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ABSTRACT

Using General William Sherman's 1864–65 military march through Georgia, South Carolina, and North Carolina during the American Civil War, this paper studies the effect of capital destruction on medium and long-run local economic activity, and the role of financial markets in the recovery process. We match an 1865 US War Department map of Sherman's march to county-level demographic, agricultural, and manufacturing data from the 1850-1920 US Censuses. We show that the capital destruction induced by the March led to a large contraction in agricultural investment, farming asset prices, and manufacturing activity. Elements of the decline in agriculture persisted through 1920. Using information on local banks and access to credit, we argue that the underdevelopment of financial markets played a role in weakening the recovery.

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1 Introduction

Conflict and other disasters have caused destruction and economic mayhem throughout human history, but the study of their effects on growth remains limited (Skidmore and Toya 2002). Furthermore, little work has examined the economic factors that may affect the recovery process, specifically the role of credit markets. In a standard neoclassical growth model, a temporary shock to capital should be followed by rapid growth, bringing the economy back to the original steady state quickly, with no consequences in the medium run. Evidence in this direction has been presented by several papers, all examining the effects of wartime destruction in the twentieth century (Ikle 1952; Davis and Weinstein 2002; Miguel and Roland 2011). In this paper, we study the long- and medium-run effects of capital destruction on local economies using General William Sherman's 1864-65 military march during the American Civil War as a shock to local capital. We estimate that capital destruction had large negative effects on both the agricultural and manufacturing sectors, with some agricultural effects persisting until 1920 because of changes to agricultural institutions. More, we also find evidence that the underdevelopment of credit markets in the postbellum period played a key role in weakening the recovery process.

Writing to Commander Henry Halleck on Christmas Eve 1864 from freshly conquered Savannah, Georgia, Union General William Sherman documented how he and his men were fighting "not only ... armies, but a hostile people" (US War Department 1901). Their effort required "mak[ing] old and young, rich and poor, feel the hard hand of war," particularly in a region of the South that had not hosted any major fighting (Carr 2015, p. 134). For more than a month, Sherman marched his men 300 miles through the heart of the Confederacy to "enforce devastation" on the seceded states of Georgia, South Carolina, and North Carolina. His military "destroy[ed] mills, houses, cotton-gins, & c.," burned railroads and telegraph lines, and confiscated over 5,000 horses, 4,000 mules, 13,000 cattle, 10.5 million pounds of corn, and 9.5 million pounds of fodder (US War Department 1901; Lee 1995). They were "aveng[ing] the national wrong [Southerners had committed by] dragging [the] country into civil war" (US War Department 1901).

How realistic was Sherman's postwar boast that his march inflicted large damage—$100 million in postbellumm dollars, he claimed—to economic infrastructure (Trudeau 2008, p. 539)? And how long did the damage persist? To credibly identify Sherman's effect, we exploit his path through

the South. We georeference an 1865 US War Department map of Sherman's march and match it to detailed county-level demographic, agricultural, and manufacturing data from US censuses, 1850--1920. As one Georgia planter put it: "I had the misfortune to be in the line of Sherman's march, and lost everything—Devon cows, Merino sheep, Chester hogs, Shanghai chickens, and in fact everything but my land, my wife and children and the clothing we had at that time" (Fite 1984, p. 1). Our strategy is to compare outcomes in misfortunate counties with geographically close and economically and demographically similar counties in the same states outside the path of destruction, before and after the Civil War.[1] This allows us to difference out the overall effects of the war as well as the shifts in economic conditions across states.[2] We show that Sherman counties look relatively similar to non-Sherman counties in the same state among several dimensions before the Civil War, limiting concerns of pretreatment differences or heterogeneity between affected and unaffected counties. Further, we show throughout that that controlling non-parametrically for a variety of 1860 county-characteristics has a limited impact on the results.

We show that Sherman effectively devastated these counties' postwar economies. In the agricultural sector, we observe a large decline in farming economic activity, measured by farming asset values. These results are significant both statistically and economically. For instance, by 1870, the value of march-county farms was 20% lower than the value of non-march-county farms.[3] Consistent with this result, we also found that investments in farm land—measured by improved land—declined following the march. Nor were these relative economic declines confined to agriculture—at least in the medium-term. The 1860–1870 growth rates of manufacturing output, capital, employment, and number of firms were also lower in march counties than in non-march counties. This confirms that local entrepreneurial activity and investments were highly affected by capital destruction.

Our results pass a series of robustness tests, strengthening our causal interpretation. We show

[1]Throughout the paper, we refer to march and non-march counties. We define march counties as those within 5 miles on either side of Sherman's march lines, that is, within a 10-mile buffer of the march lines. Non-march counties are those outside of this 10-mile area but within 100 miles of the march lines. Because our specification includes state-by-year fixed-effects, we exclude the few counties outside the three march states of Georgia, North Carolina, and South Carolina that are within the non-march zone, including counties in Alabama, Tennessee, and Virginia. We show that our results are robust to alternative buffer distances. We also exclude the vertex cities on the march from our analysis: Atlanta (captured September 2, 1864), Savannah, GA (December 10, 1864), Columbia, SC (February 17, 1865), and Goldsboro, NC (March 23, 1865).

[2]Examples of these aggregate shocks include the changes in global cotton demand and the postwar reduction in labor supply among newly freed African Americans. Wright (1986) discusses the postwar reduction in world cotton demand. Ransom and Sutch (2001) investigate the reduction in labor supply among newly freed African Americans.

[3]Our findings echo diaries from along Sherman's path. According to Rubin (2014, p. 20), one Sandersville, GA resident lamented that people "struggled with deprivation, coping with the lack of livestock and supplies" for years.

that these results are not driven by differential trends across treatment in the prewar period. We also implement an instrumental variable estimator to tackle the potential endogeneity of Sherman's path. Lastly, we estimate a placebo test using plausible alternative march routes not taken.[4] Our robustness results are discussed in detail in Section 5.

Postwar underdevelopment in credit markets played an important role in explaining the extent of the relative effects and the delay in recovery. Credit from the banking sector completely dried up after the Civil War: we document that nearly every bank and branch in Georgia, North Carolina, and South Carolina shut down right after the war. This lack of credit may have interfered with the recovery process, particularly in counties hit by Sherman. We provide three main pieces of evidence in favor of this hypothesis, using demographic data, information on local infrastructure (such as railroads), and newly digitized data on local bank and Southern credit-market conditions. First, we show that potential alternative mechanisms—a demographic shift or the lack of infrastructure—do not explain the large effects of the march, even in the decade after the Civil War. Contrary to some common beliefs about the effects of Sherman's destruction, we find that the march did not significantly affect the demographic patterns in affected counties. Neither whites nor African Americans fled march counties at faster rates than they left non-march counties. In addition, infrastructure access and investment, as measured by the number of county railroad miles in each decade from 1850–1920, was not differential across march and non-march counties.[5]

Second, we find that the lack of provision of credit made the immediate recovery more difficult in the manufacturing sector. In particular, counties that were more dependent on credit before the war experienced slower postwar manufacturing recovery following the march. Since formal credit markets were deeply impaired by the war, we measure dependence by looking at those counties where companies could have more easily accessed financing in the prewar period. Specifically, we look at counties located closer to a bank branch or where lending markets were particularly active, proxied with the number of firms tracked in the county by Dun, Boyd, & Company, a credit-ratings agency (Brennecke 2016; González, Marshall, and Naidu 2017). For these counties, we find much

[4]Our baseline specification already controls for county fixed effects—which control flexibly for time-invariant county characteristics—and state-by-year fixed effects, absorbing any time-varying shocks common to all counties within a state. We also augment this specification with a series of pre-shock controls interacted with time dummies, which condition our parameters to heterogeneity in observables across treatment in the prewar period.

[5]Our results suggest that while Sherman was very successful at destroying railroads and telegraph lines along the path of the march, that destruction was short lived. As we describe later in the paper, railroads were mostly rebuilt by 1870 and telegraph service had been restored before the end of the war.

larger effects of Sherman on manufacturing activity, suggesting that credit drying up exacerbated the recovery for businesses that were more dependent on it.[6] We also show that the negative effects of the march were much larger for industries more dependent on external finance (Rajan and Zingales 1998). This set of results underscores the importance of finance in the recovery process from a capital shock in the manufacturing sector.

Third, we also provide evidence on the importance of credit-market frictions in the recovery of the agricultural sector. As is well known, the antebellum banking sector did not serve rural and agricultural needs, especially in the South (Fite 1984; Jaremski and Rousseau 2013; Koudijs and Salisbury 2016; Nier 2007). Instead, many small farmers relied on country stores (Atherton 1949; McCurry 1997; Sparks 1932; Bremer 2011) and local wealthy elites for financing between harvests (Rajan and Ramcharan 2011; Jaremski and Fishback 2018; Martin 2016; McCurry 1997). Consistent with this narrative, we find smaller effects of Sherman's march in counties with a larger share of wealthy individuals. At the same time, we also find smaller effects in counties in which country stores—an important source of financing for small and poorer farmers—were located before the war.

Before we conclude, we examine the persistence of the effects of the march. Overall, we find that the contraction for manufacturing was short-lived: as soon as 1880, we see no effect on manufacturing. The story for agriculture is quite different as we observe that some elements of the decline persisted at least until 1920. While the value of farms and livestock activity recovered within twenty years from the march, improved land remained lower in Sherman counties until 1920. We draw two main conclusions from these mixed results. First, while the initial shock may have been similar across sectors, recovery may depend on the context or external conditions. In our case, the faster recovery of manufacturing may be related to the overall booming in the manufacturing sector experienced in the United States during the post-war period. Second, even after a recovery in prices—the value of farms and of livestock— the response to the shock may cause changes in the institutions or organization of economic activity that persist even longer. In the case of post-bellum Georgia, North Carolina, and South Carolina, that new institution was sharecropping and tenant farming. In particular, we document that the areas affected by the shock saw a shift in the

[6]While this credit mechanism does not require a direct negative effect of Sherman's march on banks, it would be reinforced by one. As we discuss later in the paper, we suspect that Sherman's march did have a negative effect on the banking sector. However, we consider this to be part of the overall treatment. We test the credit mechanism using only potential treatment intensity, based on preexisting bank or credit access rather than the post-treatment declines. However, we show results are similar when using the change in banks as treatment.

size of farms that is consistent with a rise in tenancy in response to capital destruction. While tenancy and sharecropping—land and labor institutions that addressed the financing problems of credit-constrained Southern landowners and newly freed labor—were common throughout our study states (Fite 1984; Ransom and Sutch 2001; Alston and Higgs 1982), our evidence suggests that the capital destruction may have accelerated the shift toward tenancy, and more tenancy explains the more permanent changes in agricultural investment and land allocation in these regions, as predicted by Jaynes (1986, p. 50).

With this paper, we contribute to three related literatures. The first examines the effects of capital and infrastructure destruction on economic local activity.[7] Ikle (1952) studied this in the context of the Allied bombing of German cities during World War II and found that bombed cities rebuilt fairly quickly after the war. Davis and Weinstein (2002) similarly showed rapid postwar reconstruction of Japanese cities destroyed during WWII. Miguel and Roland (2011) found that Vietnamese districts severely damaged by American bombing in the late 1960s and early 1970s also rebuilt shortly after fighting ceased. We contribute to the literature in several ways. We are the first among these papers to examine how capital destruction affects business development across different sectors.[8] Many of the previous papers focus on population and other general demographic variables, which may fail to capture important economic dimensions of the costs of capital destruction. Consistent with this hypothesis, our paper finds essentially no effect on population, but very large negative effects in both the agricultural and manufacturing sector after the crisis. Further, our analysis on the longer-run effects of the shock also provides two novel insights. First, we highlight the importance of the economic context to assess the actual effect of a shock. As we show, the reversal was quicker in manufacturing, an economic area that—unlike postbellum agriculture—was experiencing high growth overall. Second, we discuss the importance of understanding how capital shocks can change the way economic activity is organized. As we show in the paper, this institutional shift may persist over the long-run and, consistent with the literature on the role of institutions on growth (Dell 2010; Acemoglu, Johnson, and Robinson 2001), have long-run effects on growth and

[7] In examining the effects of a large, destructive capital shock, this paper is also relevant to the literature on the effectiveness of post-shock reconstruction plans. Recent examples in this area include the impact of the Marshall Plan in Italy after WWII (Bianchi and Giorcelli 2018; Giorcelli 2018) and post-Katrina rebuilding plans (e.g. Gregory 2017).

[8] In a recent working paper, Bianchi and Giorcelli (2018) study the effects of Allied bombing in Italy and also separate effects across sectors, showing variation within agriculture depending on crop perishability.

development.

