Financial Scarring and the Failure of the Freedman's Savings Bank*

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Abstract

The failure of the Freedman's Savings Bank, (FSB) one of the only Black-serving banking institutions in the early post-bellum South, was an economic catastrophe and one of the great episodes of racial exploitation in post-Emancipation history. Can events like these permanently alter financial preferences and behavior? To test this, we examine the impact of FSB collapse on insurance-holding, an alternative savings vehicle that was both accessible and extremely popular over the late 19th and early 20th centuries. We document a sharp and persistent increase in insurance demand in affected counties following the shock, driven disproportionately by Black customers. We also use FSB migrant flows to disentangle place-based and cohort-based effects. In so doing, we provide evidence identifying psychological and cultural scarring as a distinct mechanism underlying the shift in financial behavior induced by the bank's collapse. Horizontal and intergenerational transmission of preferences further help explain the shock's persistent effects on financial behavior.

JEL Codes: G21, G22, G51, G52, D63, I30, N21, N22, N31, N32

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1 Introduction

History is rife with episodes of institutional exploitation of vulnerable communities, but how consequential can these breaches of trust be—and in what ways? In this paper, we ask whether racially traumatic economic events can durably shift the financial behavior of affected groups.[1]

Specifically, we study the long-run consequences of the failure of the Freedman's Savings Bank (FSB), one of the first and only savings institutions accessible to newly-free Black Americans in the period immediately following the US Civil War. Heavily—and fraudulently—marketed toward this group as a safe place for their often meager savings, the Freedman's Savings Bank collapsed in 1874 as a consequence of speculative investments, corruption, and cronyism on the part of white bank managers (Celerier & Tak, 2021). The collapse had the effect of eliminating roughly half of Black wealth at the time (Baradaran, 2017), and historical accounts suggest that this betrayal and cataclysmic loss sowed a deep and lasting distrust of banks in the Black community (Osthaus, 1976; Fleming, 2018). With banks now viewed with skepticism, Black households sought alternative means of savings and investment. One possible alternative they turned to was life insurance—one of the principal methods of saving among households of all races and socioeconomic strata throughout the late 19th and early 20th centuries (Bullock, 1957; Goldsmith, 1955; Goldsmith & Lipsey, 1963; Temporary National Economic Committee, 1940).

To test the capacity of the Freedman's Savings Bank collapse to shift financial preferences and behaviors, we examine the impact of this shock on insurance-holding within a differences-in-differences framework. Specifically, we identify counties in the US South that ever had a Freedman's Savings Bank branch, and compare the size of the insurance-industry workforce in those counties before and after the bank's failure,

[1] Throughout this paper, and following a large related literature in health, we refer to the persistent change in attitudes and behavior induced by a traumatic event as "scarring."

relative to that in economically similar counties in the US South that did not have an FSB branch. Our measures of FSB branching are taken from Celerier & Tak (2021) and Fu (2021). Meanwhile, our measures of the insurance workforce are drawn from the decennial US Censuses of 1850-1940.

There are several advantages to focusing on insurance demand as an outcome. First, it was a popular and readily available financial product over the period of our study, representing a large fraction of total household wealth over this timeframe, and with the vast majority of households—of any race—holding at least some life insurance (Bullock, 1957; Goldsmith, 1955; Goldsmith & Lipsey, 1963; Temporary National Economic Committee, 1940). Second, this measure is readily available both at a spatially fine (i.e., county) level corresponding to FSB exposure, and comprehensively over a long period (i.e., the late 19th and early 20th century). Third, it is one of the few financial choices in this period that can be examined on a race-specific basis in existing data. This is because we are able to exploit the relatively highly integrated (or at least, racially segmented) nature of the insurance workforce, which both employed Black agents and actively sought Black customers. For contrast, and taking bank deposits as an example of an alternative outcome, it is difficult to identify the universe of Black-serving banks in this period (let alone Black dollars within banks serving a mixed clientele). Moreover, analysis of other financial assets, such as bank deposits or real estate, might conflate changes in Black preferences and demand with systemic and policy barriers to accessing these assets in a highly racially segregated setting, given that the barriers to entry for these assets were much higher in this period than for insurance.[2] Fourth, our choice of insurance demand as an outcome speaks to a large literature documenting strong and disproportionate historical demand for insurance among Black households (Williams & Jones, 1941; Yancy, 1933; Bullock, 1957)—though little has been established about the origins of these patterns. Crucially, this is not just a historical phenomenon: Black households in the present continue to place

[2]Likewise, there are to our knowledge no relevant surveys of financial preferences and holdings in this period (such as those used in Fu (2021)), let alone on a race-specific basis.

a significant fraction of savings in life insurance and are less likely to hold equities or bank accounts (Stevenson & Plath, 2002; Hayashi *et al.*, 2018). Moreover, even controlling for income and demographics, Black Americans are likelier to hold life insurance than their white counterparts (Gale *et al.*, 2022; Harris & Yelowitz, 2018). Through our study, we are able to cleanly identify at least one factor contributing to these persistent differences.

Our paper produces several key results. First, we document a sharp and statistically significant increase in the demand for insurance in US counties exposed to the shock. This increase is persistent, lasting until our data leave off nearly 70 years after the bank's collapse. It is also economically meaningful, with estimates suggesting that the Freedman's Savings Bank collapse accounts for an additional annual life insurance policy volume of \$600-\$3,600 per household (in 2023 dollars) in affected regions. Moreover, it is robust to a wide range of causal estimation approaches, and is neither an artefact of the 1873 Panic nor of broader trends in local economic development.

Second, we show that the increase in local insurance demand induced by the failure of the Freedman's Savings Bank was a race-specific phenomenon, ruling out the possibility that increases in white demand are driving our central result. Indeed, at minimum 13-20% of the marginal effect of FSB collapse on local insurance demand that we document is attributable exclusively to changes in Black insurance holdings, raising the overall share of exclusively Black-serving insurance agents by roughly 4 percentage points in these communities. These results lend credence to our interpretation of the Freedman's Savings Bank collapse as a fundamentally racialized trauma with racialized consequences.

Third, we provide evidence identifying psychological and cultural scarring as a distinct mechanism underlying the shift in financial behavior induced by the bank's collapse. Using migrants from FSB counties to distinguish potential ongoing place-based effects from those embodied in people's beliefs, preferences, and experiences, we show that non-FSB regions that received large flows of FSB migrants also saw

increases in the demand for insurance. These migrants not only brought their own demand for alternative savings vehicles with them to their new homes, shaped by their adverse experiences with the Freedman's Savings Bank. Instead, they also appear to have transmitted their preferences to others once there: both horizontally, to their non-FSB-exposed friends and neighbors, and vertically, to their non-FSB-exposed descendants and family members. These effects help to explain the wide scope and intergenerational persistence of our results.

Together, our results suggest that racially traumatic economic events can durably alter financial behavior. To the extent that these historical episodes have shaped persistent racial differences in portfolio composition, they could also have potential implications for racial gaps in long-run wealth accumulation (Derenoncourt *et al.*, 2022). Importantly, we show that episodes of historical racial exploitation can have long-reaching impacts beyond just the health realm, in which effects like these are relatively better documented (see, e.g., Alsan & Wanamaker (2018); Archibong & Annan (2021); Lowes & Montero (2021)). Likewise, and in contrast to macroeconomic studies that document discrete cohort effects of macro crises on financial preferences (Malmendier & Nagel, 2011; Graham & Narasimhan, 2004), we show that large economic shocks can also have effects on preferences that extend beyond the affected cohorts' lifetimes, perhaps because of the racialized nature of this particular economic shock and the corresponding cultural transmission. Finally, where the bulk of the literature on the Freedman's Savings Bank to date has focused on its operations and short-run effects (Fu, 2021; Celerier & Tak, 2021; Traweek & Wardlaw, 2021; Stein & Yannelis, 2020), we contribute by examining its failure, and in particular, documenting its long-run economic consequences. Moreover, we disentangle potential effects on the economic structure of FSB localities from those due to the changing beliefs and preferences of their residents, and document persistence through intergenerational transmission of both lived experience and cultural memory.[3]

[3]See, e.g., Dohmen *et al.* (2012), who, using modern German survey data, likewise document the

2 Related literature

Our study is motivated by a literature in health that documents that major breaches of institutional trust can have persistent effects on patient preferences and behaviors, with material consequences for wellbeing. These betrayals can run the gamut from mere negligence, as in the case of vaccine trials gone wrong (see, e.g., Fairley *et al.* (2023); Archibong & Annan (2021))[4] to willful exploitation, as in the case of the infamous "Tuskegee Study" (Alsan & Wanamaker, 2018), in which Black men were denied informed consent and were instead deliberately exposed to syphilis as part of a medical experiment.

Relevant to our setting, this sort of long-run effect on attitudes, often termed "scarring," appears especially prevalent in circumstances where there are elements of abuse of power, or of real or perceived targeting based on a socially vulnerable racial, ethnic, or religious identity. For instance, Archibong & Annan (2021) find that following a deadly Pfizer vaccination test undertaken in Muslim communities in Nigeria, there was a sustained rise in vaccine hesitancy among mothers residing in minority Muslim neighborhoods. Likewise, Lowes & Montero (2021) demonstrate that deleterious medical interventions by the French colonial government in early 20th century Central Africa contributed to contemporaneous medical distrust,[5] which per-

role of the local environment and of parent-child interactions in the transmission of risk and trust attitudes.

[4]In public health, exposure to negative information about vaccines has been shown to contribute to vaccine hesitancy (Fairley *et al.*, 2023; Orsini *et al.*, 2022; Martinez-Bravo & Stegmann, 2022; Archibong & Annan, 2021; Deiana *et al.*, 2022). The "Cutter Incident," in which a mismade batch of polio vaccines infected tens of thousands of Americans with the live virus, is a prime example of this phenomenon. There, a locality's historical exposure to this mid-20th-century episode was shown to raise decades-later mortality—and even present-day morbidity—from vaccine-preventable diseases (Fairley *et al.*, 2023).

[5]A distinct but related literature focuses on the origins of trust, mistrust, and distrust more generally. Many of these emphasize persistence and the role of intergenerational transmission. For instance, Nunn & Wantchekon (2011) finds that intergenerational transmission of norms from ancestors directly exposed to the slave trade can explain modern variation in trust between ethnic groups in Africa. Similarly, Nikolova *et al.* (2022) provide evidence of intergenerational transmission of distrust sowed by the brutal gulag system in the former Soviet Union. In our study, we do not focus on distrust per se, but given our empirical context, institutional distrust is likely to play a

sists today and manifests in a reduction in health-seeking behaviors. Perhaps the most notorious example in this vein is the "Tuskegee Study" mentioned above, in which Black men in the US South were purposefully exposed to syphilis without their knowledge in order to study the disease's effects. Alsan & Wanamaker (2018) provide robust evidence that public disclosure of this study precipitated a deep and persistent distrust of medical institutions among Black men, resulting in fewer physician interactions and substantially worse longevity for this group.[6]

These studies underscore that a discrete traumatic event can influence tangible outcomes for decades and even centuries, since changes in behaviors and preferences induced by the event need not be limited to the victims themselves, but may also spill over to contemporaneous observers and to subsequent generations. Indeed, research in economics and psychology suggests that the identity-based or racialized nature of these shocks may help explain their salience beyond those directly affected, and therefore, the durability and wide scope of their downstream effects (Alsan & Wanamaker, 2018; Tabellini, 2008; Gutsell & Inzlicht, 2010; Singer *et al.*, 2006).[7]

There is reason to believe that an episode of institutional racial exploitation as economically consequential as the failure of the Freedman's Savings Bank might similarly affect attitudes and behavior—albeit in the financial domain relevant to the shock.[8]. Indeed, a literature in macroeconomics and finance suggests that traumatic economic events can shape financial preferences and decision-making. For instance,

strong role in our results.

[6]Related to themes of race and institutional distrust, exposure to police violence can diminish the mental health of Black men (Bor *et al.*, 2018), and hinder the educational progress of minorities in school (Ang, 2021).

[7]Research such as that cited above finds that negative shocks resonate most with individuals who share socioeconomic characteristics with the victims, and therefore identify closely with them. Williams (2022) suggests that these shock-induced changes in cultural norms are then transmitted intergenerationally, further contributing to persistence.

[8]In this paper, we take an agnostic position as to the particular attitudes driving the shift in behavior that we document, in part because this is difficult to disentangle in the existing data. However, identifying the specific beliefs affected by shocks such as these is a worthwhile avenue for future research, given that each—e.g., a loss of trust in banks versus a change in risk preferences more broadly—has distinct policy implications and merits a distinct policy response.

exposure to severe macroeconomic shocks such as the Great Depression have been shown to increase risk aversion among those that came of economic age during the crisis (Malmendier & Nagel, 2011; Graham & Narasimhan, 2004; Malmendier & Shen, 2018).[9] Financially scarring events can also erode public trust in institutions. To wit, recent research suggests that in Germany, the 1931 banking crisis ultimately led to the scapegoating of Jews and rise of the Nazi party (Funke *et al.*, 2019). Likewise, personal experiences of fraud, exploitation, and discrimination have been shown to sow mistrust in financial institutions, resulting in low utilization of financial services (Gurun *et al.*, 2018; Dupas *et al.*, 2014; Rhine *et al.*, 2006).

While the effects in these studies tend to be shorter-lived than those documented above in the health literature—often confined to the lifetime of a single affected cohort—we hypothesize that the features that make the Freedman's Savings Bank collapse distinct as a shock likely also render its effects distinct in their scope and longevity, relative to others in the macro-financial literature on scarring. Specifically, given the institutional and racial power dynamics involved, the FSB shock may have been more salient to those nominally unexposed, with attitudes about it also more readily transmitted through pre-existing community ties. Interestingly, there is precedent for thinking about intergenerational transmission of financial preferences outside of the context of scarring events: for instance, Chiteji & Stafford (1999) suggest a role for social learning within families in the intergenerational persistence of portfolio choice among Black households. To the extent that the FSB failure changed the financial preferences and behaviors of directly-impacted cohorts, it would stand to reason that these changes may endure in part through these existing social dynamics. To better understand our shock in context, we discuss the history of the Freedman's Savings Bank next.

