
Total Wealth	3.66	50.86	12.71	8.84	8.83
(in millions)	(3.96)	(85.77)	(7.98)	(4.19)	(3.94)
N	439	25	15	16	11

Notes: Summary statistics based on county-level statistics in 1870 compiled by Haines (2010) . Only counties from Southern states are included: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. Counties with banks are defined as the 25 counties in the South with surviving register data. Counties with planned banks are the set of counties with planned bank openings. The trimmed sample of banked counties and counties with planned banks are produced by dropping counties where the number of African Americans in 1870 is less than 5000 (these are banks with planned openings), and greater than 35000(these are counties with banks) and population density in 1870 is greater than 100. Standard deviations are reported in parenthesis.

Table 1.17: County characteristics for counties with bank registers vs. counties with planned bank openings from 1860-1900

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Pop Density	Pct Blk	# Manuf	Avg Manuf Output	# Farms
Bank	0.818	0.0416	26.12	-3,501	-232.1
	(7.108)	(0.0693)	(27.67)	(7,862)	(149.2)
1(year=1870)	5.615	0.0342	53.36*	-13,177*	335.8
	(5.808)	(0.0810)	(27.26)	(7,286)	(224.5)
1(year=1880)	14.14**	0.0510	20.18	-8,560	1,292***
	(6.313)	(0.0829)	(23.70)	(8,139)	(335.8)
1(year=1890)	21.65**	0.0368	45.45**	-3,407	1,755***
	(8.892)	(0.0863)	(21.55)	(8,396)	(434.7)
1(year=1900)	29.39***	0.0121	81.45***	-3,237	2,535***
	(10.000)	(0.0908)	(21.70)	(8,128)	(606.8)
1(year=1870)*Bank	2.299	0.0250	-19.70	7,752	527.5
	(10.39)	(0.0980)	(39.87)	(8,227)	(405.4)
1(year=1880)*Bank	-1.018	0.0359	-18.05	8,376	-156.7
	(11.62)	(0.101)	(34.52)	(9,150)	(452.2)
1(year=1890)*Bank	-0.477	0.0359	25.41	3,673	-476.3
	(14.88)	(0.104)	(42.96)	(9,193)	(518.4)
1(year=1900)*Bank	-0.443	0.0365	11.21	5,194	-432.3
	(17.10)	(0.109)	(41.52)	(9,002)	(754.6)
N	130	130	130	130	130
R-squared	0.114	0.055	0.191	0.089	0.330

Notes: Summary statistics based on county-level statistics from 1860 to 1900 compiled by Haines (2010). The sample consists of only counties in the trimmed sample, which includes 15 counties with banks and 11 counties with planned banks. Counties with banks are defined as the 25 counties in the South with surviving register data. Counties with planned banks are the set of counties with planned bank openings. The trimmed sample of banked counties and counties with planned banks is produced by dropping counties where the number of African Americans in 1870 is fewer than 5000 (these are banks with planned openings), and greater than 35000 (these are counties with banks) and population density in 1870 is greater than 100. Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 1.18: Estimation Results of the Logit Model

	1870-1880 Matched Sample		1870-1900 Matched Sample	
VARIABLES	Coefficients	Std. Error	Coefficients	Std. Error
Illiteracy	-0.182***	0.058	-0.082	0.212
Age of Head Age	-0.002	0.002	0.007	0.009
Farmer	-0.506***	0.058	-1.353***	0.216
Family Size	0.137***	0.026	0.246***	0.088
# Children	-0.141***	0.032	-0.197*	0.113
Wealth dum1	0.350***	0.112	0.909**	0.443
Wealth dum2	0.337***	0.088	1.048***	0.316
Wealth dum3	0.349***	0.094	0.496	0.353
Wealth dum4	0.669***	0.075	0.868***	0.289
Mulatto dum	-0.026	0.067	0.020	0.229
County pop density	-0.018*	0.010	-0.060	0.040
County pct blk	0.971	1.049	-1.606	3.999
N	8675		2548	
Pseudo R-squared	0.168		0.171	

Notes: All variables are family characteristics measured in 1870. In both samples, the logit is estimated on sons who resided in a county with a bank within the trimmed sample. Illiteracy is a dummy variable which equals 1 if the head of household was illiterate. Farmer is a dummy variable which equals 1 if the head of household was a farmer. Family wealth is binned in the following way: a bin for 0 family wealth, a bin for family wealth between 1 and 100 inclusive, a bin for family wealth between 101 and 200 inclusive, a bin for family wealth between 201 and 400 inclusive and a bin for family wealth greater than 400. The logit regression also include birth cohort dummies. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 1.19: Balance test of matching covariates in propensity score matching

1870-1880 Matched Sample					
	Mean			t-test	
	Treatment	Control	Bias(%)	t	$p >\mid t \mid$
Wealth	207.12	195.42	5.65	-0.40	0.69
Personal Estate	70.72	75.69	-7.03	0.48	0.63
Real Estate	136.40	119.73	12.22	-0.68	0.50
Family Size	5.81	6.08	-4.65	1.24	0.21
# of Children	3.36	3.52	-4.76	0.95	0.34
Illit	68.84	66.39	3.56	-0.92	0.36
Farmer	39.70	39.53	0.43	-0.06	0.95
Head Age	37.04	37.55	-1.38	0.86	0.39
Mulatto	17.94	18.60	-3.68	0.30	0.77
1870-1900 Matched Sample					
	Mean			t-test	
	Treatment	Control	Bias(%)	t	$p >\mid t \mid$
Wealth	164.35	148.58	9.60	-0.31	0.76
Personal Estate	50.85	38.32	24.64	-0.99	0.32
Real Estate	113.50	110.26	2.85	-0.07	0.94
Family Size	5.94	5.94	0.00	0.00	1.00
# of Children	3.43	3.59	-4.66	0.81	0.42
Illit	72.35	75.88	-4.88	0.74	0.46
Farmer	37.65	38.82	-3.11	0.22	0.82
Head Age	37.76	37.37	1.03	-0.35	0.73
Mulatto	19.41	24.71	-27.31	1.18	0.24

Notes: All variables are family characteristics measured in 1870. Columns two and three report the means of the treatment and control groups respectively. Illiteracy is a dummy variable which equals 1 if the head of household was illiterate. Farmer is a dummy variable which equals 1 if the head of household was a farmer. Column "Bias(%)" displays the percentage deviation of the means of the treatment group from that of the control group: (treatment group mean - control group mean)/treatment group mean * 100. For the t-test columns, the null hypothesis of the t-test is that the treatment and control groups have the same sample means. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 1.20: Results based on matched sample

	(1)	(2)	(3)	(4)	(5)	(6)
	A. 1880 Outcomes			B. 1900 Outcomes		
VARIABLES	Literate	School	Labforce	Literate	Labforce	BetterOcc
Freedman	0.10**	0.07	-0.03	0.09*	-0.01	-0.02
	(0.04)	(0.07)	(0.02)	(0.05)	(0.02)	(0.05)
Mean	0.47	0.36	0.65	0.74	0.98	0.24
N	1,138	701	1,138	340	329	322
R-squared	0.09	0.09	0.42	0.07	0.04	0.10

Notes: The sample consists of children in the matched sample. The children were ages 10-21 in the 1880 Census and ages 30-41 in the 1900 Census. *Freedman* denotes an indicator = 1 if the son's parent was a depositor at the Freedman's Bank. Labor force is set as missing if no occupation is recorded. The outcome *BetterOcc* is an indicator = 1 if the son was in an occupation that has an occupation score greater than 20, which is the occupation score for a laborer. *BetterOcc* is set as missing if one is unemployed or no occupation is recorded. All specification include birth cohort fixed effects and 1870 family level fixed effects: wealth quartile, family head occupation, mulatto, family size, family head literacy status. The following 1870 residential county characteristics are also used as controls: population density, percent Black, percent Black as farmers, average farm output and average value of farm output. Robust standard errors are shown in parentheses. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.21: Characteristics of banked counties with different opening years

VARIABLES	(1) Pop Density	(2) Farm Output	(3) Manuf Output	(4) Total Wealth	(5) Pct Black	(6) Pct Foreign	(7) Banks	(8) Pct Blk Farmer	(9) Pct Blk Literate
1(OpenYr=1866)	83.98	0.66	-0.16	6.04	-0.04	0.03	1.04	-0.07	0.15
	(102.86)	(0.50)	(0.36)	(4.68)	(0.10)	(0.03)	(1.55)	(0.17)	(0.09)
1(OpenYr=1868)	-68.55	-0.14	-0.33	-1.56	0.07	-0.03	-0.90	0.14	-0.02
	(43.17)	(0.14)	(0.38)	(0.95)	(0.08)	(0.02)	(0.78)	(0.19)	(0.09)
1(OpenYr=1870)	25.02	0.50	0.37	1.75	-0.17	0.02	0.77	-0.05	0.04
	(39.15)	(0.42)	(0.54)	(1.97)	(0.10)	(0.04)	(0.94)	(0.17)	(0.04)
N	25	25	25	25	25	25	25	25	25
R-squared	0.07	0.09	0.09	0.11	0.12	0.07	0.05	0.04	0.17

Notes: The sample is restricted to counties in the south with surviving register data. Southern states include: AL, AR, DC, FL, GA, KY, LA, MD, MS, NC, SC, TN, TX, VA. The majority of 1870 county characteristics were taken from Haines(2010). County level bank data was taken from Fulford(2015). Robust standard errors are shown in parenthesis. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.22: Characteristics of depositors in ledger sheets vs. not in ledger sheets

	Overall		Charleston	
	No-Ledger	Ledger	No-Ledger	Ledger
Positive Wealth	21.42	20.87	11.92	15.59
	(41.03)	(40.64)	(32.44)	(36.30)
Wealth	183.96	131.84	74.87	104.21
	(1274.58)	(655.57)	(422.72)	(501.40)
Positive PE	16.45	15.62	7.79	10.58
	(37.08)	(36.31)	(26.83)	(30.77)
Personal Estate	58.44	45.32	12.63	21.78
	(628.62)	(303.14)	(53.14)	(83.41)
Positive RE	9.58	9.33	5.84	7.91
	(29.43)	(29.09)	(23.48)	(27.00)
Real Estate	125.52	86.52	62.24	82.43
	(1090.69)	(509.02)	(412.34)	(482.46)
Family Size	4.39	4.39	4.43	4.40
	(2.47)	(2.35)	(2.36)	(2.31)
Children Under 5	0.65	0.65	0.55	0.59
	(0.87)	(0.86)	(0.79)	(0.82)
Age of Family Head	35.92	36.83	37.17	38.06
	(12.46)	(12.29)	(12.86)	(12.83)
Illiteracy	68.50	67.29	53.01	58.91
	(46.46)	(46.92)	(49.97)	(49.23)
Farmer	44.33	37.43	25.30	36.41
	(49.68)	(48.40)	(43.53)	(48.15)
N	4146	5851	411	898

Notes: Summary statistics based on black depositors who were matched to the 1870 Census. Standard deviations are reported in parenthesis.

Table 1.23: Effects of proportion of wealth loss on depositor families with positive reported wealth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Children		B. Depositor		C. Adult Family		
VARIABLES	School	Labforce	Labforce	BetterOcc	Labforce	BetterOcc	Fem Labforce
PropLoss	-0.039**	0.007	0.021	-0.006	0.020*	-0.020	0.036*
	(0.015)	(0.018)	(0.013)	(0.025)	(0.012)	(0.015)	(0.018)
Mean	0.352	0.403	0.861	0.232	0.691	0.239	0.475
N	165	139	118	94	241	163	138
R-squared	0.446	0.499	0.435	0.216	0.583	0.214	0.238

Notes: The sample consists of Black depositors and their families who appeared in ledger sheers and were linked to the 1880 Census, and reported positive wealth in the 1870 Census. All regressions include a vector of controls that include bank site dummies, wealth quartile dummies, mulatto status, gender dummies, literacy status of household head, family head occupation category dummies and distance to bank. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.24: Effects of log proportion of wealth loss on depositor families

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Children		B. Depositor		C. Adult Family		
VARIABLES	School	Labforce	Labforce	BetterOcc	Labforce	BetterOcc	Fem Labforce
Log(PropLoss)	-0.035***	0.010	0.003	-0.030*	0.022**	-0.009	0.032**
	(0.012)	(0.013)	(0.009)	(0.018)	(0.010)	(0.014)	(0.016)
N	583	551	503	419	1011	709	677
R-squared	0.240	0.353	0.344	0.113	0.309	0.138	0.121

Notes: The sample consists of black depositor and their families who appeared in ledger sheers and linked to the 1880 Census. All regressions includes a vector of controls that include birth cohort dummies, bank site dummies, wealth quartile dummies, mulatto status, gender dummies, distance to bank, family head occupation category dummies and literacy status of household head. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.25: Effects of amount of wealth loss on depositor families

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Children		B. Depositor		C. Adult Family		
VARIABLES	School	Labforce	Labforce	BetterOcc	Labforce	BetterOcc	Fem Labforce
AmountLoss	-0.023***	0.007	0.000	-0.018*	0.004	-0.009	0.007
(in hundreds)	(0.004)	(0.011)	(0.006)	(0.009)	(0.006)	(0.014)	(0.010)
N	583	551	503	419	1011	709	677
R-squared	0.237	0.354	0.344	0.110	0.269	0.138	0.142

Notes: The sample consists of black depositor and their families who appeared in ledger sheers and linked to the 1880 Census. All regressions includes a vector of controls that include birth cohort dummies, bank site dummies, wealth quartile dummies, mulatto status, gender dummies, distance to bank, family head occupation category dummies and literacy status of household head. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.26: Heterogeneous effects of proportion of wealth loss on children by family size

	(1)	(2)	(3)	(4)
	A. Family Size < 5		B. Family Size ≥ 5	
VARIABLES	School	Labforce	School	Labforce
PropLoss	-0.009	-0.040	-0.032**	0.027
	(0.029)	(0.036)	(0.015)	(0.017)
N	280	254	303	297
R-squared	0.290	0.466	0.376	0.403

Notes: The sample consists of children from Black depositor families who appeared in ledger sheers and were linked to the 1880 Census. The children were ages 7-18 in 1880. Panel A consists of children from families with fewer than 5 members in 1870. Panel B consists of children from families with at least 5 members in 1870. All regressions include a vector of controls that include bank site dummies, wealth quartile dummies, mulatto status, gender dummies, literacy status of household head, family head occupation category dummies and distance to bank. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.27: Heterogeneous effects of proportion of wealth loss on children by local economic conditions

	(1)	(2)	(3)	(4)
	A. Low Growth		B. High Growth	
VARIABLES	School	Labforce	School	Labforce
PropLoss	-0.044**	0.010	-0.019	0.011
	(0.015)	(0.027)	(0.015)	(0.020)
N	210	202	373	331
R-squared	0.319	0.425	0.272	0.358

Notes: The sample consists of children from Black depositor families who appeared in ledger sheers and were linked to the 1880 Census. The children were ages 7-18 in 1880. Panel A consists of children from families who resided in counties with low economic growth rate. Panel B consists of children from families who resided in counties with high economic growth rate. Low (high) economic growth rate is defined as counties with ledger sheets with less (more) than median population density growth between 1870 and 1880. The bank locations classified as low growth are: Tallahassee, Savannah, Shreveport, Natchez, Vicksburg, New Bern, Beaufort and Charleston. The bank locations classified as high growth are: Lynchburg, Richmond, Norfolk, Atlanta, Augusta, Lexington, Baltimore and Nashville. All regressions include a vector of controls that include bank site dummies, wealth quartile dummies, mulatto status, gender dummies, literacy status of household head, family head occupation category dummies and distance to bank. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 1.28: Effects of proportion of wealth loss on children based on gender

	(1)	(2)
VARIABLES	School	Labforce
PropLoss	-0.025*	-0.006
	(0.015)	(0.016)
Female	0.125*	-0.327***
	(0.073)	(0.069)
PropLoss*Female	-0.007	0.021**
	(0.009)	(0.008)
N	583	481
R-squared	0.236	0.397

Notes: The sample consists of children of Black depositors who appeared in ledger sheers and were linked to the 1880 Census. The children were ages 7-18 in 1880. All regressions include a vector of controls that include bank site dummies, wealth quartile dummies, mulatto status, gender dummies, literacy status of household head, family head occupation category dummies and distance to bank. All control variables were measured in 1870. Standard errors are clustered at the family level. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Chapter 2

The Freedman's Bank and the Persistence of Mistrust

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Survey data show large and persistent racial gaps in the utilization of banks. Historians have long hypothesized that the collapse of the Freedman's bank in 1874 contributed significantly to the mistrust and underutilization of financial institutions by African Americans today. I will be the first to test this hypothesis using data. Using present day survey data, I find that African Americans are less likely to be banked if they reside in a county with higher exposure to knowledge of the bank collapse. In addition, for unbanked households, those who reside in a county with higher exposure to knowledge of the bank collapse are more likely to report "mistrust" in bank as the primary reason to be unbanked. Placebo effects are not present in the sample of white, Hispanic or foreign Black survey respondents, suggesting that the collapse of the Freedman's Bank can partly explain persistent gaps in the utilization of financial services by African Americans.

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2.1 Introduction

Differences in economic outcomes by race have persisted for centuries and continue up to the present day (Myrdal 1996, Duncan 1968, Collins and Margo 2011). For example, in 2016, the net worth of a typical white family, at $171,000, is nearly ten times greater than that of an African American family (McIntosh, Moss, Nunn and Shambaugh 2020). Why African American households have so much less wealth than whites remains a topic of intense discussion and debate. Blau and Graham (1990) found that, net of differences in income and other demographic factors, as much as three-quarters of the wealth gap remained unexplained. Proposed explanations for the sources behind the observed wealth disparity range from discrimination (e.g., Pager 2003, Eberhardt, Goff, Purdie and Davies 2004, Bertrand and Mullainathan 2004) to education (Hamilton and Darity 2017).

One possible reason for the large and persistent wealth gap relates to differences in savings instrument (Charles and Hurst 2002, Chiteji and Stafford 1999, Thompson and Suarez 2015). Specifically, African Americans are less likely to hold a checking account as compared to white households with similar levels of income and other demographic characteristics. According to the 2015 Federal Deposit Insurance Corporation (FDIC) National Survey of Unbanked and Underbanked Households, 18.2 percent of African Americans were shown to be unbanked[2] compared to 3.1 percent of Caucasians. Aside from economic reasons to not keep a bank account (which was the most commonly cited reason for not having an account, at 52.7 percent), almost one-third of unbanked households cited mistrust as a reason to be unbanked.

In this paper, I trace the historical roots of mistrust in banking institutions in the African American community by studying the persistent impact of a watershed event, the collapse of the Freedman's Bank. The Freedman's Bank was a savings bank established for African Americans after the Civil War, and for most African Americans, this bank was their first exposure to the banking institution. To attract depositors, the bank used dubious advertisement methods. It purposely misled depositors into believing that their savings were backed

[2]A household is considered "unbanked" if no one in the household has a checking or savings account

by the federal government. This advertisement effort was a success. During its years of operation, approximately 1 in 8 African American families held an account at the bank. However, members of the board of trustees of the bank, all white men, used the deposits of the freedman to invest in speculative enterprises. The mismanagement of the bank, coupled with the Panic of 1873, culminated in the bank's failure in 1874. Historians have estimated that more than half of the accumulated Black wealth disappeared through the mismanagement of the Freedman's Bank (Baradaran 2017)[3].

While all historians and commentators agreed upon the tragic impact of the Freedman's Bank, they held differing views on its long-term legacy. Although some historians (e.g., Harris 1968) held the view that the failure did not leave a damaging legacy, the majority of historians long hypothesized that its failure left the African American community to view most banking institutions with suspicion and mistrust. I will be the first to test this hypothesis using data and contribute to this debate. Specifically, I examine the effect of the failure of the Freedman's Bank on trust and participation in banks within the African American community today. In order to measure exposure to the knowledge of the bank collapse, I utilize surviving register records from the Freedman's Bank. From the records, I construct county-level take-up rate in banking, which is used as a proxy for the degree of exposure to the knowledge of the bank failure. Formally, the take-up rate is defined as the share of the county African American population who opened a bank account at the Freedman's Bank[4]. For the take-up rate to be a valid proxy, I assume that counties with a historical higher take-up rate experienced a higher level of exposure to the knowledge of the bank collapse.

I use data from the Current Population Survey (CPS) Un(der)banked Supplement from 2009 to 2017. Using a sample of African American households who currently reside in counties that had a Freedman's Bank, I test whether households who currently reside in a

[3]Throughout this paper, I will use the terms African Americans and Black interchangeably when referencing African Americans in the late 19th century. The Freedman's Bank was in existence from 1865 to 1973. In that time period, the vast majority of Blacks in the U.S. are African Americans.

[4]The county African American population was measured in 1870, which was four years prior to the collapse of the bank.

county with historically higher take-up rate are less likely to be banked today. I find that households who reside in a county with 10 percentage points higher historical take-up rate are 1.68 percentage points less likely to be banked today compared to households with the same demographic characteristics[5].