Second, our paper provides novel evidence on the importance of finance for growth by showing that the underdevelopment in financial markets may significantly affect the way an economy recovers from a large shock to capital. Across manufacturing and agriculture, we find that the lack of financing appeared to have detrimental effects on the ability to recover quickly from the shock. Given the frequency of disasters around the world, understanding the factors that influence the recovery process is crucial (Skidmore and Toya 2002). Previous research has shown that there is a strong relationship between local economic activity and the condition of the financial sector (e.g. Rajan and Zingales 1998, Petersen and Rajan 2002, Guiso, Sapienza, and Zingales 2004, Gilje 2017, and more recently in a historical context Carlson, Correia, and Luck 2019, Ziebarth 2013 and Lee and Mezzanotti 2014). We provide direct evidence on how the condition of financial markets may also shape the recovery from a shock. On the connection between finance and destruction, the closest paper to us is Cortés and Strahan (2017), studying the role of multi-market banks in reallocating credit towards affected area after Hurricane Katrina.[9]

Finally, our study provides evidence of the importance of capital destruction as a new channel to understand the effects of the Civil War on the postbellum South, a classic and long-debated question in economic history. Goldin and Lewis (1975) estimated that the former Confederate states had a lower per capita income than other states and that income in the South fell after the war.[10] Wages fell in the South relative to the North after the war (Margo 2002), as did output per worker in manufacturing, driven in large part by declines in relative capital intensity (Hutchinson and Margo 2006). Goldin (1979) argues that there were three main reasons for the decline in Southern per capita income after the War—loss of economies of scale, decline in relative demand for southern cotton, and a decrease in ex-slave labor force participation—generally dismissing capital destruction as a factor.[11] If capital destruction played any role in the stagnation of the postbellum Southern

[9]Both our credit channel mechanisms and our long run effect of Sherman's March through agricultural institutional change, also complement more recent work in economic history, that studies how emancipation affected the Southern financial system. Slave wealth was a frequent source of collateral (Martin 2010, 2016) and emancipation both eliminated slaves as an asset and made any lending-management practices backed by slave finance obsolete. González, Marshall, and Naidu (2017) found that slave owners were more likely than wealthy non-slave owners to start businesses before emancipation but not afterward.

[10]In 1860, prior to the start of the war, the Confederate states produced 38% of total US agricultural output and 8% of national manufacturing output. By 1870, however, even after four years of postwar recovery, Southern agricultural output remained below its prewar level in absolute terms and made up only 28% of the US total. Similarly, Southern manufacturing lagged behind the rest of the nation, making up only 5% of national output in 1870.

[11]On the decrease in labor force participation, Ransom and Sutch (2001) argued this decline was driven by a

economy, then it would most likely arise in the areas Sherman destroyed. Not only does our research design, focusing on local economic activity during and after the Civil War by comparing neighboring counties, enable us to difference out many of the confounding effects in prior work and, in turn, isolate the costs of destruction during war. But more, our findings suggest that capital destruction was a clear cost of the war in the South and mattered to weak postbellum economic development. Finally, our study provides novel evidence on the development of sharecropping and farm tenancy in the postbellum South (Shlomowitz 1979; Alston and Higgs 1982; Ransom and Sutch 2001; Fite 1984), suggesting an important role for wartime capital destruction—combined with the general scarcity of capital in the postbellum South—in the adoption of the practice.[12]

The remainder of the paper proceeds as follows. Section 2 reviews the history of Sherman's March. Section 3 describes the historical data we draw on, including the march map, census of population and manufactures data, and two measures of local credit markets in the postbellum South. Section 4 details our identification strategy. Section 5 presents results. Section 6 examines mechanisms, focusing on the role of credit markets in the recovery. Section 7 examines the persistence of results. Section 8 concludes.

2 Sherman's March and Reconstruction

2.1 The March: Historical Background

As the American Civil War progressed into 1864 and Union General Ulysses S. Grant plotted the destruction of Confederate armies in Virginia, his colleague, General William Sherman, trained his sights on the destruction of the Confederate economy and infrastructure (Trudeau 2008, p. 40). Sherman had just finished the successful Atlanta Campaign, a collection of skirmishes from

reduction in labor supply among newly freed African Americans, while Margo (2002) suggested the participation rate decline was also due to a decrease in labor demand, not an independent shift in supply. Wright (1986), on the other hand, claimed that a postwar reduction in world cotton demand—the South's staple cash crop—hurt the Southern economy most. Temin (1976) attempted to reconcile these explanations in light of the finding by Fogel and Engerman (1974) that plantation agriculture was more productive, making the loss of economies of scale argument. More recently, Khan (2015) showed that the misallocation of resources during the war due to declining geographic mobility and increasing payoffs to military technologies was short-lived and did not inhibit the long-term capacity of technological progress.

[12]Related to our work, two recent papers study the effects of emancipation as a wealth shock and trace the effects on intergenerational mobility from the antebellum to postbellum South (Ager, Boustan, and Eriksson 2019; Dupont and Rosenbloom 2016). Ager, Boustan, and Eriksson (2019) argue that while emancipation reduced the wealth of the richest slaveholders in 1870, their sons recovered their wealth and occupational status by 1880. Using the Sherman's March geographic data we collected for this paper, Ager, Boustan, and Eriksson (2019) also show that the negative effects on wealth in 1870 are significantly larger for wealthy household heads in Sherman's path compared to similar household heads in Georgia counties outside Sherman's path, with differences in wealth in 1870 as large as 40 log points.

Chattanooga, TN through northwest Georgia during the late spring and summer of 1864, which culminated in Atlanta's capture on September 2, 1864. In Chattanooga he had fought with Confederate armies led by Joseph E. Johnston and later John B. Hood. Though bloody—with 4,423 Union dead and 3,044 Confederate dead—the Atlanta Campaign was a conventional operation for its time with the two armies fighting one another in large and small battles. For his next act, Sherman had something else in mind: he planned to "enforce devastation" on the Southern states by "destroy[ing] mills, houses, cotton-gins, &c.," burning railroads and telegraph lines, and confiscating livestock and crops.[13] He wrote to Grant specifically of plans to "break roads and do irreparable damage" to the Southern transportation network (Carr 2015, p. 55).

According to Civil War historians, Sherman used the prewar 1860 US Census of Agriculture to carry out this mission (Trudeau 2008; Rubin 2014). He mapped out a march path that traversed the agriculturally richest counties in Georgia, North Carolina, and South Carolina while still following Grant's orders to capture the important Southern cities of Atlanta and Savannah, Georgia, and Columbia, South Carolina. Writing in December 1864, Sherman recalled how he "had the [1860] census statistics showing the produce of every county through which [he] desired to pass" and that he would destroy those counties most abundant in agriculture. "No military expedition was ever based on sounder or surer data," he remarked (Trudeau 2008, p. 538).

Based on our analysis of the 1860 Census of Agriculture, Sherman hit counties with a lot of railroad infrastructure to destroy and with large plantations, though differences in agricultural production fade when we compare within the three states. Figure 1 details the routes each of Sherman's forces traveled. The Union Army started in Atlanta and moved southeast to Savannah after several feints toward Macon and Augusta. Following the capture of Savannah, Sherman's forces marched north, through inland South Carolina to Columbia. The final drive took the army northeast before defeating the only Confederate army remaining in the Carolinas near Goldsboro, North Carolina.[14] In Table 1, we see that in 1860, the counties in Sherman's path—those counties inside

[13]William T. Sherman, Military Division of the Mississippi Special Field Order 120, November 9, 1864 in US War Department (1901); Lee (1995).

[14]Goldsboro was a minor railroad connection that Sherman targeted once his army began marching through North Carolina (Angley, Cross, and Hill 1995, p. 35). From March 19 to 21, 1865, the final fights of the campaign took place in Bentonville, 20 miles from Goldsboro. Sherman arrived in Goldsboro on March 23, 1865. From that point on, his army was supplied by rail from the north, ending the most destructive aspects of the campaign. Grant wrote to Sherman on April 8, telling him that "the confederate armies were the only strategic points at which to strike." (Barrett 1956, p. 198-199).

a ten-mile band of the main route shown in Figure 1—were more intensively active in agriculture. Demographically, the counties Sherman hit were more populous, but not denser or more urban, and even the raw population differences fade when we include state fixed effects (column 4). We find that the Sherman counties produced more agricultural output and included farms that were relatively more valuable, though again these differences fade with state fixed effects. At the same time, consistent with the historical evidence, we do not find any difference in terms of manufacturing activity, which overall was low across this whole region. Overall, while Sherman counties do not appear to be identical to non-Sherman counties, these results show that accounting for across-state variation, the differences in observable characteristics are relatively modest. In Section 4, we discuss how our research design accounts for underlying differences in the march and non-march counties, including the use of flexible controls and with a differences-in-differences strategy, as well as instrumenting for Sherman's final path.

[Figure 1 about here.]

[Table 1 about here.]

Based on his planning, Sherman undertook his march and wreaked substantial destruction. He officially assigned more than 3,000 infantrymen each day to "foraging." Another 3,000 likely joined in on many occasions, placing the true number closer to 6,000.[15] The men destroyed hundreds of businesses, homes, farms, railroad lines, and telegraph lines, and expropriated over 5,000 horses, 4,000 mules, 13,000 cattle, 10.5 million pounds of corn, and 9.5 million pounds of fodder.[16]

Sherman's men were not the only ones causing damage along the march path. Confederate General Joseph Wheeler and his cavalry corps—the main Confederate opposition to Sherman in Georgia and the Carolinas—were also under strict orders from their superiors to "destroy everything from which the enemy might derive sustenance" (Trudeau 2008, p. 85) and "felled trees [and] burned bridges" (Barrett 1956, p. 50). In addition, Wheeler's cavalry requisitioned mules and horses and

[15]Sherman issued orders regulating the destruction of property and foraging, but likely understood that not all of the regulations would be followed. For example, while many of the fires he ordered were managed by the Union Army Corp of Engineers, the controlled blazes often inspired infantrymen to set their own fires, which frequently grew out of hand and engulfed whole sections of towns (Trudeau 2008, p. 128, 543). In addition, while soldiers often offered to pay for their takings, the payments were frequently made in Confederate greybacks, which were not highly valuable by 1864 due to the Confederate government's large-scale printing efforts, and by war's end were of no value.

[16]William T. Sherman, Military Division of the Mississippi Special Field Order 120, November 9, 1864 in US War Department (1901); Lee (1995).

"burn[ed] up all the corn and fodder" (Glass Campbell 2006, p. 10). So severe was Wheeler in carrying out the orders that one Confederate officer remarked that "the whole of Georgia is full of bitter complaints of Wheeler's cavalry" (Bearss 1991, p. 127). Confederate General Beauregard too ordered his Georgia forces to "obstruct and destroy all roads in Sherman's front, flank, and rear..." (Trudeau 2008, p. 128).

This destruction along the march path marked the first time the war had meaningfully visited Georgia and the Carolinas (Carr 2015, p. 134). Previously, the only military actions seen in those states were the few shots fired at Fort Sumter, South Carolina, to start the war, a union blockade of the ports, and some minor skirmishes in North Carolina. The larger battles, city sieges, and troop movements had taken place farther north or west.[17] Noticing the relatively untouched countryside in the area prior to the 1864 march, one Minnesota soldier among Sherman's infantrymen wrote that "this part of Georgia [had] never realized what war was until we came through on this expedition" (Trudeau 2008, p. 526). A Hillsboro, Georgia native remarked similarly that only when Sherman marched had the "beloved [Georgia] country [been left] to desolation and ruin" (Trudeau 2008, p. 175). Even foreign observers remarked on the novelty of war to the region: writing a travelogue through the postbellum South, an English Member of Parliament recalled that Sherman marched "through States which had never had the war brought home to them, or even seen the blue uniform of their Yankee foes" (Kennaway 1867, p. 26). Sherman's destruction extended even to paperwork: in a study of bankruptcy cases in postbellum South Carolina, Thompson (2004) describes several claimants blaming Sherman for destroying their records and complicating the unwinding of debt obligations.

2.2 Reconstruction

Even before the end of the Civil War, planning for the reconstruction and reintegration of the South into the Union was underway. Reconstruction, dated by historians as from 1863 to 1877, was overseen by Presidents Lincoln, Johnson, and Grant, as well as the Republican Congress.

While rebuilding of the national railroad infrastructure was a postbellum priority, few other Reconstruction policies were focused on physical reconstruction. No attempts were made to pay back

[17] Before Sherman's army departed Atlanta on November 15, 1864, there had only been 36 fighting events in Georgia, North Carolina, or South Carolina, compared to 294 in all other states. Moreover, only 8 of the battles had had more than 500 total casualties. The median total casualties for Civil War fighting events in all states was 513.

private property owners whose capital had been destroyed during the war.[18] Nor was compensation offered to former slave owners of the Confederacy, as had been common in past emancipation of slaves elsewhere in the Western Hemisphere (Goldin 1973). Though cotton factors and other Southern interest groups proposed congressional aid and loan packages and corresponded about large-scale financing with some Northern and European firms and individuals, nothing came of these efforts. "[B]y and large the federal government offered little financial aid..." and "[n]either government aid nor large-scale, long-term private loans on the pattern envisioned by the Cotton Planters' Association were forthcoming, despite the obvious need and repeated pleas by Southern leaders..." (Woodman 1999, p. 251, 253).