[9]Exposure to large-scale conflict and natural disasters have also been shown to alter individuals' risk preferences and contribute to interpersonal mistrust (Kim & Lee, 2014; Callen *et al.*, 2014; Bernile *et al.*, 2017; Hanaoka *et al.*, 2018; Conzo & Salustri, 2019).

3 Empirical setting

3.1 The Freedman's Savings Bank

Established in 1865 during the Reconstruction Era, the primary stated goal of the Freedman's Savings Bank was to teach financial literacy and thrift to the nearly 4 million recently-freed Black people fighting an uphill battle to economic stability (Osthaus, 1976). Despite its philanthropic origins, the bank's management, composed exclusively of white Northern businessmen, engaged in increasingly speculative investing practices, cronyism, and corruption that ultimately caused the bank's collapse. Historians argue that the failure of the Freedman's Savings Bank contributed to a lasting distrust in savings institutions by Black Americans (Osthaus, 1976; Baradaran, 2017; Fleming, 2018).

The formation of the Freedman's Savings Bank stemmed from the military savings banks set up for Black troops during the Civil War (Osthaus, 1976; Baradaran, 2017; Fleming, 2018). As the Union Army advanced on the South, Union generals recruited Freedmen who fled their enslavement and sought refuge with Union Army camps (Dobak, 2011). Enlistment in the Union Army promised pay, but the Freedmen lacked a secure place to deposit their earnings, often spending the entire sum rather than saving it (Fleming, 2018). After hearing the success of military savings banks established by Union generals in ameliorating this problem, Reverend John Alvord recruited Northern philanthropists to petition Congress for a charter to establish the Freedman's Savings Bank, which was granted in 1865. Although Congress intended to establish a single bank in the District of Columbia (D.C.), the Freedman's Savings Bank would soon expand to 37 branches across 17 states and D.C. (Fleming, 2018).

Alvord selected New York City, the financial capital of the United States, as the bank's headquarters, but quickly set sights on establishing branches throughout the South. Alvord strategically selected cities for the bank's branches, prioritizing cities

with either large Black populations or large numbers of recently-paid Black troops (Osthaus, 1976). For example, the Norfolk, Virginia and Beaufort, South Carolina branches replaced the preexisting military savings banks, while the Baltimore branch serviced the city's large Black population (Osthaus, 1976). Branch employees were instructed to pursue recently-paid soldiers in an effort to maximize deposit collection (Osthaus, 1976). As the bank proved successful throughout its first three years of operation, Black communities throughout the South applied for their own branches. The bank expanded rapidly, selecting cities with successful Black economies, and establishing branches as far as Houston (Osthaus, 1976).

Although its name suggests a direct connection with the Freedman's Bureau, a government entity focused on supporting the immediate needs of the Freedmen, the Freedman's Savings Bank was managed by a board of trustees consisting of white Wall Street businessmen, and had no connection with the federal government beyond its federal charter granted by Congress. Despite this, the bank extensively advertised in Black-owned newspapers the false impression that deposits were insured by the US federal government (Celerier & Tak, 2021; Baradaran, 2017; Osthaus, 1976). The advertising successfully attracted new depositors, 20% of whom were children or students, and the vast majority of whom were low-wage workers (Celerier & Tak, 2021; Traweek & Wardlaw, 2021).

The bank's board of trustees faulted the bank's initial Congressional charter for financially handicapping the bank. A successful Black-serving bank authorizing loans for entrepreneurship and homeownership would put depositor funds to productive use and drive economic growth (Clarke, 2019; Baradaran, 2017), but the FSB's establishment as an exclusively savings institution prohibited it from making loans (Osthaus, 1976; Baradaran, 2017). Instead, the Bank invested two-thirds of deposits in US securities, allowing only a modest return for depositors (Osthaus, 1976). With promises of higher returns for the bank's depositors, the trustees successfully lobbied Congress to amend the bank's charter in 1870, authorizing management to invest depositor

funds in speculative securities (Osthaus, 1976; Baradaran, 2017). This amendment transitioned the Freedman's Savings Bank from a safe place to hold savings to a speculative investment institution, undermining the bank's core mission.

According to Celerier & Tak (2021), who compile an exhaustive dataset of both the Bank's loans and advertisements, despite the fact that roughly 90% of FSB depositors were Black, 80% of loans went to white borrowers (including 15% to elected officials, 41% to public and real estate contractors, and 13% to railroad investors), and the vast majority of loans were fraudulent and never repaid (Celerier & Tak, 2021). Further, bank management deliberately exploited depositors by intensifying advertisement efforts in response to the Bank's new charter in an attempt to enlarge the pool of deposits to plunder (Celerier & Tak, 2021). Consequently, instead of facilitating loans that benefited depositors, the bank's management effectively transferred the savings of the Freedmen to white elites.

Henry Cooke, chair of the bank's financial committee and brother of banker Jay Cooke, managed the bank's finances, making loans to several companies the Cooke family had a personal stake in (Osthaus, 1976; Baradaran, 2017). When Jay Cooke's bank failed in 1873, triggering the Financial Panic of 1873 and a run on the banks, the Freedman's Savings Bank was obligated to liquidate its sound securities to satisfy depositor demand (Osthaus, 1976). The bank may have survived the panic if not for its speculative loans, many of which were illiquid and made at low interest (Osthaus, 1976). While the bank survived the run, it ultimately could not cover its expenses and closed in July of 1874 (Osthaus, 1976). The bank's trustees elected a commission responsible for liquidating the bank's remaining assets and refunding depositors, ultimately declaring five dividends over the course of nine years amounting to 62% of total deposits owed, although only 19.8% of deposits were recouped on average (Celerier & Tak, 2021; Osthaus, 1976). Congress debated reimbursing depositors fully, but the legislation lacked sufficient political support for enactment.

The consequences of Freedman's Savings Bank's failure were severe, wiping out

half of Black wealth in its wake (Baradaran, 2017). For the Freedman, this loss was certainly consequential—but even compared to other national bank failures, the collapse of the Freedman's Savings Bank remains among the most severe in history (Celerier & Tak, 2021). Further, the Freedmen's relative inexperience in engaging with financial institutions likely exacerbated the psychologically scarring effects of this failure. Traweek & Wardlaw (2021) analyze the passbook activity of FSB depositors and find that white depositors, who comprise 10% of total depositors, were more than twice as likely to withdraw deposits following the onset of the 1873 Financial Panic than were Black depositors. Consequently, Black depositors were exploited both through false advertisements to fuel the plunder by white elites, and through their inexperience in the banking sector. The result was a wealth transfer from Black to white Americans.

Following the bank's failure, the belief that the bank was a mechanism through which whites could swindle Blacks of their economic prospects circulated throughout Black communities (Osthaus, 1976). The decimation of Black savings contributed to deep distrust of the banking institution, likely stifling Black economic development (Baradaran, 2017; Kinzer & Sagarin, 1984). According to W.E.B. Du Bois, "Not even ten additional years of slavery could have done so much to throttle the thrift of the freedmen..." than the failure of the Freedman's Savings Bank (Du Bois & Marable, 2015, p. 36). In 1913, almost 40 years after Freedman's collapse, bank president Richard Henry Boyd remarked that community elders, still scarred by the Freedman's Savings Bank collapse, had continued to instill distrust of banks in their children (Osthaus, 1976, p. 224). Consequently, Black-owned and Black-serving banks struggled to attract Black depositors for decades after the Freedman's Savings Bank collapse, with Black Americans often opting to store excess cash at home or with the Postal Savings System (Osthaus, 1976; Thieblot Jr & Fletcher, 2016; Kinzer & Sagarin, 1984).

Little is known about the effects of the rise and fall of the Freedman's Savings Bank

on Black economic development beyond the qualitative assessments of historians.[10] Interest in this topic has recently emerged, however, with scholarship examining the determinants of Black inclusion in the banking system, the effects of the Freedman's Savings Bank on its depositors' human capital, and the long-term consequences of the bank's failure (Stein & Yannelis, 2020; Celerier & Tak, 2021; Traweek & Wardlaw, 2021; Fu, 2021). Notably, Fu (2021) finds that the failure of the Freedman's Savings Bank contributed to present-day distrust of banking institutions, as 21st century Black households residing in counties with historically high exposure to the bank's failure are less likely to engage with the banking system. Evidence on the short-term effects of the bank is mixed, with some studies finding that the bank prior to its failure provided significant educational and economic benefits to its depositors (Stein & Yannelis, 2020; Fu, 2021), although this finding is a matter of debate in the literature (Celerier & Tak, 2021). Our study is the first to assess how the psychological and cultural scarring effects resulting from the failure of the Freedman's Savings Bank—above, beyond, and distinct from any effects of the bank's collapse on the economic structure of FSB localities—affected Black financial behavior throughout the 19th and 20th centuries.

3.2 Life Insurance

Facing highly segregated economic markets, Black Americans had relatively few options for savings and investment. One that was relatively more accessible to Black customers in this period, and which we focus on in this study, is life insurance.

Life insurance was popular throughout the late 19th and early 20th centuries across racial and socioeconomic lines, and represented a major source of household savings (Bryson, 1959; Temporary National Economic Committee, 1940; Goldsmith, 1955; Goldsmith & Lipsey, 1963; Bullock, 1957). These policies offered a range of attributes,

[10] A related literature on the economic history of Black-owned and Black-serving banks more broadly, however, is also emerging in economics; see, e.g., Clarke (2019).

including the opportunity to borrow against their value, but most crucially, they were used as a form of old-age savings in an era before Social Security (Goldsmith, 1955; Goldsmith & Lipsey, 1963). This was particularly true of ordinary life policies (similar to today's whole life policies), which tended to be taken out by household heads and prime-age men, were typically larger in value, and paid either an annuity or lump sum after the policyholder reached a specified age, or paid benefits to survivors in the event of the policyholder's death. Industrial life insurance policies, for contrast, tended to be taken out on behalf of women and young children, were smaller in value and with shorter maturities, and were used primarily for short-term savings (sometimes called burial insurance). A final major category of life insurance was group life, typically used by employers, and in which a single policy covered a large group. While industrial life insurance initially comprised the majority of Black life insurance holdings (Stuart, 1969; Pierce, 2013), one mid-century study suggested that Black households' insurance holdings by value in force were: ordinary life 60%, industrial life 32%, and group life 8% (Bryson, 1959).

Although traditions of insurance in the Black community can be traced to Black churches, benevolent groups, and mutual aid societies organized to provide support to members in times of crisis (Abner III, 1962; Stuart, 1969; Woodson, 1929; Southern, 1942), most Black households purchased life insurance policies from formal insurance firms post-Emancipation, whether white- or Black-owned. Insurance agents selling industrial life policies traveled door-to-door to hand-collect weekly premiums from customers, while premiums on ordinary life insurance were collected less frequently, albeit in a similar manner (Abner III, 1962; Southern, 1942). This feature of life insurance sales motivates our choice of measure for local insurance demand, discussed in more detail in Section 5. Likewise important for our analysis are the racial dynamics of the industry. White-owned firms both employed Black agents and sold policies to Black customers—whether via Black or white agents. Indeed, most Black households purchased life insurance from white-owned companies, and were likely served by white

agents. Importantly, however, while white insurance agents could sell policies to both Black and white households, Black insurance agents (whether employed by a white-owned or Black-owned firm) could only sell to Black customers (Stuart, 1969; Pierce, 2013; Southern, 1942; Kinzer & Sagarin, 1984; Bryson, 1959). Consequently, the number of Black insurance agents in a region represents a reasonable proxy for the lower bound of Black household demand for life insurance.

4 Estimation strategy

4.1 Difference-in-Differences Using Two-Way Fixed Effects

Throughout our main analysis, we adopt a difference-in-differences (DiD) identification strategy, exploiting the plausibly exogenous failure of the Freedman's Savings Bank to estimate its effect on demand for insurance by households residing in counties containing a FSB branch, relative to that in unexposed counties. Our baseline specification takes the following form:

$$AgentsPerHH_{ct} = \alpha + \beta FSB_c * POST_t + \gamma_c + \lambda_t + \epsilon \tag{1}$$

where $AgentsPerHH$ is the number of insurance agents per 1,000 households for county (or city) c in year t. The average treatment effect on the treated (ATT) is estimated by interacting $POST$, an indicator equal to 1 in years after the bank's 1874 failure,[11] with FSB, an indicator equal to 1 if the county ever housed a Freedman's Savings Bank branch.[12] A positive β indicates that difference in demand for insurance between treatment and control counties increased after the Freedman's Savings Bank failed. Our baseline specification takes a two-way fixed effects (TWFE) form,

[11] Our sample period runs from 1850-1940, inclusive, which is the period over which we have access to full-count Census data.

[12] Our main analytical sample consists of what was traditionally the South, excludingg Texas.

controlling for time-invariant characteristics of the county a particular branch was located in, captured by γ_c, and spatially-invariant characteristics of a given year, captured by λ_t. The ATT is identified if the treatment, $FSB_{ct} * POST_t$ is orthogonal to the error term, ϵ, and the parallel trend assumption holds. We cluster our standard errors by county, the level of our treatment (Abadie *et al.*, 2017).

4.2 Threats to Identification

We address several threats to identification in this analysis. First, although our period, 1850-1940, provides 3 Census years to test pre-treatment trends, the 1850-1860 Censuses notably do not enumerate slaves. The Emancipation Proclamation was signed in 1863, freeing slaves and fundamentally changing the Southern economy. Accordingly, 1870 is the first and only pre-treatment period for which information on all Southern residents is available, complicating the analysis of parallel trends. We adopt several strategies throughout the paper to address this concern, the main ones of which we detail below, and others of which appear in line with our discussion of results.