To analyze the effect of historical take-up rate on mistrust in banks today, I use a multinomial logit regression to model a household's decision to not open a checking or savings account. For unbanked households, those who reside in a county with 10 percentage points higher take-up rate are 2.78 percentage points more likely to report "mistrust" in bank as the primary reason to be unbanked. The take-up rate only seems to be predictive of mistrust; it does not significantly predict a household to be unbanked due to economic reasons. Even though I do not claim the relationship between historical take-up rate and one's propensity to bank to be causal, I show that the results are robust to controlling for a large number of contemporary and historical county-level characteristics, such as institutional quality, education, historical presence of slavery and residential segregation. In addition, I show that the relationship between the take-up rate and banking decisions do not exist for whites, Hispanics or foreign Blacks. Given that African Americans were disproportionately affected by the failure of the Freedman's Bank, historical take-up rates should not correlate with banking decisions of whites, Hispanics or foreign Blacks. In this falsification exercise, I find the correlation to be close to zero and statistically insignificant.

After establishing that there exist a persistent correlation between the failure of the Freedman's Bank and contemporary banking decisions made by African American respondents, I investigate whether this relationship can be mitigated by personal factors such as higher education, higher income, or community factors such as the presence of Black-owned banks. While I do not find that having higher levels of education or income change the relationship between take-up rate and banking decisions, I do find the presence of black banks to be an important factor. The results show that African Americans residing in a county with a

[5]Demographic controls include household income, family size, family composition type, education level of the household head, labor force participation of the household head.

Black-owned bank are less impacted by the failure of the Freedman's Bank, suggesting that the unwillingness to bank within the African American community can be mitigated by the presence of Black-owned banks.

Following Nunn and Wantchekon (2011), I decompose the effect of the Freedman's Bank collapse into two channels: its effect on factors internal to the individual, and its effect on factors external to the individual. I do so by constructing a second proxy to measure the knowledge of the bank failure. Specifically, this measure quantifies the exposure of each non-Southern county to news of the bank collapse. Historically, the vast majority of African Americans resided in the South. Hence, African American households that do not reside in the South today either movers during the Great Migration or descendants of the movers. By focusing on African American households who do not reside in the South today, I aim to separate out the effect of the bank failure on trust working through internal factors that are geographically mobile – such as individuals' internal beliefs and values – versus through external factors that are less geographically mobile, such as institutional structures. To construct this second measure, I rely on random variation generated by historical railroad network used during the Great Migration interacting with the locations of the Freedman's Bank branches. The measures exploit two sources of variation: proximity to a rail line and the number of collapsed branches/bank accounts along that particular rail line. I find that respondents who live in a non-Southern county with higher exposure of the bank failure are less likely to be banked. In addition, if they are unbanked, those that live in an area with higher exposure are more likely to report "mistrust" as the primary reason to be unbanked. The results suggest that the relationship between the bank failure and contemporary unwillingness to bank arises partially from a change in internal norms and beliefs caused by the failure itself.

This paper builds on and contributes to several strands of literature. First, my findings elucidate factors that influence the demand for banking among African Americans. Research has pointed to discrimination in the credit market (Munnel et al. 1996, Blanchflower et al. 2003), paucity of banks in the local community (Wang and Zhang 2020, Toussaint-Comeau

et al. 2019) and parental influence for investment choices (Chiteji and Stafford 1999) as reasons for un(der)banking in the African American community. In addition, studies have shown that the decision to be unbanked often centers around negative banking experiences or perceptions fostering mistrust toward mainstream financial institutions (Rhine et al. 2006). In this paper, I present evidence that mistrust generated by a historical episode may be another important demand constraint and that this mistrust can be directly linked to the choice to be unbanked.

Other studies, mostly focused on developing economies, have explored the role of mistrust as a limiting factor for the take-up and active use of bank accounts. Bachas et al. (2017) and Dupas et al. (2016) demonstrated that mistrust is an important reason why individuals choose to stay out of the formal banking system. In addition, low trust could explain the low participation rates and low account usage in experiments that offered savings account with no fees or minimum balance requirements. While it was not proven empirically, Dupas et al. (2016) suggested that banking scandals in Kenya decades ago potentially generated mistrust that limit the extent to which people seek out information about available financial services today.

This paper is related to a broader literature on the historical origin of present-day conditions. [6] More specifically, my paper is directly related to the literature that evaluates the historical roots of (mis)trust. On the theoretical side, Giuso et al. (2009) outlined how beliefs can be updated and transmitted through generations. Empirically, various studies have shown that modern cultures of (mis)trust in medicine (Lowes and Montero 2021, Alsan and Wanamaker 2018), in the judicial system (Becker et al. 2016) and in other members in the community (Nunn and Wantchekon 2011) have historical roots. Trust has been shown to matter for economic development in a variety of settings (Greif 1989, Knack and Keefer 1997, Fafchamps 2006, Algan and Cahuc 2010), Hence, understanding this historical process can be used to gain insights and inform public policies. For example, Lowes and Montero (2021)

[6] Studies have shown that current day gender roles (Alesina, Giuliano and Nunn 2011), fertility (Fernandez and Fogli 2009), voting behavior (Williams 2017), and even inter-ethnic violence (Voitgtlander and Voth 2012) can trace their roots back to historical events or practices.

have shown that historical negative experience with the health sector affects health-seeking behavior and mistrust in medicine in future generations. In addition, they showed that World Bank projects in the health sector are less successful in areas with greater historical exposure of medical malpractice. I contribute to this literature in several ways. First, my paper is the first to address the root of mistrust in the financial institution, an important but previously unstudied topic. Second, I am the first to test the hypothesis of whether the failure of the Freedman's Bank has persistent impact on mistrust in the financial institution.

The remainder of the paper proceeds as follows: Section 2 provides the historical background on the creation and failure of the Freedman's Bank. Section 3 outlines the conceptual framework behind the persistence of beliefs and norms. Section 4 presents the data. Section 5 describes the estimating equation and results that show the relationship between the Freedman's Bank failure and banking decision today. Section 6 presents the multinomial logit regression to model the reasons to be unbanked. Section 7 examines whether the bank failure impacted banking decisions through internal cultural norms. Section 8 concludes.

2.2 Historical Background

On March 3rd, 1865, a bill to establish the Freedman's Bank, formally known as The Freedman's Savings and Trust Company, was passed by Congress and presented to President Abraham Lincoln. The object of incorporation, as stated in the Freedman's Bank Act, was to receive deposits offered "by or in behalf of person heretofore held in slavery in the United States, or their descendants." Even though the bill to establish the bank was passed by Congress, the Freedman's Bank was nationally chartered private bank not affiliated with the federal government. At the time of incorporation, the Charter stated that at least two-thirds of the deposits must be invested in United States securities, and the rest of the deposits be kept as "available funds" to be used for current needs. In addition, the books of the bank were to be available for inspection by Congress at all times. Hence, according to the provisions of the original Charter, no misuse of bank funds seemed possible. On the same day, the bill

was signed by President Lincoln, and the organizers soon began plans to establish the bank and solicit deposits.

The incorporators' original plan was to establish headquarters in New York City with branch banks in each Southern state. Organization and expansion proceeded rapidly, and by the end of 1865, 10 branches had been organized. The expansion proceeded from 1865 to 1870, by which time 37 branches had been established in total, 33 of which were in the South[7]. Within a few years, the total number of depositors reached 70,000 with $57 million (adjusted for inflation) in deposits.

In the process of growing the assets of the bank, local bank officers tried many ways to promote the bank, often deceitful. In addition to simply advertising the bank's benefits in newspapers, advertisements would tout and exaggerate its connection to notable government officials. For example, bank advertisements included phrases such as "Abraham Lincoln's Gift to the Colored people, his signature to the Bill one of the last acts of life", and quotes by Major General Howard which said, "I consider the Freedman's Savings and Trust Company to be greatly needed by the colored people, and have welcomed it as an auxiliary to the Freedman's Bureau"(Davis 2003). From General Howard's quote, one can see that the Freedman's Bank was closely affiliated with the Freedman's Bureau. Unlike the Freedman's Bank, the Freedman's Bureau was a government agency that was created to assist newly liberated freedman and to undertake the general relief efforts following the Civil War. Bureau offices and branch banks were often in the same room and bank officials often wore the Union Army uniform to solicit deposits. In promoting the bank's connection with the Bureau and notable government officials, many depositors were led to believe that the Freedman's Bank was under the guarantee of Congress. This notion is echoed by the Douglass Report (1876): "In regard to this bank 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."

[7]The branches located in New York, Pennsylvania, West Virginia and Missouri were not considered Southern.

Despite its rapid expansion and popularity, problems began to plague the Freedman's Bank after 1870. In 1870, the Charter was amended such that one half of the portion of deposits formerly invested in United States securities might be invested in notes and bonds secured by real estate mortgage. Soon after the Charter amendment, the resources of the bank were tied up in poor quality loans such that it was impossible to realize upon them without a long delay. Moreover, the board of trustees saw a steady decay in its moral character. Jay Cooke, a prominent financier and railroad bond speculator, was in control of the finance committee of the Freedmen's Bank. Due to his connections, he was able to borrow at one time $500,000 of the freedmen's deposits. Other members of the finance committee, along with Cooke, treated the Freedmen's Bank as a dumping ground for their own bad private claims and those of friends.

The mismanagement of the bank, coupled with the Panic of 1873, culminated in the bank's failure in 1874. Alarmed by the Panic of 1873, depositors requested withdrawals, which resulted in bank runs at several branches[8]. Since the bank made loans of questionable character, they were unable to realize these loans to meet the needs of the depositors. To try to deal with the crisis, the Freedman's Bank had to sacrifice its best securities and borrow at ruinous rates. However, a full report of the Comptroller of the Currency was released soon after the crisis, revealing the dire state of the bank. Realizing that the bank was beyond redemption, the bank president and the trustee closed the bank on June 28, 1874.

At the time of the bank failure, $2,993,790.68 was due to 61,144 accounts. The bank only had $400 of United States securities in the vaults and $31,689.35 in cash from the branches. As soon as it was seen that the bank had failed because of improper management, a widespread demand arose that the government reimburse the depositors. From 1875 to 1883, the bank went into liquidation and commissioners made efforts to provide dividends for the depositors. In the process, a portion of depositors were able to receive 3/5 of their original deposits. However, the majority of small deposits were never claimed. And, many depositors

[8]The authorities were forced to require the depositors to give 60 days or more notice before drawing out deposits

believed that the dividends paid to them by the commissioners were simply interest on their deposit, and that sooner or later their original deposits will be returned to them.

In later years, many historians [9] took the view that the failure of the bank was disastrous for the African American community. W.E.B. DuBois once said, "Not even ten additional years of slavery could have done so much to throttle the thrift of the freedmen as the mismanagement and bankruptcy of the series of savings banks charted by the Nation for their special aid."

2.3 Conceptual Framework

For the African American community, the failure of the Freedman's Bank was a watershed event. The bank itself was an important institution because the this was the first bank that was created to primarily serve the African American community. For most depositors, this was their first contact with any banking institution. Its failure, which destroyed more than half of accumulated Black wealth at that time, was a traumatic event that destroyed trust in banking institutions. In the years following the bank failure, social scientists have pointed to the failure of the Freedman's Bank as a reason African Americans underused and remained wary of banking institutions[10].

Considering that cultural beliefs are "rules-of-thumb" that develop to aid decision-making, it's natural to hypothesize that the large cost suffered from the failure of the Freedman's Bank increased the return to rules-of-thumb based on mistrust relative to rules-of-thumb based on trust, resulting in the development of a culture of greater mistrust in all financial institutions (Boyd and Richerson (1985, 2005). In this paper, I test whether the failure of the Freedman's Bank created persistent mistrust and underutilization of banks in the African American community over 100 years after the event.

There are several explanations for this persistent behavior. First, according to the theo-

[9] W.E.B. Du Bois in "The Souls of Black Folk," Benjamin Brawley in "Short History of the American Negro," and Booker T. Washington in "Story of the Negro"

[10] See Osthaus (1976) and Baradaran (2017)

ries outlined by Guiso, Sapienza and Zingales (2008) and Tabellini (2008), parents encourage traits in investing that they feel are useful for the next generation. For the families who lost wealth due to the bank failure, parents find it beneficial to teach their children to be wary of banks. Hence, the persistence of mistrust towards banks may reflect the high level of mistrust among families that were the most impacted by the bank failure.

Another potential explanation is that the Freedman's Bank failure, partially caused by the Panic of 1873, resulted in poorer institutional quality in banks. The underdevelopment of the Southern banking institution in the years immediately following the Civil War is unsurprising. But even up to and beyond the turn of the 20th century, the South lacked an efficient system for banking and credit. Compared to other regions in the country, the South had significantly fewer banks per capita (James 1981). The failure of the Freedman's Bank and the Panic of 1873 could have hindered the recovery of the Southern financial system and contributed to its continuing relative underdevelopment. Hence African Americans are distrustful of banks, and underbanked as a result, not directly because of the bank failure, but due to the comparatively poorer quality of the local banking institutions.

It is also possible that our results arise because the failure of the Freedman's Bank impeded the development of Black banking. In a summary report written by Arnett Lindsey on the state of Black banking in the United States fifty years after the failure, he stated "the so-called governmental aid which was given in establishing the Freedman's Bank proved to be an almost insurmountable obstacle for the Negroes who later attempted to organize banks of their own" (Baradaran 2017). In the period following the Civil War, the majority of non-minority-owned banks in existence were unwilling to provide basic financial institutions (Ammons 1996). Hence, banks established by African Americans served as the major outlet for African Americans to gain access to loans and other banking services[11]. If failure of the Freedman's Bank was a hindrance to the development of Black banks in the U.S., then it could have contributed and prolonged the feeling of mistrust towards banking institutions within the African American community.

[11]The first Black bank was established in 1888, 14 years after the failure of the Freedman's Bank.

In this paper, I will not be able to distinguish between these finer transmission mechanisms. Instead, my aim is to empirically investigate whether there exists any relationship between the failure of the Freedman's Bank and bank utilization and mistrust in the African American community today. In addition, I explore whether this relationship is partially based on beliefs and rules-of-thumb, which are internal to the individual and transmitted from parents to children through generations.

2.4 Data

2.4.1 Outcome Variable

I measure the household's banking behavior using data from the Un(der)banked Supplement to the Current Population Survey (CPS). The Un(der)bank Supplement was conducted by the Federal Deposit Insurance Corporation (FDIC) in partnership with the U.S. Census Bureau. Its primary purpose was to estimate the percentage of U.S. households that do not have a bank account and to identify the reasons why households are not participating in banking institutions.

The supplement was first added to the January 2009 CPS basic monthly survey. In 2011, 2013, 2015 and 2017, it has appeared as a supplement to the June CPS basic monthly survey. My sample included all households who answered the Un(der)banked Supplement across these years who can be geographically located within a metropolitan area or a county[12] . Within the Un(der)banked Supplement, I focus on the following two questions:

1. Do you or does anyone else in your household have a checking or savings account now?

2. What is the main reason why no one in your household has an account?

The answer to the first question reflects the household's decision to participate in banking in-

[12]Metropolitan areas (CBSA) are mapped to a county using the CBSA to FIPS county crosswalk provided by NBER: `https://data.nber.org/data/cbsa-fips-county-crosswalk.html`. For the rest of my analysis, I limit my sample to households who live in the central part of the metropolitan area. Hence, I create a one-to-one mapping from CBSA to county.

stitutions[13]. For unbanked households, the survey then asked for the reason to be unbanked. For the second question, the survey provided the following choices:

1. Do not have enough money to need a bank account
2. Do not trust banks
3. Other reasons/none of the above

Out of the many reasons that the respondent can choose from to answer Q2, "Don't trust banks" was always listed as one of the choices[14]. Hence, within the unbanked households, I use the answer to the second question to gauge whether their decision was based on distrust in the banking institution.

2.4.2 Control Variables

While the CPS Un(der)banked Supplement contains many household level characteristics (such as income, education level, employment status...) that influence the demand to open a bank account, we know little regarding the "supply" of banks in the respondents' place of residence. FDIC publishes information on the location of all banks and their branches for all survey years. I use the total number of banking offices [15] aggregated at the county level to capture the availability of banks.

Aside from bank availability in the locality, there are concerns that other place-based characteristics would also influence a family's choice to bank. Alesina and Ferrara (2000) suggest that racial heterogeneity of the community strongly influences trust. Hence, I obtain county-level data from the 2010 Census to control for potential contemporary factors, such as urbanization and racial composition, that could influence both mistrust in bank and banking decisions.

[13]I limit my sample to heads of households. Hence for the rest of the paper, the respondent is answering the survey question for the rest of his/her household.

[14]This supplement has revised the reasons respondents can choose from to answer Q2 many times over the years. In the main analysis, I harmonized the choices offered across years.

[15]including only brick and mortar offices, excluding retail offices, cyber offices and partial service offices

Grosjean (2014) show that the persistence of cultural norms depends on the quality of formal institutions. Following Grosjean (2014) and Williams (2017), I take the number of newspapers per capital, first recorded in the 1840, to act as a measure of general historical institutional quality. However, there exist vast differences in the institution quality experienced by white and African Americans. Formal and cultural institutions, such as Black codes, racial violence and Jim Crow, were created to exclude African Americans from participating in banking. I use two measures in order to capture the degree of historical institutional barriers faced by African Americans. First, I use the proportion of slaves in 1860 as a proxy for racist institutional structures[16]. Second, a measure created by Logan and Parman (2017) is included to capture the degree of segregation on the county level in 1880 and 1940.

2.4.3 Summary Statistics

Table 2.1 displays the contemporary and historical characteristics of the counties that were surveyed as part of the CPS Un(der)banked Supplement. The table divides the counties into two samples: the counties that had a Freedman's Bank and counties without a Freedman's Bank[17]. Panel A shows the contemporary characteristics where most of the variables were measured in 2010 using the Census[18]. Counties in both samples are very urban. Compared to counties that did not have a Freedman's Bank, The Freedman's Bank counties tend to have less total population and a higher percentage of residents who are Black. Panel B presents the historical characteristics of counties in both samples[19]. One can see counties that had a Freedman's Bank had slightly less population than the counties without a Freedman's Bank. Similar to the contemporary pattern observed between the two samples,

[16]Acharya et al. (2016) found that the presence of slavery lead to racist institutions. The usage of proportion of slaves in 1860 as a proxy for racist institution is first implemented by Williams (2017).

[17]Most of the counties that had a Freedman's Bank are located in the South, with the exception of New York and Philadelphia. Hence, in this sample, the majority of counties that had a Freedman's Bank were in the South and all of the counties that did not have a Freedman's Bank are not in the South.

[18]The exception is the number of banks. The number of banks corresponds to the year the surveys were taken: 2009, 2011, 2013, 2015 and 2017. Table 2.1 shows the average number of banks in a county between 2009 and 2017.

[19]Note that for many characteristics measured at or before 1870, the number of counties in the non-Freedman's Bank sample decreased from 87 to 44. This is because many counties had yet to be formed in 1870, hence county characteristics at or before 1870 do not exist.

46 percent of the population in counties with a Freedman's Bank were African Americans compared to 2 percent in the non-Freedman's Bank sample. Because the majority of African Americans in 1870 were illiterate, adults who resided in counties with Freedman's Bank were more likely to be illiterate compared to adults who lived in a non-Freedman's Bank sample.

Table 2.2 presents the individual and household characteristics of the African American respondents who were surveyed in the CPS Un(der)banked Supplement. Like Table 2.1, Table 2.2 splits the sample into respondents who reside in counties that had a Freedman's Bank, and respondents who live in counties that never had a Freedman's Bank. Panel A displays the summary statistics for all African American respondents in the survey. Individuals who reside in a county that had a Freedman's Bank are slightly more likely to have a bank account compared to those who live in a county without a Freedman's Bank, even though the difference is not statistically significant. In terms of other demographic characteristics, such as one's age or marital status, the respondents are very similar across the two samples. Panel B of Table 2.2 shows the characteristics of unbanked African American respondents from both samples. Compared to the general African American respondents, the unbanked tend to be younger, less likely to be married and less likely to have gone to college. On average, approximately 12 percent of the unbanked respondents listed "mistrust in bank" as the primary reason they do not have a bank account. Within those who are unbanked, there are no statistically significant differences in demographic characteristics between those who reside in a county with a Freedman's Bank versus without a Freedman's Bank.

2.5 The Failure of the Freedman's Bank and Contemporary Banking Decisions

2.5.1 Estimating Equation

In order to investigate the relationship between the failure of the Freedman's Bank and the individual's current banking decision, I begin my analysis by using the historical county-

level take-up rate in the Freedman's Bank as a proxy to measure the magnitude of the bank failure in a county. The sample is limited to African American families in the CPS Un(der)banked Supplement who reside in a county that had a Freedman's Bank[20]. I estimate the following OLS equation:

$$BANKED_{i,c,t} = \beta_1 + \beta_2 \text{TAKE-UP RATE}_c + \mathbf{X'}_{i,t}\mathbf{\Gamma} + \mathbf{X'}_c\mathbf{\Omega} + \lambda_t + \epsilon_{i,c,t} \tag{2.1}$$

where i indexes individuals, residing in county c in time t. $BANKED_{i,c,t}$ is an indicator variable that takes the value 1 if the individual has a checking or savings account at a bank. λ_t denotes survey year fixed effects, which are included to capture time-specific factors, such as the great recession, that may affect the decision to bank. TAKE-UP RATE$_c$ measures the proportion of the local African American community who held an account at the Freedman's Bank in the 1870s. Formally, the county-level take-up rate in banking is defined as half the total number of records within each branch register divided by the county African American population in 1870[21]. The variation in the take-up rate across branches is illustrated by Figure 2.1.

$$\text{TAKE-UP RATE}_c = \frac{\text{total number of register records within each branch * 0.5}}{\text{total county African American population in 1870}}$$

The coefficient of interest in β_2, the estimated relationship between the severity of the Freedman's Bank failure at the respondents' place of residence and their decision to bank.