Instead, Reconstruction focused on the reintegration of the South into the nation and the legal revolution that ultimately granted citizenship and de jure rights to the formerly enslaved African-American population.[19] Along with constitutional amendments abolishing slavery, guaranteeing citizenship, and granting the right to vote, Congress enacted several statutes enabling Reconstruction. The creation of the Freedmen's Bureau, the federal agency tasked by Congress with transitioning former slaves into freedom, involved starting schools for both children and adults, as well as provision of food and medical care to newly freed African Americans. The Freedman's Saving and Trust Company, also known as the Freedman's Savings Bank, was chartered in 1865. It made loans to African-American veterans and newly freed slaves (Stein and Yannelis 2019). Directly relevant to our identification strategy is that we have not found any qualitative historical evidence of Reconstruction varying in implementation or focus—from the location of Freedmen's Bureau schools or Freedman's banks, to the protection or enforcement of constitutional amendments—across Sherman march and non-march counties.[20]

[18]Even Thaddeus Stevens, the powerful Radical Republican congressman from Pennsylvania, could not extract repayment for destruction. An iron foundry he owned near Caledonia, PA was destroyed by a raid led by Confederate General Jubal Early, an uncompensated loss Stevens later estimated at $50,000 (Egerton 2014, p. 212–213).

[19]Logan (2018) estimates the effects of black politicians elected during Reconstruction on taxation and public spending. The new rights of the formerly enslaved were largely stripped at the end of the Reconstruction period when the planter class regained power in the South (Naidu 2010).

[20]Bolder plans for a more radical Reconstruction, such as Thaddeus Stevens' call to confiscate the landholdings of the 70,000 largest Southern landowners and use the proceeds to give land to the formerly enslaved, pay veterans' pensions, and pay off the national debt, were not enacted (Jaynes 1986, p. 10).

3 Historical Data

To investigate the economic effects of the war on march counties relative to non-march counties, we gather historical data from several sources.

To start, we classify counties' march status using the 1865 War Department map of Sherman's troop movements, shown in Figure 1. In the map, each line indicates the center of march lines for each of five Sherman military units: the Right Wing (the 13th and 17th Army Corps), the Left Wing (the 19th and 20th Army Corps), and the Cavalry. We digitally trace each of these lines and consider as "march" counties all counties within five miles on either side of a line. Non-march counties are those outside this 10-mile-wide band, but within 100 miles of the lines and located in Georgia, North Carolina, or South Carolina. Figure 2 shows the march and non-march counties in our sample. We select five miles as our primary march bandwidth based on historical accounts that the marching soldiers and foragers did not stray far from the main body of the army (Trudeau 2008, p. 234). Our results are robust to alternative march-distance definitions, as shown in Appendix Section A.[21]

[Figure 2 about here.]

Second, we gather economic and demographic county-level data before and after the march from the US censuses of 1850-1920. Haines (2010) provides decadal, county-level, agricultural production and asset value data, as well as demographic information for each county, from the Census of Population, the Census of Agriculture, and the Census of Manufactures. Newly digitized versions of the Census of Manufactures report manufacturing data at the county-by-industry level in 1860, 1870, and 1880 (Lee 2015b). From both sources, we extract data pertaining to the counties in the states of Georgia, North Carolina, and South Carolina. County-level data are standardized to 1860 county borders, using the county-intersection procedure described in Hornbeck (2010).

Because the 1870 census data were collected in a South that was still recovering from the Civil War, one might worry about data quality. In particular, it would be problematic if under-enumeration in march counties were more severe than in non-march counties.[22] Heterogeneity in

[21] Given the high variation in quality of contemporary maps, the troops assigned to forage may have occasionally done so farther than 5 miles from the main line of the army. In the Appendix, we also show robustness to various definitions of the control county set.

[22] See Steckel (1991) and King and Magnuson (1995) on under-enumeration in historical census data generally. Reid

data quality across treatment could potentially bias our results in an unknown direction, affecting the reliability of our estimates.

We use data on contemporary marriage records to show that data quality does not seem to be different across treatment and control counties in the census. Specifically, we test whether men who appear in county marriage records in Georgia and North Carolina, two of our sample states, between 1868 and 1872 are differentially likely to also appear in the 1870 census based on whether the men were married in a march county or a non-march county.[23] Table A.1 shows the match rates for each state and county type, finding no difference across treatment. This suggests that there was not differential under-enumeration across march and non-march counties and allays our concerns about comparing census outcomes across the two groups of counties.

To investigate the mechanisms behind any march and non-march differences, we augment the Haines (2010) demographic information with three other data sets. The first are infrastructure data from Donaldson and Hornbeck (2016) based on Atack, Bateman, Haines, and Margo (2010): decadal railroad maps that indicate the exact location of all railroad lines from 1850 through 1920. We intersect these railroad lines with 1860 US county boundaries to calculate the number of railroad-track miles in each county in each decade.

The second two data sources used to consider the mechanisms driving our results are local credit data from a pair of newly digitized sources. We hand-collect bank-level entries in two Merchants & Bankers Registers, 1859 and 1864, and firm-level records from Dun, Boyd, & Company's *The Mercantile Agency's 1860 Reference Book*.[24] The bank registries provide the name, capitalization amount, and county of location for the approximately 1,800 state- and nationally-chartered US banks in 1859 and 1864.[25] The Dun, Boyd, & Company book lists all firms tracked by this credit-rating firm based in New York as of 1860. For each firm, the book lists the name, city, and three credit ratings. In various formats, historical DB data have recently been used by several papers in

(1995) documents under-enumeration in the 1870 census in North Carolina of African Americas, particularly union veterans.

[23]The 1868–1872 county marriage records were digitized by the genealogical website FamilySearch.org. The marriage records are collected locally by state and county governments, which contrasts with the census data collected by federal agents. Marriage records from South Carolina have not been digitized; Koudijs and Salisbury (2016) face similar data restrictions. The name matching procedure follows the machine learning approach to record linkage developed in Feigenbaum (2016).

[24]The bank registries were scanned by the University of Wisconsin. We digitized them into a machine-readable, manipulable format.

[25]The banks in 1859 were only state-chartered, as national-charter banks were not authorized until after the National Banking Act of 1863 (Jaremski 2013).

economic history and finance (Brennecke 2016; González, Marshall, and Naidu 2017).[26] We match the city of each firm to an 1860 county and calculate the number of credit-tracked firms in each county. The bank and DB data proxy for different types of local credit availability before the march, allowing us to shed light on the importance of credit in driving our results. Following the historical arguments about the important role of country stores and local merchants in antebellum Southern credit markets in Koudijs and Salisbury (2016), we also collect census data on country stores from the 1840 census.

4 Identification Strategy

Sherman's objective was to "enforce devastation" on the South; our objective is to measure how effective and persistent Sherman's devastation was, estimating the direct effect of the capital destruction caused by Sherman's march on the local economy. We begin our analysis by comparing agricultural and manufacturing outcomes before and after the march, across march and non-march counties. Depending on the outcome type, this differences-in-differences fixed effect approach produces our two primary estimating equations.

We start by looking at the agricultural sector using county level data. Initially, we focus on the medium-run impact of the shock (1850-1890), but in our final section, we test for the presence of longer-term effects using data through 1920. We focus on two sets of variables. First, we look at the effect of the march on the economic value of farming activities, measured by the value of the farms and the value of livestock. Second, we examine the role of the march in affecting the investment in farming land. In line with previous work (e.g. Hornbeck 2010), we proxy investment with the share of land improved for farming.[27] We estimate the following specification:

$$Y_{cst} = \beta_t 1[Sherman]_c + \delta_c + \delta_{st} + X_c\theta_t + \epsilon_{cst} \tag{1}$$

[26] Brennecke (2016) provides a very detailed explanation of the data, the way they were collected, and the general business model of Dun, Boyd, & Company around this period.

[27] The Census of Agriculture defines improved acres of farmland as land in farms cleared for tillage, grazing, grass, or lying fallow; unimproved areas are defined as uncultivated land connected to farms, including both fertile and waste acres. This measure is consistently employed across the 1850–1920 US Censuses of Agriculture. For an extensive discussion on the role and determinants of improved land in the South around the Civil War, see the work by Majewski and Tchakerian (2007). We define share of land improved for farming as the acres improved in census year t scaled by the farmland area in the county in 1860. We scale improved acreage so that the units are easy to understand and in accord with Table 1 but fix the denominator as farmland 1860 so that the denominator is not determined by Sherman's March. In practice, the county fixed effects make scaling irrelevant in the regressions and results are numerically identical to using unscaled improved land.

where Y_{cst} is agricultural outcome Y in county c and state s at time t, $1[Sherman]_c$ is an indicator equal to 1 if county c is within five miles of any Sherman's march line, δ_c are county fixed effects, δ_{st} are state-by-year fixed effects, X_c are non-parametric controls for 1860 county characteristics interacted with year indicators, and ϵ_{cst} is the error term. We interact the Sherman's march indicator with a full set of year-indicator variables—excluding 1860—to estimate the difference in the outcomes between the march and non-march counties in each year, relative to the year 1860 difference. Our sample includes all counties within 100 miles of any march line.[28] Negative estimates of β_t for $t > 1860$ indicate lower agricultural outcomes in march counties relative to non-march counties following the war. Standard errors are always clustered at county-level.

There are three important elements of this specification. First, the county fixed effects controls flexibly for time-invariant county characteristics such as the quality of soil, climate, or latitude and longitude. Second, the state-by-year fixed effects absorb any time-varying shocks common to all counties within a state such as changes in the demand for cotton, state-specific business cycles, or changes in state policy. Third, on top of our baseline results, we augment this main specification with a set of control variables that account for the heterogeneity in county characteristics in 1860. We use these controls to absorb the cross-county variation across treatment that was presented in Section 2 based on Sherman's planning of the path. In particular, we control for size of the county, measured in square miles; population; size of the agricultural output; and intensity in cotton production (Bleakley and Lin 2012, Donaldson and Hornbeck 2016), share of plantation farms, share of manufacturing employment, slave intensity, and railroads per squared acres in 1860.[29] To avoid imposing a specific parametric relationship between the 1860 county characteristics and the changes in economic activity, we control for each of these characteristics by dividing the sample in four quartiles and interacting these quartile indicators with year-dummies.[30] As we discuss in the results section, our findings generally suggest that the addition of these controls has little effect on

[28] Our sample only includes counties in the three states where Sherman marched because, with state-by-time fixed effects, we need within-state variation in the treatment to estimate Sherman effects.

[29] In the main analyses, this means the set of counties that are 100 miles from the march and are in North Carolina, South Carolina, and Georgia. We define the intensity in cotton production as the dollar value of ginned cotton production per total farming acres; a plantation farm as a farm with more than one hundred slaves in 1860; and slave intensity as the average number of slave per farm. More discussion on the variable construction is provided in the data section.

[30] The only exception to this procedure is the variable measuring railroads, which is highly skewed, with many zero, and therefore for which constructing four quartiles would not be possible. For this variable, we simply split the sample at the first quartile.

our estimates, in particular in the medium-run.[31]

For manufacturing, our production, capital, revenue, and employment data are more detailed and we observe outcomes at the industry, county, and year levels in the decennial censuses of 1860, 1870, and 1880. To take advantage of this rich industry-level data, we also consider an alternative empirical specification. We estimate a collapsed difference-in-difference model, where we compare the 1860-1870 growth rate of manufacturing outcomes at industry level across march and non-march counties. Using the data at industry-county level allows us to control for time-varying industry shocks in the South, as well as the level of county development in manufacturing. Furthermore, collapsing the outcome makes it easier to accommodate the different data structure and therefore avoid a large number of missing values that the previous log-level specification would entail, particularly in the postbellum period. The specification is:

$$\Delta Y_{cg(i)s,1870-1860} = \beta_M 1[Sherman]_c + \delta_{g(i)} + \delta_s + X_c + \epsilon_c \tag{2}$$

where $Y_{cg(i)s}$ is the percentage change between 1870–1860 in manufacturing outcome Y in county c, industry group $g(i)$, where i denotes industry, and state s, $1[Sherman]_c$ is an indicator equal to 1 if county c is within five miles of any Sherman's march line, and ϵ_c is the error term.[32] Furthermore, we augment this specification with industry-group fixed effects $\delta_{g(i)}$ and state fixed effects δ_s.[33] The industry-group fixed effects control flexibly for industry-group characteristics such as demand. The state fixed effects absorb any shocks common to all counties within a state such as changes in the state-specific business cycles or state policy changes. Exactly like in the previous specification, we also control for county-specific 1860 characteristics. The sample is again all counties within 100 miles of any march line. Negative estimates of β_M indicate lower manufacturing growth rates in march counties relative to non-march counties from 1860 to 1870. Standard errors are always clustered at county-level.

[31] As presented in previous versions of the paper, the results are similar if we control for 1860 county characteristics using continuous controls.

[32] Growth rates are winsorized at 1% at each tail.