Our primary identification concern is the endogenous selection of FSB branch locations. The historical record provides ample information regarding the determinants of branch locations (Osthaus, 1976). In the early years of the bank's formation, 1865-1868, branches were established in cities occupied by Black troops to persuade them to deposit their pay. Black troops occupied 163 counties throughout the Reconstruction Era. The timing of pay disbursement and ease of access for bank officials to reach Black troops plausibly exogenously determined the location of the Bank's early branches.

The Bank's later branches, 1869-1871, were endogenously located in cities where a branch was deemed economically viable. Strategic placement of branches in growing cities would positively bias our results, as unmeasured regional economic growth

potential is likely correlated with the selection of branch locations. We analyze the failure of early and late branches both together and separately, relying on the plausible exogeneity of the early branch locations, and recognizing the potential upward bias among later branch locations. In our two-way fixed effects specifications, we condition on race-specific pre-treatment trends in covariates likely correlated with branch location and insurance uptake. We further employ a battery of alternative specifications and estimation techniques to minimize potential bias and probe the robustness of our results.

We outline our main empirical approaches to overcoming threats arising from selection into treatment and potential parallel trends violations briefly below. We posit that utilizing a combination of econometrically distinct and complementary approaches helps to address concerns regarding causal identification.

4.3 Doubly Robust Methods

First, we supplement our two-way fixed effects results using propensity score methods to aid identification in equation 1. Propensity score methods rely on the assumption that selection into treatment is random conditional on a set of predictive covariates. This set of pre-treatment covariates performs best if correlated with both the treatment and the outcome Garrido *et al.* (2014). Instrumental variables, however, should be excluded from the covariate vector as these variables do not address the problem of confounding.

Our first specification estimates a semiparametic DiD model using stabilized inverse-probability weights (IPW) developed by Abadie (2005). We estimate the probability of treatment assignment using 1870 county averages by race of literacy and employment rates, as well as wealth, urban status, socio-economic status (proxied by occscore), and family size. Data for these exercises come from the decennial US Census (details in Section 5). IPW methods estimate a counterfactual by plac-

ing high weights on treated counties with a low probability of treatment and control counties with a high probability of treatment. This estimate can be combined with outcome regression techniques developed by Heckman *et al.* (1997), resulting in the doubly-robust (DR) DiD estimators developed by Sant'Anna & Zhao (2020). Specifically, these estimation methods use a vector of pre-treatment covariates to provide consistent estimates of the ATT if either the propensity score model or the outcome regression model is specified correctly. The first specification, DRIPW, estimates the propensity score model via maximum likelihood and the outcome regression model using ordinary least squares. The second specification, DRIMP, improves on this specification by estimating propensity scores using the inverse probability tilting estimator and weighting the outcome regression by a function of these estimated probabilities. This estimator is both doubly-robust for consistency and inference. These estimators help reduce bias in TWFE estimates when there are covariate trends in both treatment and control groups (Sant'Anna & Zhao, 2020).

The doubly-robust DiD methods require correctly specified propensity score and outcome regression models to develop a counterfactual control group. Such a method may be sensitive to the choice of covariates and availability of relevant data. The synthetic DiD method, developed by Arkhangelsky *et al.* (2021), is robust to these concerns. Specifically, the synthetic DiD estimator constructs a counterfactual control group using both pre-treatment values of the dependent variable, $AgentsPerHH$, and time effects of the control group. The estimator is doubly-robust, in that it needs only the unit weights constructed from the pre-treatment period, or time weights constructed from the control group, to be effective at removing bias to produce consistent ATT estimates. We employ the synthetic DiD estimator as our final robustness check using doubly-robust methods.

4.4 Instrumental Variables

Second, we adopt an instrumental variable estimation approach to address the endogeneous selection of counties into treatment. The required instrument must predict the counties selected for a Freedman's bank branch, but be orthogonal to a location's baseline household demand for insurance. This identification method is strengthened by the ample historical record of FSB management's selection process. Specifically, the historical record makes clear that FSB branches were initially located in counties with a high population of Black Union soldiers, as it was originally intended to encourage thrift among this population and to absorb their paychecks. Accordingly, we use $MaxBlackTroops$, the maximum number of Black troops in a county during reconstruction, as our primary instrument. We further interact this instrument with $Occupation$, which measures the maximum total number of troops in a county. The rationale for this is to account for the accessibility of Southern counties by the Northern bank managers and potential customers, who required safe passage to a bank branch since the South remained hostile towards the Freedmen and their associates in the aftermath of the Civil War. For this measure, we use the Mapping Occupation dataset by Downs & Nesbit (2016), which provides the location and date of white and Black army troops from 1865-1880

We argue that our proposed instruments satisfy the two identifying assumptions. First, both historical accounts and empirical evidence demonstrate the predictive power of our instruments, indicating that the instruments are relevant. Further, we contend that $MaxBlackTroops$ only influences $AgentsPerHH$ through the financial scarring induced by the failure of the Freedman's Savings Bank. Our data indicates that Union Army troops were highly mobile, and therefore their presence was unlikely to affect long-term local preferences for life-insurance holdings. We also present a second instrument, $Contraband$, which indicates the total number of so-called "contraband camps" in each county. Contraband camps were refugee camps for ex-slaves,

and are predictive of the location of FSB branches. Data on the location of contraband camps come from Cooper (2014). These instruments provide exogenous variation in the location of FSB branches and identify local average treatment effects of the failure of the FSB on household insurance holdings.

5 Data

Our primary outcome of interest is the number of insurance agents per 1,000 households at the county level over the period 1850-1940. We compute these measures from complete-count, decennial US Census data from 1850-1940 Ruggles *et al.* (2019), using consistent 2016 county borders. This period allows us to control for pretreatment characteristics (1850-1870) and to analyze post-treatment outcomes (1880-1940).[13] To compute the number of insurance agents per thousand households, we divide the total number of individuals employed as insurance agents within a county in each census year by the total number of unique households in that county and year, and multiply by 1,000. Although microdata on household insurance holdings are unavailable for the period of this study, we propose that agents per thousand households is a sufficient proxy. This measure of insurance holdings is supported by the characteristics of the insurance market throughout the 19th and 20th centuries, as discussed in Section 3.2. In particular, agents selling industrial life insurance, one of the primary insurance policy types purchased by Black households, collected weekly premiums door-to-door, and therefore had a natural limit of households they could market. Agents selling ordinary life insurance often collected premiums by mail, but were still bound by an upper limit of households. According to data from Stalson (1942), an ad-

[13]To compute measures at the level of geographically consistent counties, we merge Census data with crosswalks developed by Berkes *et al.* (2022), which geolocate most individuals in our data to a latitude and longitude and assigns a corresponding 2016 county and state. We assign the remaining individuals to a modern county using historical county crosswalk data from Eckert *et al.* (2020). The unit of analysis is county for the majority of observations, and city for the minority of cities that are independent from counties.

ditional insurance agent is associated with new insurance sales of approximately $600 per household on average, in 2023 dollars. Figure 1, which provides an 1870 snapshot of the spatial distribution of insurance agents in our sample, indicates substantial within- and across-state variation in baseline insurance activity.

Notably, we are unable to distinguish insurance sales by race of household. While white insurance agents sold insurance to both white and Black households, Black insurance agents only marketed to Black households. Accordingly, we compute two measures for Black-specific insurance demand. The first measure represents the number of Black insurance agents per 1,000 Black households, and is a correlate of Black insurance holdings. The second measure is the share of all insurance agents who are Black, and indicates exclusively-Black insurance holdings relative to mixed Black-and-white insurance holdings. These measures must be interpreted cautiously, however, as a study of Black insurance holdings in 1954 Baltimore indicated that 84% of insurance in force was purchased from white-owned insurance companies (Bryson, 1959). Further, we have no indication of how this statistic varies across space and time. Accordingly, we focus our main analysis on total insurance agents, and conduct robustness checks using the race-specific measures.

We further use the 1860 and 1870 US Censuses to calculate pre-treatment county-level variables we expect to correlate with both the location of FSB branches and insurance demand. Specifically, we calculate county-level averages of employment rates, literacy rates, occupational scores, wealth, urban status, and family size, by race of the household head. While the 1860 measures capture pre-treatment-assignment county characteristics, the full Black population was not enumerated until the 1870 Census. We therefore rely on race-specific county characteristics from 1870, which could in theory be contaminated by assignment to treatment group from 1865-1870, but precedes the 1874 failure of the Freedman's Savings Bank.

We take Freedman's Bank locations, number of depositors, and year opened from Fu (2021) and Celerier & Tak (2021). The explanatory variable of interest is FSB,

a binary variable equal to unity if the county or city ever received a FSB branch, interacted with $POST$, a binary variable equal to unity in years after the Bank's 1874 failure. FSB branches were spread throughout the United States, ranging from Houston to New York City, although the vast majority of branches were located in the South. We drop New York City and Philadelphia from this analysis to restrict our analysis to the Southern economy. We further drop Houston from the analysis as this branch closed prior to the bank's failure. We therefore restrict our main analysis to all counties within a Southern state that ever received a FSB branch. Branch locations are depicted in 2.

We draw on a range of other data to facilitate our IV and doubly-robust identification strategies, as well as to explore mechanisms and robustness. We discuss these sources and methods in line with those analyses in later sections of the paper.

6 Main Results

6.1 Two-Way Fixed Effects

Table 1 uses a two-way fixed effects approach to estimate the effect of the Freedman's Savings Bank failure on local insurance demand, here given by the number of insurance agents per 1,000 households. All specifications are restricted to the South, include county and year fixed effects, and cluster standard errors by county.

Column 1 presents the simplest two-way fixed effects specification, which suggests that following the failure of the Freedman's Savings Bank in 1874, counties with at least one FSB branch saw a statistically significant increase of nearly 4 agents per 1,000 households.[14] For context, based on insurance industry statistics compiled in

[14]We interpret our results throughout this paper as reflecting a change in insurance demand rather than a change in insurance supply. There is little reason to believe that insurance supply would have changed differentially in FSB counties relative to non-FSB counties following the bank's collapse, particularly given that none of the fundamentals determining eligibility, price, coverage, or the costs of offering plans changed discontinuously over time and space in ways that were correlated with

the 1930s, one additional agent could be expected to generate new insurance sales of approximately $600 per household in 2023 dollars (Stalson, 1942). Columns 2 and 3, which add state-by-year fixed effects and state trends, respectively, show similar significance and magnitudes.

Given potential concerns about the endogenous placement of FSB branches—namely, that they may have been placed in economically vibrant locations that were predisposed to insurance products even prior to FSB collapse—in Columns 4-6 we add to our standard specification trends in 1860 Black and white covariates, 1870 Black and white covariates, and 1870 Black covariates, respectively. These covariates include the urban population, literacy rate, average occscore, employment rate, wealth, and family size. Because the characteristics of the local Black population—the group of primary relevance to our analysis—are poorly measured prior to the end of the Civil War,[15] we place greater emphasis on the results in Columns 5 and 6. Indeed, we view the approach in Column 6 as being that which best addresses the particular endogeneity concerns of this setting, given that the motivation for FSB branch placement was to identify localities with large and relatively affluent *Black* populations specifically rather than large and relatively affluent populations more generally. Because of this, we use this specification going forward in the paper when comparing results across two-way fixed effects and alternative estimation strategies. Here, we see that the addition of local economic and demographic trends in Columns 4-6 lowers the estimated treatment effect of FSB failure to roughly 1.1-1.6 agents per 1,000 households, an effect which remains strongly statistically significant.

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the shock. The only scenario in which insurance supply might have changed in correlation with the shock would be if, for instance, seeing a gap in the financial services market following the FSB failure, insurance companies aggressively stepped up their marketing and sales efforts in these (but not other) localities in the post period. This, however, seems implausible as a major explanation of our overall results.

[15] 1860, a Census year prior to Emancipation, is the last year in the Census prior to the establishment of FSB branches, the bulk of which opened in the mid-to-late 1860s. 1870 presents us the closest reasonable snapshot of Black population, education, wealth, and income in a year prior to FSB collapse.

Table 1: Two-Way Fixed Effects

	DV: Insurance Agents Per 1,000 Households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FSB × Post	3.952***	4.011***	3.921***	1.215***	1.128***	1.598***	3.548***	4.921***
	(0.356)	(0.353)	(0.357)	(0.411)	(0.402)	(0.375)	(0.385)	(0.682)
Observations	8,352	8,352	8,352	8,352	8,352	8,352	8,262	8,136
R-squared	0.713	0.737	0.725	0.763	0.771	0.754	0.701	0.695
Fixed Effects	County, Year	County, Year, State × Year	County, Year	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	No	State	1860 Covariates	1870 Covariates	1870 Black Covariates	No	No
Cluster	County	County	County	County	County	County	County	County
Sample	South	South	South	South	South	South	Early South	Late South

Notes: Each column is a separate regression of *AgentsPerHH* on *FSB*, an indicator for Freedman's Savings Bank exposure, interacted with *Post*, an indicator for years subsequent to the Bank's 1874 failure, along with the noted fixed effects and time trends. Column 4 (5) includes time trends of 1860 (1870) white and Black county averages of employment, literacy, wealth, urban status, socio-economic status, and family size. Column 6 includes time trends of the same controls, but only 1870 Black county averages. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Treated counties in Column 7 are restricted to branches that opened prior to 1869. Treated counties in Column 8 are restricted to branches that opened after 1868. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

Finally, based on the idea that the endogeneity of FSB branch placement may have been stronger amongst branches built later in the bank's history, as the bank responded to lessons learned from its earlier branches and fine-tuned its expansion strategy, we separately estimate the effects for branches built up to and including 1868 ("early branches," Column 7), and branches built 1869 and after ("late branches," Column 8). Consistent with this hypothesis, effect sizes are slightly larger for late branches than for early ones.