The vector $\mathbf{X'}_{i,t}$ denotes a set of individual and household-level covariates, which include fixed effects for the respondent's age, gender, marital status, education level, employment status. Fixed effects for the household income brackets, family size and number of children are also included in $\mathbf{X'}_{i,t}$. The vector $\mathbf{X'}_c$ captures the contemporary and historical characteristics of the county in which the respondent lives. I included log county population,

[20]Specifically, the sample is limited to African American families in 19 counties. Some counties with a Freedman's Bank were not sampled in the CPS Un(der)banked Supplement. Other counties with a Freedman's Bank did not have data to define the take-up rate.

[21]Fu (2020) finds that approximately 50 percent of depositors live within the county boundary where the bank was located.

percent Black and percent urban measured in 2010 to encapsulate county characteristics at the time of the survey. In addition, the number of banks in the county is included for each survey year. The historical characteristics include: date of county formation, newspaper per capita measured in 1840, proportion of slaves measured in 1860, measures of the degree of residential segregation in 1880 and 1940, proportion of Black illiterate men, proportion of white illiterate men, proportion of African Americans who were farmers and the log annual manufacturing wage measured in 1870. Many of the explanatory variables in Equation (1) do not vary across individuals. Rather they vary at the county level (e.g., TAKE-UP RATE$_c$ and $\mathbf{X}'_c$). Given the potential for within-group correlation of the residuals, the standard errors are adjusted for potential clustering at the county level.

To claim that the relationship between take-up rate and contemporary banking decision is causal, identification requires that the take-up rate be "as good as random." A potential source of bias is that counties with higher take-up rate had residents with certain persistent characteristics driving the effect, aside from cultural mistrust caused by the bank failure. Fu (2020) shows that variation in the take-up rate across different counties mainly arises from the persuasiveness and advertisement ability of the local branch officers. In that study, the take-up rate was correlated against various county-level demographic variables and measures of urban-ness. Fu (2020) finds that while the take-up rate is uncorrelated with the majority of county-level variables, it is related to the percentage of the local Black population who were farmers. Counties where a larger percentage of the local Black population were farmers have a lower take-up rate. In this study, I control for the historical proportion of African Americans who were farmers, as well as all the additional controls described previously.

Even after controlling for a large number of observable characteristics, one might still be worried that the presence of unobservable characteristics is driving both historical take-up rate and contemporary banking decisions made by African Americans. Due to the fact that I cannot account for all possible sources of bias, I am not claiming that the relationship between take-up rate and contemporary banking decisions is causal in this paper. Future work should strive to identify the causal relationship on the effect of the Freedman's Bank

failure on banking decisions and mistrust today.

2.5.2 OLS Baseline Estimates

Table 2.3 reports the OLS estimate of Equation (1). The results are first presented with only individual and household characteristic controls, then with contemporary county-level controls, and finally with the full set of contemporary and historical controls. When only controlling for individual and household level characteristics, the estimates show no significant relationship between the historical Freedman's Bank take-up rate and contemporary decision to bank. However, after controlling for the number of banks in one's county, and other contemporary county-level characteristics, the β_2 coefficient becomes statistically significant and larger in magnitude. A 10 percentage point increase in the take-up rate is associated with a 1.68 percentage point decrease in the probability of holding a bank account today. The results remain statistically significant after controlling a large number of additional historical county-level characteristics and the magnitude becomes even larger. A 10 percentage point increase in the take-up rate is associated with a 15 percentage point decrease in the probability of holding a bank account today, everything else being equal.

In Table 2.3, the proportion of Blacks who held an account at the Freedman's Bank in the 1870s is used as a proxy to measure the magnitude of the Freedman's Bank failure at the county level. The estimated coefficient for TAKE-UP RATE$_c$ is negative and statistically significant. This is consistent with the hypothesis that the failure of the Freedman's Bank lowered one's probability to open a bank account, controlling for income, education level and the supply of banks. For robustness, I utilize an alternative measure of the take-up rate which defines the take-up rate as the proportion of total county population that opened an account at the Freedman's Bank (Table 2.7 in the appendix). The results are similar: I continue to find a significant negative correlation between historical take-up rate and contemporary decision to bank.

2.5.3 Falsification Tests

Although the previous findings support the hypothesis that the failure of the Freedman's Bank negatively correlates with the decision to bank for African Americans, this finding is consistent with other interpretations. For example, the failure of the Freedman's Bank after the Panic of 1873 could have resulted in long-term stagnation in the development of all financial institutions. This could have led to individuals, regardless of race, who reside in counties heavily impacted by the Freedman's Bank failure to be unbanked, simply due to the poorer quality of the local financial institution. To check against alternative explanations, I perform a number of falsification exercises.

To check this alternative explanations, I consider whether there exists a relationship between historical take-up rate and the contemporary banking decision of whites, Hispanics and foreign Blacks. For most African Americans[22], Freedman's Bank was their first exposure to the banking institution. Considering that the vast majority of depositors were African Americans[23], its failure should only predict banking within the African American community. The results are presented in Table 2.8 in the appendix. Panel A of Table 2.8 shows that there exist no relationship between the historical take-up rate and the banking decision of whites, even when controlling for contemporary and historical county-level characteristics. Panel B of Table 2.8 shows the correlation for Hispanics. The coefficient on TAKE-UP RATE$_c$ for the Hispanic group is statistically insignificant and slightly positive. Panel C of Table 2.8 considers the relationship between historical take-up rate and being banked for foreign Blacks. One should expect that the failure of the Freedman's Bank should only have a significant and persistent impact for African Americans who are descended from individuals with knowledge of the bank failure. Hence, the impact should be heavily attenuated or null for foreign Blacks. The findings confirm that there exists no relationship between historical take-up rate and banking decisions for foreign Blacks who currently reside in counties that had a Freedman's Bank.

[22]Excluding those who were freed and lived in the north prior to the Civil War
[23]Fu (2020): 70% of depositors were Black

2.5.4 Heterogeneity

In this section, I explore whether the relationship between the historical severity of the Freedman's Bank failure and banking decisions vary by the education level, household income and the presence of Black-owned banks in a locality. First, I investigate whether the persistence effect of the Freedman's Bank failure is potentially mitigated by education. For this analysis, I interact the take-up rate with an indicator variable that the respondent has some college experience. Table 2.9 in the appendix presents the results. The results show that respondents with some college education are 10 percentage points more likely to have a bank account. However, higher education level does not change the relationship between the severity of the bank failure and decision to be banked.

Second, I examine whether the relationship between the severity of the bank failure and decision to be banked varies based on household income. I define "high-income" households to be those who earn above $50,000 annually. Table 2.10 in the appendix shows that while respondents from high-income households are approximately 45 percentage points more likely to hold a bank account, there exists no differential relationship between the severity of Freedman's Bank failure and banking decisions based on household income. For unbanked individuals, they cite "not enough money" as the biggest reason they do not hold a checking or savings account. This result suggests that the failure of the Freedman's Bank impacted individuals' decision to bank through channels other than income.

Lastly, I investigate whether the presence of local Black-owned banks impacts the relationship between the magnitude of the bank failure and the decision to open a bank account. Historically, African American banks focused on customers left unserved because of segregation and racial discrimination (Ammons 1996, Dymski and Mohanty 1999). For African Americans, Black-owned banks promise and offer refuge from exploitation. Lack of trust in the banking institution, coupled with an up-tick in racial violence, led to a revival of Black banking after 2016 (Baradaran 2017). According to the Federal Deposit Insurance Corporation (FDIC), there are approximately 20 Black-owned banks in 2017. [24]. For each

[24]Minority depository institutions (MDIs) are depository institutions where 51 percent or more of the stock

county that had a Freedman's Bank. I create an indicator variable that equals 1 if a county has at least one Black-owned bank. Table 2.11 in the appendix shows that the presence of Black-owned banks mitigates the relationship between the magnitude of the bank failure and the decision to open a bank account for African Americans. The results suggest that African Americans residing in localities that were heavily impacted by the failure of the Freedman's Bank might choose to be unbanked due to fears of exploitation, but are more willing to participate at a Black-owned banks.

2.6 The Failure of the Freedman's Bank and Trust in Banks

For unbanked African American households, the CPS Un(der)banked Supplement asked the respondents for their main reason to be unbanked. Given the three types of reasons to be unbanked, I model the respondents' reason to not open a checking or savings account using a multinomial logit. Let X_{ij} denote the vector of explanatory variables for respondent i to choose reason j. Limiting the sample to unbanked African American residing in a county that had a Freedman's Bank, the probability (P) that respondent i chose reason j to be unbanked is determined as

$$P_{ij} = \frac{exp(\beta' x_{ij})}{\sum_{j=1}^{4} exp(\beta' x_{ij})} \tag{2.2}$$

The vector of explanatory variables x_{ij} includes the county-level take-up rate of the Freedman's Bank, individual- and household-level controls, contemporary and historical county-level controls. The individual- and household-level controls include: age, number of children and fixed effects for household income bracket, gender, marital status, education level and employment status. The contemporary county-level controls are log total population, per-

is owned by one or more "socially and economically disadvantaged individuals." In addition to the ownership test, institutions are considered MDIs if a majority of the board of directors is minority and the community the institution serves is predominately minority. There were approximately 150 MDIs in 2017 where 15 percent of the MDIs were designated as Black or African American. The FDIC publishes information about the MDIs on an annual basis, including the names of the banks and the city in which they are located in.

cent Black, percent urban and the number of banks in 2010. The historical controls include: county formation date, newspaper per 100,000 residents in 1940, proportion of slaves in 1860, log annual manufacturing wage, proportion of Black farmers and percent Black illiterate in 1870, and the degree of residential segregation in 1880 and 1940.

Table 2.4 presents the average marginal effect estimates from the multinomial logit model. Columns 1-3 of Table 2.4 reports the marginal effect of changing the take-up rate on the probability of reporting "mistrust" as the main reason to be unbanked. The Freedman's Bank take-up rate is a significant predictor for present day mistrust in banks. For unbanked households, those who reside in a county with 10 percentage points higher take-up rate are 2.55 percentage points more likely to report "mistrust" in bank as the primary reason to be unbanked. However, the take-up rate does not significantly predict a household to be unbanked due to economic reasons (e.g., do not have enough money to need a bank account).

As a robustness check, I model mistrust using a linear probability model. In the sample of unbanked African American respondents, the outcome variable is an indicator variable that takes the value 1 if a respondent noted mistrust in banks as the primary reason for being unbanked. The variable takes the value 0 for all other reasons. Table 2.12 in the appendix reports the result: respondents living in a locality with 10 percentage points higher take-up rate are 2 percentage points more likely to have mistrust in banks. The results are very similar in magnitude as the multinomial logit regression, which confirms the previous findings.

2.7 Effects of the Freedman's Bank Failure on Internal Norms versus External Factors

So far, the evidence presented suggests that the failure of the Freedman's Bank contributed to mistrust in the banking institutions and plays a role in the African American community's hesitancy to bank today. One potential explanation for the persistence of mistrust is through internal norms. Those with knowledge of the Freedman's Bank failure in the

late 1800s became less trusting of banks, hence their descendants remain less trusting today. There could be another possible reason for the persistence of reduced trust in banks within the African American community today. The failure of the Freedman's Bank coincided with the collapse of many other financial institutions, resulting in a permanent deterioration of the quality of banking structures in Southern cities today. If this effect persists, then people today may have lower levels of trust simply due to poor institutional quality.

In this section, I attempt to distinguish between the two channels. I do so by focusing on a sample of African Americans who do not reside in the South[25]. Up until 1910, more than 90 percent of the African American population lived in the South. Beginning in the early twentieth century, Southern-born African Americans began to relocate to the North in search of better social and economic opportunities, creating a phenomenon known as the Great Migration. Hence, the majority of African Americans who currently do not reside in the South either came from the South, or are descendants of individuals who partook in the Great Migration. By focusing on a sample of movers, or descendants of movers, I am exploiting the fact that when individuals relocate, their cultural beliefs, norms and values move with them, but their external environment is left behind. Therefore, if the failure of the Freedman's Bank primarily affects mistrust in banks through internal factors, then African Americans who live in the North or in the West today would also be hesitant to bank. If the failure of the Freedman's Bank affects trust primarily through external factors, like the deterioration of banking institutions in Southern cities, then I would expect that African Americans who live in the North or in the West today are much less likely to be unbanked due to mistrust.

For each non-Southern county in the CPS Un(der)banked Supplement, I create a measure to capture the degree of historical "exposure" to news of the Freedman's Bank failure. The variation in this exposure variable is based the interaction between the locations of the Freedman's Bank and settlement patterns during the Great Migration. Historians have

[25]I define the South as states that were in the Confederacy during the Civil War. Hence this sample excludes individuals who reside in South Carolina, Mississippi, Florida, Alabama, Georgia, Louisiana, Texas, Virginia, Arkansas, Tennessee and North Carolina.

emphasized that train routes played an outsized role in shaping migratory patterns and information networks. There are three major migratory patterns that emerged based on train routes [26]. First, African Americans in Mississippi tended to migrate to Midwestern cities via the *Illinois Central Railroad*. Second, African Americans in South Carolina and Georgia migrated up the East coast through the *Southern Railway* and then through the *Pennsylvania Central*. Third, those in Louisiana had concentrated migration to cities in the West through the *Central Southern Rail*. Figures 2.3 to 2.6 reproduce maps of the four major lines of railroad from the turn of the century.

In Figure 2.2, I map the locations of the Freedman's Bank along with the four train routes that are instrumental in shaping the migratory patterns of African Americans during the Great Migration: the *Illinois Central*, the *Southern Railway*, the *Pennsylvania Central* and the *Central Southern Railway*. The interaction between the branches of the Freedman's Bank and the train network create a different amount of "exposure" of the bank failure news along each rail line. There are various ways to capture the degree of exposure to the Freedman's Bank failure along a line. One way is to measure the number of branches along a rail line. There were 4 branches of the Freedman's Bank along the *Illinois Central*, 13 along the *Southern Railway*, 5 along the *Pennsylvania Central* and 4 along the *Central Southern Railway*[27]. Because each branch location varies in size, there also exist variation in how many accounts were opened along a rail route. For example, approximately 24,000 accounts were opened along the *Illinois Central*, 68,000 along the *Southern Railway*, 39,000 along the *Pennsylvania Central* and 12,000 along the *Central Southern*[28]. Whether exposure is

[26]Black et al. (2010) first described the three migratory pattern groupings during the Great Migration. Black and coauthors also empirically confirmed these three prominent migratory patterns using the 1970 U.S. Census and the Duke SSA/Medicare dataset. Boustan (2010) also discussed persistent migratory patterns through train routes and gives references to the extant literature.

[27]The following branches were located along the Illinois Central: New Orleans, Natchez, Vicksburg, Memphis. Branches along the Southern Railway are: Macon, Atlanta, Mobile, Augusta, Chattanooga, Knoxville, Lexington, Louisville, Raleigh, Norfolk, Richmond, Lynchburg, Alexandria* and DC. Along the Pennsylvania Central, the branches are: DC, Alexandria*, Martinsburg*, Baltimore, Philadelphia, New York and St. Louis. Branches along the Central Southern Railway are: St. Louis, Little Rock, New Orleans, Shreveport and Houston*. The branches denoted with an asterisk closed prior to the bank failure on July 1874. Hence, they were not counted in the number of branches along a rail line.

[28]The number of accounts opened along a rail line only counts branches with surviving register records.

captured by the number of branches along a rail, or the number of accounts opened, one can see that the *Central Southern Railway* was the least exposed while the *Southern Railway* was the most exposed.

For any destination cities on the aforementioned rail lines, I use either the number of branches or the total number of accounts along a line to capture the "exposure" to news of the Freedman's Bank failure. However, many counties in the CPS Un(der)banked Supplement are not located directly on one of these rail lines. To capture the magnitude of the historical "exposure" to news of the Freedman's Bank failure for any non-Southern county, I first determine which rail line is the closest to this county. Assuming that rail line a is the closest to county c, then the exposure variable is created by dividing the number of branches or the total number of accounts by the distance to rail line a. To formalize, the first variable to gauge the degree of exposure to news of the bank failure on the county-level is

$$EXPOSURE1_c = \frac{\text{Number of branches along the closest rail line}}{\text{Distance to the closest rail line}} \tag{2.3}$$

for any county not directly on the rail line. The second variable to capture the degree of exposure to news of the bank failure for counties not on the rail line is

$$EXPOSURE2_c = \frac{\text{Number of bank accounts opened along the closest rail line}}{\text{Distance to the closest rail line}} \tag{2.4}$$

The exposure variables are created based on the intuition that the saliency of the Freedman's Bank failure would be the highest in the destination cities directly on the rail lines. For counties farther away from the rail line, only a fraction of the information regarding the bank failure arrives because fewer migrants choose to move there. Figure 2.7 in the appendix plots the share of African American migrants to a particular county against the distance to the closest rail line. The majority of movers during the Great Migration settled in cities within 50 miles of the rail line.

2.7.1 OLS Estimating Equation

To examine whether historical exposure to news of the Freedman's Bank failure impacts banking decisions through internal norms, I estimate the following equation on the sample of African Americans who do not reside in the South:

$$BANKED_{i,c,t} = \beta_1 + \beta_2 log\text{EXPOSURE}_c + \mathbf{X'}_{i,t}\mathbf{\Gamma} + \mathbf{X'}_c\mathbf{\Omega} + \lambda_t + \epsilon_{i,c,t} \tag{2.5}$$

for individual i, living in county c who took the survey in year t. EXPOSURE$_c$ is one of the two exposure variables mentioned previously and the variation is at the county level. Because the distributions of both types of EXPOSURE$_c$ are highly left skewed, I will report estimates using the natural log of the exposure measure. As before, the outcome of interest $BANKED_{i,c,t}$ is whether respondent i has a checking or savings account at a bank. $\mathbf{X'}_{i,t}$ represents individual- and household- level controls including: dummies for family income bracket, age, gender, marital status, family size, number of children, education level and employment status. $\mathbf{X'}_c$ is a vector of county controls that captures both contemporary and historical characteristics of a location. $\mathbf{X'}_c$ includes contemporary controls such as log total population, percent Black, percent urban, number of banks measured in 2010. It also includes variables like distance to a Freedman's Bank, county formation date, newspaper per 100,000 residents in 1840, log annual manufacturing wage in 1870, proportion of Black farmers in 1870, percent Black (white) illiterate in 1870 and measures of residential segregation in 1880 and 1940. In this estimation, time fixed effects are included in λ_t and the standard errors are clustered at the county level.

2.7.2 OLS Results

The estimates of Equation (5) are reported in Table 2.5. Columns 1-3 of Table 2.5 reports the estimates using the first exposure measure, $EXPOSURE1_c$, based on the number of branches along a rail line. Column 1 reports estimates of Equation (5) controlling for only individual and family-level characteristics. It shows that a 10% increase in the exposure mea-

sure decreased an individual's propensity to open a bank account by 1.37 percentage points. After controlling for contemporary county-level characteristics, the estimated coefficient remains very similar in magnitude and statistically significant. However, once controlling for historical county-level characteristics, the coefficient on $log\text{EXPOSURE1}_c$ becomes slightly smaller in magnitude but still statistically significant at 10 percent. The results show that a 10% increase in the exposure measure decreased an individual's propensity to open a bank account by 1.02 percentage points. It is important to note that the sample size drops significantly once I control for historical county-level characteristics. This is because many counties in the West were not in existence in 1840. Hence, the counties that remain in the sample after controlling for historical characteristics are largely located in the Midwest.

Columns 4-6 of Table 2.5 shows the relationship between the second type of exposure measure, based on the number of accounts along a rail line, on the propensity of being banked. Controlling for only individual and family-level characteristics, the results show that a 10% increase in the exposure measure is associated with a 0.36 percentage point decrease in the propensity to bank. The coefficient is negative and statistically significant. After controlling for contemporary and historical county-level characteristics, the coefficient became larger in magnitude: a 10% increase in the second type of exposure measure decreased the propensity to bank by 1.11 percentage points. Comparing the estimated coefficients in Columns 4-6 versus those reported in Columns 1-3, one can see that the estimated coefficient that used the second exposure is much smaller compared to the coefficient of the first exposure measure. However, both results show that an increase in the exposure to news of the bank failure has a small but statistically significant impact on the propensity to open a bank account. The results overall suggest that exposure of the bank failure has a small but persistent impact on the decision to open a bank account for African Americans through internal norms.

As a robustness check, I consider falsification tests on other groups of respondents. Specifically, I test whether the exposure measures are associated with the propensity to bank for whites, Hispanics and foreign Blacks. The results are reported in Table 2.13 in the appendix. There exist no consistent pattern between either of the exposure measures with

banking decisions made by whites, Hispanics or foreign Blacks.

2.7.3 Multinomial Logit Estimation and Results

In this exercise, I model the reason to not have a bank account on the sample of unbanked African American respondents who do not currently reside in the South and do not live in a county that had a Freedman's Bank. In this sample, I want to estimate the marginal effect of changing the exposure measure on the probability of reporting "mistrust" as the primary reason to be unbanked. The vector explanatory variables in this exercise remain largely the same as the last multinomial logit exercise, with the exception that the take-up rate is substituted by the exposure measures.