[33] The industry group is generated using the industry groupings introduced in the Census of Manufactures in 1900. These groupings were precursors to the Standard Industrial Classification 2-digit groupings, which were introduced in the Census of Manufactures in 1939. To apply these year 1900 groupings to the 1860 to 1880 data, we convert all 1860 to 1880 industry classifications into year 1900 industry classifications. The procedure we use for the conversion is based on Lee (2015a). We also run these regressions excluding the pre-march, 1860 county controls, X_c. The results are robust to the more parsimonious specification.

5 Sherman and Economic Activity

Using the empirical setting discussed in the previous section, we demonstrate how the destruction unleashed on Georgia, South Carolina, and North Carolina by Sherman's March harmed both agriculture and manufacturing in the medium-run, including large decreases in values in 1870. We also show the robustness of these results, employing an instrument for the march path, estimating null effects on placebo marches, and varying our treatment and control bandwidths.

5.1 Main Analyses

Comparing march to non-march counties using our differences-in-differences specification, we find economically large and statistically significant post-march differences among agricultural outcomes. Table 2 shows the results. After the march, the value of farming activities and investment in agriculture declined substantially. Our results show that the value of farms and livestock declined by 20% and 14% respectively. We also find that march counties experienced a significant drop in the amount of improved land, which in 1870 declined almost 15% more in affected counties relative to the control group. These findings are similar across the specifications with and without controls. Overall, this analysis shows that the capital destruction caused by Sherman's march substantially affected the local agricultural economy, with the effects still visible in 1870, six years later.[34]

[Table 2 about here.]

Importantly, these results are not driven by differential trends across treatment before the march. In each specification, we also estimate the "effect" on agricultural outcomes in 1850 relative to the base year 1860. Across the outcomes, we find that the 1850 effect is both non-significant and small in economic magnitude. These results provide reassuring evidence on the quality of our empirical model. In the post-1870 period, our results suggest some persistence of the march's effects. However, this evidence is statistically weaker and varies across outcome measures. We will investigate the long-run persistence of the shock in the second half of the paper; for now, we focus on the evidence in Table 2 that Sherman had a large medium-run effect on agriculture activity.

[34]These negative results are consistent with the historical record. In the immediate antebellum period, farming was done with "Sherman horses," the old, "sore-backed" and "abused" animals the Union Army had swapped for fresh rides along the path of the march (Rubin 2014, p. 50-51).

Turning to data on postbellum manufacturing, we gauge how the march also reduced manufacturing activity. As a first step, we analyze county manufacturing aggregates in Table 3, using the same exact methodology that we employed for agriculture. While we are constrained slightly by the limited nature of the 1850 Census of Manufactures, we observe declines in employment, capital, manufacturing establishments, and production in the Sherman counties after the war, echoing our agricultural findings.[35] However, while these results are all relatively large, they are quite imprecise. We find a highly significant effect of the march only when looking at capital, which declined 30% more than in non-Sherman counties. The magnitude of the effects are similar across other outcomes, but the estimates tend to be statistically non-significant.

We believe that this lack of statistical precision can be explained by three key facets of the county-aggregated manufacturing data. First, in the 19th century there was a great deal of heterogeneity in manufacturing specialization across counties. In our data, very few counties specialized in the same set of industries. Second, the period around the Civil War was characterized by a lot of transformation in the manufacturing sector (Engerman 1966). Third, demand dynamics may also have played an important role, as the war itself led to an increase in demand for certain industries while impairing others. This heterogeneity across industries—even if unrelated to the location of Sherman's march—may increase the noise in the data and could therefore make it harder to detect march effects in aggregate data.

[Table 3 about here.]

To overcome these limitations, we have collected county-by-industry data at the decade level for 1860, 1870, and 1880 that enable us to account for any such county and industry heterogeneity, including time-varying industry shocks or variation across counties in manufacturing specialization.[36] Using these county-by-industry data, we examine the differential growth rates from 1860 to 1870 along four manufacturing outcomes for march and non-march counties in Table 4. Overall, we find that the number of establishments grew substantially more slowly in march counties than in non-march counties following the war, as did value added, employment and capital. These results are large in magnitude: for instance, in the baseline specification, Sherman counties experienced about

[35] Establishment data are not available for 1850.

[36] The growth rates are winsorized at 1% to reduce the impact of outliers in our analysis.

a 50% lower establishment and employment growth relative to non-Sherman counties. While this effect is large in absolute terms, the magnitude should be considered within the context of the Southern manufacturing sector between 1860 and 1880, decades characterized by large transformations and, in some counties, rapid growth. The addition of county-level controls does not qualitatively impact our results. Moreover, as with the agricultural results, these differences in manufacturing growth rates were not simply the result of pre-march trends, as we find no difference in aggregate manufacturing growth.[37]

[Table 4 about here.]

We also see substantial declines in the lumber industry in Sherman counties, a geographically common industry explicitly targeted by Sherman's troops because of its importance to the war effort. We are able to measure these lumber industry effects because our data is at the county by industry level. Prior to the march, 75% of the counties in the sample had at least one sawmill or lumber establishment, making the industry the most geographically prevalent manufacturing industry in the data. We find suggestive evidence of relative declines in lumber, even with far fewer observations than in our county-by-industry growth-rate analysis.[38] Table A.2 indicates that both capital and the number of establishments among lumber mills in march counties grew 50% more slowly than it did in non-march counties between 1860 and 1870. Value added and employment also grew more slowly, but the coefficients on these effects are less precisely estimated.

Overall, the capital destruction following General Sherman's march led to a strong contraction in land values, agricultural investments, and manufacturing output. These effects are not driven by differential trends in economic outcomes across march and non-march counties. Sherman succeeded in bringing destruction to the parts of the South through which he marched, and the economic effects of the march were still evident more than five years afterwards.

5.2 Robustness

In this section, we provide three main robustness tests to our results. First, we employ an instrumental variable estimator to address the potential endogeneity of the march path. Second, we

[37] We do not have data at the industry level prior to 1860; therefore this pre-trend test can only be performed at the aggregate manufacturing sector level and not by industry-group.

[38] Sherman also targeted the pine forests and resin pits in the Carolinas, the main inputs for the large South Carolina turpentine and tar industries (Carr 2015, p. 91).

create placebo marches, connecting other large cities in the South to ensure that our results are not driven by a county's geographic position relative to urban areas. In both cases, the robustness tests support our argument: the instrumental variables estimates largely confirm our main results and the placebo marches show no effects. Third, we show that the results are similar across different definitions of treatment and control groups.

5.2.1 Robustness: Instrumental Variables Estimator

Sherman's path was not a random walk. As historians have documented—and as we confirmed—the course was plotted based on available economic data from the 1860 census. Even if the selection we have documented would likely work against our findings, we are still concerned about endogeneity. One worry would be that some unmeasured, time-varying county characteristic was correlated with the path Sherman took and explains our results. Our inclusion in the main specification of both county fixed effects and state-by-decade fixed effects, as well as flexible controls based on variables measured in 1860, makes this unlikely, but it cannot rule this possibility out.

To assuage these concerns, we instrument for Sherman's march path with the straight-line path between the vertices of the actual march. To replicate the approximate width of the march, we define as treated any county within 15 miles of the straight line between the march vertices.[39] Sherman was specifically targeting the main cities—Atlanta, Savannah, and Columbia—but many of the counties between these cities were likely hit only because they happened to be on the way.[40] This instrument is likely to satisfy the standard exclusion restrictions: counties between these cities should not be expected to have grown less quickly but for Sherman destroying many of them during the war. The F-stat on the first stage is 126.4, suggesting that although Sherman took deviations from the straight line, particularly when approaching the coastal area, the path of the march can roughly be approximated by a straight line between the main cities.

Overall, this straight-line IV analysis bolsters the findings of our main estimates: Sherman's march had large, negative economic effects on both agriculture and manufacturing. The results,

[39] In the main results, we defined as treated any counties within five miles on either side of one of Sherman's units' march lines. However, because the march was undertaken by five units (the 13th and 17th Army Corps in the Right Wing, the 19th and 20th Army Corps in the Left Wing, and Kilpartick's Cavalry), to replicate the effective width of the true treatment with the straight-line instrument, we expand the treatment radius to be 15 miles on either side of the line.

[40] Exactly because of our endogeneity concerns about the cities, these three cities are dropped from all analyses, as is Goldsboro, the town where the march ended after the Confederate opposition surrendered. These exclusions do not affect the results.

which are shown in Tables A.3 for agriculture and A.4 for manufacturing, are similar in sign and magnitude to the OLS results in Tables 2 and 4, though they are less precise. This analysis confirms our main result.

5.2.2 Robustness: Placebo March

As a second robustness test, we develop placebo march lines to show that differences in postbellum economic responses across counties lying between cities and counties not between cities cannot explain our results. While the straight-line instrument addresses the concern that an unmeasured, time-varying county characteristics correlated with the path Sherman chose could explain the differential postwar agricultural and manufacturing outcomes, this placebo test exploits inevitable variation across counties that are located between the major cities compared with the rest of the sample. This is problematic if these counties are exposed to different shocks in the postbellum economy because of their special location between major economic hubs.

To implement this robustness test, we construct *all* possible comparable march paths in the Confederacy outside Georgia, North Carolina, and South Carolina.[41] The straight-line distance from Atlanta to Savannah is 270 miles; Savannah to Columbia is 164 miles. In this spirit, we create all possible connections of three Confederate cities—defined as counties with more than 2000 urban residents in 1860—where each line segment is between 100 and 300 miles. Excluding paths through the three Sherman states, there are 852 paths, many in Texas, Louisiana, and Mississippi, and others in Virginia. We then define treatment and control indicators in the usual way for straight-line marches—treatment for counties within 15 miles of the paths, controls counties within 100 miles—and estimate separate regressions for each placebo path, echoing the form of equation (1).

We plot the distribution of t-statistics from the effect of these placebo marches in 1870 against the Sherman t-statistics from Table 2 in Figure 3. In all six specifications, the Sherman t-statistic is smaller (more negative) than 95% of the placebo marches; in five of the six it is smaller than 97.7% of the placebos.[42] No placebo march has more negative t-statistics than we found for Sherman's march

[41] We avoid placebo paths through the three Sherman states to preclude historically destroyed counties and their neighbors from being in the placebo sample at all.

[42] In a corresponding analysis for the manufacturing results in Table 3, we find fewer than 2% of placebo marches yield t-statistics as negative as those of Sherman's effect on capital in columns 3 and 4. For employment, establishments, and the value of production, in five of six specifications fewer than 15% of placebo marches yield as negative t-statistics as the real Sherman's march but because we do not find a statistically significant effect of Sherman on these outcomes in this specification, it is not surprising that the placebo march analyses for those outcomes are less compelling.

in all six specifications and only ten placebo marches are smaller in three or more specifications.

[Figure 3 about here.]

5.2.3 Robustness: Different Treatment Definitions

Lastly, we also show that our results are not driven by the specific definition of treatment and control counties that we use in our main results. As discussed before, our main results consider as treated the counties within 5 miles of Sherman's march and as a control those within 100 miles. The treatment bandwidth is based on the historical record: Sherman's troops could only "forage" so far away from the relative safety of the main army. We chose the 100-mile control bandwidth as a reasonable tradeoff between including more counties from Georgia, North Carolina, and South Carolina and not including counties in those states that were very different from the counties Sherman laid to waste. In Figure 4a, we test the robustness of our bandwidth choices by presenting estimates of the 1870 effect of the march across different treatment definitions—5, 10, 15, 20, and 25 miles—keeping the control group constant. We find that these alternative definitions of treatment provide very similar results, both statistically and economically. For instance, the estimates using 10 miles are always indistinguishable from the 5-mile results across all the main agricultural outcomes. However, as we use a broader definition of treatment, we tend to find smaller and less significant results. In fact, this finding is reassuring about our identification: as we increase the bandwidth for the treatment, we are bundling together both counties that were struck by Sherman with areas that were most likely unaffected.

We also show that our control-bandwidth choice is not driving our findings in Figure 4b. We repeat the same procedure as above, but keeping the treatment definition as constant (at 5 miles) and changing the control. Specifically, we consider bandwidths of 25, 50, 75, 100, 125, and 150 miles as potential control groups. Again, we find similar results: the point estimates of our 1870 Sherman effects are extremely stable across specifications. The effects are generally less precisely estimated when we employ a very tight control group—25 or 50 miles—reflecting both the small sample size and the possible contamination of treatment into the control counties.

[Figure 4 about here.]

Overall, these robustness tests increase our confidence that the relative declines we detect in Sherman counties were indeed due to the destructive path of Sherman's march. In the Appendix,

we also show that the results on manufacturing, presented originally in Table 4, are robust to alternative treatment and control bandwidth definitions (Figure A.2), though the estimates are a bit more stable when varying control distance rather than treatment definitions.

6 The Role of Credit Markets in the Extent of Capital Devastation

The capital destruction caused by Sherman's march had strong negative effects: the march led to a contraction in investments and asset prices in the agricultural sector and a reduction in manufacturing growth in the decade after the war. At least in part, economic activity appeared to have recovered by 1880 or 1890—within twenty or thirty years from the march. Overall, these results confirm that the effects of capital destruction may generate large medium-run costs for an economy.