In the Appendix, we present several further checks on our results. First, we account for the possibility that because FSB locations tended to be more urban by design, our results may merely reflect the long-term trajectories of urban, economically ascendant localities more generally. To this end, in Table 13, we present results controlling for a county's urban status, and results estimated exclusively on a sample of counties with an above-median share of urban population—i.e., a sample in which the control group more closely resembles the treated counties economically and demographically. Though these adjustments slightly lower effect sizes, they remain extremely similar to our main results in sign (positive), size (ranging from 2.401-3.232 agents per 1,000 households), and statistical significance (significant at the 1% level).

Second, we account for the possibility that our results reflect not the FSB failure itself, but rather pent-up demand for insurance among the newly-emancipated Black population, most of whom were unable to participate in these markets prior to the end of the Civil War. Put another way, we ask: had the entire Black population in our sample been free to purchase insurance much earlier in time (and in particular, prior to our first post-Emancipation observation in 1870), would our FSB-failure results disappear? Table 14 suggests that this is not the case: both specifications that track the demand for insurance in the South pre- versus post-Civil War as a function of the percent enslaved, and specifications that estimate pre-Emancipation correlations between insurance demand and a Southern county's share of Black population that was free, produce small, negative, and statistically insignificant results. Moreover, in a more econometrically like-with-like comparison to our main results, we show that when estimating our main two-way fixed effects specification on the sample of states, New York and Pennsylvania, which both had FSB branches and were also free states prior to 1870,[16] effects range from a statistically significant 0.780-2.260—roughly 50-60% of the estimated effect sizes in our main (Southern) analytical sample. That is, assuming that baseline Black demand for insurance is similar in New York and Pennsylvania to that in the South, and assuming that FSB treatment effects are similar across these locations, we can conclude that even while a fraction of our main results may be due to pent-up demand for insurance in the South (represented by the smaller treatment effects estimated in the NY-PA sample compared to our main sample), there exists a large and significant causal effect of FSB failure on insurance demand above and beyond this (represented by the positive and significant effects estimated for NY and PA).

[16]Note that FSB branches outside the traditional South, including those in New York City and Philadelphia, are excluded from our main analysis.

6.2 Doubly-Robust Methods

Our second approach to causal identification involves a range of doubly-robust estimation methods, which we present in Table 2. As outlined in Section 4.3, the aim of these methods is to overcome threats to inference arising from the possibility of selection into FSB status and potential parallel trends violations corresponding to such endogenous treatment.

In Column 1, we present inverse probability weighted results, where these weights are based on 1870 county-level Black and white averages for urban population, literacy rates, employment rates, occupational income, wealth, and family size. These variables are also used to balance treatment status in Columns 2 and 3, which present DRIPW and DRIMP estimation results, respectively. Column 4, our preferred doubly-robust specification, presents results of the same estimation strategy used in Column 3, but restricts the balancing covariates to Black county-level averages in 1870. Columns 1-4 paint a consistent picture: even after adjusting for potential bias arising from endogenous FSB branch placement, there remains a substantial, positive, and statistically significant effect of FSB failure on local insurance demand. Crucially, the event studies in Figure 3 show that the DRIMP specifications (with 1870 covariates in Panel A, and with 1870 Black covariates in Panel B) overcome concerns over differential pre-trends.

It is worth noting that accounting for potential endogeneity in this manner attenuates the results compared to those produced in the naive two-way fixed effects specifications, such as in Column 1 of Table 1; rather than increasing the agents per 1,000 households by roughly 4, here, FSB failure increases this figure by roughly 1-2. Notably, the results of our preferred two-way fixed effects specification, which incorporates 1870 Black covariate trends (see Column 6 in Table 1), are nearly identical to the doubly-robust estimates in Columns 1-4 of Table 2 in terms of sign, significance, and magnitude. This is perhaps unsurprising, given that controlling for trends

in the variables likely to determine treatment status is an approach very similar in spirit to those formalized more rigorously in methods such as IPW and DRIMP. This concordance gives us additional confidence in relying on the two-way fixed effect specification with 1870 Black covariate trends in those rare situations where doubly-robust methods cannot be implemented in our data.

Finally, in Column 5, we present the results of synthetic difference-in-difference estimation. Although these rely on a slightly different econometric approach than the doubly-robust methods showcased in Columns 1-4 in that they construct a counterfactual control group using weights calculated from both pre-treatment values of $AgentsPerHH$ and time effects of untreated counties rather than by balancing on 1870 covariates, they, too, produce similar results—namely, a statistically significant increase of 2.604 agents per 1,000 households. Figure 4 illustrates these synthetic difference-in-difference results graphically, where we can see the treated and control groups following each other tightly in the pre-collapse period, and diverging sharply thereafter—particularly after 1880, when we hypothesize that many Black Americans may have first had sufficient funds following Emancipation to invest in insurance products.

6.3 Instrumental Variables

Yet another approach to establishing causal effects is to implement an instrumental variables strategy. Here, motivated by historical evidence that FSB branches were seen initially as an institution to absorb the wages of and foster thrift among Black troops during Reconstruction (including Freedmen who were recruited during the Civil War with the promise of future pay), we use as our primary instrument the maximum number of Black troops stationed in a county over the period 1865-1874.

As an alternate instrument for FSB treatment status, we use the number of contraband camps in a county over the period 1860-1865. Contraband camps were areas

Table 2: Doubly-Robust Methods

	DV: Insurance Agents Per 1,000 Households				
	Doubly Robust				Synthetic Control
	(1)	(2)	(3)	(4)	(5)
ATT	1.259***	1.370***	1.082**	1.796***	2.604***
	(0.475)	(0.445)	(0.474)	(0.370)	(0.383)
Observations	8,352	8,352	8,352	8,352	8,352
Method	IPW	CS DRIPW	CS DRIMP (70)	CS DRIMP (70B)	SDID
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Cluster	County	County	County	County	County

Notes: Each column presents an average treatment on the treated (ATT) estimate of the effect of exposure on $AgentsPerHH$. Columns 1-3 are estimated using pre-treatment assignment time-invariant covariates, including 1870 white and Black county averages of employment, literacy, wealth, urban status, socio-economic status, and family size. Column 5 replicates Column 4, but uses only Black-specific 1870 covariates. Column 5 is estimated using synthetic difference-in-differences. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

surrounding Union-held positions where escaped slaves and other Black individuals affiliated with the Union Army established a base. Since as of late 1861, US policy held that escaped slaves were to no longer be returned to the South, refugees to these camps were deemed "contraband of war," in reference to their status as the former property of the enemy. Many at these camps soon became involved in Union Army efforts, including on a paid basis, making them an ideal target for FSB participation following the conclusion of the Civil War.

Finally, we interact both of these instruments, which proxy the presence of a relatively dense and affluent Black population in the early post-Bellum period, with an instrument—the maximum Union (by then, US) Army troops stationed over the period 1865-1874—that captures the ease and safety with which both the newly-free Black population of the South could participate in public life, and the Northern bank managers could service FSB branches.

All three instruments and their interactions are strongly predictive of FSB adoption (see IV first-stage results in Table 12), and we do not expect for the historical presence of Black troops, contraband camps, or Reconstruction-era US troops to di-

rectly affect either the baseline demand for insurance in these localities, or—more crucially still—the post-FSB-failure *change* in the local demand for insurance.[17]

In Table 3, we present the IV results. These indicate, irrespective of choice of instrument, a statistically significant increase of roughly 4-6 agents per 1,000 households in FSB counties following the collapse of the Freedman's Savings Bank. These estimates, while slightly larger than those produced by two-way fixed effects and doubly-robust methods, tell a similar story: local insurance demand rose in the wake of the bank's failure. Furthermore, the reported test statistics underscore the relevance and exogeneity of our instruments.[18]

Given the similarity of the results produced across the choice of instrument, and the straightforward correspondence of the "Black Troops" measure to the historical motivation for FSB branch placement, going forward, we take the specification given in Column 1 as our preferred IV specification.

6.4 Summary

Using a variety of fundamentally different estimation strategies, each with its own distinct approach to overcoming endogeneity concerns, we have generated estimates that nevertheless tell a consistent story: following the failure of the Freedman's Savings Bank in 1874, counties with a branch location saw sharp, sustained, and statistically

[17]For instance, historical troop density does not systematically predict counties that had or that would come to have over the ensuing century a more heavily urban, affluent, or Black population—these being significant determinants of baseline insurance demand (not reported).

[18]The underidentification test reported in Table 3 is a test for instrument relevancy, where the null holds that the particular endogenous regressor in question is unidentified. Rejection of the null, which is the case here, indicates that the instrument is relevant, i.e., it predicts FSB location in the first stage. This test is estimated using (Baum *et al.*, 2022). The Weak IV confidence sets provide robust bounds of the estimated treatment effect robust to relevant but weak IVs. This is estimated using (Sun, 2018). The effective F statistic is the first stage f statistic adjusted for non-homoskedastic errors (Olea & Pflueger, 2013), and is estimated using (Pflueger & Wang, 2020). A value over 10 is typically deemed sufficient (Andrews *et al.*, 2023). Finally, the overidentification test is a test for instrument exogeneity. It requires 2 instruments and assumes that one of the instruments is exogeneous. Rejection of the null indicates that instruments are endogenous, while failure to reject the null (which is the case in Column 2 and arguably only marginally not the case in Column 4) indicates that the exclusion restriction is valid.

Table 3: Instrumental Variables

	DV: Insurance Agents Per 1,000 Households			
INSTRUMENTS	Black Troops (interacted with)		Contraband Camps (interacted with)	
		Union Troops		Union Troops
	(1)	(2)	(3)	(4)
FSB × Post	5.225***	6.360***	3.939***	5.540***
	(0.874)	(0.772)	(0.753)	(0.690)
Observations	8,352	8,352	8,352	8,352
R-squared	0.711	0.703	0.713	0.709
Fixed Effects	County, Year	County, Year	County, Year	County, Year
Cluster	County	County	County	County
Effective F-Stat	11.564	19.588	11.002	43.137
UnderID P-Value	0.000	0.000	0.001	0.000
OverID P-Value		0.377		0.068
Weak IV Robust CS	[3.928 - 8.080]	[5.341 - 8.790]	[2.373 - 5.802]	[5.098 - 7.246]

Notes: Each column is a separate instrumental variable regression of $AgentsPerHH$ on FSB, an indicator for Freedman's Savings Bank exposure, interacted with *Post*, an indicator for years subsequent to the Bank's 1874 failure, along with the noted fixed effects and time trends. All specifications instrument for endogenous selection into treatment, indicated by FSB. The primary instrumental variable in Columns 1-2 is $BlackTroops$, the maximum number of Black troops that occupied a county during reconstruction. The primary instrumental variable in Columns 3-4 is $ContrabandCamps$, the total number of contraband camps within a county. Columns 2 and 4 fully interact the primary instrumental variable with a second instrument, $UnionTroops$, the maximum number of Union troops occupying the county during reconstruction. The presented p-values result from Kleibergen-Paap LM underidentification tests. Rejection of the null hypothesis suggests that the excluded instruments are relevant. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

significant increases in the demand for insurance. These increases were economically significant as well, with estimates ranging from roughly 1 to 6 additional agents per 1,000 households, equivalent to a roughly $600-$3,600 per household total increase in annual policy volume (in 2023 dollars) between the pre-collapse and post-collapse periods in these counties.

For convenience in considering these results all together, we summarize our preferred specifications across these three broad classes of methods in Table 4. Column 1 features the simple two-way fixed effects estimation, Column 2 features two-way fixed effects with trends in 1870 Black covariates, Column 3 features DRIMP estimation based on 1870 Black covariates, Column 4 features synthetic differences-in-differences, and Column 5 features an IV approach using Black Troops as an instrument. Here, we see estimates ranging from 1.598 to 5.225 additional agents per 1,000 households in FSB counties post-collapse, all statistically significant.

To address the possibility that 1850 and 1860 observations are not especially informative in a setting where the majority of the Black population was enslaved at the time, and therefore prohibited from the sorts of financial activity at issue in our study, in Table 15 we re-estimate Table 4 dropping 1850 and 1860 from the pre-collapse period. The results remain statistically significant, with magnitudes that are only marginally smaller than in the full sample period. Indeed, our preferred strategy, the DRIMP 1870 Black covariates specification, yields results that are nearly identical: 1.791 in the restricted sample, versus 1.796 in the standard sample, with both coefficients significant at the 1% level. Accordingly, we conclude that our results are not an artifact of incomplete or inaccurately measured pre-Emancipation data.

In the remainder of the paper, we present results using all five of the specifications laid out in Table 4 wherever econometrically feasible.[19] The rationale for this

[19]In specifications 1) relying on an unbalanced panel, 2) where there are multiple distinct treatment groups (as in Table 8 and Table 6), or 3) where doubly-robust methods would be conceptually inappropriate since FSB branch locations do not dictate treatment assignment (therefore obviating the particular endogeneity concerns these methods are meant to address; as in the tables throughout

Table 4: Main Results: Summary

	DV: Insurance Agents Per 1,000 Households				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	3.952***	1.598***			5.225***
	(0.356)	(0.375)			(0.874)
ATT			1.796***	2.604***	
			(0.370)	(0.346)	
Observations	8,352	8,352	8,352	8,352	8,352
R-squared	0.713	0.754			0.711
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South

Notes: This table reproduces the main results using each of the core methods shown. DRIMP methods use 1870 Black covariates. *** p<0.01, ** p<0.05, * p<0.1

approach is that while any one technique may have idiosyncratic strengths and weaknesses, the fact that so many diverse estimation strategies all yield similar results ultimately lends confidence to our overall conclusions. In the rare cases where the doubly-robust methods cannot be estimated, we present results using the specification in Column 2 (two-way fixed effects with 1870 Black covariate trends), which is closest both in spirit and in estimated results to our most preferred estimation method, the DRIMP 1870 Black covariates specification in Column 3.