The average marginal effect estimates of changing the exposure measure on the probability of reporting "mistrust" are presented in Table 2.6. Panel A shows the estimates using the first type of exposure variable which is based on the number of Freedman's Bank branches along a rail line. Panel B shows the estimates that use the second type of exposure measure which is based on the total number of accounts opened along a train route. From Panel A, we can see that the first exposure variable is not a significant predictor for present day mistrust in banks when only individual and family-level controls are included. After including contemporary and historical county-level controls, the exposure variable becomes a significant predictor for reporting "mistrust" as the main reason to be unbanked. The results show that those who reside in a location with 10% higher exposure to news of the bank failure are 0.36 percentage points more likely to cite "mistrust" as the reason to be unbanked. Panel B presents the marginal effect when the exposure measure is based on the total number of bank accounts along the rail line. After controlling for the full set of contemporary and historical county-level characteristics, the results show that living in a county with 10% higher exposure are 0.27 percentage points more likely to distrust banks. Columns 4-9 shows the average marginal effect of the exposure measure on other reasons to be unbanked. Interestingly, the exposure measures seems to decrease the likelihood of respondents reporting economic reasons as the primary reason to be unbanked.

Even when the relationship between mistrust and the exposure measure is modeled as a linear probability model, the same pattern emerged. Among unbanked African American respondents who do not reside in the South, those who live in localities with more exposure to the news of the bank failure are more likely to not have a bank account due to mistrust (see Table 2.14 in the appendix). Overall, the results in this section suggest that the persistent effect of the Freedman's Bank failure on banking behaviors, identified in Section 5 and Section 6, arises partially from a change in the internal norms and beliefs of the descendants of those affected by or who knew of the failure. In this paper, I cannot rule out that the bank failure impacted mistrust and banking decisions of African Americans today through a long-term deterioration of institution quality. In the future, it will be fruitful to explore methods to quantify to what degree each channel, internal norm versus institutional quality, contributed to this persistent relationship.

2.8 Conclusion

Historians and other social scientists have hypothesized that the failure of the Freedman's Bank has a lasting legacy that left the African American community wary and distrustful of banks. In this paper, I empirically test this hypothesis. First, I use the county-level take-up rate of banking as a proxy for the severity of the bank failure at the local level, and I find that African Americans currently residing in a county with a higher take-up rate are less likely to hold a checking or savings account at a bank, holding individual and household characteristics constant. The relationship is robust to controlling for a large number of contemporary and historical county variables. I then model the household's reason to be unbanked using a multinomial logit and I find that households who reside in a county with a higher take-up rate are more likely to report "mistrust" in banks as the primary reason they remain unbanked.

I then explore whether the relationship between the failure of the Freedman's Bank and contemporary banking decisions can be explained by internal norms passed down from

parents to children. By focusing on a sample of movers or descendants of movers from the Great Migration who no longer live in the South, I show that households who reside in locations with greater exposure to the news of the bank failure are less likely to be banked and are more mistrustful of banks. The result suggests that the bank failure altered the trust of banks for the African American community through internal factors, such as norms, beliefs or values.

Many policy makers are very concerned with lowering the number of unbanked households. For the African American community, increased inclusion in the formal financial system would create greater opportunity to reinvest and grow their wealth. This paper highlights mistrust in banks, stemming from an historical event, as a potential barrier to opening a bank account in the African American community. Additionally, the paper shows that the presence of Black-owned banks can mitigate the relationship between the bank failure and willingness to open a bank account. In addition to the various strategies that are already in place to increase financial inclusion, policy makers should also consider the prosperity of Black-owned banks as an important factor in the efforts to increase financial inclusion within the African American community.

2.9 Figures and Tables

Figure 2.1: Take-up rate at the Freedman's Bank

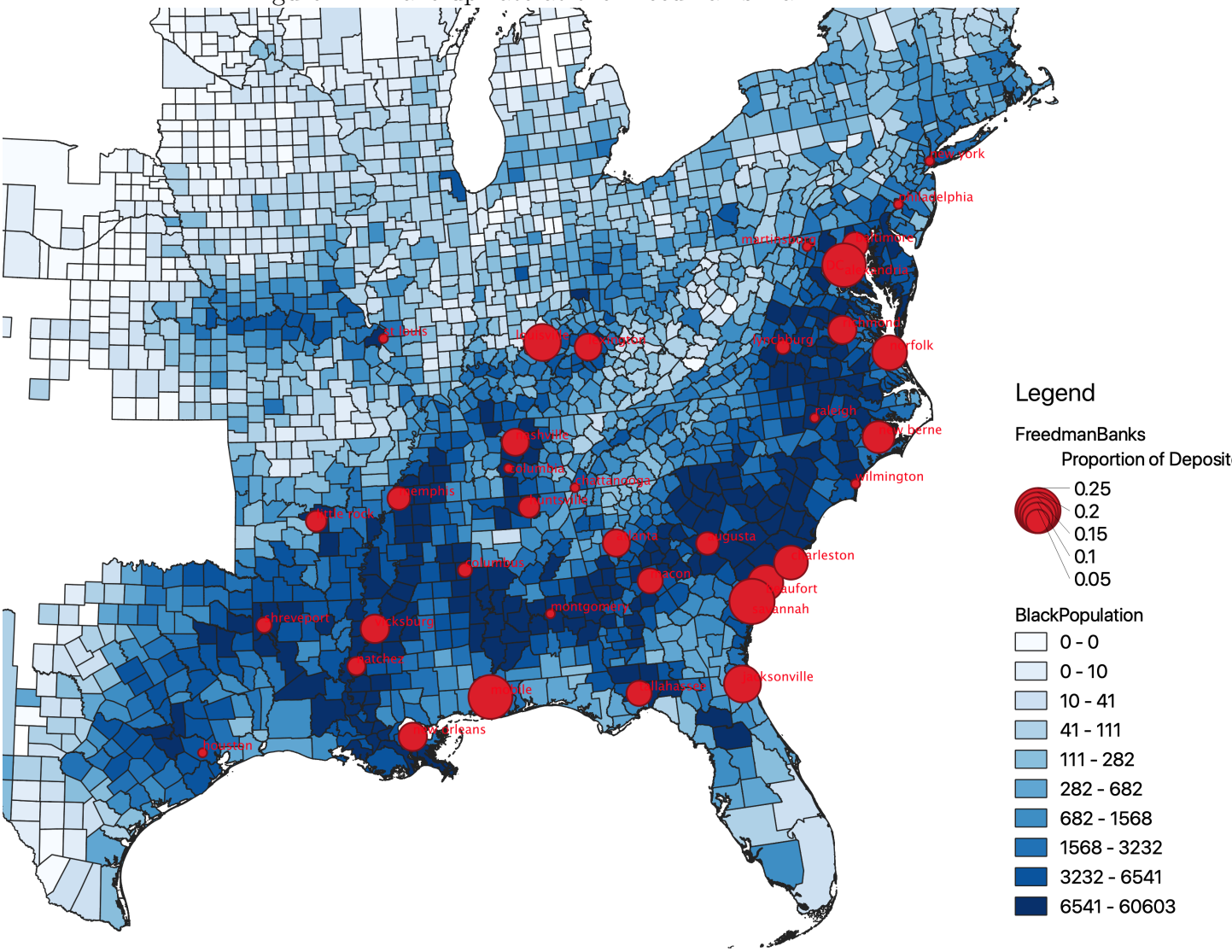


Figure 2.2: Location of Freedman's Bank branches against historical rail lines

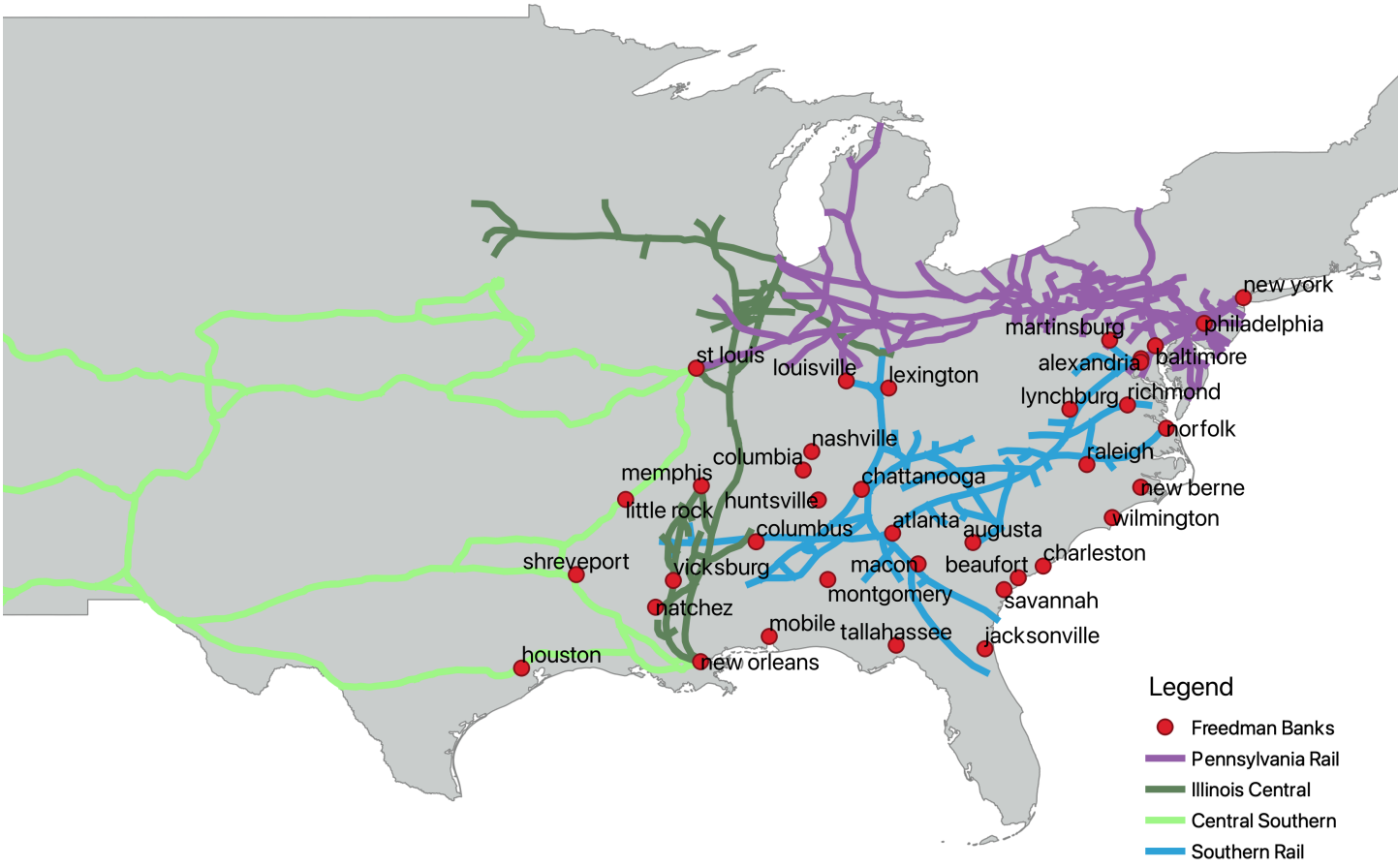


Table 2.1: County-level summary statistics

	Freedman's Bank Counties			Non-Freedman's Bank Counties		
	Mean	Sd	N	Mean	Sd	N
Panel A: 2010 Variables						
Totpop (in thousands)	466.657	249.058	19	917.638	125.765	87
Percent Blk	0.396	0.138	19	0.097	0.080	87
Percent Urban	0.939	0.064	19	0.926	0.057	87
Number of Banks	129.368	66.337	19	205.759	239.632	87
Panel B: Historical Variables						
1870 Totpop (in thousands)	72.580	78.224	19	98.202	80.692	44
1870 Percent Blk	0.458	0.177	19	0.018	0.023	44
1870 Percent Illiterate (Male over 21)	0.415	0.157	19	0.072	0.043	44
1870 Number of Banks	4.444	3.276	19	1.419	3.065	44
1860 Proportion Slave	0.342	0.190	19	N/A	N/A	N/A
1840 Newspapers per 100,000 people	31.024	21.269	19	19.827	13.110	41
Date of Initial County Formation	1783.111	58.211	19	1826.287	56.389	87

Note: County total population, percent Black, percent urban in 2010 are statistics derived from the 2010 Census. Data on the number of banks come from the FDIC. County total population, percent Black, percent illiterate are statistics derived from the 1870 Census. The proportion of slave comes from the 1860 Census. The National Historical Geographic Information System contains information on the number of newspaper per 100,000 people in 1840. The date of county formation come from the National Association of Counties website.

Table 2.2: Household-level summary statistics

	Freedman's Bank Counties			Non-Freedman's Bank Counties		
	Mean	Sd	N	Mean	Sd	N
Panel A: All						
Banked	0.784	0.412	2658	0.742	0.438	2912
Age	51.061	17.450	2658	49.982	17.099	2912
Female	0.622	0.485	2658	0.615	0.487	2912
Married	0.204	0.403	2658	0.224	0.417	2912
Family Size	2.072	1.361	2658	2.204	1.452	2912
Num Children	0.633	1.077	2658	0.730	1.159	2912
Labforce	0.565	0.496	2658	0.571	0.495	2912
Any College	0.496	0.500	2658	0.518	0.500	2912
Panel B: Unbanked						
Mistrust	0.121	0.326	456	0.116	0.320	571
Age	46.130	15.857	576	44.773	16.110	751
Female	0.675	0.469	576	0.658	0.475	751
Married	0.082	0.274	576	0.088	0.283	751
Family Size	2.243	1.561	576	2.205	1.534	751
Num Children	0.877	1.332	576	0.870	1.295	751
Labforce	0.481	0.500	576	0.489	0.500	751
Any College	0.259	0.438	576	0.309	0.462	751

Note: Summary Statistics are derived from African American respondents who answered the CPS Un(der)banked Supplement from 2009 to 2017. The respondents resided in a counties that had a Freedman's Bank, and counties without a Freedman's Bank.

Table 2.3: OLS estimates of the take-up rate and the decision to bank

VARIABLES	(1) Banked	(2) Banked	(3) Banked
TakeUp Rate	-0.00494	-0.0168**	-0.152***
	(0.00508)	(0.00630)	(0.0388)
Constant	0.702***	0.734**	-0.333
	(0.131)	(0.307)	(1.004)
Observations	2,658	2,658	1,191
R-squared	0.261	0.270	0.279
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.4: Multinomial logit estimates of the take-up rate and the reason to be unbanked

	(1) Mistrust	(2) Mistrust	(3) Mistrust	(4) Economic	(5) Economic	(6) Economic	(7) Other	(8) Other	(9) Other
TakeUp Rate	0.0263***	0.0263*	0.0255*	0.0288*	0.0102	-0.00685	-0.0551***	-0.0366	-0.0186
	(0.00580)	(0.0106)	(0.0122)	(0.0131)	(0.0282)	(0.0304)	(0.0147)	(0.0280)	(0.0311)
Observations	576	576	559	576	576	559	576	576	559
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The table displays the average marginal effects. The sample consists of unbanked African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. "Mistrust" corresponds with the reason "do not trust banks". "Economic" corresponds to the reason "do not have enough money to need a bank account". "Other" corresponds to the reason "other reasons/none of the above". All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.5: OLS estimates of the internal exposure to the bank failure and the decision to bank

VARIABLES	(1) Banked	(2) Banked	(3) Banked	(4) Banked	(5) Banked	(6) Banked
log(Exposure 1)	-0.137***	-0.136***	-0.102*			
	(0.0276)	(0.0283)	(0.0586)			
log(Exposure 2)				-0.0365***	-0.0332*	-0.111***
				(0.0136)	(0.0182)	(0.0292)
Constant	1.033***	1.122**	3.600***	1.138***	1.304**	2.680***
	(0.0696)	(0.434)	(1.216)	(0.122)	(0.550)	(0.795)
Observations	2,912	2,912	1,709	2,912	2,912	1,709
R-squared	0.273	0.276	0.307	0.260	0.261	0.294
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that was not in the South and never had a Freedman's Bank. The variable Exposure 1 is defined by $\frac{\text{number of branches along the closest rail line}}{\text{distance to the closest rail line}}$. The variable Exposure 2 is defined by $\frac{\text{number of accounts opened along the closest rail line}}{\text{distance to the closest rail line}}$. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.6: Multinomial logit estimates of the internal exposure to the bank failure and the reason to be unbanked

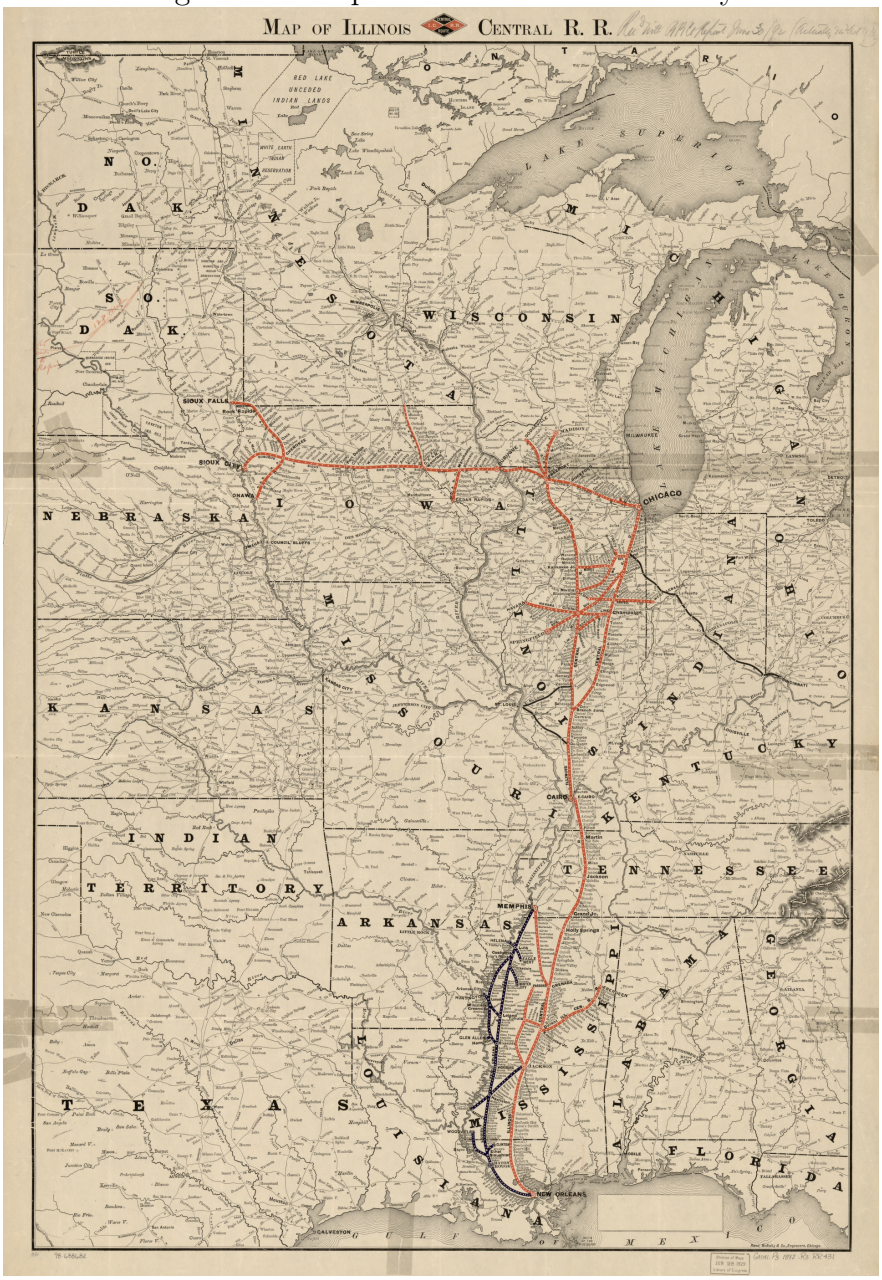
	(1) Mistrust	(2) Mistrust	(3) Mistrust	(4) Economic	(5) Economic	(6) Economic	(7) Other	(8) Other	(9) Other
Panel A: Exposure 1									
log(Exposure 1)	-0.00353	-0.000784	0.0356*	-0.0421**	-0.0382	-0.0441*	0.0457	0.0390	0.00848
	(0.0199)	(0.0189)	(0.0154)	(0.0148)	(0.0200)	(0.0198)	(0.0261)	(0.0219)	(0.0296)
Observations	826	826	748	826	826	748	826	826	748
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Exppsure 2									
log(Exposure 2)	0.0164	0.0121	0.0277*	-0.0216	-0.0367*	-0.0295	0.0052	0.0246	0.00181
	(0.0089)	(0.0105)	(0.0133)	(0.0229)	(0.0158)	(0.0162)	(0.0285)	(0.0189)	(0.0220)
Observations	826	826	748	826	826	748	826	826	748
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The table displays the average marginal effects. The sample consists of unbanked African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that is not in the South and never had a Freedman's Bank. The variable Exposure 1 is defined by $\frac{\text{number of branches along the closest rail line}}{\text{distance to the closest rail line}}$. The variable Exposure 2 is defined by $\frac{\text{number of accounts opened along the closest rail line}}{\text{distance to the closest rail line}}$. "Mistrust" corresponds with the reason "do not trust banks". "Economic" corresponds to the reason "do not have enough money to need a bank account". "Other" corresponds to the reason "other reasons/none of the above". All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

2.10 Appendix

2.10.1 Additional Figures and Tables

Figure 2.3: Map of Illinois Central Railway



Source: Rand Mcnally And Company, and Illinois Central Railroad Company. Map of Illinois Central R.R. [Chicago, 1892] Map. https://www.loc.gov/item/98688682/.