The previous literature in this area has devoted little attention to understanding factors that might make capital destruction more or less devastating to a local economy in the short- or medium-run, or to how such underlying heterogeneity might affect the recovery process (Skidmore and Toya 2002). This decision is partially motivated by the fact that—in line with the predictions of standard neoclassical growth models—previous work has found that the negative effect of capital shocks tend to dissipate very fast. For instance, the seminal work by Ikle (1952) found that the negative effects of Allied bombing of German cities during World War II disappeared within a couple of years after the war. Similar results were found in the context of the Japanese WWII reconstruction (Davis and Weinstein 2002), and the Vietnam War (Miguel and Roland 2011). At the extreme, some papers find essentially no effect of large natural disasters, even in the very short run (Porcelli and Trezzi 2019).

In light of our results, we believe that studying the variation in the costs of destruction in the medium-run is crucial to fully understand these negative events. First, our analyses show that capital destruction can have a large impact in economic activity, even several years after the negative events. Therefore, understanding the factors that may cause a larger downturn from the same destructive shock or impede a faster and more efficient recovery process may provide important insights in how to reduce these intermediary welfare costs. Second, even if the level of economic activity may return to the pre-shock level, capital destruction may affect resource allocation, which directly can have important implications for economic growth.

In this paper, we argue that the lack of developed financial markets played an important role

in the reconstruction after the Civil War. One of the key implicit assumption of the standard neoclassical growth model is the presence of perfect financial markets.[43] If this assumption fails, the economy will experience a slower recovery from a temporary shock to the stock of capital and, potentially, it may never achieve full convergence to the pre-shock equilibrium. In fact, if agents are financially constrained, reconstruction efforts are limited by their financial slack, rather than driven by investment opportunities only. More broadly, the presence of imperfections in financial markets should affect the overall path to recovery. In our setting, this might affect which counties are more or less severely shocked as soon as 1870, only a few years after Sherman's destruction.

The weakness of credit markets after the Civil War suggests that financing considerations may have played an important role in the weak immediate postbellum recovery. Woodman (1999, p. 300-301) notes that in contrast to antebellum cotton farmers, "the postwar grower seldom delayed the sale of his crop... Growers usually sold their cotton quickly, ordinarily to the nearest buyer [because] [t]hey lacked the financial independence as well as the ability to secure the credit required to delay a sale." In general, the US banking sector was not particularly developed even before the war (Jaremski 2013), and this underdevelopment was more acute in the South (Ransom and Sutch 2001). Bensel (1991) argues that the South was capital-starved, and this was particularly true in the rural and agrarian parts of the region. Indeed, the 1859 Merchants & Bankers Register data reveal that both North Carolina and South Carolina had only 2.9 banks for every 100,000 people, and Georgia had 6.2 banks per 100,000, compared with the national average of 7.1 per 100,000.

Formal credit markets were not particularly developed during this period, but the banking sector in the South was also deeply impaired by the war and slow to recover to even these meager antebellum levels (James 1981). The bank registries indicate that as of 1864, no state- or nationally-chartered bank existed in Georgia, North Carolina, or South Carolina.[44] Banks started to re-enter these states only in 1866 and even by 1870 only a handful of banks were operating.[45] As Atack and Passell (1994) note, this modest "growth in the number of banks after the war masks a sharp reduction in total bank assets from prewar levels." Figure 5 provides suggestive evidence that access to

[43] An alternative to developed financial markets is to have a planned economy where the government can optimally reallocate resources to foster recovery, as discussed by Miguel and Roland 2011 in the case of Vietnam.

[44] The Comptroller of the Currency's 1864 Report to the US Treasury documents the same fact. At the same time, the Southern banking sector collapsed as a whole during the war: according to Jaremski (2013), 170 of 223 banks closed in 1863 and 1864.

[45] By 1870, Georgia still had only 9 banks; North Carolina 6; and South Carolina a mere 3.

formal credit was negatively impacted by the Sherman's march. There are two other important factors to consider to interpret this period. First, unlike the other destruction settings previously cited, this underdeveloped financial sector was not compensated by large public programs to help private reconstruction. Second, the overall weakness in credit was exacerbated by emancipation—which removed one important source of collateral for landowners (González, Marshall, and Naidu 2017; Martin 2010, 2016; Jaynes 1986, p. 31)—and the default of the Confederate States on their debt obligations.

Furthermore, the supply of formal credit from banks did not serve rural and agricultural needs during this period, especially in the South (Fite 1984; Jaremski and Rousseau 2013; Koudijs and Salisbury 2016; Nier 2007).[46] As we will discuss more in detail later, most of the funding in the agricultural sector was coming from local country stores (Atherton 1949; McCurry 1997; Sparks 1932) or from wealthier landowners providing credit for other farmers during bad times or between harvests (Jaremski and Fishback 2018; McCurry 1997; Rajan and Ramcharan 2008, 2011).[47] To some extent, this different financing model for the agricultural sector could have reduced frictions that specifically characterized credit in the agricultural sector. Because these institutions were still important in the postbellum period for agriculture, in our analyses we will exploit directly the differences between agricultural and manufacturing sectors.

The Southern financial sector was weak after the war, but did financing issues play an important role in exacerbating the Sherman shock and retarding the recovery process? The remainder of this section provides three pieces of empirical evidence that are consistent with this hypothesis. First, we show that the two leading alternative channels affecting the slow relative recovery—a demographic shift after the war and the destruction of public infrastructure—are not supported in the data. Second, using both cross-industry and cross-location variation, we provide direct evidence of the importance of the banking sector to explain the lower growth in manufacturing after the capital shock. Third, we show that in the agricultural sector, too, frictions in the credit market are relevant in explaining the extent of the decline in 1870. Since the banking sector played little

[46] According to Fite (1984, p. 27), the Southern banking system in this period "was entirely inadequate to meet rural needs."

[47] For instance, Rajan and Ramcharan (2011) argue that before the Great Depression, local landowning elites actively tried to restrict the development of a local banking sector to maintain monopoly power in the provision of credit to small farmers. While Jaremski and Fishback (2018) disagree in part with some of the conclusions in Rajan and Ramcharan (2011), both papers highlight the importance of local landowning elites in the provision of credit for farmers.

role in financing agriculture in this period in the South, our evidence will be based on a different identification strategy. Consistent with the history of agricultural finance in the antebellum and postbellum South, we show that counties with more pre-war high-wealth individuals recovered from the march faster. In the same direction, the presence of antebellum country stores—often run and financed by local wealthy elites—also had a significant positive impact on the recovery.

6.1 Alternative Channels: Demographic Shifts and Infrastructure

The effects of capital destruction may be magnified if the shock also affected the demographic structure of the population; in the postbellum South, that could mean reducing the labor supply of whites or newly freed blacks. Ransom and Sutch (2001) argue that changes in labor supply help explain the postbellum decline in economic activity in the South as a whole compared with the North. For the enslaved populations of Georgia and the Carolinas, the arrival of Union troops signaled freedom. Catton (1988, vol. 3, p. 415-416) estimates that more than 10,000 slaves were freed during the march. Moreover, Sherman not only freed the slaves in his path, but he also signed Field Order No. 15, which allowed the freed slaves to settle outside the march path in abandoned coastal plantations (Trudeau 2008, p. 521). Ransom and Sutch (2001) estimate high rates of out-migration among freed people throughout the South, but we will investigate whether that out-migration differed between march counties and non-march counties.

In addition to potentially divergent postwar demographic patterns, the rebuilding and development of new public infrastructure in the postbellum period could have been different between march and non-march counties. We know that wartime destruction of infrastructure varied between the march and non-march counties because Sherman explicitly targeted the railroads and telegraph lines in his path. Prior to the march, Georgia, North Carolina, and South Carolina had more than 2,700 miles of railroad track. Sherman laid siege to this track by assigning a large share of his men to the specific job of destroying the tracks and nearby depots, warehouses, station buildings, and bridges (Carr 2015, p. 69). His soldiers sent home vivid letters describing how they would lift up track in concert, soften the steel with bonfires, wrap the track around trees, and bend it into bows known as "Sherman's neckties" (Carr 2015, p. 70).[48] Between Atlanta and Savannah alone, Sherman claimed to have destroyed 310 miles of track (Trudeau 2008, p. 533). These claims may

[48] Barrett (1956) describes in detail the Union army process of destroying a railroad, including the need to bend the wrap into a twisted doughnut shape known as a "Lincoln gimlet" (Barrett 1956, p. 51).

be exaggerations, as much of the destruction was incomplete and his men concentrated more on pulling up rails and breaking ties than on fully destroying rail paths and grounds. Postwar re-laying of track came fairly quickly (Trudeau 2008, p. 92). Nevertheless, the potential for differential infrastructure across march and non-march counties following Sherman's march could also help explain the observed economic differences.

Using county-level data on demographic structure and infrastructure, we find that neither channel is particularly useful in explaining our results in either the medium term or the long term. Table 5 shows the results of estimating equation 1 on the demographic and infrastructure outcomes. Columns 1 and 2 indicate that there were not systematically different postwar in- or out-migration rates overall; columns 3 and 4 indicate no differences for migration of newly freed African Americans across the march and non-march counties.[49] Demographics do not appear to explain much of the economic effects of the march. Looking across both total population and share of African American, we find effects that are very small in size and highly insignificant. Our results echo many histories of the postbellum South: if newly freed slaves "showed a reluctance to leave the places where they had lived and worked" (Glass Campbell 2006, p. 49), that reluctance was not differential across counties decimated by Sherman and not.

Similarly, the last two columns show that differences in infrastructure, as measured by county railroad miles, were also small. This is consistent with the historical record: Atack and Passell (1994, p. 378-379) note that while rail and telegraph lines were "destroyed with great vigor by the Union," the "repairs were immediate." According to Rubin (2014, p. 154), many travelers remarked on the speed with which the Georgia Central Railroad was rebuilt in the few years after the war. The telegraph repair was even more rapid: as soon as December 13, 1864, while Sherman was still sieging Savannah, the Southern Telegraph Company had already repaired many of the cut wires, quickly reestablishing communication between Macon and Augusta. Given these rapid repairs and the exaggerated reports of railroad destruction by Sherman, it is perhaps unsurprising that postwar infrastructure was not different across march and non-march counties.

[Table 5 about here.]

These results suggest that differences in either demographic composition or infrastructure cannot

[49] We find similar results on a battery of other demographic outcomes such as sex ratio.

explain the Sherman effects. Importantly, this is true both in the long and medium-run, since we do not find any significant effects on these outcomes even in 1870, six years after the starting of the events.

6.2 Credit in the Manufacturing Recovery

The previous analyses suggest that shifts in demographics and public investments do not appear to explain the economic distress in 1870. For the rest of this section, we provide some direct evidence on the importance of the financial channel in explaining the relative downturns from the Sherman destruction. Using both county and industry variation, we begin by exploring the role of credit in the near-term manufacturing recovery.

Jaremski (2014) documents the importance of a formal banking sector in encouraging the growth of manufacturing during this period. Banks and credit were both scarce in the postbellum period. However, for some areas of the South, this scarcity was not a new phenomenon: banking coverage throughout Georgia and the Carolinas was quite variable in the antebellum period as well. Therefore, if a weak banking sector made the recovery more difficult, we would expect to find larger negative effects in counties where banks were active before the war. The intuition for this test is that the location of a bank can be used as a proxy for places where the manufacturing sector is characterized by high demand for bank lending. Since all banks disappeared after the war, businesses that located closer to banks should have—all else equal—suffered more than businesses that were already unbanked before 1860 if financial constraint was a key driver of the recovery. Given the nature of the decline in bank activity, this test is equivalent to testing whether the shock was larger in counties where banks declined the most by the end of the war. Indeed, in Table A.7, we show that results are similar if we measure the banking shock directly as change in banks between 1859 and 1870.[50]

To test this hypothesis, we collect county-level data on the number of banks in 1859 and we define as more dependent on credit markets those counties that are located closer to a bank in 1859 (Figure A.1). This sorting identifies places where credit markets were more active before the war, and therefore, since all 1859 banks were destroyed by 1864 (Figure 5), where the contraction

[50]To do this, we calculate the growth rate of banks from 1859 to 1870 and split the sample at the median growth rate. This measure captures more directly the heterogeneity across counties in the banking decline after the war. However, as also suggested by Figure 5, this measure may be endogenous to the march itself as post-1864 banking recovery could be a function of the recovery in manufacturing. Therefore, we prefer the main specification based only on the pre-treatment distribution of 1859 banks, not the growth rate of banks.

of credit caused by the march was more costly. If access to credit helps explain the postbellum manufacturing recovery, we expect that counties more dependent on credit should suffer more for a given level of destruction. Importantly, in these specifications we control for industry fixed-effects to control for the fact that the type of business located closer to banks may have been operating in a different industry.