7 Mechanisms & Additional Checks

In the following section of the paper, we dive deeper into how, why, and for whom the Freedman's Savings Bank collapse shifted financial behavior. We also provide evidence that our results are not primarily driven by confounding factors. Instead, we show that these persistent changes in portfolio choice reflect a change in the

Section 7.4), the doubly-robust methods cannot be implemented, and we instead rely on the other methods at our disposal.

affected group's attitudes toward traditional banking institutions.[20]

7.1 Are Results General or Race-Specific?

The Freedman's Savings Bank targeted Black prospective customers, and it is the bank's Black depositors that ultimately bore the brunt of its failure. With the FSB collapse serving as a racialized economic shock, we might then also expect to see a racialized response. Put another way, if Black residents of FSB counties were those most exposed to FSB failure—whether through direct loss of wealth or through the event's salience in the broader community—we would expect our main results to be driven disproportionately by the shifting behavior of a county's Black residents. In this section, we test whether the results we have documented up to this point are a general phenomenon, driven by the majority white households (or white and Black households in equal measure), or whether the sharp rise in the demand for insurance post-collapse is a phenomenon specific to the local Black population.

To do so, we exploit the fact that white insurance agents could sell to all customers irrespective of race, while Black insurance agents could only sell to Black customers.[21] Thus, an absolute increase in Black insurance agents following FSB failure could only arise from changing demand on the part of Black customers. Moreover, a *relative* increase in Black agents would indicate a *disproportionate* surge in demand by Black versus other-race customers.[22]

[20] In our study, we do not distinguish between potential effects on behavior through changes in risk aversion, trust in banks, or trust in white-owned/white-staffed institutions: they are observationally equivalent in the data currently available. While isolating the particular psychological phenomena involved remains a matter for future research, we simply interpret our results as reflecting a change in attitudes and preferences, whatever the specific underlying factors.

[21] Historical records indicate that Black households purchased insurance primarily from white agents. However, insurance was an unusually racially "integrated" profession in this period, and Black agents were also allowed to practice, even in the South. While white agents could serve customers of all races, Black agents were restricted to Black customers—in large part because insurance premium collections were conducted door-to-door in a heavily racially segregated setting.

[22] It is not implausible that white customers might respond to the FSB shock, even if they were not as directly affected. Firstly, some white depositors would have faced a loss of wealth upon the bank's collapse. To wit, roughly 10% of FSB depositors were white, although many white depositors

Table 5: Effects on Race-Specific Insurance Demand

	DV: Black Insurance Agents Per 1,000 Black Households				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	1.270***	0.492***			1.811***
	(0.168)	(0.187)			(0.383)
ATT			0.533***	1.281***	
			(0.182)	(0.179)	
Observations	8,352	8,352	8,352	8,352	8,352
R-squared	0.314	0.336			0.312
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South

Notes: The dependent variable is *BlackAgentsPerBlackHousehold*. Column 1 presents results from our standard two-way fixed-effects specification, and Column 2 adds 1870 Black county-level covariate trends. Column 3 presents results from our standard DRIMP specification based on 1870 Black covariates. Column 4 presents results from our standard synthetic diff-in-diff specification. Column 5 presents results from our main IV specification, wherein the number of Black troops instruments for FSB locations. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

Table 5 presents results where the outcome of interest is the number of Black insurance agents per 1,000 Black households. All five estimation approaches show a positive and statistically significant impact of FSB collapse on Black insurance demand, with estimates falling in the 0.5-1.8 range.

For comparability with the main results on agents per 1,000 households that we present in Table 4, in Table 16 we present results on the impact of FSB failure on the number of Black insurance agents per 1,000 households (of any race). These results, too, are all statistically significant and positive, ranging from roughly 0.3-0.8 Black agents per 1,000 households. These results imply that approximately 13-20% of the

were eventually compensated for at least some portion of their losses. Secondly, word of a local bank failure might have traveled beyond the local Black community, and local white residents could have in theory shifted their financial behavior in response to this information. However, we hypothesize that the FSB shock was fundamentally a shock specific to the Black population for a number of reasons: because greater numbers of Black depositors were affected, because Black depositors were more severely affected, because the event likely had greater salience within the greater Black population, and because the Black population likely had fewer options for financial-sector and especially banking participation than their white counterparts following the collapse of a Black-serving institution such as the FSB.

marginal agents added in the wake of the bank's failure were Black. Thus, while any of the 1.5-5.2 additional agents documented in Table 4 could have theoretically been added in response to increasing Black demand, at least 13% of these agents were added exclusively to serve Black customers.

Finally, we estimate the effects of the FSB failure on the share of the insurance workforce that is Black. These results are slightly noisier, perhaps reflecting among other things variation in the size of the pre-collapse (almost exclusively white) insurance workforce, and the fact that white insurance agents could be and in many cases likely were deployed to satisfy rising Black demand. While one specification (Column 3) suggests a small but statistically insignificant negative effect on the share of insurance agents who were Black, four of our five estimation approaches (Columns 1, 2, 4, and 5) show a positive impact on the share of insurance agents who were Black, with three of these specifications (Columns 1, 4, and 5) indicating strongly statistically significant effects: namely, a roughly 4-10 percentage point increase in the share Black.

Together, these results suggest either that Black customers' preference for interacting with Black as opposed to white agents rose in response to the FSB failure shock, or that Black demand for insurance more generally rose in response to the shock. The latter is more plausible given that Black customers in the Reconstruction and later Jim Crow South likely had little influence, even indirectly, over the staffing decisions of insurance firms. Either way, however, these results suggest that the FSB failure was a racialized rather than a general phenomenon. These results also help assuage concerns about differential trends and the potentially endogenous placement of branches in economically ascendant regions: while we might expect insurance demand to rise more over time in dense and economically vibrant localities relative to localities without these growth prospects, we would not expect it to rise differentially by race—and save for a scenario in which the white-serving insurance market was saturated, we would especially not expect it to rise disproportionately among a less

financially-advantaged group.

7.2 Do Results Merely Reflect the 1873 Panic?

The Freedman's Savings Bank collapse coincided with—and indeed was precipitated by—another major banking shock, the Panic of 1873. With these shocks both sharing an identical post period in our data, could it be that our results are simply picking up an 1873-Panic effect per se, rather than a Freedman's Savings Bank one? Or, from another angle: is the response to the FSB collapse *sui generis* (and perhaps reflective of its status as an unusually and racially exploitative episode), or does it reflect the way people respond to bank failures more broadly? If the FSB failure is "just another" bank failure, then we would expect that more severely-hit areas in the 1873 Panic should also see an increase in insurance demand, whether in general or on a race-specific basis—and more importantly, that controlling for this crisis should eliminate our main FSB failure effect. The 1873 Panic, then, can serve as a placebo test of sorts.

To examine this, we collect annual deposit data for the universe of (non-FSB) nationally-chartered banks from the annual reports of the Office of the Comptroller of the Currency. We calculate the severity of the 1873 Panic in a county as being the negative of the percentage change in total deposits for all non-FSB nationally-chartered banks in the county between 1873 and 1874, for counties that had such a bank.[23] Counties with an above-median severity value are deemed to have had a severe experience of the 1873 Panic, and all other counties are deemed not to have had an especially severe experience. We then estimate a two-way fixed effects specification with 1870 Black covariate trends (our preferred specification for when DRIMP methods cannot be implemented due to the existence of multiple treatment groups) wherein, first, the 1873 Panic effect is estimated separately, and second, the

[23]We take the negative of the percentage change so that larger values represent a more severe contraction.

Panic's effects are estimated alongside and in interaction with the FSB failure's effects. The results of this analysis are presented in Table 6.

Column 1 shows that the 1873 Panic had a positive and significant impact on the number of insurance agents per 1,000 households, albeit one about half the size of our analogous estimates of the impact of the FSB failure (see Column 2, Table 4). This indicates that exposure to a severe experience of the 1873 Panic also appears to have shifted financial behavior—whether because of growing distrust in banks, rising risk aversion, or other factors—although Column 3 indicates that the 1873 Panic did not have the sort of racialized impact that the FSB collapse did. This is perhaps unsurprising considering that depositors of all races were exposed to the 1873 Panic, while those exposed to the FSB collapse were almost exclusively Black. These results, however, suggest that to some extent, a shift toward insurance may be a natural response to banking crises generally, and that the larger effect observed in response to the FSB collapse is then reflecting both this effect and an effect related to the peculiarities of that specific bank's failure—one that was seen as a particular betrayal of a vulnerable community.

In Column 2, we add measures of the FSB shock and its interaction with severe treatment by the 1873 Panic. Here, we see that our main FSB effects on total insurance demand survive even after controlling for the 1873 Panic, and indeed are about twice the size of the 1873 Panic effect. Moreover, we see in Column 4, which estimates the results of this specification on Black agents per 1,000 Black households, that whereas the 1873 Panic has no significant effect on race-specific insurance demand, the FSB shock does. These results therefore suggest that there is a FSB-failure effect above and beyond an 1873-Panic effect, and that the former can be viewed as a race-specific shock in a way that the latter likely wasn't.

Table 6: Impact of 1873 Panic on Insurance

DV:	**Agents Per 1,000 HH**	**Agents Per 1,000 HH**	**Black Agents 1,000 Black HH**	**Black Agents 1,000 Black HH**
	(1)	(2)	(3)	(4)
Severe Panic × Post	0.897***	0.761***	0.117	0.0199
	(0.215)	(0.217)	(0.116)	(0.108)
FSB × Post		1.491***		0.389*
		(0.510)		(0.216)
Severe Panic × FSB × Post		-0.116		0.269
		(0.652)		(0.341)
Observations	8,352	8,352	8,352	8,352
R-squared	0.752	0.755	0.335	0.336
Fixed Effects	County, Year	County, Year	County, Year	County, Year
Trend	1870 Black Covariates	1870 Black Covariates	1870 Black Covariates	1870 Black Covariates
Cluster	County	County	County	County
Sample	South	South	South	South

Columns 1 and 3 are full-sample regressions of the outcome listed in the column header on *SeverePanic*, an indicator for an above-median value of 1873 Panic severity interacted with *Post*, an indicator for years subsequent to both the 1873 Panic and the FSB's 1874 failure, along with the noted fixed effects and time trends. Columns 2 and 4 add to this specification the interaction of on *FSB*, an indicator for Freedman's Savings Bank exposure, with *Post*, as well as a triple interaction between *SeverePanic*, *FSB*, and *Post*. The triple-interaction term captures the effect of FSB exposure above and beyond that of 1873 Panic exposure. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

7.3 Do Results Merely Reflect Broader Local Development Patterns?

Is it possible that our results on insurance demand are confounded by long-run trends in local economic development, including the growing size, sophistication, and diversity of the financial sector in affected regions? This might be a particular concern if FSB branches were located in areas with better growth prospects. If this were the case, we might expect that in FSB counties, other financial services, including banking, real estate, and securities, would be growing at the same rate as—or perhaps even faster than—insurance.[24]

To test this, in Table 7 we examine whether insurance's share of the financial-sector workforce is growing in response to the failure of the Freedman's Savings Bank.[25]

[24]The underlying assumption is that staffing across these types of services responds similarly to changes in demand.

[25]We find that the overall size of the financial-sector workforce, including non-insurance occupations, is growing over this period, particularly in FSB localities (not reported).

Table 7: Impact on Insurance's Share of Financial Employment

	DV: Share of Financial-Sector Workers in Insurance				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	5.404***	3.760**			5.765
	(1.388)	(1.549)			(6.069)
ATT			-2.011	7.582***	
			(3.344)	(1.526)	
Observations	8,352	8,352	8,352	8,352	8,352
R-squared	0.369	0.371			0.369
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South

Notes: The dependent variable is $PercentInsuranceIndustry$. Column 1 presents results from our standard two-way fixed-effects specification, and Column 2 adds 1870 Black county-level covariate trends. Column 3 presents results from our standard DRIMP specification. Column 4 presents results from our standard synthetic diff-in-diff specification. Column 5 presents results from our main IV specification, wherein the number of Black troops instruments for FSB locations. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

There, we find broadly consistent evidence of a statistically significant and roughly 4-7 percentage point increase in the share of insurance agents in the county's financial industry. Results from IV estimation are similarly large and positive, but statistically insignificant, while results from DRIMP estimation are modestly negative and statistically insignificant.

A related way to address concerns regarding endogenous branch placement and the potential conflation of FSB-specific effects with broader local economic development trajectories might be to look for FSB spillovers in places which did not themselves have an FSB branch, but were nearby to counties that did. While potentially sharing information or customer flows with FSB locations, these areas would not share the underlying fundamentals that made locations attractive for FSB branching and, therefore, the theory goes, potentially predisposed to higher insurance demand.

To explore this, we present in Table 8 binned estimates of insurance demand in FSB and nearby non-FSB counties (Column 1), and continuous estimates of insur-

ance demand among non-FSB counties within set radii of FSB counties (Columns 2-4). Column 1 suggests the existence of spillovers within roughly 15 miles of an FSB county, after which effects dissipate. This likely reflects some combination of information flows to neighboring counties regarding the FSB collapse (where word of the bank's failure may have changed local attitudes toward insurance and banking despite no direct prior engagement with the FSB) and the spatial displacement of financial activity (a non-trivial fraction of FSB depositors lived outside the county, and to the extent that some in neighboring counties held accounts at the FSB, their depository activity would have been captured in FSB-county figures, since banking in this period was done in-branch and would require travel to FSB counties; their insurance-related activity, however, would be reflected in their own county's figures, since insurance sales/collections were done door-to-door). Columns 2-4, which iteratively expand the distance window out from 25 miles, to 50 miles, to 100 miles out from FSB counties, similarly indicate that among non-FSB counties, post-collapse surges in insurance demand diminish with distance. Together, these results are suggestive of FSB engagement (whether through information or prior banking activity) among nearby locations, and of significant insurance demand effects even in plausibly-exposed locations that did not share potentially confounding determinants of FSB placement such as urban status.