Figure 2.4: Map of Southern Railway

Source: Southern Railway, U.S. Southern Railway and connections. [Buffalo, 1897] Map. https://www.loc.gov/item/98688812/.

Figure 2.5: Map of Pennsylvania Central Railway

Source: Mendel, Edward, and Pennsylvania Railroad. A correct map of the Pennsylvania Central Rail Road with its branches & connections, the shortest & quickest route between the east & west. [Chicago, -58, 1854] Map. https://www.loc.gov/item/98688761/.

Figure 2.6: Map of Central Southern Railway

Source: United States Department Of Commerce And Labor. Bureau Of Statistics, and Rand Mcnally And Company. Map exhibiting the several Pacific railroads. [Chicago Rand, McNally & Co, 1883] Map. https://www.loc.gov/item/gm71000841/.

Figure 2.7: Distance to railroad and Black migrant share

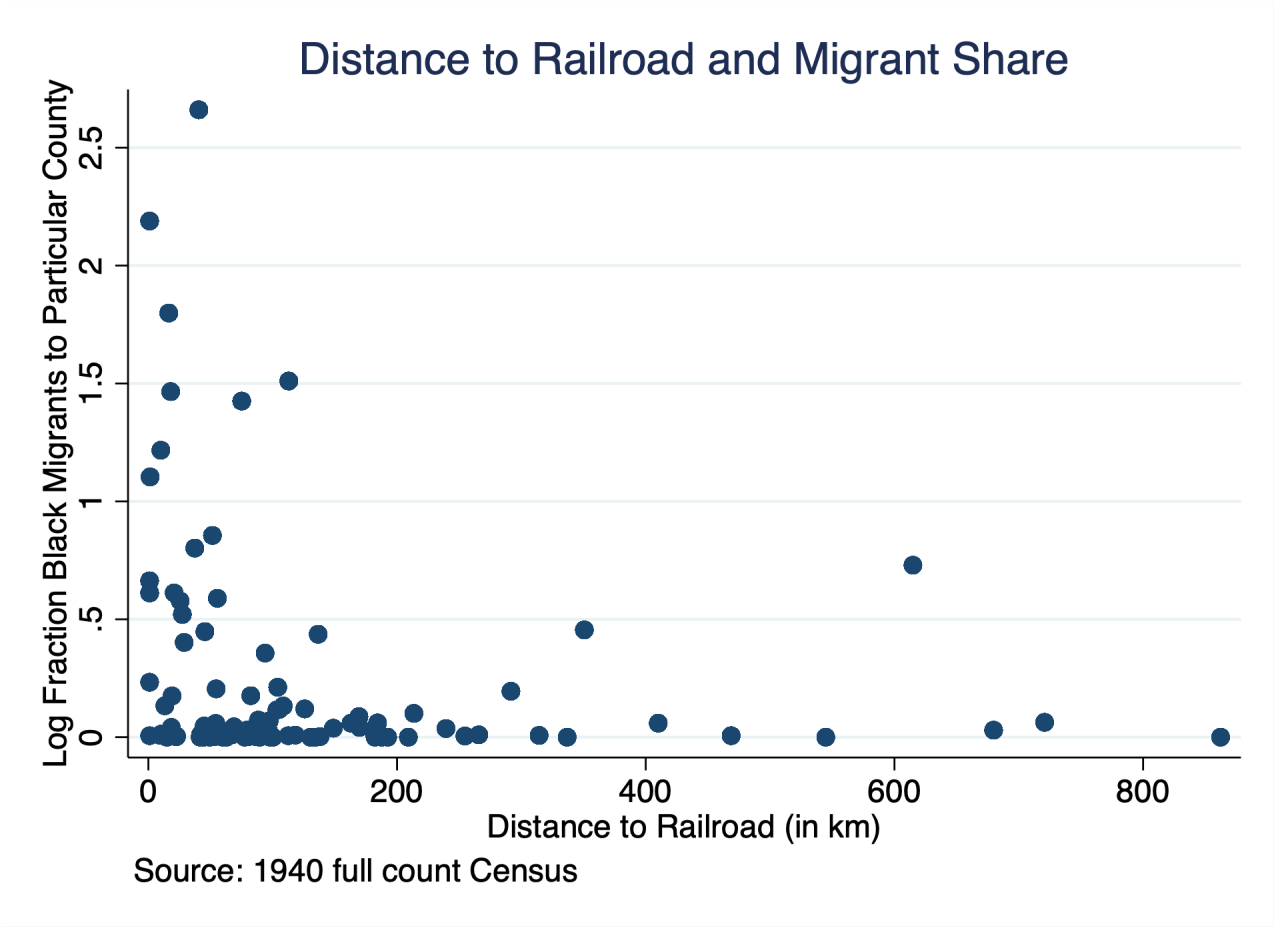


Table 2.7: OLS estimates of the alternative take-up rate and the decision to bank

VARIABLES	(1) Banked	(2) Banked	(3) Banked
TakeUp Rate Alt	-0.0236*	-0.0236*	-0.271**
	(0.0117)	(0.0117)	(0.0950)
Constant	0.697**	0.697**	0.517
	(0.295)	(0.295)	(0.878)
Observations	2,658	2,658	1,191
R-squared	0.269	0.269	0.276
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate Alt is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total county population in 1870}} * 1/10$. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.8: Falsification exercise of the take-up rate and decision to bank

VARIABLES	(1) Banked	(2) Banked	(3) Banked	(4) Banked
Panel A: Whites				
TakeUp Rate	-0.00365	-0.00868		
	(0.00342)	(0.00753)		
TakeUp Rate Alt			-0.00472	-0.0205
			(0.00448)	(0.0187)
Constant	0.328	2.020***	0.340	2.044***
	(0.312)	(0.304)	(0.306)	(0.289)
Observations	3,527	1,794	3,527	1,794
R-squared	0.146	0.176	0.146	0.176
Family Controls	Yes	Yes	Yes	Yes
Contemporary Controls	Yes	Yes	Yes	Yes
Historical Controls	No	Yes	No	Yes
Panel B: Hispanics				
TakeUp Rate	0.00132	0.197		
	(0.0133)	(0.175)		
TakeUp Rate Alt			-0.00728	0.208
			(0.0228)	(0.662)
Constant	1.192	-5.848	1.090	-4.281
	(1.267)	(5.261)	(1.273)	(6.672)
Observations	465	144	465	144
R-squared	0.349	0.503	0.349	0.501
Family Controls	Yes	Yes	Yes	Yes
Contemporary Controls	Yes	Yes	Yes	Yes
Historical Controls	No	Yes	No	Yes
Panel C: Foreign blacks				
TakeUp Rate	-0.00770	-0.0266		
	(0.0170)	(0.325)		
TakeUp Rate Alt			-0.0111	-0.247
			(0.0296)	(0.871)
Constant	-0.810	-6.906	-0.820	-2.883
	(1.493)	(19.89)	(1.489)	(20.66)
Observations	283	86	283	86
R-squared	0.535	0.753	0.535	0.754
Family Controls	Yes	Yes	Yes	Yes
Contemporary Controls	Yes	Yes	Yes	Yes
Historical Controls	No	Yes	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of non-African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. Panel A presents results for non-Hispanic whites. Panel B presents results for Hispanics. Panel C presents results for foreign Blacks. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch}*0.5}{\text{total African American population in county in 1870}} * 1/10$. TakeUp Rate Alt is defined by $\frac{\text{the total number of accounts opened at a branch}*0.5}{\text{total county population in 1870}} * 1/10$. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.9: Take-up rate and decision to bank, heterogeneity based on education

VARIABLES	(1) Banked	(2) Banked	(3) Banked
TakeUp Rate	0.00233	-0.00874	-0.144***
	(0.00607)	(0.00631)	(0.0388)
College	0.102***	0.0999***	0.0861***
	(0.0274)	(0.0260)	(0.0289)
College*TakeUp Rate	-0.0124	-0.0126*	-0.0124
	(0.00825)	(0.00720)	(0.00720)
Constant	0.668***	0.698**	-0.387
	(0.129)	(0.301)	(1.022)
Observations	2,658	2,658	1,191
R-squared	0.265	0.274	0.282
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. College is an indicator variable that equals one if the respondent had some college education. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.10: Take-up rate and decision to bank, heterogeneity based on income

VARIABLES	(1) Banked	(2) Banked	(3) Banked
TakeUp Rate	-0.00417	-0.0157*	-0.150***
	(0.00752)	(0.00803)	(0.0397)
Higher Income	0.457***	0.450***	0.485***
	(0.0610)	(0.0605)	(0.0574)
Higher Income*TakeUp Rate	-0.00308	-0.00456	-0.00550
	(0.0125)	(0.0137)	(0.0126)
Constant	0.701***	0.738**	-0.317
	(0.133)	(0.304)	(1.000)
Observations	2,658	2,658	1,191
R-squared	0.261	0.270	0.279
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. Higher Income an indicator variable that equals one if the household reported annual income higher than $50,000. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.11: Take-up rate and decision to bank, heterogeneity based on Black MDI

VARIABLES	(1) Banked	(2) Banked	(3) Banked
TakeUp Rate	-0.00811	-0.0202**	-0.160***
	(0.00663)	(0.00913)	(0.0351)
Blk MDI	-0.0285	-0.0776	-0.110***
	(0.0399)	(0.0620)	(0.0357)
Blk MDI*TakeUp Rate	0.0250	0.0567*	0.0826***
	(0.0182)	(0.0289)	(0.0164)
Constant	0.711***	-0.462	-1.511*
	(0.124)	(0.565)	(0.754)
Observations	2,658	2,658	1,191
R-squared	0.262	0.269	0.280
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. Blk MDI is an indicator variable that equals one if the household reside in a county with at least one Black minority deposit institution(MDI). All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.12: OLS estimates of the take-up rate and the reason to be unbanked

VARIABLES	(1) Mistrust	(2) Mistrust	(3) Mistrust
TakeUp Rate	0.0257***	0.0200**	0.104***
	(0.00816)	(0.00788)	(0.0349)
Constant	0.0751	-0.105	3.657**
	(0.153)	(0.543)	(1.609)
Observations	576	576	287
R-squared	0.159	0.172	0.262
Family Controls	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes
Historical Controls	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of unbanked African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that had a Freedman's Bank. TakeUp Rate is defined by $\frac{\text{the total number of accounts opened at a branch} * 0.5}{\text{total African American population in county in 1870}} * 1/10$. Mistrust is an indicator variable that equals 1 if the respondent listed "do not trust banks" as the primary reason to be unbanked. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.13: Internal exposure to the bank failure and the decision to bank, falsification

VARIABLES	(1) Banked	(2) Banked	(3) Banked	(4) Banked	(5) Banked	(6) Banked
Panel A: Whites						
log(Exposure 1)	0.00152	0.00592	0.0119			
	(0.0119)	(0.0122)	(0.0197)			
log(Exposure 2)				-0.00274	0.0105**	0.00936
				(0.00406)	(0.00507)	(0.0153)
Constant	0.876***	0.791***	1.160***	0.899***	0.687***	1.097***
	(0.0476)	(0.101)	(0.385)	(0.0617)	(0.105)	(0.344)
Observations	13,333	13,333	4,259	13,333	13,333	4,259
R-squared	0.125	0.126	0.157	0.125	0.127	0.157
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes
Panel B: Hispanics						
log(Exposure 1)	-0.0404	-0.0563	-0.0276			
	(0.0373)	(0.0353)	(0.184)			
log(Exposure 2)				-0.0206	-0.00417	0.00262
				(0.0143)	(0.0208)	(0.113)
Constant	0.0900	0.0169	4.405**	0.992***	0.929***	2.875*
	(0.182)	(0.313)	(1.747)	(0.136)	(0.257)	(1.661)
Observations	4,331	4,331	867	4,331	4,331	867
R-squared	0.218	0.221	0.321	0.195	0.197	0.286
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes
Panel C: Foreign blacks						
log(Exposure 1)	-0.0163	0.0378	0.214			
	(0.110)	(0.102)	(0.206)			
log(Exposure 2)				0.0178	0.0807*	0.222
				(0.0185)	(0.0413)	(0.157)
Constant	0.683***	1.173**	3.464	0.526***	0.311	2.196
	(0.128)	(0.582)	(2.562)	(0.190)	(0.698)	(2.246)
Observations	476	476	262	476	476	262
R-squared	0.275	0.282	0.354	0.276	0.285	0.358
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of non-African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that was not in the South and never had a Freedman's Bank. The variable Exposure 1 is defined by $\frac{\text{number of branches along the closest rail line}}{\text{distance to the closest rail line}}$. The variable Exposure 2 is defined by $\frac{\text{number of accounts opened along the closest rail line}}{\text{distance to the closest rail line}}$. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Table 2.14: OLS estimates of the internal exposure to the bank failure and the reason to be unbanked

VARIABLES	(1) Mistrust	(2) Mistrust	(3) Mistrust	(4) Mistrust	(5) Mistrust	(6) Mistrust
log(Exposure 1)	0.0134	0.0168	0.0888**			
	(0.0427)	(0.0409)	(0.0399)			
log(Exposure 2)				-0.0337	0.00994	0.0676**
				(0.0250)	(0.0225)	(0.0309)
Constant	0.117	-0.159	1.736	0.421	-0.272	1.218
	(0.150)	(0.587)	(1.267)	(0.297)	(0.597)	(1.241)
Observations	751	751	471	751	751	471
R-squared	0.077	0.100	0.177	0.082	0.101	0.178
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary Controls	No	Yes	Yes	No	Yes	Yes
Historical Controls	No	No	Yes	No	No	Yes

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the county level are in parentheses. The sample consists of unbanked African American respondents who took the CPS Un(der)banked Supplement from 2009 to 2017 and who reside in a county that was not in the South and never had a Freedman's Bank. The variable Exposure 1 is defined by $\frac{\text{number of branches along the closest rail line}}{\text{distance to the closest rail line}}$. The variable Exposure 2 is defined by $\frac{\text{number of accounts opened along the closest rail line}}{\text{distance to the closest rail line}}$. Mistrust is an indicator variable that equals 1 if the respondent listed "do not trust banks" as the primary reason to be unbanked. All regressions include year fixed effects. The regressions are re-weighted by the weight specific for households in the Un(der)banked Supplement. Family controls include: dummies for family income bracket, age, gender, marital status, family size, number of children, education level, employment status. Contemporary county controls include: log of number of banks, log of total population in 2010, percent Black in 2010 and percent urban in 2010. Historical county controls include: county formation date, number of newspaper per 100,000 residents in 1840, proportion of slaves in 1860, log of manufacturing wages in 1870, proportion of whites literate in 1870, proportion of Blacks literate in 1870, proportion of Black farmers in 1870 and measures of residential segregation in 1880 and 1940.

Chapter 3

Income Maintenance Programs and Occupation Turnovers: Evidence from Mincome

Xuanyu Fu [1], UCLA

This paper investigates whether low-income individuals with a guaranteed annual income had a higher likelihood of occupation turnover using data from the Manitoba Basic Annual Income Experiment (Mincome), which randomized assignment of families to the treatment groups. I find that guaranteed income treatment increased the probability of an occupation switch by 8 and 11 ppts respectively for males and wives, where most of the individuals who switched were above the age of 35. On the other hand, the probability of an occupation switch is 13 ppts lower for single women in treatment with younger women driving most of this difference. From survey results, I find that occupation turnovers resulted in non-pecuniary gains for all switchers. Overall, these results suggest that when relieved of financial pressure, a subset of low-income individuals were more likely to switch occupations. In addition, those in the treatment group accrued more

[1]I am especially grateful to Rodrigo Pinto, Adriana Lleras-Muney, Moshe Buchinsky and Ricardo Perez-Truglia for their comments and suggestions. This project also benefited from comments made by numerous participants at the UCLA Applied Microeconomics Proseminar. All errors are my own.

non-pecuniary gain simply because they switched occupations more often.

3.1 Introduction

Occupation turnover remains an important topic in labor economics. It affects not only the growth of wages but also non-pecuniary rewards for work[2]. So far, occupation turnover is understudied in low-income families. Most of the existing literature focusing on such turnover has used datasets that pooled individuals from across the income distribution, which potentially masks the different behaviors of low-income individuals[3]. The labor turnover of low-income individuals may differ from that of the general population for several reasons. First of all, these individuals are more credit constrained, and thus their turnover behavior is likely to be hampered by their financial situation. Second, low-income individuals tend to be low-skilled as well. The general population tends to switch jobs within their specific skill set, which means that they often switch employers, but not job title. Low-skilled individuals with less-specialized skill sets may switch occupations more often than the general population.

In the 1970s, U.S. and Canada conducted several income maintenance experiments (IMEs) that randomly allocated low-income families into treatment groups which guaranteed a basic annual income[4]. The IMEs were implemented as a negative income tax. The negative income tax scheme guaranteed a specified cash benefit for families with no other income. However, the amount provided was reduced at a specified rate as other sources of income rise. More recently, policy makers in various countries are renewing their interests in basic income experiments as a way to combat poverty and inequality[5]. Due to the experimental nature of these cash assistance programs, the various IMEs that was conducted in the 1970s provide the perfect setting to study occupation turnover of low-income individuals when they are relieved of credit constraint for the duration of the experiment. In addition, studying

[2]See Akerlof et al. (1988) and Bartel and Borjas (1981).

[3]For example, researchers often utilize the National Longitudinal Survey of Older and Young Men, or datasets from the U.S. Bureau of Labor Statistics.

[4]Families were randomly allocated into treatment and control groups conditioned on income brackets.

[5]See `https://www.vox.com/future-perfect/2020/2/19/21112570/universal-basic-income-ubi-map` for a description of all the countries that have experimented with basic income.

the labor market behaviors of participants in IMEs from the 1970s can inform the design of new basic income policies around the world.

In this paper, I investigate occupation turnover for low-income individuals using data from the Manitoba Basic Annual Income Experiment (Mincome), which was an IME carried out in Manitoba, Canada between 1974 and 1979. Like the IMEs conducted in the United States, Mincome implemented the guaranteed income using a negative income tax. For households in the treatment group, they were assigned to groups with varying levels of cash benefit guarantees and benefit reduction rates. The experiment collected 11 waves of survey data from households both in the treatment and control group. Each survey not only contains detailed demographic characteristics about each member of the household, but it also documents their labor market participation, occupation and attitudes about their jobs. To analyze the labor market behavior of individuals in different types of households, I study the difference in occupation turnover between the treatment and control groups separately for males, females in double-headed households (wives), and females in single-headed households.

This study finds that, when relieved by financial pressure, males and wives under treatment showed a higher probability of occupation switch compared to the control. Specifically, the probability of an occupation switch increased by 8 and 11 percentage points for males and wives respectively when they received a guaranteed income. The results are fairly robust to various specifications that consider the possibility of non-random attrition and selection into employment. On the other hand, I find that single females were less likely to switch occupation while in treatment: the probability of occupation switch was 13 percentage points lower for single women in treatment. By analyzing heterogeneity in treatment effects across age, I find that the increase in occupation turnover for males and wives was driven by older workers being induced to switch under treatment, while the decrease in turnover by single women was driven by younger women being more reluctant to switch occupations under treatment.

In addition to examining whether an individual switch occupation, I investigate whether

occupation turnover results in non-pecuniary gains, and whether those in treatment experience differential gains after an occupation switch compared to the control group. In this setting, the non-pecuniary gains are measured by attitudinal questions posed in surveys. Those that experienced non-pecuniary gains post-occupation switch expressed higher interests and enjoyment in their new job and report a higher sense of job satisfaction. Throughout the duration of the experiment, I find that occupation switch is associated with non-pecuniary gains for individuals both in the treatment and control groups. However, who received guaranteed annual income did not experience differential non-pecuniary benefits. Overall, one can conclude that the treatment group received more non-pecuniary benefits because they switch occupation more frequently. Lastly, inspired by existing literature that examine the relationship between tax rates and entrepreneurship, I examine whether households assigned to different benefit reduction rates in the treatment group experience differential rates of occupation turnover[6]. In the Mincome setting, I did not find differential occupation turnover for low-income individuals across different reduction rates.

This paper contributes to and build on three strands of literature. First, there is a large strand of literature that investigates the causes of job and occupation turnover and how turnover impacts future wage growth and job satisfaction. Bartel and Borjas (1988) demonstrated that labor turnover affects both wage growth across jobs and within jobs for men. The wage gains from turnover appear to be positive for young men and zero or negative for older men. This difference stems from the nature of job turnover: older men quit mainly due to dissatisfaction with the current job, while younger men quit to find a better paid job. Topel and Ward (1992) found that most of the job-changing occurs in workers below the age of 35, and found evidence that mobility declines with age as workers sort themselves into "good" employment matches. Aside from wage growth, Akerlof et al. (1988) and Altonji and Paxon (1988) showed that job changes lead to an increase in job satisfaction. In my paper, I analyze the occupation turnover for a specific group of individuals: low-income households.

[6]See Gentry and Hubbard (2002, 2004) on the relationship between nonlinearities in taxes and entrepreneurship.

The results of my paper suggest that credit constraint and the uncertainty time period of job searches can impede occupation turnover for low income families. In addition, I find that occupation switchers report higher job satisfaction after the switch. This paper shows that aside from effects on wage growth, non-pecuniary rewards should also be accounted for when attempting to answering the question of whether mobility "pays" for low-income workers.