We find that the negative effect caused by the march is much stronger for those counties that were ex ante more dependent on finance (Table 6, top panel). Formally, we first divide the sample into counties that were above or below the median distance to a bank in 1859. We consider counties below the median distance as counties that experienced significant wartime credit disruption.[51] We then estimate manufacturing change regression from equation 2, interacting the march treatment with the indicator of finance access. We find that Sherman's damage on manufacturing was stronger in counties that also experienced the disappearance of a local banking network.

[Figure 5 about here.]

To bolster our mechanism, we present two additional results. First, we use data on the number of firms receiving a credit rating from Dun, Boyd, & Company in 1860 as an alternative way to measure locations in which credit services were frequent. We define as more dependent on credit markets those counties that have firms which appear to have had credit extended according to the 1860 Dun report (Figure A.1). March counties with Dun-tracked firms in 1860 were more adversely affected by the march than march counties without Dun-tracked firms, as shown in the middle panel of Table 6. This regression also demonstrates that when places with more credit relationships external to the firm were damaged, recovery was slower. Hence, the credit channel played an important role in the postbellum rebuilding and can help explain the divergent economic results across march and non-march counties.

Second, we find that the negative effects were much larger when looking at industries generally more dependent on external finance. Following the seminal work by Rajan and Zingales (1998), finance research has documented—across different countries and periods—that industries that de-

[51] In 1859, 51 of the 248 counties in Georgia, North Carolina, and South Carolina contained a bank. The median county centroid distance to an 1859 bank was 23.35 miles. Counties with a bank are considered 0 miles from a bank. Atack and Passell (1994, p. 392) suggest that trips to banks out of county or farther, taking two days, could have been prohibitively costly for small farmers who had "neither the time nor the skills" to negotiate with bankers out of town. Further, these bankers would know little of the small farmers' credit risk or land and might be hesitant to lend.

pend more on external financing to fund investments tend to be more responsive to shock to credit. To overcome the lack of data in our period, we measure dependence on external finance at industry-level using data collected in the 1920s for public and private firms by Nanda and Nicholas (2014).[52] Then, we use this measure to identify the group of industries that are more dependent on external finance, and we test whether the effects of capital destruction were stronger in the set of more dependent industries. In the bottom panel of Table 6, we present our results. When looking at capital, employment, and number of establishments, we find that industries more dependent on external finance experienced a larger decline in affected counties. In economic magnitude, the effect for the high-dependent group is on average double than for the low-dependent group across all the specifications.

[Table 6 about here.]

Our investigation of the mechanisms driving our differential economic results across march and non-march counties indicates that access to credit played an important role, while demographic and infrastructure differences were less crucial. In particular, counties that were more dependent on financing before the war experienced a more difficult recovery, as the war also led to a severe disruption in credit markets. These findings are consistent with the historical literature showing the rapid rebuilding of infrastructure in the postwar period (Atack and Passell 1994) and the importance of credit for manufacturing growth later in American history (Nanda and Nicholas 2014; Ziebarth 2013; Lee and Mezzanotti 2014). Similarly, growth of industries more dependent on external finance was more affected, even five years after the end of the war.[53] Overall, these different tests seem to highlight how financing considerations affected the recovery process in the manufacturing sector. However, the postbellum Southern economy was more agrarian than industrial and banks had a limited role in the agricultural sector. Instead, credit for farmers often came from local large

[52]The data is presented in Table A.4 of the published version of Nanda and Nicholas (2014). Since our industry composition is not coded exactly like the one in their study, we hand-match the two data sets based on industry names and description.

[53]To the extent that Sherman's march directly impaired financial intermediaries, our main estimates may partially capture the negative degree of this direct effect. However, when we examine heterogeneity in Sherman's effects, our results do not rely on an assumption that Sherman had a differential effect on the banking sector because we use antebellum access to banking and credit as our measure of treatment intensity. While it may be the case that banks in Sherman counties recovered slower, as Figure 5 shows, any differential recovery in banking activity across Sherman and non-Sherman counties is second-order compared with the overall drop in banking activity in the South during and after the Civil War, a decline experienced similarly across all counties in our sample.

landowners or local stores. In the next section, we investigate this type of credit in the recovery from Sherman's March.

6.3 Agricultural Recovery and Credit

To provide evidence of the importance of credit-market imperfection for the agricultural sector, we present two results that exploit the institutional characteristics of this sector around the Civil War.

As previously discussed, the agricultural sector was not particularly dependent on banks during this period (Fite 1984; Jaremski and Rousseau 2013; Koudijs and Salisbury 2016; Nier 2007). The consensus among historians is that "banks chose not to provide service to rural areas," and therefore concluded that there was no evidence that "rural banks provided (...) credit to small farmers" (Nier 2007, p. 152). Instead, funding for farmers in the antebellum period came mostly from two alternative sources. First, country stores and local merchants played an important role in providing credit to farmers (Atherton 1949; McCurry 1997; Sparks 1932). The importance of country stores and furnishing merchants did not diminish after the war (Woodman 1999, p. 303-307). In their review of the historical evidence on this subject, Koudijs and Salisbury (2016) document that country stores provided loans to farmers in the form of open accounts that ran for 12 to 15 months. Second, a large part of the funding to medium and small farmers came from local wealthy individuals operating in the agricultural sector (Rajan and Ramcharan 2011; Jaremski and Fishback 2018; Koudijs and Salisbury 2016). Local wealthy elites, like country stores, advanced poorer farmers money on their future crops (McCurry 1997) and, in some cases, also directly loaned money to smaller farmers who wanted to purchase land or improve it (Martin 2010). In the postbellum period, the cotton factor system that supplied many rich landowners with their own loans "whether a novice or a practiced businessman, initially functioned much as he had during the prewar period" (Woodman 1999, p. 254).

Building on these institutional details, we test the importance of financing consideration in explaining the recovery period in two ways, examining the effects of antebellum local wealth and country stores on the recovery process.

First, we test whether the presence of wealthy individuals affected the recovery process. As previously discussed, a high density of wealthy individuals and households should have generated positive externalities by providing smaller farmers with an alternative source of funding in the

absence of formal financial institutions. At the same time, local wealthy individuals were less likely to be financially constrained, which in aggregate may also reduce local financing needs. Though very wealthy planters did occasionally go bankrupt—future South Carolina Governor Wade Hampton is one notable example—small farmers and merchants were much more likely to appear at bankruptcy court (Thompson 2004, p. 73-74). One concern with this approach is that the Civil War may have been particularly costly for the wealthiest Southerners, making pre-war level of wealth in a county not informative about the presence of wealth after the march. However, recent work by Ager, Boustan, and Eriksson (2019) suggests that this is unlikely, with a finding that families who were very wealthy in the South before the war tended to still be high-wealth afterward, with little of the emancipation or war shock transmitted intergenerationally to the sons of the planter elite. Importantly for us, they show that this result holds both generally across the South and specifically in counties directly affected by Sherman.

Therefore, either directly or by providing funding to others, the presence of wealthy individuals could facilitate the funding of reconstruction and reduce the costs related to the lack of external credit in the local area. We start by drawing from the 1850 census, which reports individual level data on wealth, to measure the density of very wealthy individuals in a county. The 1850 census is the only antebellum census for which data on wealth is digitized for the complete population.[54] Using this data, we identify in each county the number of individuals who have wealth in the top 5% of the overall distribution. Then, we defined as counties with high density of wealthy individuals those that are in the top quartile in terms of share of individuals in the top 5% of wealth distribution relative to total number of individuals reporting some wealth. We later discuss several alternatives to this approach. Based on the previous discussion, our hypothesis is that, if financial frictions mattered in the recovery, places with a high share of large landowners should enjoy a better recovery after the large shock to capital of Sherman's march. To test this idea, we augment our standard regression model interacting our treatment dummy and time dummy with the high-wealth

[54]The federal census only asked about wealth only in three decades: 1850, 1860, and 1870. However, the complete count including the wealth questions of neither 1860 nor 1870 have been fully digitized yet. One limitation of the 1850 census is that wealth is defined as only real-estate wealth rather than total wealth. Since we need data only to measure the relative level of wealth concentration across counties, we think this data limitation is unlikely to be important. High land wealth tends to correspond to high non-land wealth. For instance, in 1870 individuals in the top 10% of real estate wealth have a 68% chance of being in the top 10% in personal wealth as well. The overlap between the top of the real estate and personal wealth distributions is similar for other thresholds of wealth, including top 5% or 1%. We construct these numbers using the IPUMS 1% sample of the 1870 census for white men in GA, NC, and SC.

variable. Since the main coefficients of interest will be on a triple interaction, we simplify the time dummies to before and after the march indicators, reporting in the main result in the top panel of Table 7 only the average effect after the march.[55]

In line with our hypothesis, we find that counties in Sherman's path with a large share of wealthy individuals experienced a lower decline in land prices and farm values following the destruction. The same holds for agricultural investments, but in this case the effect is weaker statistically. Furthermore, when looking at the effect by decades, it is clear that this effect is not driven by any differential trend in this group of high-wealth counties within Sherman, but it is the response of these counties to the economic shock (Appendix Table A.5). These differences are also relatively large in magnitude, suggesting that this mechanism can explain a large part of the variation in Sherman's effects. These results are robust to the alternative measures of wealth at county level.[56]

We show that the effects of country stores are similar (bottom panel of Table 7). Country stores were an important source of funding for agriculture. Measuring country stores during this period is difficult because—unlike banks—there is no external data with systematically collected information about them. However, the 1840 census has data on the number of country stores by county (Koudijs and Salisbury 2016). To the extent that their distribution persisted over time, this data can be used to analyze the differential recovery of agriculture across areas that are located inside or outside the country store network. Following the previous set of analyses, we identified counties in which small farmers had significant access to country stores by looking at those counties on the top quartile of the distribution of country stores in the data. Then, we test whether these counties experienced a lower decline in economic activity caused by Sherman's march and capital destruction. Across the different outcomes, we find that counties with more country stores experienced a smaller decline in agricultural activity and land investment than places with fewer. The magnitude suggests that essentially all the negative effects of capital destruction were concentrated in counties with a smaller supply of country stores measured in 1840. Since country stores were crucial to providing working capital financing to agriculture, this evidence is consistent with the view that lack of financing

[55]Since we are collapsing the time dimension to only pre vs. post, this model is estimated only using 1850–1890. The results are similar with the full sample, as the results by decade in Table A.5 suggest.

[56]In particular, we find that using a more extreme definition of wealthy individual—top 2%—provides very similar results in the top panel of Table A.6. In the bottom panel of Table A.6, we show that the results are qualitatively identical if we define wealth density relative to the overall white population rather than only individuals with some wealth in the census.

played an important role in the recovery process. Our results echo an 1880 census report on cotton production that found "the importance of the country stores in financing and marketing the cotton crop... With few exceptions, the furnishing merchant system prevailed in the cotton areas of every Southern state" (Woodman 1999, p. 307).

[Table 7 about here.]

Overall, this section has provided evidence for the importance of a lack of financing to explain the initial severity of capital destruction and its immediate recovery. In principle, this is a reasonable hypothesis given that financial markets were not well developed in the South in the nineteenth century and they deteriorated further after the Civil War. This section has provided three tests that highlight the importance of financing considerations to explain the recovery from the shock. First, we have shown that alternative leading explanations for a lack of recovery in the medium and long term—a demographic shift or lack of infrastructure—do not appear to be confirmed by data. Second, we have provided direct evidence of the importance of credit for variation in the manufacturing downturn. In particular, we have shown that growth in manufacturing was especially affected by Sherman's march in places where credit was more extensive prior to the Civil War and in industries more dependent on external finance. Lastly, we have shown that the agricultural downturn was much more severe in counties without a high density of wealthy individuals or country stores, two important sources of external capital for the agricultural sector in the South at this time. While none of these tests are perfect, combined these analyses provide convincing evidence that credit-market imperfections are responsible for part of the severe devastation in the medium-run following the march.

7 Persistent Effects of Sherman's March

Our results suggest that the capital shock caused by the march led to a significant medium-run contraction in both the manufacturing and agricultural sector. Did these effects persist over the long run? In this section, we extend the time dimension of our main analyses.

Within the agricultural sector, we find that the county-level shock is associated with negative effects. However, these effects are more precisely estimated and larger in magnitude for our measure of investment in agricultural land. In particular, the initial decline of about 15% in the share of

improved land remains extremely stable in magnitude and in statistical significance at least until 1920 (Figure 6). For the other agricultural outcomes, while we find that the negative effect of the march significantly declines between 1870 and 1880, our estimated coefficients remain negative and still sizable in magnitude—at least without controls—but generally non-significant.

[Figure 6 about here.]

At the same time, we find no evidence of persistence in the manufacturing sector. As we show in the second panel of Table 4, differences between affected counties declined dramatically by 1880. This result suggests that while manufacturing contracted in the medium-run, over a longer horizon the effect of the march on manufacturing was small.