7.4 Did the FSB Collapse Shape Beliefs and Preferences?

If persistently higher insurance demand is largely a function of the way that the failure of the Freedman's Savings Bank shaped the local economy—for instance, by wiping out wealth or by reducing the opportunities to bank—then the effects of FSB collapse ought to stay local. If, however, the FSB failure changed attitudes among those exposed, then FSB-induced demand for insurance is likely to travel with migrants.

Table 8: Effects by Distance to FSB Counties

	DV: Insurance Agents Per 1,000 Households			
	Full Sample	0-25 mi	0-50 mi	0-100 mi
	(1)	(2)	(3)	(4)
FSB × Post	1.172***			
	(0.390)			
0-15 mi × Post	1.225**			
	(0.526)			
15-30 mi × Post	0.0519			
	(0.111)			
Distance to FSB × Post		-0.122***	-0.0205***	-0.00187
		(0.0369)	(0.00739)	(0.00234)
Observations	8,579	702	2,862	6,183
R-squared	0.736	0.749	0.710	0.703
Fixed Effects	County, Year	County, Year	County, Year	County, Year
Trend	1870 Black Covariates	1870 Black Covariates	1870 Black Covariates	1870 Black Covariates
Cluster	County	County	County	County
Sample	South	South Excl FSB	South Excl FSB	South Excl FSB

Notes: Column 1 is a full-sample regression of $AgentsPerHH$ on FSB, an indicator for Freedman's Savings Bank exposure, interacted with *Post*, an indicator for years subsequent to the Bank's 1874 failure, along with the noted fixed effects and time trends. Column 1 also contains interactions between *Post* and indicators for non-FSB counties within 0-15 and 15-30 miles of an FSB county. Columns 2-4 present regressions interacting *Post*, an indicator for years subsequent to the Bank's 1874 failure, with the distance in miles from an FSB county, along with the noted fixed effects and time trends. These columns are restricted to non-FSB counties within 25, 50, and 100 miles of an FSB county, respectively. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

To distinguish place-based effects from effects embodied in people, we trace cohorts of migrants from FSB counties to non-FSB counties, and examine their impact on insurance demand in their new destinations. We hypothesize that higher demand for insurance in locations receiving larger concentrations of out-migrating FSB cohorts indicates that the FSB failure likely had a role in changing the tastes and preferences of exposed individuals, above and beyond any effects it may have had on the economic structure of FSB localities themselves.

To test this possibility, we shift our attention to states in the North and West of the US, i.e., outside of our main analytical sample. We calculate, for each county in this new, non-FSB sample, the extent of the FSB migrant presence there in a given year from 1880 to 1940.[26] FSB migrant counts are obtained by using IPUMS MLP linked census records. In our simplest classification, we identify as FSB-exposed any Black individual who lived in the index county in the index year, and lived in an FSB county for at least one Census year between 1870 and the index year. In alternate specifications, we expand and contract this definition to explore different mechanisms for preference transmission and persistence. For more on data and methods used in our migration analysis, see Appendix C.3.1.

7.4.1 Migration as a Transmission Mechanism: Own FSB Exposure

In Table 9, we estimate the impact of exposure to FSB migrants on insurance demand in the non-Southern US—i.e., a region that did not have FSB branches, and therefore could not have experienced FSB effects directly.[27] The exposure of a destination county to FSB migrants is alternately defined as the share of the local Black

[26] We cannot identify pre-collapse migrants (i.e., those who had migrated out by 1870 but who were found in an FSB county in a Census prior to 1870) because the overwhelming majority of the Southern Black population was enslaved until after the 1860 Census. Moreover, Black migration rates in the few years immediately following the conclusion of the Civil War were undoubtedly relatively low.

[27] We exclude New York, Pennsylvania, and Texas because although they are outside our main (Southern) sample, they had FSB branches and therefore do not present a clean group in which to test the influence of FSB migrants.

population that were FSB migrants (Columns 1, 2, 5, and 6), or the share of the local population of Black migrants from the South that were FSB migrants (where these Southern migrants are defined in a similar manner to FSB migrants; Columns 3, 4, 7, and 8). The rationale for the denominators in these definitions is to establish the influence of FSB migrants relative to that of a group that is otherwise very similar to them. For example, as in the case of the latter definition, to the extent that Black individuals, Southern individuals, or even migrants have higher baseline preferences for insurance, or choose similar destinations compared to people of other types, comparing Black FSB migrants to similar non-FSB Black Southern migrants allows us to better capture the effect attributable to FSB status specifically.

Column 1 indicates that in localities where the Black population had a larger share of FSB migrants, there was a statistically significant positive effect on the number of insurance agents per 1,000 households. An estimated effect size of 4.798 here implies that going from a location with no FSB migrants to a location with the mean FSB share of the Black population would result in 0.014 additional agents per 1,000 households.

Column 2 adds controls for the growth rates of the Black population, the white population, and the Southern-origin population in order to account for the possibility that regions that are growing rapidly, or that have growing sub-populations with a high baseline taste for insurance (in particular, sub-populations, like the Black population and the Southern population, that FSB migrants will mechanically inflate), may naturally see a rise over time in the local demand for insurance. The sign, magnitude, and significance are largely unchanged by the addition of these controls, whether in this or any of the other specifications (Columns 4, 6, and 8) where they are added.

Column 3, which presents the same specification as in Column 1, but with FSB exposure now defined as the share of the local population of Black Southern migrants that were FSB migrants, we see once again a significant and positive effect on in-

surance demand: 2.146 additional agents per 1,000 households, which at the mean value of the exposure variable is equivalent to roughly 0.07 additional agents per 1,000 households. Thus, it appears that even when comparing the impact of FSB migrants on their destinations relative to the impact of another group with similar characteristics and preferences, there is an increase in local insurance demand associated with their FSB status specifically.

To facilitate a more like-with-like comparison given that migrants typically tended to choose more urban destinations, in Columns 5-8, we restrict the sample to urban counties in the non-South. Like their analogous full-sample results, these results are also statistically significantly positive, and are very similar in magnitude.

In the Appendix, we estimate the specifications with population growth controls discussed above, but on race-specific outcomes including Black agents per 1,000 households and the share of insurance agents who were Black. Those results, provided in Table 18, underscore that what we document in this paper is a race-specific phenomenon: they indicate a consistent, positive, and statistically significant relationship between exposure to FSB migrants and Black-driven increases in local insurance demand outside the South.

We also estimate specifications subdividing FSB-exposed migrants into two mutually exclusive categories: first, those migrants who resided in an FSB county in the Census years flanking the bank's collapse, 1870 and 1880,[28] and second, those migrants that resided in an FSB county sometime after those two decades. We conceive of the first group as those who were there to observe the collapse first-hand, and the second group as those with place-based (or local-cultural-memory-based) experience, that is, indirect exposure to the event's aftershocks, including stories, warnings, and other relevant information that may have circulated in affected communities. Sep-

[28]Note that at present, we are unable to further subdivide this group into FSB depositors versus non-depositors. The relatively recent emergence of linkable FSB passbooks may allow us to explore this option, though we expect that the linkable number of individuals who were FSB depositors and who also migrated may be low enough to preclude a rigorous county-level analysis.

arating these groups allows us to shed light on the roles of both event salience and locally-held information effects in our results. The first set of these results are presented in Table 19, which estimates the impact of both types of exposure in the same regression. There we see that exposure to both "first-hand" and "place-based" FSB migrants predict statistically significantly higher local insurance demand, regardless of whether the implicit reference group is the Black population or the population of Black Southern migrants. The effects are almost uniformly statistically significant, and are always significant when using the latter denominator (we prefer this definition since it relies on a more like-with-like comparison to FSB migrants). In our preferred specifications (Columns 3 and 4), and based on the mean county's values, first-hand effect sizes are a little over three times the size of place-based effects. In Table 20, we estimate specifications 1-4 in Table 9, but including a control for the share of the local FSB migrant population with first-hand exposure. Consistent with the results in Table 19, our main results on migrant transmission survive, and a higher share of first-hand exposed is consistently positively (and often statistically significantly) associated with local insurance demand.

Together the results in Tables 9 and 18 strongly suggest that the FSB failure's impact on the demand for insurance is not simply a function of the way the bank's collapse changed the financial landscape in FSB counties. Instead, they point to a role for tastes and preferences formed in FSB counties in response to the bank's failure, and carried to new locations that had neither FSB experiences of their own nor substantial pre-existing insurance demand. Moreover, the results in 19 and 20 suggest a role for both first-hand exposure to the FSB's collapse as well as place-based exposure over the ensuing decades—the latter encompassing some combination of both continued exposure to the structural economic effects of the shock, and the stories and memories passed on through generations and enshrined in the lore of scarred locations.

Table 9: Impact of Migrants' Own FSB Exposure

	DV: Insurance Agents Per 1,000 Households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FSB Exposure (per Black Pop)	4.798***	4.617***			6.224**	5.906**		
	(1.407)	(1.348)			(2.965)	(2.902)		
FSB Exposure (per Southern Black Pop)			2.146***	2.101***			1.703***	1.668***
			(0.305)	(0.297)			(0.303)	(0.296)
Observations	8,655	8,655	8,655	8,655	4,475	4,475	4,475	4,475
R-squared	0.393	0.400	0.406	0.412	0.454	0.460	0.465	0.470
Fixed Effects	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	County	County	County	County	County	County	County	County
Sample	Non-South	Non-South	Non-South	Non-South	Urban N-S	Urban N-S	Urban N-S	Urban N-S

Notes: Each column is a separate regression with the dependent variable $AgentsPerHH$. The variable $FSBExposure(perBlackPop)$ measures the share of the destination-county Black population who were post-1870 Black migrants from the FSB counties in our main analytic sample. $FSBExposure(perSouthernBlackPop)$ is a similar measure, but the denominator is the destination-county Black population who are non-FSB post-1870 Black migrants from the South. Columns 2, 4, 6, and 8 control for Black, white, and Southern-origin population growth. All specifications are restricted to states without an FSB branch and to post-treatment years. Columns 5-8 are further restricted to urban counties. To aid in interpreting effect sizes, we present the following means: FSB Exposure (per Black Pop) Mean: 0.003; FSB Exposure (per Southern Black Pop) Mean: 0.0339. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

7.4.2 Sources of Intergenerational Persistence: Family FSB Exposure

The previous set of results suggest that information and preferences passed on from those who experienced the FSB failure may be important to explaining the spread and durability of the event's impact on financial behavior. To explore this mechanism further, in this section we test whether there is evidence that FSB exposure exclusively through family connections can also influence insurance demand.

Specifically, we define FSB migrants as above, but now focus on the destination-county impact of those individuals who were either co-resident with or descended from an FSB migrant, but who never themselves lived in an FSB county. This definition of FSB exposure is indirect, and posits that FSB experiences and related advice, information, and beliefs may be passed on through generations within the household.

We present these results in Table 10. In each case, we estimate the effect of direct (own FSB) and indirect (family FSB) exposure within the same regression in order to establish whether there is a family effect on local insurance demand above and beyond that related to the presence of people from FSB counties. In all cases, family exposure is positively associated with destination-county insurance agents per 1,000 households. In our preferred specifications, which look at the impact of FSB family

Table 10: Impact of Family FSB Exposure

	DV: Insurance Agents Per 1,000 Households			
	(1)	(2)	(3)	(4)
Family Exposure (per Black Pop)	0.572	0.488		
	(1.178)	(1.137)		
FSB Exposure (per Black Pop)	4.690***	4.525***		
	(1.386)	(1.328)		
Family Exposure (per Family of Southern Black Pop)			1.250***	1.207***
			(0.451)	(0.441)
FSB Exposure (per Southern Black Pop)			1.609***	1.586***
			(0.299)	(0.292)
Observations	8,655	8,655	8,655	8,655
R-squared	0.393	0.400	0.411	0.417
Fixed Effects	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	No	Yes	No	Yes
Cluster	County	County	County	County
Sample	Non-South	Non-South	Non-South	Non-South

Notes: Each column is a separate regression with the dependent variable $AgentsPerHH$. The variable $FSBExposure(perBlackPop)$ measures the share of the destination-county Black population who were post-1870 Black migrants from the FSB counties in our main analytic sample. $FSBExposure(perSouthernBlackPop)$ is a similar measure, but the denominator is the destination-county Black population who are non-FSB post-1870 Black migrants from the South. Family exposure refers to those who were not themselves FSB migrants, but who were co-resident with or descended from FSB migrants (or non-FSB Southern migrants in the case of the denominator in $FamilyExposure(perFamilyofSouthernBlackPop)$). Columns 2 and 4 control for Black, white, and Southern-origin population growth. All specifications are restricted to states without an FSB branch and to post-treatment years. To aid in interpreting effect sizes, we present the following means: Family Exposure (per Black Pop) Mean: 0.002; Family Exposure (per Southern Black Pop) Mean: 0.0285; FSB Exposure (per Black Pop) Mean: 0.003; FSB Exposure (per Southern Black Pop) Mean: 0.0339. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

exposure relative to that of the analogous Southern population (Columns 3 and 4), family exposure is also strongly statistically significant. At the mean values for these variables, the impact of exposure through FSB family members is very similar in magnitude to that of exposure through one's own FSB experiences.[29] These results suggest that the transmission of tastes and experiences to those who were putatively untreated by the shock may have been an important mechanism by which the shock persistently altered financial behavior, including intergenerationally. This result also accords with the findings in Chiteji & Stafford (1999), which have emphasized the importance of social learning in the intergenerational transmission of portfolio-choice preferences, particularly among Black households.

[29]When using exposure measures per Black population, the family exposure effect for the mean county is more like one tenth of the own-exposure effect.

7.4.3 Did FSB Migrant Tastes Spread to New Friends and Neighbors?: Exposure to Community Influencers

In this section, we provide further evidence that the the rise in insurance agents in response to FSB migrants was not solely to satisfy FSB migrants' own demand. Specifically, and building on our results regarding family transmission and social learning, we ask whether in fact there is evidence that FSB migrants also passed their experiences, tastes, and preferences on to non-FSB friends and neighbors in their new homes.