Second, this paper contributes to the literature examining the relationship between tax rates and occupation turnover. The decision of whether to switch occupations relies on one's degree of risk aversion, which can be influenced by marginal tax rates. A string of related literature examines how nonlinearities in taxes can potentially shift risk aversion, resulting in changes in job and occupation turnover. In particular, Gentry and Hubbard (2002) analyzed how nonlinearities in income taxation impact job impacts entrepreneurship. They find that progressive marginal tax rates discourage entry into self-employment and business ownership. Their 2004 paper used the same method to analyze the decision to switch jobs and arrived at similar conclusions. In this paper, I exploit the fact that Mincome randomly assign households into different benefit reduction rates within the treatment group and find that individuals do not experience different rates of occupation turnover across the different reduction rates.

Lastly, my paper is related to the large strand of literature that used the various IMEs designed in the 1970s to examine the impact of guaranteed income on labor market outcomes, family structure, health...etc[7]. The purpose of the original wave of federally funded social experiments was to test whether income maintenance would reduce work effort of the poor. As expected, the majority of the literature focuses on the labor supply response. Most papers that focused on labor supply outcomes concluded that these programs caused moderate reduction in work effort, but the treated individuals did not completely withdraw from the labor force. The paper that is the most similar to mine is Price and Song (2018). In this paper, they investigate the long-term effects of an IME conducted in the United States and

[7]Munnel (1986) and Widerquist (2005) reviewed and summarized the findings from previous literature that looked at outcomes from the various income maintenance experiments.

found that treatment induced workers who took time off during the experiment to switch to jobs that required less education. Hence, when they do return to work, they switched to jobs that paid similar wages but were worse in non-monetary ways. This, in turns, decreased their post-experimental earnings and increased disability benefits. In my study, I do not consider the occupation turnover of individuals who temporarily dropped out of the labor market during the duration of the experiment. However, for workers in treatment who remain in the labor market, I find that treatment induced higher occupation turnover and they switched to occupations that are better in non-monetary ways, at least in the short-term.

The remainder of the paper proceeds as follows: Section 2 outlines potential mechanisms to explain why individuals were more likely to switch occupation when they received a guaranteed income. Section 3 provides information on the set-up and implementation of Mincome. Section 4 describes the data and variables used in this paper. Section 5 outlines the empirical strategies used to analyze whether treatment induced occupation turnover. Section 6 discusses the results and Section 7 concludes.

3.2 Discussion of Mechanism

There are a few plausible theories to explain why individuals are more likely to switch occupations on guaranteed income. According to the classic theory of compensating wage differential, people may work in less enjoyable careers for higher pay. Hence, even without work, treated individuals with guaranteed income provided the IME, could be prompted to search and switch to lower paying but more enjoyable occupations. Survey questions that gauge an individual's preference and utility associated with an occupation could shed some light on this mechanism. If this mechanism holds, the data would show that individuals report lower labor market wages and higher job satisfaction post-occupation switch.

In addition, low-income families are typically severely credit-constrained. Even if a job switch is desired, stemming either from a desire to find a better job or dissatisfaction with the current job, many families may delay such decisions because they hold uncertainty over the

length of the job search/unemployment period. Credit constrained families are typically unable to save enough to guard against these uncertainties. Being treated in an IME effectively acts as insurance against uncertain length of job search, potentially inducing individuals to act on the previously suppressed desire to switch jobs.

In the discussion of mechanism, it is important to mention related theories on how taxation impacts risk aversion and labor market turnover Even though the focus of this paper is not on taxation, these theories provide insight into how individuals' behavior shifts when faced with different incentives payoffs. The theories outlined by Gentry and Hubbard (2002, 2004) predicted ambiguous direction on the likelihood of labor market turnover when facing a progressive tax system due to countervailing incentive and insurance effects. Greater tax progressivity can offer insurance through the tax system against uninsured idiosyncratic risk, while lowering the potential returns to investment at the same time. Income maintenance, like the tax structure, provides both insurance and disincentive to investment. But given the first two mechanisms mentioned, the insurance effect is likely to outweigh the disincentive effect, inducing more individuals to switch occupations.

3.3 The Mincome Experiment

Mincome, short for Manitoba Basic Annual Income Experiment, was a federally funded multi-million-dollar income maintenance study that occurred in Canada in the 1970s[8]. This IME was modeled after the IMEs conducted in the United States in a similar time period. During the American War on Poverty, interest in guaranteed income among policy makers sparked a series of large-scale IMEs. The U.S. conducted the New Jersey IME from 1968 to 1972, the Rural IME from 1869 to 1973, the Gary IME from 1971to 1975 and the Seattle/Denver IME from 1970 to 1978[9]. At the heart of these experiments was the question: to what extent would a guaranteed income cause able-bodied individuals to reduce their

[8]Mincome cost 17 million in 1975 Canadian dollar.

[9]The New Jesey IME was conducted in New Jersey and Pennsylvania. The Rural IME was conducted in Iowa and North Carolina. The Gary IME was conducted in Gary, Indiana.

hours of work or exit the labor force? The American War on Poverty attracted attention in Canada. By 1970, the Department of National Health and Welfare emphasized the potential of guaranteed income as an anti-poverty measure, resulting in political support and funding for an IME to be implemented in Canada. Mincome, like its U.S. counterparts, was implemented as a negative income tax. The negative income tax scheme includes a cash transfer amount (called the guarantee) for which a family is eligible if it has no other income, and a benefit reduction rate at which the guarantee is reduced as other income rises. The Mincome experiment has eight treatment arms, each with a unique combination of guarantee and benefit reduction rate. The various treatment arms are outlined in Table 3.1. Mincome also enrolled a control group which faced the same progressive tax schedule as others in Manitoba at the time.

Mincome was implemented in the cities of Winnipeg and Dauphin, Manitoba. The experiment was split into three sites: urban Winnipeg, rural Winnipeg and Dauphin. The characteristics of the two cities chosen for this IME are very different. Winnipeg is the capital and largest city in the province of Manitoba, with a population of 450,000. Dauphin is town in Manitoba with a population of less than 10,000. The experiment officially began in 1974. Payments starting in 1975 and lasted for three years thereafter. The treated families were aware of the limited lifespan of the experiment. In addition, the continuation of participation in Mincome was conditioned on completing a periodic survey every four months in addition to filing a monthly income reporting file (IRF)[10]. To be eligible for Mincome, families must have had an able-bodied head of household under 58 years old, with annual income less than $13,000 for a family of four[11]. The participating families received payments on a monthly basis based on the IRF received the previous month plus an adjustment amount[12]. Even though payment was distributed on a monthly basis, the accounting period was on an annual basis. Thus, actual monthly payments over the year were reconciled with their

[10]Families assigned to the control group were also asked to complete a monthly IRF. They received $10 (1975 Canadian dollars) for each completed IRF.

[11]$13,000 in 1975 Canadian dollars is equivalent to $40,404 in 2016 U.S. dollar.

[12]The adjustment occurs because reported income above the break-even level is "carried forward" and counted as income in months when income falls below the break-even level.

entitlement based upon the total income within the same year. At the end of the calendar year, underpayments to families were corrected and overpayments were recovered.

There are two important things to note regarding the details of the experiment. First, the Mincome experiment in Winnipeg was conducted separately from the experiment in Dauphin. In the urban site of Winnipeg, the participants were interviewed and assigned to the eight treatment arms and control group prior to the beginning of the experiment. The participants represented only a small subset of the population of Winnipeg, so in theory, no spillover effects should be observed. On the other hand, in Dauphin, there existed only one treatment (treatment with guarantee =$3800 and benefit reduction rate =0.5). Aside from the original wave of enrollees, any families in Dauphin who met the income requirement could be subsequently enrolled anytime during the experiment, leading to a potential general equilibrium framework[13]. For ease of identification, this study will focus on the partial equilibrium experimental design in the urban site in Winnipeg and results should be interpreted in such a setting accordingly.

Second, Mincome utilized a variant of the Watts-Conlisk assignment model to allocate families to experimental treatment in order to minimize cost of implementation[14]. Prior to the experiment, each family was assigned to a cell based on the normal income group. With this assignment design, a higher fraction of families in the high normal income category was assigned to the treatment arm with a high break-even level to reduce transfer payment cost. Unlike simple random assignment, the Watts-Conlisk model results in a sample where the household characteristics are not orthogonal to the experimental treatments and the experimental treatments are not orthogonal to each other. As other researchers have noted, a simple comparison between treatment and control does not provide the true treatment effect[15]. As Ashenfelter and Plant (1990) pointed out, a simple re-weighting scheme would yield an unbiased estimate of the expected experimental effect. The method outlined in Ashenfelter and Plant (1990) will be the method used throughout this paper.

[13]Researchers estimated that approximately 30% of the residents in Dauphin participated in Mincome.
[14]This model was also used to assign families in the New Jersey IME and the Seattle/Denver IME.
[15]See Keely and Robins (1980) and Ashenfelter and Plant (1990)

3.4 Data

This paper uses the data collected from the periodic surveys filled out by the households every four months. The surveys include information regarding employment, income, wealth, work attitudes and other demographics characteristics for 920 intact families[16]. The dataset consists of 11 surveys, in which the first two were conducted prior to rolling out the payments. In particular, the first survey was the baseline survey. Information from the baseline survey was used to determine the normal income group, which forms the basis for stratification. Table 3.2 displays summary statistics calculated using data from the baseline survey; t-tests suggest that the treatment and control groups were balanced. The second survey was the enrollment survey, which the families had to fill out to begin receiving payments. In this paper, I consider the baseline and enrollment surveys to be the first year of the experiment (1974). Year two of the experiment spans surveys 3-5, which was the first year of the payment cycle. Year three spans surveys 6-8 and year four spans surveys 9-11, which were the second and third year of Mincome payments respectively. Due to the design of the experiment, families were not interviewed at the same time every survey. Instead, interviews were conducted continuously for each survey. For example, a family might have been interviewed in January for survey 3 while another family was interviewed in March for the same survey. However, the order of the interviews was preserved in the sense that the family surveyed in January would be surveyed in May for survey 4; the families surveyed in March would be surveyed again in July. Because families were surveyed at different times, I annualized the data for comparison purposes.

I construct the occupation turnover measure using the occupation code reported by the respondents in the periodic surveys. The occupation code used was derived from the Canadian Classification and Dictionary of Occupations. The turnover measure is a dummy which equals one for year t if an individual experienced a change in occupation anytime in year t and was employed for the majority of the year prior to the switch. If an individual was unem-

[16]Intact families are defined as households with no head split or head join from the start to the end of the experiment.

ployed or attritioned from the experiment, then the turnover measure was coded as missing. Because the turnover measure was constructed from occupation codes, a few caveats must be noted. First of all, this measure does not capture job switches within the same occupation definition. Moreover, the occupation code cannot differentiate between switch into self-employment versus being an employee. Arguably, this measure is an imperfect measurement of job turnover. However, it should be noted that because low-skilled workers tend to be less specialized, low income workers are much more likely to switch occupations when they switch jobs (e.g. switch from being a waiter to a cashier) compared to workers on the higher end of the skill distribution. And if there are reasons to believe that many workers would like to switch into more enjoyable occupations after being selected into treatment, i.e. the compensation differential story, then this occupation turnover should capture switches induced by this mechanism.

To measure non-pecuniary benefits, this paper took advantage of the attitudes file that was incorporated into three of the eleven periodic surveys. Work oriented survey questions are of particular interest because they capture the non-monetary aspects of an individual's job. The head of households was asked these job orientation questions in surveys 1, 7 and 11. The participants were given a statement by the interviewers where one would respond to the statement by telling the interviewer the magnitude to which they agreed with the statement. The coding categories for these questions were: 1= strongly agree, 2 = agree somewhat, 3 = neither agree nor disagree, 4 = disagree somewhat, 5 = strongly disagree. The following statements were posed to the heads of households:

1. "It is more important for a job to offer opportunity than security."

2. "Some of my main interests and pleasures in life are connected with my work."

3. "I don't really enjoy most of the work I do, but I feel I must do it in order to have other things that I want and need."

4. "It is extremely important to have a higher income."

In this study, I measure non-pecuniary gains by analyzing how much did an individual agreed with statements 2 and 3 following an occupation switch. If a respondent agrees more with statement 2 after an occupation switch, then this would be counted as non-pecuniary gains. Similarly, respondents would experience non-pecuniary gains if they agree less with statement 3 following an occupation switch.

While the Mincome surveys offer an abundance of information, this dataset unfortunately does not accurately measure wages. The survey reports occupation codes, income, and hours worked three times a year. However, the exact timing of the switch within the four-months intervals remains unknown. This means that one cannot disentangle income and hours worked before and after the exact moment of the switch. With accurate wage data, one can analyze if occupation switchers were switching into higher/lower paying positions and establish whether the compensating differential story is a valid mechanism. Since all participants in the Mincome IME were required to submit their tax returns, the administrative tax data would provide accurate wage measures including the exact time of occupation switch if they could be obtained; but this is left for future work.

3.5 Empirical Strategy

3.5.1 Guaranteed Income Treatment on Occupation Turnover

To determine the causal effect of whether assignment into the treatment groups induce individuals to switch occupations, I estimated the following ordinary least squares regression separately for males, wives and single-headed females.

$$OCCTURNOVER_{it} = \beta_1 + \beta_2 TREAT_i + \lambda_t + \epsilon_{it} \tag{3.1}$$

where $OCCTURNOVER_{it}$ is an indicator variable that takes the value 1 for individual i in year t if I observe this individual to change occupation and was employed for the majority of the year prior to the switch. $TREAT_i$ is an indicator variable that takes the value 1 if an

individual was assigned into a treatment group that received guaranteed income.

Since the occupation turnover measure was dichotomous, the following probit model was also estimated to relax the assumption of linearity.

$$P(OCCTURNOVER_{it}) = \Phi(\beta_1 + \beta_2 TREAT_i + \lambda_t) \tag{3.2}$$

The observations are pooled over all the years in which payments were distributed, and the standard errors are clustered at the family level. In addition, the re-weighting method outlined in Ashenfelter and Plant (1990) was used to correct for random assignment condition on normal income group due to the Watts-Conlisk model for all regressions[17].

The un-biasedness of the OLS and the consistency of the probit estimates relies on the assumption that $E[TREAT_i, \epsilon_{it}] = 0$. Even though given a normal income group, randomization was guaranteed, there are still factors that threaten exogeneity. In the Mincome setting, non-random attrition and selection into employment lead to endogenous treatment observations and biased results. Selection into employment could depend on personal characteristics such as income earned by spouse, unemployment duration, unemployment benefit or macro characteristics such as regional unemployment rate[18] . Regarding attrition, the potential causes could include: low break-even income, meager payment for completing surveys and IRFs, high cost of filing paperwork, or a household member receiving job offers in another city. Figure 3.1 shows the attrition in each year of the experiment by family types. As shown in the figure, single individuals were more likely to attrition, especially due to moving. Single individuals tend to be younger, have less attachment to the workforce, and more likely to switch occupations. This shows that there is a potential to underestimate the occupation turnover rate due to attrition. Another problem to be aware of is that if systematic attrition occurs at a different rate for the treatment and control groups, then the

[17]Keeley and Robbins (1980) showed that including assignment variables indicating one's normal income group will yield unbiased results under the Watts-Conlisk assignment model, and Price and Song (2016) used this method to analyze the Seattle/Dever IME in a recent paper. This paper also tried including the assignment variables instead of using the reweighting method, and the results changed very little.

[18]Studies such as Brown (2013) and Prasad (2003) have also dealt with selection into employment.

two groups are no longer comparable. I check the balance between treatment and control groups in year 2 and year 4 for the non-attrited sample. The results are shown in Table 3.10 in the appendix. There are some statistically significant differences between the treatment and control group to note in this table. First, the percentage of homeownership is different between control and treatment in year 2. Second, the treatment group has more young children compared to controls in both years, suggesting that families with young children are more likely to stay with the program, or that the experiment encouraged those in treatment to have more children. Due to attrition, households in the treatment and control group differed in dimensions that could affect occupation turnover.

In this paper, the issues of attrition and selection into employment will be dealt with in two ways. First, I estimate a treatment effect bound for non-random sample selection using the methods outlined in Lee (2009). Compared to other selection correction estimators, the bounds developed by Lee (2009) have the advantage of requiring few assumptions while yielding informative bounds. To implement this procedure, one needs to assume random assignment of treatment and monotonicity about the selection mechanism. While the treatment bounds are informative, it is worthwhile to derive a precise treatment effect at the cost of imposing stronger assumptions, especially if the derived bounds are not very tight. Hence, after estimating a Lee bound, I use a Heckman (1979) two-step estimator to account for attrition and selection into working assuming joint normality of the error terms. Because the occupation turnover is only observed for those who were employed and did not attrition, the selection equation is modeled jointly using Equation (3) - Equation (5).

$$OCCTURNOVER^{*}_{it} = \beta_1 + \beta_2 TREAT_i + \lambda_t + \epsilon_{it} \tag{3.3}$$

$$D_{it} = \mathbb{1}(Z'_{it} + \eta_{it} > 0) \tag{3.4}$$

$$OCCTURNOVER_{it} = D_{it} * OCCTURNOVER^{*}_{it} \tag{3.5}$$

and that $\begin{pmatrix} \epsilon_{it} \\ \eta_{it} \end{pmatrix} \mid \begin{pmatrix} TREAT_i & Z_{it} \end{pmatrix} \sim N(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix})$.

In Equation (4), Z_{it} is a vector of variables that determines both the decision to work and the decision to drop out of the program. For example, the number of young children and family income are variables that can impact both decisions. Having young children impacts the labor market participation for the caretaker in the household, while making families less likely to drop out of the problem. A higher household income is associated with a decreased probability of women entering the labor market and a higher chance of dropping out of the experiment[19]. In addition, there are certain variables that only affect one of the two decisions. For example, families who owned a home were less likely to attrition, while possibly having no impact on the labor supply decision on the extensive margin for the family members. In this specification, any variables that impact either one or both of the decisions - household income, income of spouse, number of children, age of head of household, homeownership and welfare received by government - are included as part of Z_{it}.

3.5.2 Marginal Tax Rate and Occupation Turnover

Gentry and Hubbard (2004) suggests that there is a causal relationship between marginal tax rate and job switching. Since IMEs experimentally subject families to different benefit reduction rate (marginal tax rates) conditional upon their normal income group, the design of the experiment offers a clean identification of the causal effect of marginal tax rate on occupation turnover. In this study, I estimate the effect of marginal tax rate on occupation switch using the following specification:

$$OCCTURNOVER_{it} = \beta_1 + \beta_2 MTR1_i + \beta_3 MTR2_i + \lambda_t + \epsilon_{it} \tag{3.6}$$

Since families in treatment were assigned to different treatment arms with the following marginal tax rates: 0.35, 0.50 or 0.75. $MTR1_i$ is a dummy with the value of one if the

[19]Higher income households might be more likely to drop out of the experiment because their income is above the break-even level.

family was assigned to a treatment arm with marginal tax rate of 0.35, and $MTR2_i$ is a dummy with a value of one if the family was assigned to a treatment arm with marginal tax rate of 0.50. In this analysis, families from the control group are excluded since they face the more complicated combined provincial and federal tax schedule of Manitoba, Canada. β_2 and β_3 are causal estimates of how much more likely an individual was to switch occupation when facing a marginal tax rate of 0.35 and 0.50, compared to 0.75. A probit estimation with the same specification was estimated to allow for non-linearity. In addition, correction for selection into employment and attrition for both the least square and probit models was included using the aforementioned selection mechanisms.

3.5.3 Occupation Switch and Non-Pecuniary Benefits

To estimate whether there were non-pecuniary rewards in the event of an occupation switch, I estimate the following least square regression:

$$\Delta ATTITUDE_{it} = \beta_1 + \beta_2 OCCTURNOVER_{it} + \beta_3 OCCTURNOVER_{it} * TREAT_i + \lambda_t + \epsilon_{it} \tag{3.7}$$

$\Delta ATTITUDE_{it}$ measures the change in how much the respondent agreed with work-related statements in the periodic surveys attitudes file[20]. From the 11 periodic surveys, the attitude file appear in surveys 1, 7 and 11. $\Delta ATTITUDE_{it}$ at $t = 7$ measures the change in attitude from survey 1 to survey 7. Similarly, $\Delta ATTITUDE_{it}$ at $t = 11$ measures the change in attitude from survey 7 to 11. $OCCTURNOVER_{it}$ at $t = 7$ is an indicator variable that equals 1 if an individual switched occupation anytime between survey 1 and 7 and this individual was employed in both survey 1 and survey 7. The same definition of $OCCTURNOVER_{it}$ holds for when $t = 11$. In this analysis, neither β_2 nor β_3 carries a causal interpretation. However, the signs of the coefficients are still informative. β_2 points to the correlation between attitude change and occupation switch and β_3 points to whether attitudinal changes differ between occupation switchers in control versus those in treatment.

[20]Specific work-related statements are described in the data section.

Lastly, I investigate whether attitudinal changes that occurs with occupation switch differ based on the age of the individual. Previous studies have found that younger workers are more likely to quit for a higher paid job while older workers are more likely to quit because they are dissatisfied with their current job. Since I currently do not have accurate measurement of wages at this stage, I am unable to confirm/deny if younger workers switched into higher paying occupations. However, this analysis will shed light on whether non-pecuniary benefits associated with occupation switch differ by age group. Specifically, I run Equation (7) separately for younger and older workers, where younger workers are defined as those under the age of 35 at the time of survey[21].