Taking our agricultural and manufacturing results together, these differences in persistence suggest two main takeaways. First, even when the overall economy suffers in the short- or medium-run, different sectors may experience different long-term responses. In this case, while some elements of the shock persisted for agriculture, there is no evidence of the same persistence for manufacturing. In part, these differences may stem from the different stage of development across sectors. While the agricultural sector was a mature industry at the time, manufacturing was still at a more infant stage in the South. Manufacturing grew dramatically in the postbellum period; after declining from \$71M in 1860 to \$57M in 1870, Southern manufacturing and mining output grew to \$100M in 1880 (Engerman 1966).

Second, even when economic activity recovers, some elements of the shock may still be evident years later. Within the agricultural sector, asset prices appear to have completely recovered at some point between 1880 and 1900.[57] At the same time, improved land remained significantly lower at least until 1920. One hypothesis that may rationalize these results is that the recovery from the shock led to a change in the organization of agriculture. This change in agricultural institutions could then influence the way inputs are used, both in the short run and—to the extent that changes in institutions tend to be sticky over time—the long run, even if the underlying economic damage of the march was mostly in the short run.

In particular, we focus on the rise of farm tenancy and sharecropping during the postbellum period. After the Civil War and emancipation, the organization of southern agriculture was in

[57]The magnitude of the estimates becomes smaller and smaller as we arrive at the turn of the century, suggesting a complete recovery.

flux. New labor arrangements used in the late 1860s through 1880 included gang labor, squad labor, and family-based labor, as well as cash tenancy, share tenancy, and sharecropping (Ransom and Sutch 2001; Shlomowitz 1979; Alston and Higgs 1982; Bloome and Muller 2015; Wright 1986). Eventually, though not uniformly throughout the South, a system of tenancy emerged as the main labor and land market in the postbellum South (Shlomowitz 1979; Fite 1984).[58] Our argument is that the capital destruction caused by the shock may have magnified the shift of agriculture toward sharecropping and tenant farming and away from owner-managed land.

How does tenancy fit with the main takeaways above? First, as we have emphasized earlier, the recovery from the march was hindered by a general scarcity of capital. With capital scarce, new land and labor institutions like sharecropping and tenancy could be optimal for land-owners trying to overcome financial constraints in the postbellum period.[59] Ransom and Sutch (2001), Woodman (1977), Alston and Higgs (1982), and other historians and economic historians of the postbellum South have all pointed to financial constraints as one explanation for the rise of tenancy and sharecropping in this period.[60] Jaynes (1986, p. 50) puts it clearly:

> What we would expect is that regions facing the most severe credit constraints would have a high incidence of share payments. Planters not especially constrained by credit rationing would therefore never adopt fully deferred money wage contracts ... This thesis strongly implies that the incidence of full postharvest and specifically share payments

[58] Sharecropping and tenancy were not brand-new in the postbellum period (Reid 1976) but they were rare before emancipation. Tolnay (1999) describes three main forms of tenants: cash tenants rented land by paying in cash, share tenants rented land and paid with a share of crop yield, and sharecroppers rented land, farm equipment, and animals and paid with a larger share of crop yield. Following recent work on tenancy and sharecropping, we consider cash tenancy, share tenancy, and sharecropping as one broader category of labor and land arrangement. We do this for three reasons. First, as Alston and Kauffman (1997) note, in some years enumerators at the Census of Agriculture seem to have confused their own definitions and misclassified farms between categories. Second, rates of tenancy and sharecropping are correlated at the county-level and the effects of tenancy on other outcomes are similar; for example, see Bloome and Muller (2015); Bloome, Feigenbaum, and Muller (2017) on the effects of tenancy on marriage. Third, and most important, while each arrangement varied, all had the same structure of land owners outsourcing the management of parts of their land to farm tenants and being paid only at harvest, in contrast to a wage labor system with overseers or owner-managers.

[59] Tenancy may have been an optimal solution for financially constrained land-owners, in either the short or long run, but it may have, overall, increased constraints on the postbellum Southern agricultural economy. According to Shlomowitz (1979), even landowners paying wages were sometimes forced to write contracts with uneven payment schedules, waiting until after harvest to settle accounts for workers who were supposed to be paid a regular wage.

[60] Shlomowitz (1979) and others argue that sharecropping developed and evolved according to postbellum market forces as landowners, planters, freed African Americans, and other southern laborers negotiated the post-emancipation agricultural economy. Beyond financial constraints, there are other arguments for the rise of sharecropping in this era. Share contracts may ensure full-year farm labor for planters in a way that (breakable) wage contracts would not. In addition, sharecropping contracts shift risk—though the shift is likely to be away from landowners and towards croppers. Finally, Shlomowitz (1979) emphasizes monitoring advantages of sharecropping and tenancy compared to wage farming.

should have been highest in regions where the devastation of property was greatest.

Second, the reallocation of land from owner-managed farms to tenants may explain the larger share of land left unimproved in these counties. Both owners and tenants may be less likely to invest in improvements, either because tenants had only a temporary claim on the land and were generally poor or because monitoring the upkeep or depreciation of costly improvements would be expensive for owners not actively managing the farm.

As first step, we document that tenancy was mostly concentrated in smaller farms. According to the 1880 Census of Agriculture, which is the first census to record information on tenancy, farms under 50 acres were much more likely to be operated by a sharecropper or tenant farmer—rather than by the land owner—in Georgia, South Carolina, and North Carolina (Figure 7).[61] Approximately 75% of all small (under 50 acres) farms were sharecropped or tenant farmed in the Sherman states. Above this size threshold, the vast majority of farms were owner managed.[62]

[Figure 7 about here.]

If Sherman's March led to relatively higher adoption of tenancy, we would expect to find a large increase in the number and share of farms that were smaller than 50 acres in Sherman counties after the Civil War. To examine this question, we return to our main empirical specification, looking at changes in the number of farms of different sizes.[63] We consider both the log number of farms of each size and the share of farms of each size. Results are reported in Figure 8.

Overall, we find that Sherman's March increased the number of farms of less than 50 acres, at the same time reducing the presence of larger farms. This change in the distribution of farms is driven in part by changes in farm organization around the center of the size-distribution. The largest increase is of farms between 10 and 49 acres. On the other side, the contraction was particularly large among farms between 100 and 499 acres. These patterns are consistent with the distribution of tenancy

[61] In this subsection, we make use of data from the Census of Agriculture on farm sizes. The data collected by the census indicate the size of farms as operated not as owned. That is, if a land owner with 1000 acres rents the land out in 10-acre farms to 100 tenant farmers, the census will record this as 100 10-acre farms. For more detail, see Virts (1987). Specifically, this makes the farm size distribution data an excellent resource for understanding the use of the land in each census year but it does not allow us to assess inequality in land ownership or wealth with much confidence.

[62] In 1890, we find a similar distribution of size across tenant and owner-managed land. However, 1880 and 1890 are the only years in which the Census of Agriculture collected data on the distribution of farm sizes according to ownership or tenancy status.

[63] We are slightly limited by the data in assessing the pre-treatment period: the Census of Agriculture collected information on the farm size distribution starting in 1860, not in 1850 as with our other analyses.

shown in Figure 7. Overall, we see in Figure 8 that the distribution of farm sizes shifted toward the commonly tenant-farmed or sharecropped sizes. These relative changes in farm size in Sherman and non-Sherman counties were occurring in the context of changes in farm sizes throughout the South (Reid 1973).

The changes in farm size are consistent with our argument that the destruction from Sherman's March led to more sharecropping and tenancy. However, this analysis cannot be used to directly quantify the extent to which capital destruction may have contributed to the rise in tenancy. The key issue is that the Census of Agriculture only began collecting counts of farms or acres in tenancy or sharecropping in 1880. This is not surprising: sharecropping and tenant farming were exceedingly rare in the antebellum period, though they did exist in some areas (Reid 1976). Therefore, a specification that directly estimated the impact of the march on tenancy cannot be estimated under similar assumptions. Despite this limitation, the evidence in this section confirms that the shock was followed by a change in the way agriculture was organized, which may have persisted in the long run.

[Figure 8 about here.]

As a last step, we examine whether this shift in farm size distribution led to any significant shift in land inequality. This analysis is useful for two reasons. First, understanding the overall effect on inequality in land can provide useful information to evaluate the welfare impact of this change. Second, as we discuss more in detail below, this analysis can be used to provide indirect evidence that our previous evidence was not driven by differential trends in farm concentration across counties. To measure land inequality, we construct an index of farm size inequality as in Nunn (2008). This index is a Gini coefficient based on the distribution of farm size by acreage.[64]

[Table 8 about here.]

Using the usual specification, we find that farm size inequality increased substantially after Sherman's march in march counties relative to non-march counties (Table 8). Compared with the 1860 difference in inequality across march and non-march counties, the 1870–1890 differences were

[64]Farm size is categorized in seven bins. The farm size bins are, in acres, 0 to 9, 10 to 19, 20 to 49, 50 to 99, 100 to 499, 500 to 999, and over 1,000. We construct two Gini coefficients based on these data, one setting each farm to the median of each bin and another setting each farm to the minimum of each bin. For the largest bin size, we use 1000 in both cases.

between 5% and 19% larger. Farm sizes became economically significantly more concentrated and unequal in response to the Civil War's capital destruction. This evidence is consistent with the previous evidence on farm-size distribution, which has shown that the shift in farm size was mostly concentrated in the middle of the distribution, leaving almost unaffected both very large and very small farms, therefore increasing inequality.

This analysis allows us to provide some indirect evidence on the lack of pre-trend before 1860. Though we do not have information on farm sizes in 1850, we are able to construct a different proxy for inequality in the prebellum period and show that there are no differential pre-trends across Sherman and non-Sherman counties. We construct a slave-holding Gini index, which should still capture variation in wealth that is related to concentration, from data collected in both 1850 and 1860. Consistent with this claim, the residualized (for population, latitude and longitude, county size, and state fixed effects) correlation between the two measures in 1860, the only year in which both exist, is 0.34, which suggests that the latter is a reasonable proxy measure of the former in 1850.[65] In the fifth and sixth columns of Table 8, we show that the slave-holding measure indicates that inequality was not trending differentially prior to Sherman across march and non-march counties. This effect is both small in magnitude and statistically non-significant. This evidence suggests that the changes in farm distribution that are discussed in this section are not likely to stem from differential trends in farm-size before the war.

8 Conclusion

When General William Sherman began his march in Atlanta in August 1864, he sought to "make old and young, rich and poor, feel the hard hand of war." He and his men foraged for 300 miles through Georgia, North Carolina, and South Carolina and "enforce[d] devastation" on the rebel states, "aveng[ing] the national wrong [Southerners had committed by] dragging [the] country into civil war."[66]

By the time Sherman received the surrender of Confederate General Joseph E. Johnston at Bennett Place in North Carolina in April 1865, the Union general and his soldiers had wreaked

[65]The slave-holding Gini is constructed similarly, using 21 bins counting the number of slave-holdings of each certain size in terms of slaves. The slave-holding bins are 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 to 14, 15 to 19, 20 to 29, 30 to 39, 40 to 49, 50 to 69, 70 to 99, 100 to 199, 200 to 299, 300 to 499, 500 to 999, and over 1,000. In 1850, the slave-holding data are from a full sample of the 1850 slave census that we constructed. In 1860, we draw the slave-holding data from Haines (2010).

[66]Letter from Sherman to Halleck, December 24, 1864 in US War Department (1901).

significant economic damage on parts of the Confederacy. Nor did this damage end at the conclusion of the war. Both agricultural and manufacturing outcomes fell significantly in march counties relative to nearby non-march counties in 1870. In this context, we argue that the lack of credit in the postwar American South explains part of the slow recovery following the war. Furthermore, we show that agricultural investment remained persistently lower in counties destroyed by Sherman for more than a half-century and changes to the organization of agriculture and the distribution of land persisted as well. Our results suggest that large capital shocks may persistently shift the way economic activity is organized even after overall agriculture activity recovers.

Capital destruction caused by war or natural disaster is a frequent event in human history. It is important to understand its consequences—in both the short and long run—and the factors that affect the recovery. This paper has shown that the economic costs of large property and infrastructure destruction can be substantial and persistent. Furthermore, we have highlighted the importance of a developed financial sector to reduce such effects. Examining whether these effects are confined to total war or to infrastructure-focused campaigns like Sherman's March, or could be the consequence of more traditional military-centered battles, is an area for future research. Similarly, it may be that the non-economic long-term effect of total war campaigns—on outcomes like political beliefs or trust—is large and persistent. Lastly, future research can explore various policies that can be put in place to create a more effective and efficient reconstruction.