To do so, we identify individuals who would have had an outsize influence on their community's views and behavior. This is in part motivated by historical records indicating that distrust of banks and recommendations surrounding life insurance were communicated and reinforced by trusted figures in the Black community, such as pastors and local elders. We create two main classifications of influential community members, each meant to capture slightly different elements of information transmission. The first group is those in "social" occupations, by which we mean occupations which naturally would have brought these individuals into contact with many others in their community, and in interactions where information is commonly exchanged. This group includes social workers, boot blacks, bartenders, barbers, and non-clergy religious workers. The second group is those in "leader" occupations, by which we mean occupations associated with respect, wisdom, and authority. This group includes clergy, authors, journalists, lawyers, public administrators, and teachers. As in our earlier analysis, we identify members of these occupational groups who were also FSB migrants, and examine their impact on insurance demand in their new communities. We hypothesize that these individuals should have an impact on local financial behavior that is disproportionate to that induced by FSB migrants in other occupations because they not only have the same experiences as these migrants, having also been exposed to the FSB shock, but they also have a larger and more trusted

megaphone to influence the public with their views.

Table 11 presents these results. In Columns 1 and 2, we present results where exposure to these groups is defined as the number of FSB members of these occupations as a share of the overall number of people in those occupations in the destination county. That is, we can think of this measure as capturing how FSB-inflected these occupations are in a given county, or how likely one is to receive FSB messaging when interacting with people in these occupations. In Columns 3 and 4, we change these definitions to the number of FSB migrants in these occupations as a share of all FSB migrants. This formulation of exposure essentially asks if what matters is how influential the local FSB population is—i.e., how trusted are the FSB communications one receives. In Columns 5 and 6, we provide a simpler measure of exposure to influential FSB-origin community members, which is simply the size of the FSB-origin group in these occupations as a share of the locality's Black population. Across all specifications, the results are consistently positive and statistically significant: the larger the FSB-migrant share of these influential occupations, the larger these occupations' share of the local FSB-migrant population, and the larger their share in the overall Black population, the greater the number of insurance agents per 1,000 households.[30] As might be expected, the impact (based on the mean county) of those in leadership occupations is consistently 2-3 times larger than for those in social occupations.

Our results separating place from people effects provide compelling evidence that the Freedman's Savings Bank collapse scarred individuals rather than just localities, and in ways that persistently altered their financial behavior. Moreover, analysis of the influence of FSB migrants and their families suggests that rising insurance demand in migrant-receiving locations may reflect not only their mechanical presence—i.e., the arrival of groups with a taste for insurance—but also the influence of this group

[30]One might be concerned that if wealthier people buy more insurance, we are simply picking up the relative affluence of people in these occupational groups. While that might certainly be a consideration in the results in Columns 3-6, the specifications in Columns 1 and 2 implicitly control for this by drawing comparisons relative to other, non-FSB individuals in these professions.

Table 11: Impact of Exposure to FSB Community Influencers

	DV: Insurance Agents Per 1,000 Households					
	(1)	(2)	(3)	(4)	(5)	(6)
FSB's Share of Social Occs	3.207**	3.055**				
	(1.250)	(1.239)				
FSB's Share of Leader Occs	5.165***	4.996***				
	(0.945)	(0.932)				
Social Occs' Share of FSB			6.031***	5.926***		
			(1.471)	(1.472)		
Leader Occs' Share of FSB			4.074***	3.984***		
			(0.991)	(0.981)		
FSB Social Occs per Black Pop					205.4***	202.8***
					(54.11)	(54.41)
FSB Leader Occs per Black Pop					253.6**	248.3**
					(100.1)	(96.01)
Observations	8,655	8,655	8,655	8,655	8,655	8,655
R-squared	0.400	0.406	0.399	0.405	0.397	0.403
Fixed Effects	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	No	Yes	No	Yes	No	Yes
Cluster	County	County	County	County	County	County
Sample	Non-South	Non-South	Non-South	Non-South	Non-South	Non-South

Notes: Each column is a separate regression with the dependent variable $AgentsPerHH$. The right-hand-side variables are all given in rates, where the numerator is the number of Black people in either 'social occupations" or "leader occupations," respectively, who migrated from FSB counties after 1870. Social occupations include social workers, boot blacks, bartenders, barbers, and non-clergy religious workers. Leader occupations include clergy, authors, journalists, lawyers, public administrators, and teachers. In the first two rows, the denominator is the total Black population in those occupations in the destination county. In the next two rows, the denominator is the total Black post-1870 FSB migrant population in the destination county. In the final two rows, the denominator is the total Black population in the destination county. Columns 2, 4, and 6 control for Black, white, and Southern-origin population growth. All specifications are restricted to states without an FSB branch and to post-treatment years. To aid in interpreting effect sizes, we present the following means: FSB's Share of Social Occs Mean: 0.0021; FSB's Share of Leader Occs Mean: 0.0039; Social Occs' Share of FSB Mean: 0.0014; Leader Occs' Share of FSB Mean: 0.0034; FSB Social Occs per Black Pop Mean: 0.0000; FSB Leader Occs per Black Pop Mean: 0.0000. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

and their experiences on the broader non-FSB communities of which they became a part. Durability in these patterns is further explained by the transmission of these preferences to descendants of affected individuals, irrespective of their location.

8 Conclusion

The collapse of the Freedman's Savings Bank was one of the most catastrophic bank failures in the history of the United States (Celerier & Tak, 2021). Instituted as a philanthropic organization and chartered to provide a safe savings bank for the recently-Emancipated in the immediate aftermath of the Civil War, fraudulent conduct by the bank's white management engendered the conditions for the bank's failure, resulting in the destruction of roughly half of Black wealth at the time (Baradaran, 2017). We document the effects of this racially exploitative and culturally scarring event on the financial behavior of Black Americans throughout the late 19th and early 20th

centuries.

Specifically, we study the effect of exposure to the failure of the Freedman's Savings Bank on Black households' life insurance holdings, which as a savings vehicle was a historically popular alternative to banking. Using a difference-in-difference framework to estimate the effect of the bank's failure on the local demand for insurance, we find that the Freedman's Savings Bank collapse accounts for 1-6 additional agents per thousand households in treated counties—an effect that is lasting, statistically significant, economically meaningful, and robust to a battery of causal estimation techniques. Critically, we find that at minimum 13-20% of this effect is driven exclusively by Black households, underscoring the racialized nature of this shock.

We also provide evidence identifying changing beliefs and preferences as a key mechanism behind the shifting behavior we document. In particular, we use the movement of FSB-exposed migrants into regions without organic FSB exposure to distinguish between the shock's effects on local economic structure, which were confined to FSB localities, and its effects on the tastes and attitudes of prospective savers, which traveled with exposed cohorts. These results show that the effects of the Freedman's Savings Bank's failure persisted across time and space through both intergenerational and community transmission of culture and beliefs.

The Freedman's Savings Bank was explicitly conceived as a teaching tool for a financially inexperienced and vulnerable group of Americans. Our paper shows that through the bank's exploitative mismanagement, its failure seems to have taught some lasting lessons of its own. Taken as a whole, our results suggest that the collapse of the Freedman's Savings Bank durably altered financial beliefs and behavior—not just among those directly affected, but also among their friends, neighbors, and descendants. These findings not only may help explain the historical prevalence of life insurance as a savings strategy among Black households, but also, to the extent that these results help explain the origins of systematic racial differences in portfolio composition, they may also have significance to ongoing debates surrounding the

present-day racial disparities. To wit, recent literature has implicated racial differences in portfolios—in particular, the concentration of Black wealth in lower-risk, lower-return asset classes—as an important contributor to persistent racial wealth gaps (Derenoncourt *et al.*, 2022; Kuhn *et al.*, 2020). In this context, the failure of the Freedman's Savings Bank may cast a longer shadow on the prospect of shared American prosperity—and its scars may be even farther-reaching—than previously understood.

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A Appendix: Empirical Setting

B Appendix: Data

Figure 1: Insurance Agents per 1,000 Households, 1870

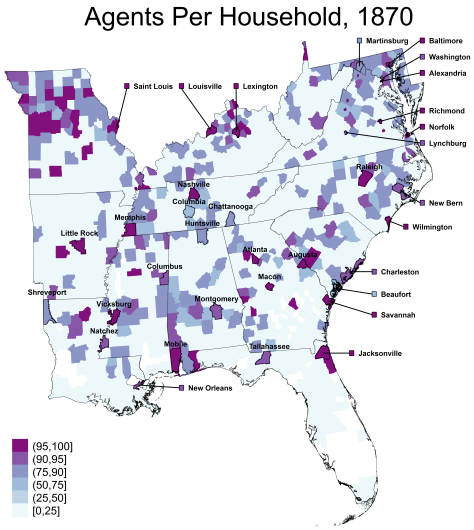
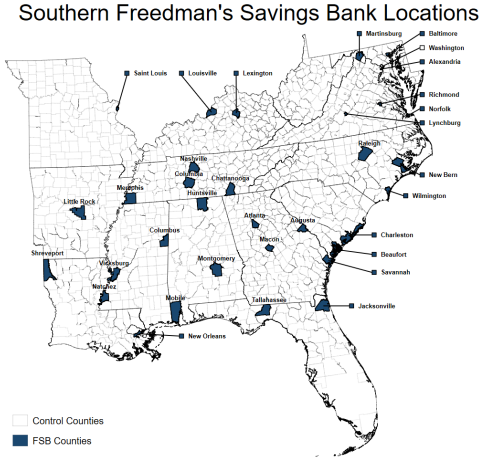


Figure 2: Freedman's Savings Bank Counties

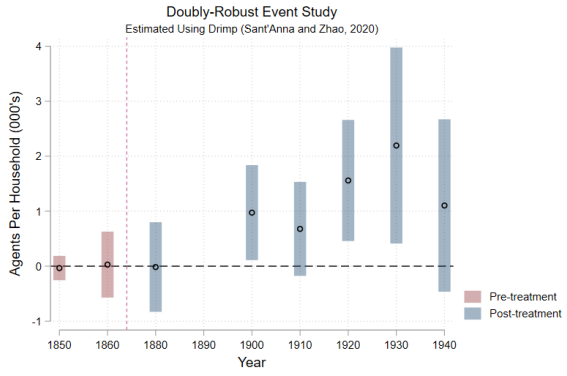


C Appendix: Results

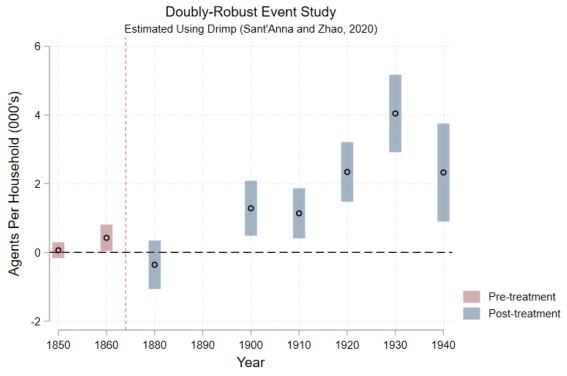
C.1 Main Results

C.1.1 Additional Evidence: Doubly-Robust Methods

Figure 3: DRIMP Event Studies

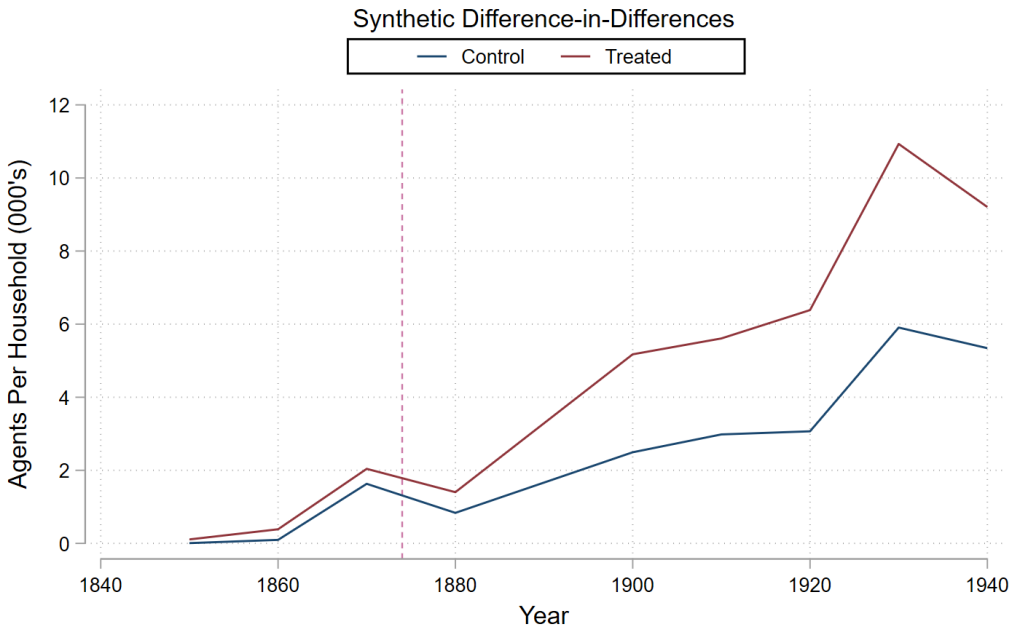


1870 White & Black Covariates



1870 Black Covariates

Figure 4: Synthetic Differences-in-Differences



C.1.2 Additional Evidence: Instrumental Variables

Table 12: Instrumental Variables: First-Stage Results

	DV: FSB × Post			
	(1)	(2)	(3)	(4)
Union Troops × Post		3.88e-05***		3.11e-05***
		(6.98e-06)		(7.40e-06)
Black Troops × Post	0.000157***	0.000114**		
	(4.34e-05)	(5.33e-05)		
Black Troops × Union Troops × Post		-3.61e-09		
		(1.17e-08)		
Contraband Camps × Post			0.0729***	0.0313
			(0.0207)	(0.0197)
Contraband Camps × Union Troops × Post				1.11e-05***
				(3.84e-06)
Observations	8,352	8,352	8,352	8,3522
R-squared	0.730	0.770	0.718	0.783
Fixed Effects	County, Year	County, Year	County, Year	County, Year
Cluster	County	County	County	County
F-Statistic	13.010	22.038	12.378	48.532