3.6 Results

3.6.1 Guaranteed Income Treatment on Occupation Turnover

Figure 3.2 shows the difference in means between treatment and control groups in the probability of occupation turnover for each year that income maintenance payments were received; the bars included represent 90% confidence intervals. The raw difference in means is presented separately for males, wives and single females. Note that these naïve patterns do not account for the Watts-Conlisk assignment algorithm, selection into employment and the possibility of non-random attrition. Nonetheless, it is informative to visualize the raw difference in means between treatment and control.

Panel A of Figure 3.2 shows that there exist statistically significant differences in the percentage of occupation switch between treatment and control groups for males, with those in the treatment group having a higher percentage of occupation switching. Even though year 3 isn't statistically significant, those who were in the treatment still had a higher percentage of occupation switching. Panel B shows that the pattern for wives is similar. Those assigned to treatment were more likely to switch occupation in all years, with the difference in year

[21]Topel and Ward (1992) showed that most of the job switches in an individual's career occurs before the age of 35.

4 being statistically significant. On the other hand, the behavior for single females shows a different trend (Panel C of Figure 3.2). The naïve figure shows that the control group exhibited a higher percentage of occupation switching compared to the treatment group.

To account for the Watts-Conlisk assignment model, Table 3.3 shows the weighted OLS and probit estimates from observations pooled across all years. For the probit specifications, the table reports the marginal effects. The OLS results show a statistically positive relationship between probability of occupation switch and being selected into treatment for both males and wives. From the probit estimates, the magnitude of the marginal effect is very similar to that of OLS. Being in treatment increases the probability of switching occupation by 7.8 percentage points for males and 11.7 percentage points for wives. Similar to the pattern shown in Panel C of Figure 3.2, Column 5 and Column 6 of Table 3.3 shows that being in treatment decreases the probability of switching occupation by 13.5 percentage points for singe females. In Table 3.11 in the appendix, I present estimates separately for each year in which the experiment was active. Results for the OLS regression separated by year largely corroborate the naïve interpretation from Figure 3.2, with the exception that the treatment effects on single females are no longer statistically significant for any of the years.

Despite correcting for the Watts-Conlisk assignment model, the results presented in Table 3.3 are still biased since there exist nonrandom selection into employment and/or nonrandom attrition. To take these possibilities into account, I estimate Lee bounds (2009) for the OLS specification. The treatment effect bounds for the male, wives and single female samples are presented in Table 3.4. Table 3.4 used data pooled across all years; lee bound estimates using data separately for each year are presented in Table 3.12 in the appendix. Both Table 3.13 and Table 3.12 in the appendix show that the treatment bounds are fairly wide and only the upper bounds are statistically significant and positive. The results largely hold when the bounding exercise is done separately for each year. For single females, only the lower bound shows a statistically negative relationship between treatment and probability of occupation switch.

Due to the wide bounds of the estimated treatment effects, it becomes important to pin

down more precise estimates that take attrition and selection into account. In order to do this, more stringent assumptions must be imposed. The two-step Heckman correction for selection bias was done for both the least square and the probit specifications. Table 3.5 displays the results from the OLS specification using the Heckman two-step correction. An inverse mills ratio was constructed and included in the regressions. The treatment coefficients remain similar in magnitude after correcting for selection into employment and attrition. The results show that being in treatment increases occupation turnover by 8.4 percentage points for males and 11.3 percentage points for wives, and both coefficients are statistically significant at 5 percent. For single females, being in treatment decreases occupation turnover by 12.8 ppts. To allow for non-linearity, results from probit estimation using Heckman correction are shown in Table 3.13 in the appendix. The probit results imply that being in treatment increases the probability of occupation switch for males by 4.51 percentage points, which is half the magnitude found in the OLS specification. In addition, being in treatment increases the probability of occupation switch by 10.24 percentage points for wives and decreases the probability of switch by 13.12 percentage points for single women. The marginal effects shown for probit are statistically significant at 10 percent for men and wives, but not statistically significant for single-headed females. Results from OLS regressions conducted separately by year using the Heckman two-step correction are available in Table 3.14 in the appendix. Table 3.14 in the appendix shows that treatment coefficients are statistically significant only for the last year in which payments were received for males and wives, and none of the annual treatment coefficients are statistically significant for single females.

Note that all the treatment coefficients under the OLS Heckman correction in Table 3.5 lie within the bounds computed in Table 3.4. Overall, the results show that if one assumes random selection into employment and random attrition, then being in treatment in the IME increases the probability of occupation switch for males and females in double-headed households and decreases the probability of occupation switch for single-headed females. Taking into account selection and assuming joint normality, the results remain significant

for both males and wives, but only marginally significant for single-headed females. Overall, the findings suggest that working individuals in double-headed households are more likely to switch careers if they are placed into an income maintenance program. Single females in treatment exhibited no change, or even decreased probability of switching occupations under treatment. The preliminary results suggest that members in double-headed households are more likely to partake in occupation switching while in treatment[22]. One potential explanation is that individuals in double-headed households are more likely to be credit constrained. Especially with children, it is very risky for low-income parents to quit their job. With the uncertainty of the length of job search and unemployment, credit constrained families simply cannot afford to quit. And these families can only afford to switch occupations when they are offered a steady stream of income under the income maintenance experiment. But for single individuals who are typically not as credit constrained, many would choose to switch occupation regardless of treatment under the IME[23]. Another potential explanation is that the insurance effect provided by Mincome outweighs the disincentive effect for individuals in double-headed households but the disincentive effect outweighs the insurance effect for single-headed families.

3.6.2 Marginal Tax Rate and Occupation Turnover

Because the Mincome IME experimentally assigned families into varying levels of benefit reduction rates (marginal tax rate, or MTR), it provides a useful set-up to test the causal effect of marginal tax rate on behavioral responses that relate to risk, such as occupation switching. Gentry and Hubbard (2004) showed that an increase in MTR causes a decrease in the probability of job switching. To test whether MTR affects occupation turnover, the control sample was dropped and the families placed in different marginal tax rate groups were compared against one another. The OLS results with and without correcting for selec-

[22]In the male sample, 80 percent are married and 17 percent are single individuals. For females, 60 percent are married, 20 percent are single individuals and 20 percent are single mothers.

[23]This explanation does not explain the behaviors exhibit by single mothers, who are likely more credit constrained.

tion are presented in Table 3.6. As oppose to the findings of Gentry and Hubbard (2004), Table 3.6 shows that there is no systematic relationship between marginal tax rate and the probability of occupation switch for either males, wives or single females. For males and wives, the probability of an occupation switch for any given treatment marginal tax rate is not statistically different from that of another treatment marginal tax rate; the results hold after correcting for selection using the Heckman 2-step estimator. For single females, the probability of an occupation switch being in a treatment group with a 50 percent marginal tax rate is 15 percentage points less than that of women assigned to a group with a 75 percent marginal tax rate. However, the data show no difference in occupation turnover between single women assigned to a group with 35 percent MTR and a group with 75 percent MTR, revealing no systematic difference in occupation turnover for different MTRs.

3.6.3 Occupation Switch and Non-Pecuniary Benefits

To examine whether non-pecuniary benefits changed after an occupation switch, an occupation turnover indicator was regressed on measurement of attitude changes to gauge the correlation between the two. Table 3.7 presents the results[24]. All attitude variables were recorded as 1 if the respondent strongly agreed with the statement and as 5 if a respondent strongly disagreed. Hence, following an occupation turnover, the β_2 coefficient in Equation (7) should be negative if a respondent agreed more with an attitude statement, If a respondent agreed less with the statement following an occupation switch, the β_2 coefficient would be positive.

Column 1 and 2 in Table 3.7 correspond to the attitude statement: "Some of my main interests and pleasures in life are connected with my work." The results show that respondents were more likely to agree with this statement after an occupation switch. However, occupation switchers who were in treatment shows no difference in attitude change compared to those assigned to the control group. Column 3 and 4 in Table 3.7 correspond to the

[24]Only the head of household in double-headed households was asked to participate in the attitude file. Hence the vast majority of the respondents to the attitude file are males.

following attitude statement: "I don't really enjoy most of the work I do, but I feel I must do it in order to have other things that I want and need". The β_2 coefficient is positive, which means respondents agreed less with this statement post occupation switch. The coefficient is only marginally significant and the sentiment change of occupation switchers in the treatment group is not different from the control group[25]. The columns labeled "Change in Job Opportunity" and "Change in Income Importance" correspond with the statements "It is more important for a job to offer opportunity than security" and "It is extremely important to have a higher income" respectively. There exist no statistically significant differences in attitudes on opportunity importance and income importance post-occupation switch. Overall, the results presented in Table 3.7 show that individuals reported higher enjoyment from their job and less likely to feel like they need the job to pay their bills after switching occupation. However, the non-pecuniary gains were not unique to occupation switchers placed in treatment; the improved sentiment was shared among all who switched careers.

3.6.4 Heterogeneous Effect Based on Age

Previous studies focusing on job turnover have found that most switches within a life-cycle occur prior to the age of 35 because young workers are finding better employment matches as they sample new jobs. This study has already shown that being in guaranteed income treatment increases occupation switching for men and wives pooling across all ages, but it is unclear if being in treatment would affect younger and older workers' occupation turnover probabilities differentially. Table 3.8 examines the effect of guaranteed income treatment on occupation switch based on age[26]. The results show that older males and older wives had a higher probability of occupation switch while in treatment. However, being in treatment had no significant effect on occupation turnover for younger individuals, even after accounting for selection into employment and non-random attrition. In addition, for single women, the results imply that younger single females were less likely to switch occupation while assigned

[25]The statistical significance is lost after including interaction terms.

[26]There are two age groups. The younger group includes workers under 35 and the older group includes workers at and above the age of 35.

to treatment across all specifications.

The differential treatment effect between younger and older individuals could be potentially explained by quit intention and credit constraint. Younger workers are more likely to switch occupation on average, as seen by the higher constant in all regressions. If younger workers are sampling jobs and switching occupations to find the right career fit, then it is not surprising that no differential response could be observed by being in treatment because they are more likely to switch occupations no matter what. The older individuals, especially in double-headed households, are more likely to have children and face more financial constraints compared to the younger workers. For them, the income maintenance treatment can provide financial insurance in the case of prolonged job search or unemployment. This could explain why older males and wives have a higher probability of occupation switch under income maintenance treatment. The reason why younger single females were less likely to switch occupations when sorted into treatment even after accounting for selection and attrition is harder to explain. Exploring the explanation for this phenomenon can be an interesting avenue for future research.

Finally, to analyze the possibility that the non-pecuniary gains from occupation switching fell more into a certain age group, I estimate Equation (7) separately for younger and older men in double-headed households. The two attitude statements I analyze are "Some of my main interests and pleasures in life are connected with my work" and "I don't really enjoy most of the work I do, but I feel like I must do it in order to have other things that I want and need". The results are shown in Table 3.9. Columns 1 and 2 correspondent with the first statement while Columns 3 and 4 correspond to the second statement. Panel A of Table 3.9 suggest that older workers tended to enjoy their occupations more after a switch. Younger workers, on the other hand, did not report higher job satisfaction post occupation switch (Panel B of Table 3.9). This corroborates the theory that older occupation switchers are more likely to quit because they are dissatisfied with their old job while younger switchers often quit to switch to a higher paying job.

3.7 Conclusion

In this paper, I utilize the setting of the Manitoba Basic Annual Income experiment to show that guaranteed income treatment increased the probability of occupation turnover for males and wives by 8 and 11 percentage points respectively, where most of the individuals who switched occupations were above the age of 35. In addition, the results show that single women were marginally less likely to switch occupations while in treatment, with the younger women driving most of this difference. While I cannot analyze whether workers increased their wage follow an occupation switch, the results show that occupation switchers enjoyed non-pecuniary gains following the switch, especially for the older workers. Occupation switchers assigned to guaranteed income treatment did not experience more non-pecuniary gains compared to the control group. However, those in the treatment group accrued more utility from occupation switching simply because they switched more often.

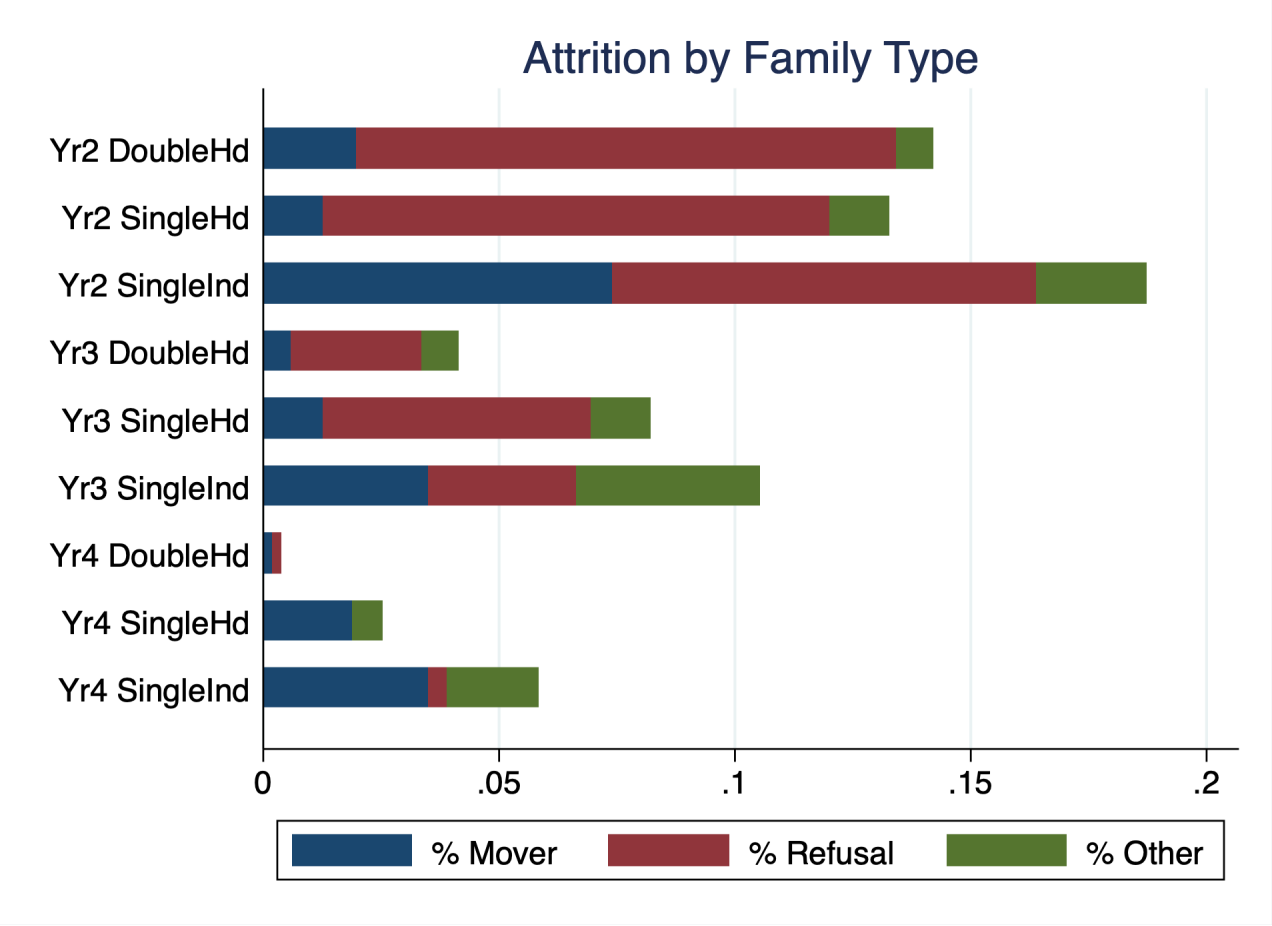
There are a few caveats to be noted for this study. First of all, this study utilized the urban Winnepeg site of the Mincome experiment. This site provides a partial equilibrium setting that guaranteed income to a small portion of the low-income families in an urban setting in Canada. Hence, the results of this study should not be extrapolated to a general equilibrium setting in which basic income maintenance is provided to all low-income families. Secondly, while this study presents theories on the mechanisms behind the relationship between income maintenance and occupation turnover, I cannot conduct tests to definitively confirm or disprove these theories. Last but not least, this study lacks data to tackle an integral question related to labor turnover: did occupation turnover induced by treatment in an IME result in higher wages for these low-income families? Even though I find non-pecuniary benefits for occupation switchers, I cannot determine whether occupation switches resulted in wage increase and whether there exists heterogeneous effect base on the age of the worker. If this question can be tackled, it has the potential to shed light on the wage trajectories for those families who were induced to switch occupations while in treatment.

There are several avenues for future work. First, it will be fruitful to obtain the IRF

and tax returns for participants in Mincome. This will allow researcher to analyze the wage changes after occupation switching in the short term. Obtaining this information will also illuminate whether guaranteed income has any long-term effect on one's wage trajectory, retirement decision, or other labor market outcomes. Second, researchers should examine the Mincome experiment in Dauphin. Policy makers today are becoming more interested in the idea of universal basic income. While the partial equilibrium setting in Winnipeg provides useful insight, the Dauphin site offers the perfect setting to analyze the effect of guaranteed income where the assistance is provided to all interested and eligible families.

3.8 Figures and Tables

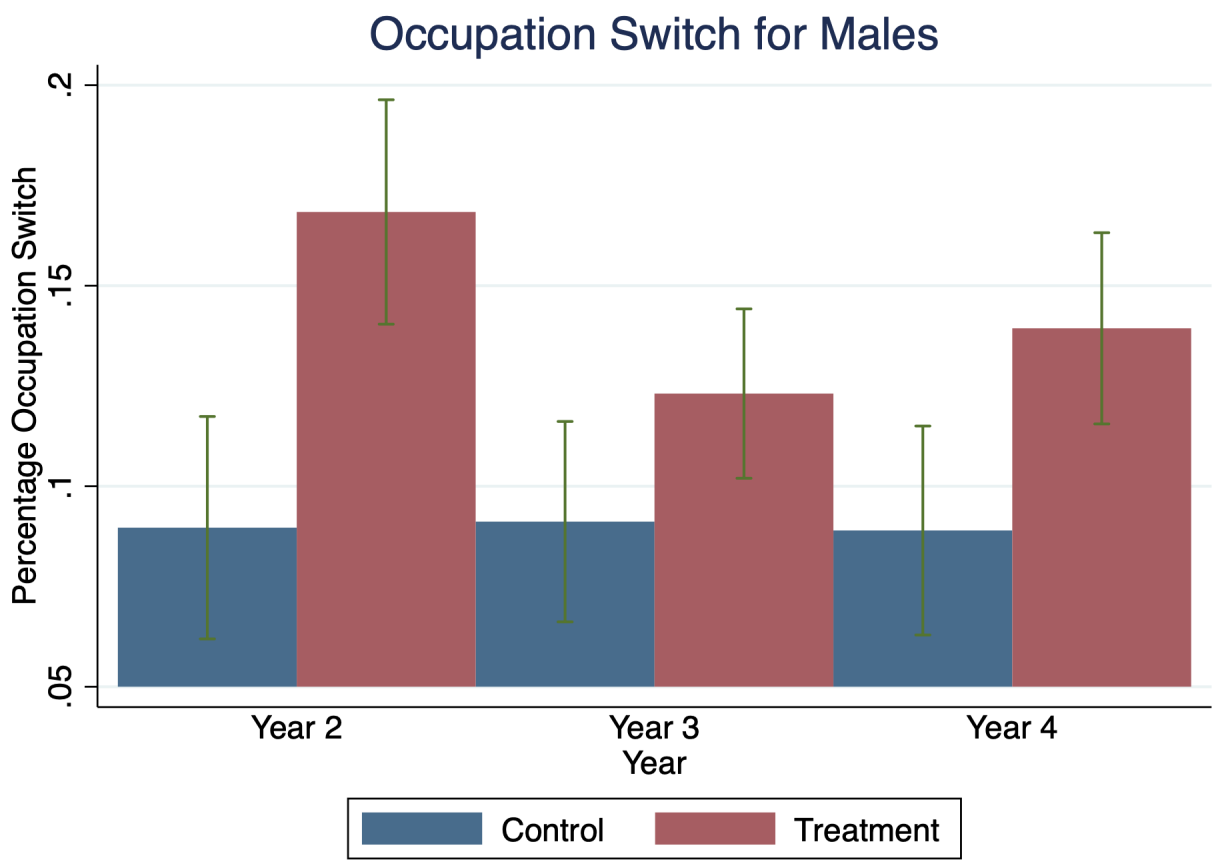
Figure 3.1: Attrition by family types



Note: "DoubleHd" stands for double-headed households with a husband and a wive. "SingleHd" stands for single-headed household with one parent and children. "SingleInd" stands for single individuals.

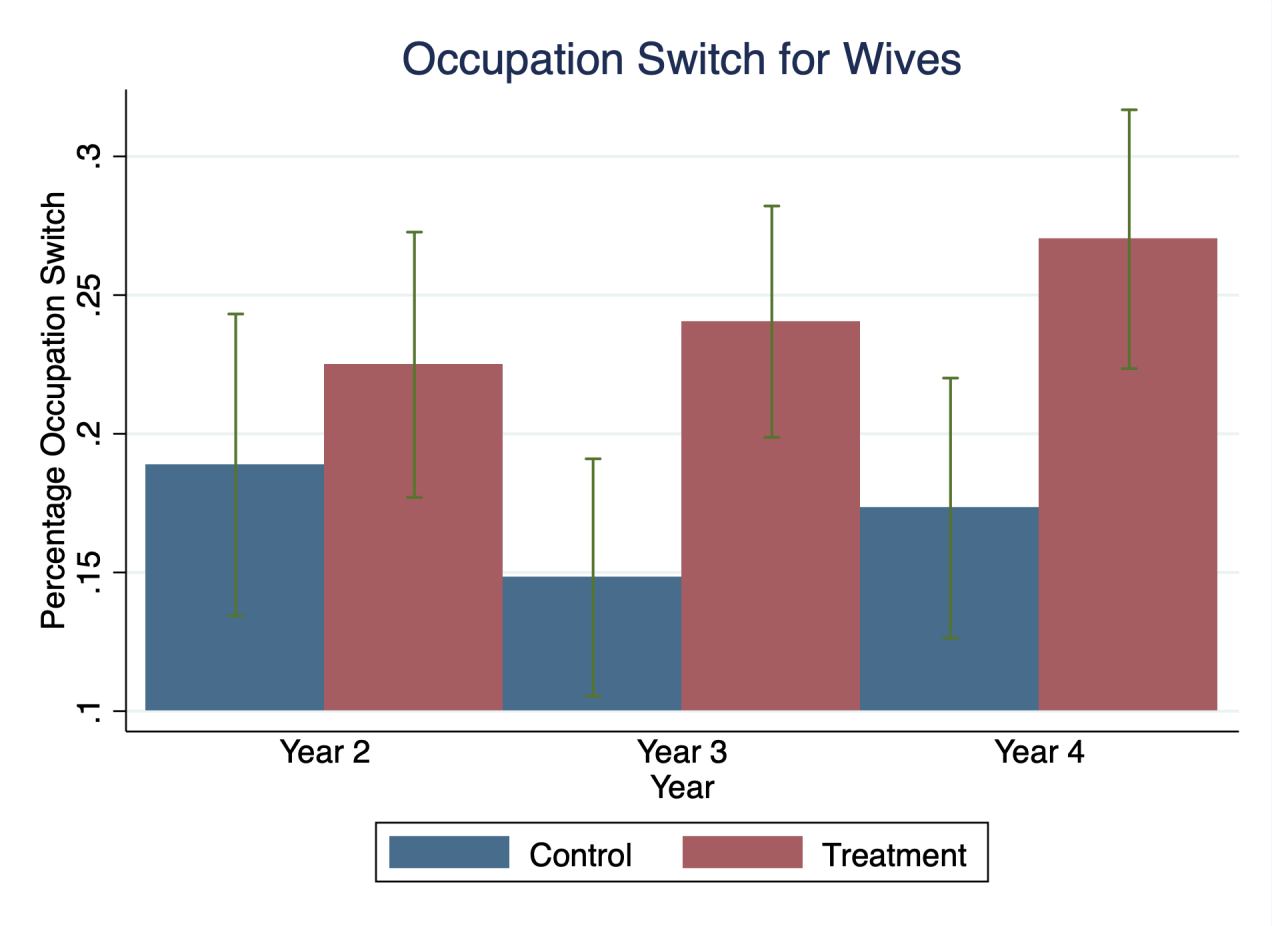
Figure 3.2: Difference in mean probability of occupation switch, by year

Panel A:



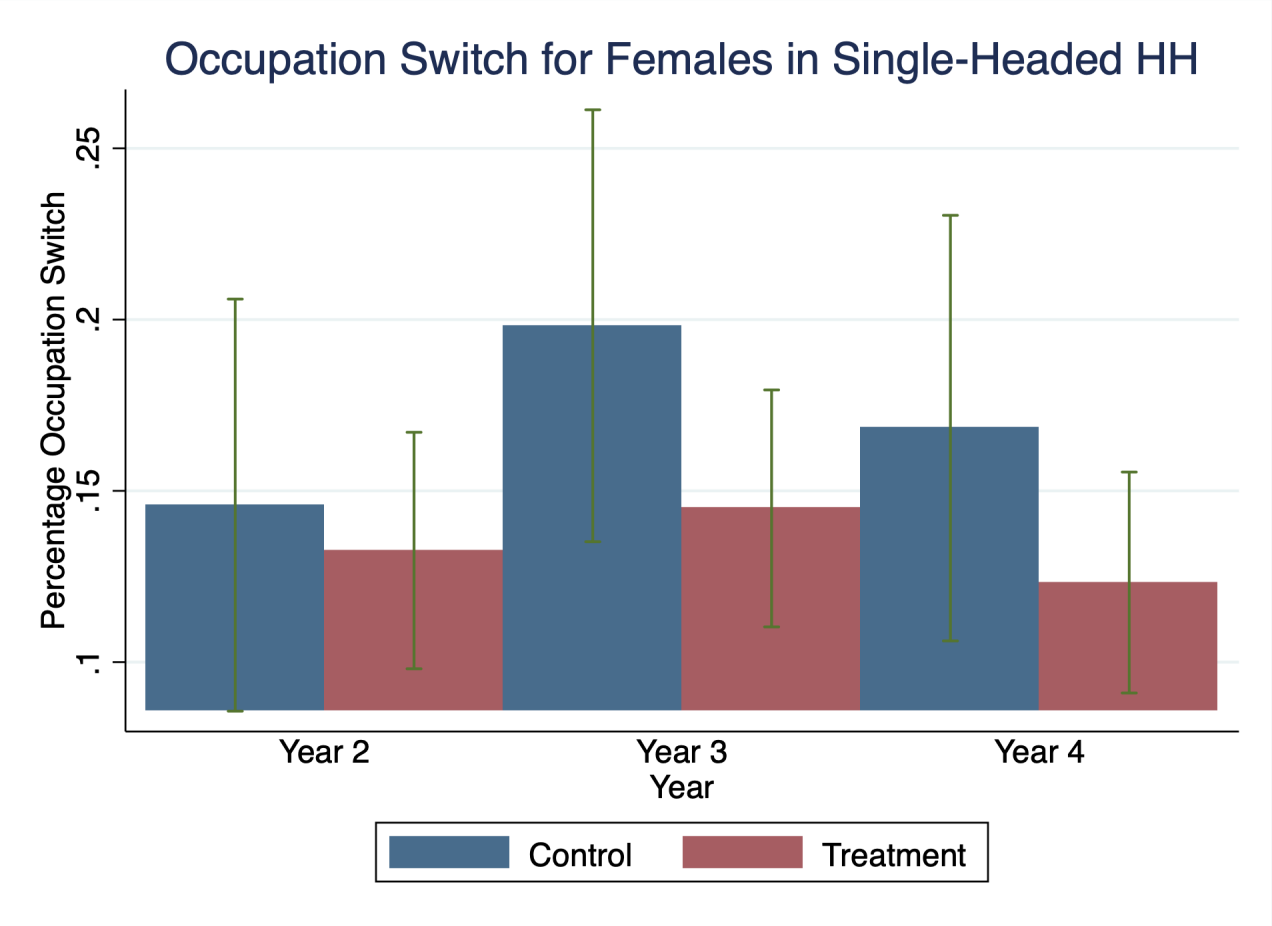
Note: Bars represent 90% confidence intervals.

Panel B:



Note: Bars represent 90% confidence intervals.

Panel C:



Note: Bars represent 90% confidence intervals.

Table 3.1: Mincome treatment arms

		Benefit Reduction Rate		
		0.35	0.50	0.75
Guarantee	3800	Plan 1	Plan 3	
	4600	Plan 2	Plan 4	Plan 7
	5400		Plan5	Plan 8

Source: Mincome Manitoba Technical Report 1

Table 3.2: Summary statistics from baseline survey

	Control	Treatment	p-value
Earned Income	7023.17	7428.48	0.27
	(3742.69)	(4341.39)	
Gov't Transfer	905.92	808.19	0.29
	(1258.93)	(1457.35)	
HH Head Hrs Worked	1551.82	1582.96	0.58
	(1047.85)	(1123.56)	
Home Ownership	0.37	0.36	0.39
	(0.48)	(0.48)	
Children age 0-5	0.81	0.90	0.27
	(0.97)	(1.06)	
Children age 6-15	0.98	1.01	0.36
	(1.34)	(1.44)	
Age 16+	2.19	2.18	0.42
	(0.96)	(1.01)	

Note: Standard deviations are shown in parentheses. Each cell under the p-value column reports the result of a single regression of the dependent variable given by the row variable on the treatment dummy. Each regression include dummy variables for each assignment group. All monetary values are measured in 1975 Canadian dollars.

Table 3.3: Pooled OLS and probit estimates of occupation turnover

VARIABLES	(1) OLS Male OccSwitch	(2) Probit Male OccSwitch	(3) OLS Wives OccSwitch	(4) Probit Wives OccSwitch	(5) OLS Single Fem OccSwitch	(6) Probit Single Fem OccSwitch
Treat	0.0777**	0.240*	0.117**	0.331**	-0.135*	-0.397**
	(0.0390)	(0.124)	(0.0468)	(0.133)	(0.0690)	(0.198)
Constant	0.244***	-0.766***	0.340***	-0.607***	0.363***	-0.348*
	(0.0358)	(0.103)	(0.0477)	(0.102)	(0.0669)	(0.188)
Observations	958	958	503	503	390	390
(Pseudo) R-squared	0.010	0.008	0.026	0.021	0.040	0.034

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects. The pseudo R-Squared is presented for probit specification. The marginal effects for the probit specifications are computed as average derivatives of the probability of occupation switch with respect to treatment in the IME.

Table 3.4: Pooled Lee bounds for treatment effects on occupation turnover

VARIABLES	(1) Male Lee Bound	(2) Wives Lee Bound	(3) Single Fem Lee Bound
lower	0.0448	0.0538	-0.285**
	(0.0443)	(0.0629)	(0.111)
upper	0.0925***	0.259***	-0.0874
	(0.0351)	(0.0797)	(0.0677)
Observations	2,520	2,024	1,120

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects.

Table 3.5: Pooled Heckman OLS correction

VARIABLES	Male OccSwitch	Wives OccSwitch	Single Fem OccSwitch
Treat	0.0844**	0.113**	-0.128*
	(0.0377)	(0.0466)	(0.0691)
IMR	-0.178***	-0.0632	-0.0502
	(0.0357)	(0.0438)	(0.0410)
Constant	0.553***	0.400***	0.408***
	(0.0631)	(0.0645)	(0.0795)
Observations	1,128	503	390
R-squared	0.082	0.030	0.047

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects. "IMR" stands for the inverse mills ratio. The selection equation used to construct the inverse mills ratio includes treatment plan dummies, homeownership, age, age squared, race dummies, number of children under 5, annual household income, annual welfare received and annual earning by spouse.

Table 3.6: Marginal tax rate and occupation turnover

VARIABLES	(1) OLS Male OccSwitch	(2) OLS Wives OccSwitch	(3) OLS Single Fem OccSwitch	(4) Heckman Male OccSwitch	(5) Heckman Wives OccSwitch	(6) Heckman Single Fem OccSwitch
0.35 MTR Dummy	0.0638	-0.109	-0.0458	0.0881	-0.103	-0.0654
	(0.0591)	(0.0830)	(0.0852)	(0.0547)	(0.0818)	(0.0882)
0.50 MTR Dummy	0.0514	-0.0926	-0.159**	0.0619	-0.0951	-0.176**
	(0.0533)	(0.0753)	(0.0679)	(0.0512)	(0.0734)	(0.0708)
IMR				-0.204***	-0.0809	-0.560**
				(0.0472)	(0.0525)	(0.233)
Constant	0.342***	0.526***	0.323***	0.625***	0.638***	0.342***
	(0.0452)	(0.0685)	(0.0578)	(0.0827)	(0.1000)	(0.0610)
Observations	748	307	276	748	307	276
R-squared	0.010	0.021	0.048	0.105	0.030	0.051

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects. "IMR" stands for the inverse mills ratio. The selection equation used to construct the inverse mills ratio includes treatment plan dummies, homeownership, age, age squared, race dummies, number of children under 5, annual household income, annual welfare received and annual earning by spouse.

Table 3.7: Attitude changes after occupation switch

VARIABLES	(1) Enjoyment	(2) Enjoyment	(3) Negative Feeling	(4) Negative Feeling	(5) Job Opportunity	(6) Job Opportunity	(7) Income Importance	(8) Income Importance
OccSwitch	-0.216*	-0.366*	0.208*	0.267	-0.0420	0.0608	0.00465	0.0513
	(0.120)	(0.200)	(0.122)	(0.173)	(0.133)	(0.220)	(0.0877)	(0.100)
OccSwitch*Treat		0.209		-0.0821		-0.142		-0.0645
		(0.218)		(0.196)		(0.234)		(0.121)
Constant	-0.0469	-0.0468	0.00627	0.00622	0.0469	0.0468	0.287***	0.287***
	(0.0912)	(0.0913)	(0.0981)	(0.0983)	(0.0958)	(0.0960)	(0.0681)	(0.0681)
Observations	640	640	640	640	644	644	643	643
R-squared	0.009	0.010	0.008	0.008	0.000	0.001	0.047	0.047

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions include survey fixed effects.

Table 3.8: Heterogeneous treatment effects on occupation turnover by age

VARIABLES	(1) OLS Male OccSwitch	(2) OLS Wives OccSwitch	(3) OLS Single Fem OccSwitch	(4) Heckman Male OccSwitch	(5) Heckman Wives OccSwitch	(6) Heckman Single Fem OccSwitch
Panel A: Older						
Treat	0.0927*	0.163*	-0.113	0.112**	0.159**	-0.120
	(0.0479)	(0.0679)	(0.0930)	(0.0492)	(0.0680)	(0.0944)
IMR				-0.106**	-0.0291	-0.279**
				(0.0522)	(0.0218)	(0.1140)
Constant	0.179***	0.235***	0.233**	0.337***	0.283***	0.237**
	(0.0468)	(0.0734)	(0.0872)	(0.0886)	(0.0907)	(0.0881)
Observations	416	215	132	441	215	132
R-squared	0.018	0.061	0.033	0.047	0.064	0.035
Panel B: Younger						
Treat	0.0604	0.0901	-0.152*	0.0582	0.0869	-0.166*
	(0.0568)	(0.0617)	(0.0881)	(0.0498)	(0.0610)	(0.0860)
IMR				-0.222***	-0.0696	-0.308
				(0.0496)	(0.0517)	(0.3580)
Constant	0.299***	0.414***	0.419***	0.693***	0.514***	0.436***
	(0.0505)	(0.0615)	(0.0867)	(0.0881)	(0.0977)	(0.0870)
Observations	542	288	258	687	288	258
R-squared	0.006	0.028	0.049	0.122	0.035	0.050

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects. "IMR" stands for the inverse mills ratio. The selection equation used to construct the inverse mills ratio includes treatment plan dummies, homeownership, age, age squared, race dummies, number of children under 5, annual household income, annual welfare received and annual earning by spouse.

Table 3.9: Heterogeneous attitude changes after occupation switch by age

	(1)	(2)	(3)	(4)
		Negative		Negative
VARIABLES	Enjoyment	Feeling	Enjoyment	Feeling
Panel A: Older				
OccSwitch	-0.400**	0.175	-0.385**	-0.0254
	(0.177)	(0.264)	(0.178)	(0.127)
Constant	-0.0250	-0.0825	0.262**	0.367***
	(0.135)	(0.154)	(0.108)	(0.0859)
Observations	258	257	367	366
R-squared	0.031	0.002	0.023	0.069
Panel B: Younger				
OccSwitch	-0.0993	0.219	0.338	0.0654
	(0.160)	(0.137)	(0.208)	(0.124)
Constant	-0.0780	0.0643	-0.287*	0.169
	(0.124)	(0.129)	(0.170)	(0.111)
Observations	382	383	277	277
R-squared	0.002	0.014	0.019	0.026

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions include survey fixed effects.

3.9 Appendix

3.9.1 Additional Tables

Table 3.10: Summary statistics of non-attrited sample in year 2 and year 4

	Year 2			Year 4		
	Control	Treatment	p-value	Control	Treatment	p-value
Earned Income	13507.04	13023.25	0.24	11487.04	11428.43	0.79
	(9478.44)	(8403.01)		(6083.37)	(8912.38)	
Gov't Transfer	924.28	877.80	0.73	806.97	764.84	0.98
	(1255.44)	(1106.97)		(1025.57)	(1242.01)	
HH Head Hrs Worked	1464.01	1369.50	0.33	1408.75	1379.74	0.94
	(977.48)	(731.14)		(724.65)	(777.64)	
Home Ownership	47.43	36.15	0.05	58.09	55.34	0.85
	(50.10)	(48.12)		(49.52)	(49.81)	
Children age 0-5	0.92	1.06	0.06	0.99	1.18	0.04
	(1.01)	(1.13)		(1.04)	(1.14)	
Children age 6-15	1.06	1.05	0.93	1.08	1.11	0.63
	(1.35)	(1.44)		(1.31)	(1.46)	
People age 16+	2.19	2.15	0.57	2.24	2.21	0.42
	(0.88)	(0.94)		(0.82)	(0.92)	

Note: Standard deviations are shown in parentheses. Each cell under the p-value column reports the result of a single regression of the dependent variable given by the row variable on the treatment dummy. Each regression include dummy variables for each assignment group. All monetary values are measured in 1975 Canadian dollars.

Table 3.11: Separate annual OLS estimates of occupation turnover

VARIABLES	(1) Year 2 OccSwitch	(2) Year 3 OccSwitch	(3) Year 4 OccSwitch
Panel A: Male			
Treat	0.101*	0.0254	0.104*
	(0.0525)	(0.0538)	(0.0548)
Constant	0.229***	0.221***	0.215***
	(0.0397)	(0.0426)	(0.0419)
Observations	338	306	314
R-squared	0.011	0.001	0.012
Panel B: Wives			
Treat	0.124	0.133*	0.0933
	(0.0803)	(0.0774)	(0.0794)
Constant	0.336***	0.213***	0.264***
	(0.0614)	(0.0552)	(0.0597)
Observations	180	157	166
R-squared	0.015	0.020	0.009
Panel C: Single Fem			
Treat	-0.110	-0.175	-0.120
	(0.0950)	(0.110)	(0.0924)
Constant	0.343***	0.499***	0.276***
	(0.0815)	(0.0897)	(0.0793)
Observations	146	123	121
R-squared	0.009	0.022	0.016

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment.

Table 3.12: Separate annual Lee bounds for treatment effects on occupation turnover

VARIABLES	(1) Year 2 Lee Bound	(2) Year 3 Lee Bound	(3) Year 4 Lee Bound
Panel A: Males			
lower	0.0585	0.0101	0.0930
	(0.0666)	(0.0747)	(0.0730)
upper	0.131**	0.0346	0.106*
	(0.0577)	(0.0587)	(0.0597)
Observations	630	630	630
Panel B: Wives			
lower	0.0873	0.0829	0.0269
	(0.109)	(0.0908)	(0.100)
upper	0.209*	0.304**	0.254*
	(0.124)	(0.145)	(0.136)
Observations	506	506	506
Panel C: Single Fem			
lower	-0.343***	-0.271	-0.222
	(0.0904)	(0.169)	(0.190)
upper	-0.0362	-0.130	-0.101
	(0.112)	(0.127)	(0.104)
Observations	280	280	280

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment.

Table 3.13: Pooled Heckman probit correction

VARIABLES	(1) Male OccSwitch	(2) Wives OccSwitch	(3) Single Fem OccSwitch
Treat	0.154*	0.300**	-0.403*
	(0.0934)	(0.144)	(0.206)
	[0.045]	[0.102]	[-0.131]
Constant	-0.768***	-0.602*	-0.308
	(0.0913)	(0.350)	(0.520)
Observations	1,812	1,495	847

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. All regressions also include year fixed effects. The variables in the selection equation are not displayed in the regression result. The variables used are: treatment plan dummies, homeownership, race, age, age squared, number of children under 5, annual household income and earnings of spouse if married. The marginal effect, displayed in brackets, are computed as average derivatives of the probability of occupation switch with respect to treatment.

Table 3.14: Separate annual Heckman OLS correction

VARIABLES	(1) Year 2 OccSwitch	(2) Year 3 OccSwitch	(3) Year 4 OccSwitch
Panel A: Male			
Treat	0.0768	0.0310	0.102**
	(0.0479)	(0.0491)	(0.0479)
IMR	-0.175***	-0.166***	-0.234***
	(0.0462)	(0.0478)	(0.0569)
Constant	0.566***	0.473***	0.528***
	(0.0752)	(0.0751)	(0.0803)
Observations	409	360	359
R-squared	0.081	0.032	0.110
Panel B: Wives			
Treat	0.0919	0.0984	0.129*
	(0.0754)	(0.0719)	(0.0709)
IMR	-0.0397*	0.0197	-0.0893
	(0.0236)	(0.0643)	(0.0617)
Constant	0.405***	0.194*	0.347***
	(0.0741)	(0.105)	(0.0960)
Observations	180	157	166
R-squared	0.016	0.012	0.033
Panel C: Single Fem			
Treat	-0.0858	-0.113	-0.107
	(0.0920)	(0.105)	(0.0876)
IMR	-0.802	1.688	-0.161
	(1.198)	(1.566)	(0.363)
Constant	0.369***	0.369***	0.286***
	(0.0861)	(0.0984)	(0.0788)
Observations	146	123	121
R-squared	0.007	0.038	0.014

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the family level are in parentheses. All regressions are re-weighted to account for non-random assignment. "IMR" stands for the inverse mills ratio. The selection equation used to construct the inverse mills ratio includes treatment plan dummies, homeownership, age, age squared, race dummies, number of children under 5, annual household income, annual welfare received and annual earning by spouse.

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