Notes: Each column presents first-stage results underlying the results reported in the main instrumental variable regression table. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

C.2 Robustness and Mechanisms

C.2.1 Urban Status

Table 13: Accounting for Urban Status

	DV: Insurance Agents Per 1,000 Households		
	Baseline	Urban Control	Above Med Urban
	(1)	(2)	(3)
FSB × Post	3.952***	3.232***	2.401***
	(0.356)	(0.368)	(0.367)
Urban		7.084***	
		(0.295)	
Observations	8,352	8,352	2,286
R-squared	0.713	0.788	0.821
Fixed Effects	County, Year	County, Year	County, Year
Trend	No	No	No
Cluster	County	County	County
Sample	South	South	Urban South

Notes: The dependent variable is $AgentsPerHH$, and is regressed on FSB, an indicator for Freedman's Savings Bank exposure, interacted with $Post$, an indicator for years subsequent to the Banks 1874 failure, along with the noted controls and fixed effects. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. Column 3 is restricted to urban counties in these states. *** p<0.01, ** p<0.05, * p<0.1

C.2.2 Pent-Up Demand

Table 14: Accounting for Pent-Up Demand

	DV: Insurance Agents Per 1,000 Households							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percent Enslaved × Post-CW	-0.0208				-0.624**			
	(0.0808)				(0.257)			
Percent (Free) Black		-0.476				-0.646		
		(0.323)				(0.831)		
FSB × Post			2.260***	0.780**			2.075***	1.031**
			(0.478)	(0.383)			(0.479)	(0.454)
Observations	3,135	3,422	1,160	1,160	872	1,618	1,025	1,025
R-squared	0.455	0.607	0.841	0.881	0.536	0.609	0.861	0.889
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	No	No	1870 B	No	No	No	1870 B
Cluster	County	County	County	County	County	County	County	County
Sample	South	US	NY & PA	NY & PA	Urban South	Urban US	Urban NY & PA	Urban NY & PA
Years	1850-1870	1850-1860	1850-1940	1850-1940	1850-1870	1850-1860	1850-1940	1850-1940

Notes: Each column is a separate regression with dependent variable *AgentsPerHH*. Explanatory variables, samples, and year ranges vary across specifications. *** p<0.01, ** p<0.05, * p<0.1

C.2.3 Pre-Period Checks

Table 15: Restricting Pre- Period to 1870

	DV: Insurance Agents Per 1,000 Households				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	2.914***	0.880**			3.964***
	(0.321)	(0.343)			(0.772)
ATT			1.791***	2.914***	
			(0.368)	(0.323)	
Observations	7,826	7,826	7,826	7,826	7,826
R-squared	0.738	0.764			0.737
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South
Years	1870-1940	1870-1940	1870-1940	1870-1940	1870-1940

Notes: This table reproduces the main results using each of the core methods shown, but with the pre-collapse period restricted to 1870. DRIMP methods use 1870 Black covariates. *** p<0.01, ** p<0.05, * p<0.1

C.2.4 Race-Specific Demand

Table 16: Effects on Black Insurance Agents per 1,000 Households

	DV: Black Insurance Agents Per 1,000 Households				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	0.523***	0.276***			0.777***
	(0.0749)	(0.0769)			(0.164)
ATT			0.280***	0.523***	
			(0.0802)	(0.0828)	
Observations	8,352	8,352	8,352	8,352	8,352
R-squared	0.402	0.460			0.396
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South

Notes: The dependent variable is *BlackAgentsPerHousehold*. Column 1 presents results from our standard two-way fixed-effects specification, and Column 2 adds 1870 Black county-level covariate trends. Column 3 presents results from our standard DRIMP specification based on 1870 Black covariates. Column 4 presents results from our standard synthetic diff-in-diff specification. Column 5 presents results from our main IV specification, wherein the number of Black troops instruments for FSB locations. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

Table 17: Effects on Share of Insurance Agents Black

	DV: Share of Insurance Agents Black				
	TWFE	TWFE 70B	DRIMP	SDID	IV Black Troops
	(1)	(2)	(3)	(4)	(5)
FSB × Post	4.316***	0.481			10.00***
	(0.912)	(0.819)			(3.047)
ATT			-0.894	4.183***	
			(0.920)	(0.860)	
Observations	8,352	8,352	8,352	8,352	8,352
R-squared	0.267	0.315			0.264
Fixed Effects	County, Year	County, Year	County, Year	County, Year	County, Year
Trend	No	1870 Black Covariates	No	No	No
Cluster	County	County	County	County	County
Sample	South	South	South	South	South

Notes: The dependent variable is *PercentBlackAgents*. Column 1 presents results from our standard two-way fixed-effects specification, and Column 2 adds 1870 Black county-level covariate trends. Column 3 presents results from our standard DRIMP specification based on 1870 Black covariates. Column 4 presents results from our standard synthetic diff-in-diff specification. Column 5 presents results from our main IV specification, wherein the number of Black troops instruments for FSB locations. All models are restricted to primarily Southern states, including Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Missouri, North Carolina, South Carolina, Tennessee, Washington D.C., and West Virginia. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

C.3 Migration Analysis

C.3.1 Migration Data and Methods

We follow FSB-exposed Southern migrants over time and space by using the IPUMS Multigenerational Longitudinal Panel, which links individuals across censuses (Ruggles *et al.*, 2019). Starting in 1870, we assign exposure to the FSB collapse to migrants if these migrants have ever lived in a county containing an FSB branch, and consider individuals directly exposed if they lived in an FSB county in either 1870 or 1880. This approach allows us to compare the scarring effects of direct exposure to the FSB collapse with intergenerational indirect effects. To further probe intergenerational indirect exposure, we track family members of FSB migrants who never resided in an FSB county.

For this analysis, we drop states that ever had an FSB branch, and instead measure county-level exposure to migrants from FSB counties. We measure relative exposure

to the FSB by dividing the number of FSB migrants who ever lived in a FSB county by the total Black population, total Southern Black migrant population, total Southern Black Urban migrant population, and total Black out-of-state migrant population. Although these variables capture different measures of FSB exposure, an increase in these measures is associated with an increase in the recipient county's exposure to the FSB. We calculate our dependent variables, as described above, using this linked-subset of the complete-county census data.

Lastly, we test the role of social networks and community leaders in spreading information about the FSB to unexposed Black communities. Specifically, we classify individuals as plausible social and leadership connections through which information spreads. To facilitate this analysis, we assign individuals as a social or leadership connection using occupations as recorded in the census. We classify individuals as social connections if they work as social workers, boot blacks, bar tenders, barbers, or non-clergy religious, and as leadership connections if they work as clergy, authors, journalists, lawyers, public administrators, and teachers. Individuals in these occupations engaged with a large number of individuals every day. Accordingly, we expect FSB-exposed individuals in these occupations to disseminate negative information about the FSB to their communities, thereby horizontally transmitting distrust in the banking system.

With these data in hand, we employ a TWFE estimator to estimate the effects of migrants from FSB counties on insurance holdings. We estimate the following equation:

$$AgentsPerHH_{ct} = \alpha + \beta FSBMig_{ct} + \gamma_s + \lambda_t + \epsilon \tag{2}$$

Where $AgentsPerHH$ is agents per 1,000 households in county c and year t, and $FSBMig$ is our measure of county-level exposure to FSB migrants. Our baseline measure of $FSBMig$ is the fraction of a county's Black population that migrated from

an FSB county. We control for state and year fixed effects, and in some specifications further control for white, Black, and southern population growth.

Our sample is restricted to states that never contained an FSB branch such that we measure insurance holdings in destination counties of the Great Migration. We further restrict our sample to the years after the FSB's 1874 failure such that all FSB migrants are exposed to the Bank's failure. A causal effect of FSB migrants on insurance holdings is identified if a migrant's choice of destination counties is uncorrelated with the destination's pre-existing preferences for insurance holdings.

C.3.2 Additional Migration Results

Table 18: Impact of Migrants' Own FSB Exposure on Race-Specific Demand

DV:	**Black Agents per 1,000 Black HH**				**Share of Insurance Agents Black**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FSB Exposure (per Black Pop)	0.954***		2.069**		0.244*		0.510*	
	(0.361)		(0.993)		(0.133)		(0.270)	
FSB Exposure (per Southern Black Pop)		0.214*		0.260**		0.0639*		0.0414
		(0.111)		(0.107)		(0.0357)		(0.0279)
Observations	8,655	8,655	4,475	4,475	8,655	8,655	4,475	4,475
R-squared	0.003	0.003	0.032	0.032	0.006	0.006	0.028	0.028
Fixed Effects	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	County	County	County	County	County	County	County	County
Sample	Non-South	Urban N-S	Non-South	Urban N-S	Non-South	Urban N-S	Non-South	Urban N-S

Notes: Each column is a separate regression with the dependent variable listed in the column header. The variable $FSBExposure(perBlackPop)$ measures the share of the destination-county Black population who were post-1870 Black migrants from the FSB counties in our main analytic sample. $FSBExposure(perSouthernBlackPop)$ is a similar measure, but the denominator is the destination-county Black population who are non-FSB post-1870 Black migrants from the South. All specifications control for Black, white, and Southern-origin population growth, and are restricted to states without an FSB branch and to post-treatment years. Columns 2, 4, 6, and 8 are further restricted to urban counties. To aid in interpreting effect sizes, we present the following means: FSB Exposure (per Black Pop) Mean: 0.003; FSB Exposure (per Southern Black Pop) Mean: 0.0339. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

Table 19: Impact of Migrants: First-hand vs Place-based Exposure

	DV: Insurance Agents Per 1,000 Households			
	(1)	(2)	(3)	(4)
FSB First-hand Exposure (per Black Pop)	4.832	4.780		
	(4.482)	(4.457)		
FSB Place-based Exposure (per Black Pop)	4.796***	4.606***		
	(1.583)	(1.520)		
FSB First-hand Exposure (per Southern Black Pop)			2.097***	1.997***
			(0.420)	(0.404)
FSB Place-based Exposure (per Southern Black Pop)			1.187***	1.200***
			(0.321)	(0.323)
Observations	8,655	8,655	8,655	8,655
R-squared	0.393	0.400	0.398	0.404
Fixed Effects	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	No	Yes	No	Yes
Cluster	County	County	County	County
Sample	Non-South	Non-South	Non-South	Non-South

Notes: Each column is a separate regression with the dependent variable $AgentsPerHH$. The variable $FSBFirst-handExposure(perBlackPop)$ measures the share of the destination-county Black population who were Black migrants from the FSB counties in our main analytic sample and who were observed in FSB counties in either/both Census years bookending the FSB failure, i.e., 1870 and/or 1880. $FSBFirst-handExposure(perSouthernBlackPop)$ is a similar measure, but the denominator is the destination-county Black population who are non-FSB post-1870 Black migrants from the South, observed in a non-FSB Southern county in 1870 and/or 1880. $FSBPlace-basedExposure$ refers to Black migrants from FSB counties who were observed in an FSB county no earlier than 1900, and the denominator for $FSBPlace-basedExposure(perSouthernBlackPop)$ is defined correspondingly. Columns 2 and 4 control for Black, white, and Southern-origin population growth. All specifications are restricted to states without an FSB branch and to post-treatment years. To aid in interpreting effect sizes, we present the following means: FSB First-hand Exposure (per Black Pop) Mean: 0.0005; FSB Place-based Exposure (per Black Pop) Mean: 0.0026; FSB First-hand Exposure (per Southern Black Pop) Mean: 0.0132; FSB Place-based Exposure (per Southern Black Pop) Mean: 0.0072. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1

Table 20: Impact of Migrants: First-hand Exposure Controls

	DV: Insurance Agents Per 1,000 Households			
	(1)	(2)	(3)	(4)
Share of FSB Exposure First-hand	0.907***	0.879***	0.240	0.224
	(0.221)	(0.217)	(0.221)	(0.217)
FSB Exposure (per Black Pop)	4.242***	4.079***		
	(1.307)	(1.256)		
FSB Exposure (per Southern Black Pop)			2.075***	2.036***
			(0.308)	(0.300)
Observations	8,655	8,655	8,655	8,655
R-squared	0.395	0.402	0.406	0.412
Fixed Effects	State, Year	State, Year	State, Year	State, Year
Pop Growth Controls	No	Yes	No	Yes
Cluster	County	County	County	County
Sample	Non-South	Non-South	Non-South	Non-South

Notes: Each column is a separate regression with the dependent variable $AgentsPerHH$. $FSBExposure(perBlackPop)$ measures the share of the destination-county Black population who were post-1870 Black migrants from the FSB counties in our main analytic sample. $FSBExposure(perSouthernBlackPop)$ is a similar measure, but the denominator is the destination-county Black population who are non-FSB post-1870 Black migrants from the South. The variable $ShareofFSBExposureFirst-hand$ measures the number of the destination-county Black population who were Black migrants from the FSB counties in our main analytic sample and who were observed in FSB counties in either/both Census years bookending the FSB failure, i.e., 1870 and/or 1880, divided by the total number of post-1870 Black FSB migrants. Columns 2 and 4 control for Black, white, and Southern-origin population growth. All specifications are restricted to states without an FSB branch and to post-treatment years. To aid in interpreting effect sizes, we present the following means: FSB First-hand Exposure (per Black Pop) Mean: 0.0005; FSB Place-based Exposure (per Black Pop) Mean: 0.0026; FSB First-hand Exposure (per Southern Black Pop) Mean: 0.0132; FSB Place-based Exposure (per Southern Black Pop) Mean: 0.0072. Standard errors are robust and clustered by county. *** p<0.01, ** p<0.05, * p<0.1