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Table 1: Sherman Target County Characteristics in 1860

	County Means		Difference	
	March	Non March	Unconditional	Conditional on State
Demographics				
Population (000s)	13.12	10.76	2.36**	0.47
	(7.69)	(7.85)	(1.19)	(0.95)
Density (000s per sq mile)	22.31	21.80	0.52	0.31
	(10.20)	(10.64)	(1.60)	(1.67)
Average Age	18.28	18.21	0.07	0.14
	(0.81)	(1.13)	(0.16)	(0.14)
Share Urban Population	1.94	2.08	-0.13	-0.22
	(8.66)	(9.49)	(1.41)	(1.47)
Agriculture				
Farm Value ($M)	2.54	1.68	0.85***	0.34*
	(2.16)	(1.41)	(0.25)	(0.18)
Agricultural Output (per sq mile)	1496.02	1287.01	209.01*	166.69
	(748.65)	(832.36)	(123.73)	(127.52)
Percent Improved Farm Land	32.31	28.97	3.34	3.48
	(15.18)	(13.32)	(2.11)	(2.19)
Slaves per Farm	12.75	8.91	3.84**	1.94
	(8.78)	(10.61)	(1.55)	(1.53)
Cotton Intensity	2.48	1.63	0.85***	0.56*
	(1.62)	(2.13)	(0.31)	(0.29)
Plantation County	0.45	0.18	0.27***	0.22***
	(0.50)	(0.39)	(0.06)	(0.06)
Area (sq miles)	667.09	535.81	131.28***	59.83
	(403.02)	(286.79)	(48.85)	(41.11)
Manufacturing				
Manufacturing Value Added (000s)	65.81	68.12	-2.31	-7.46
	(98.01)	(110.61)	(16.39)	(16.60)
Manufacturing Establishments	36.17	26.26	9.91	12.21
	(74.36)	(38.15)	(7.62)	(7.48)
Total Employment in Manufacturing	133.50	134.89	-1.39	-9.09
	(203.56)	(184.47)	(28.89)	(28.48)
Infrastructure				
County Close to Canal or River	0.71	0.59	0.12	0.12
	(0.46)	(0.49)	(0.07)	(0.08)
Railroad Density (miles per sq mile)	3.05	1.79	1.26***	1.05**
	(3.15)	(2.57)	(0.42)	(0.43)
Observations	58	167	225	225

Notes: Columns 1 and 2 report unconditional means of county characteristics in march and non-march counties, with standard deviations in parentheses below. Columns 3 and 4 report the difference between the means, unconditionally in Column 3 and conditional on state fixed effects in Column 4, with standard errors in parentheses below. The Agricultural data from the US Census of Agriculture, 1860. Manufacturing data from the US Census of Manufactures, 1860. Demographic data are from the US Census of Population, 1860. All agricultural data are measured per square mile in each county. A plantation county is a county that is in the top quartile in share of farms with more than 100 slaves. Percent Improved Farm Land is the share of farm land that is improved. Cotton Intensity is the amount of cotton output in 1860 per 100 acres of total farm land. County close to canal or river are counties that are closer than the median distance in our sample to either a canal or river. More information is provided throughout the draft about these variables when used.

Table 2: Differences in Agricultural Outcomes Relative to 1860 Difference, by Sherman March Exposure, 1850-1920

	Log Value of Farms		Log Value of Livestock		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x 1850	0.042	0.011	0.034	0.018	0.067	0.022
	(0.059)	(0.079)	(0.033)	(0.039)	(0.044)	(0.058)
Sherman x 1870	−0.197**	−0.194**	−0.139***	−0.128**	−0.148**	−0.126**
	(0.077)	(0.077)	(0.050)	(0.057)	(0.061)	(0.063)
Sherman x 1880	−0.040	0.049	−0.033	0.017	−0.135***	−0.093**
	(0.059)	(0.053)	(0.037)	(0.030)	(0.046)	(0.044)
Sherman x 1890	−0.076	0.028	−0.103**	−0.047	−0.157***	−0.120***
	(0.075)	(0.070)	(0.045)	(0.045)	(0.048)	(0.045)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	1123	1123	1123	1123	1123	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.000	0.025	0.000	0.018	0.025	0.017

Each column is a separate county-year level regression of the indicated agricultural outcome on an indicator equal to one if the county is within five miles of Sherman's march, interacted with the displayed decade indicators, plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march and all decades, 1850-1920. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3: Differences in Manufacturing Outcomes Relative to 1860 Difference, by Sherman March Exposure, 1850-1880

	Log Employment		Log Capital		Log Establishments		Log Value of Production	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman x 1850	0.018	0.012	−0.005	−0.096	0.000	0.000	0.091	0.041
	(0.192)	(0.197)	(0.209)	(0.219)	(.)	(.)	(0.225)	(0.221)
Sherman x 1870	−0.213	−0.127	−0.346**	−0.412***	−0.133	−0.142	−0.218	−0.177
	(0.146)	(0.135)	(0.155)	(0.150)	(0.175)	(0.157)	(0.175)	(0.170)
Sherman x 1880	−0.022	−0.011	−0.260	−0.258	−0.090	−0.059	−0.156	−0.008
	(0.182)	(0.162)	(0.167)	(0.170)	(0.171)	(0.152)	(0.184)	(0.162)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes	No	Yes
Observations	845	845	844	844	640	640	844	844
Clusters	224	224	224	224	224	224	224	224
Adjusted R^2	0.032	0.015	0.011	0.004	0.031	0.029	0.000	0.039

Each column is a separate county-year level regression of the indicated manufacturing outcome on an indicator equal to one if the county is within five miles of Sherman's march, interacted with the displayed decade indicators, plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march and all decades, 1850-1880. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 4: Change in Manufacturing Outcomes, by Sherman March Exposure, 1860-1880

	Change in Manufacturing Outcomes from 1860 to 1870							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.863***	-1.391***	-0.486**	-0.830***	-1.038***	-2.065***	-0.455***	-0.672***
	(0.312)	(0.507)	(0.199)	(0.271)	(0.383)	(0.711)	(0.137)	(0.195)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.002	0.030	0.005	0.044	0.004	0.052	0.007	0.044
	Change in Manufacturing Outcomes from 1860 to 1880							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.075	-0.072	0.406	0.309	0.104	-0.122	-0.004	-0.137
	(0.321)	(0.351)	(0.372)	(0.390)	(0.524)	(0.599)	(0.171)	(0.187)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.026	0.045	0.033	0.049	0.015	0.026	0.089	0.117

Each column is a separate county-industry level regression of the percentage change between 1860 and 1870 in the column indicated manufacturing outcome on an indicator equal to one if the county is within five miles of Sherman's march plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all reported industries in all counties within 100 miles of the march. The sample is unbalanced because not all industries are present in all counties. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 5: Demographic and Infrastructure Outcomes, by Sherman March Exposure, 1850-1920

	Population		Black Share		Log Railroad Miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x 1850	0.051*	0.047	0.048	0.071	0.307	0.360
	(0.028)	(0.037)	(0.045)	(0.054)	(0.214)	(0.271)
Sherman x 1870	-0.024	-0.030	-0.003	-0.003	0.048	0.031
	(0.019)	(0.021)	(0.032)	(0.036)	(0.166)	(0.137)
Sherman x 1880	0.021	0.015	0.057	0.037	-0.022	-0.059
	(0.023)	(0.024)	(0.040)	(0.042)	(0.182)	(0.149)
Sherman x 1890	-0.047	-0.052	-0.034	-0.075	-0.051	-0.020
	(0.042)	(0.043)	(0.062)	(0.064)	(0.203)	(0.167)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	1123	1123	1123	1123	688	688
Clusters	225	225	225	225	198	198
Adjusted R^2	0.044	0.038	0.031	0.037	0.033	0.013

Each column is a separate county-year level regression of the indicated demographic or infrastructure outcome on an indicator equal to one if the county is within five miles of Sherman's march interacted with decadal indicators plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; and railroad miles. To flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 6: Change in Manufacturing Outcomes from 1860 to 1870, by Sherman March Exposure and Finance Access

	Bank Status							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	0.130	-0.158	0.155	-0.067	0.148	-0.585	-0.073	-0.228
	(0.350)	(0.495)	(0.223)	(0.303)	(0.377)	(0.599)	(0.088)	(0.186)
Bank County	1.734***	1.487***	0.986***	0.676***	1.851**	1.203**	0.868***	0.604***
	(0.610)	(0.445)	(0.324)	(0.230)	(0.885)	(0.589)	(0.241)	(0.171)
Sherman x Bank	-1.755***	-1.772**	-1.132***	-1.167***	-2.094***	-2.191**	-0.677***	-0.640**
	(0.582)	(0.795)	(0.347)	(0.422)	(0.724)	(1.099)	(0.234)	(0.298)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.016	0.049	0.019	0.061	0.015	0.068	0.027	0.059

	Dun, Boyd, and Company Status							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.493*	-0.807**	-0.297*	-0.528**	-0.454	-1.129**	-0.296***	-0.433***
	(0.272)	(0.389)	(0.165)	(0.211)	(0.297)	(0.473)	(0.111)	(0.144)
Any DB firms	5.142**	4.870***	3.117***	2.723***	6.974**	6.235**	2.601***	2.231***
	(2.241)	(1.707)	(1.061)	(0.771)	(3.477)	(2.549)	(0.793)	(0.636)
Sherman x Any DB firms	-3.739	-3.985	-1.910*	-2.193*	-5.931*	-7.407*	-1.602*	-1.635*
	(2.297)	(2.535)	(1.109)	(1.200)	(3.502)	(3.862)	(0.820)	(0.952)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.053	0.075	0.065	0.088	0.070	0.106	0.081	0.092

	External Finance Dependence by Industry							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.606*	-1.141**	-0.301	-0.650**	-0.658*	-1.689**	-0.354**	-0.583***
	(0.358)	(0.509)	(0.227)	(0.283)	(0.371)	(0.671)	(0.137)	(0.192)
Sherman x High Fin. Dep.	-0.953*	-0.926*	-0.688**	-0.664*	-1.413**	-1.389**	-0.373	-0.331
	(0.533)	(0.546)	(0.331)	(0.338)	(0.676)	(0.694)	(0.232)	(0.234)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.002	0.030	0.006	0.045	0.005	0.053	0.007	0.044

Each column is a separate county-industry-year level regression of the change from 1860 to 1870 in the indicated manufacturing outcome on the displayed interaction terms, fixed effects, and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. More info on variables are in the text where we discuss the main specification. DB firms refers to the number of Dun, Boyd, and Company-tracked firms in the county as of 1860. The sample is all counties within 100 miles of the march. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 7: Agricultural Outcomes, by High Wealth, Country Stores, and Sherman March Exposure, 1850-1890

	Antebellum Local Wealth Density: Share in Top 5%					
	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x Post	-0.215***	-0.127*	-0.170***	-0.138***	-0.165***	-0.138***
	(0.077)	(0.071)	(0.051)	(0.043)	(0.050)	(0.048)
High Wealth 1850 x Post x Sherman	0.241**	0.201**	0.180**	0.193***	0.030	0.018
	(0.105)	(0.100)	(0.076)	(0.058)	(0.103)	(0.096)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	898	1123	898	1123	898	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.048	0.009	0.031	0.004	0.017	0.005
	Local Country Store Status					
	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x Post	-0.192***	-0.108*	-0.189***	-0.141***	-0.267***	-0.204***
	(0.066)	(0.064)	(0.044)	(0.041)	(0.056)	(0.047)
High Store 1840 x Post x Sherman	0.252**	0.218*	0.299***	0.272***	0.323***	0.269***
	(0.109)	(0.111)	(0.076)	(0.070)	(0.086)	(0.080)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
High Store X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	1123	1123	1123	1123	1123	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.005	0.001	0.011	0.001	0.030	0.001

Each column is a separate county-year level regression of the indicated agricultural outcome on an indicator equal to one if the county is within five miles of Sherman's march, interacted with an indicators for post 1860 decades and a dummy for high density of High Wealth Individuals in 1850, plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march and all decades, 1850-1890, as discussed in the paper. The dummy for High Wealth is equal to one for those counties that are in the top quarter in terms of share individuals in the top 5% of the overall wealth distribution in 1850. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 8: Farm Concentration by Sherman March Exposure, 1850-1920

	Farm Size Gini				Slave Holding Gini	
	Median Bin		Minimum Bin		Median Bin	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x 1850					-0.006	0.002
					(0.008)	(0.009)
Sherman x 1870	0.060**	0.050*	0.089***	0.077**		
	(0.026)	(0.026)	(0.032)	(0.030)		
Sherman x 1880	0.138***	0.068***	0.187***	0.098***		
	(0.031)	(0.021)	(0.047)	(0.028)		
Sherman x 1890	0.126***	0.063***	0.172***	0.087***		
	(0.030)	(0.021)	(0.045)	(0.027)		
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	No	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	898	898	898	898	412	412
Clusters	225	225	225	225	225	225
Adjusted R^2	0.05	0.26	0.08	0.22	0.01	0.11

Each column is a separate county-year level regression of the indicated inequality measure on an indicator equal to one if the county is within five miles of Sherman's march interacted with decadal indicators, plus the noted fixed effects and controls. The sample is all counties within 100 miles of the march. The gini measure in columns 1 to 4 is constructed following Nunn (2008) and uses farm size data from the Census of Agriculture, 1850-1920. The gini measure in columns 5 and 6 is constructed using the same procedure, except using slaveholdings rather than farm size. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

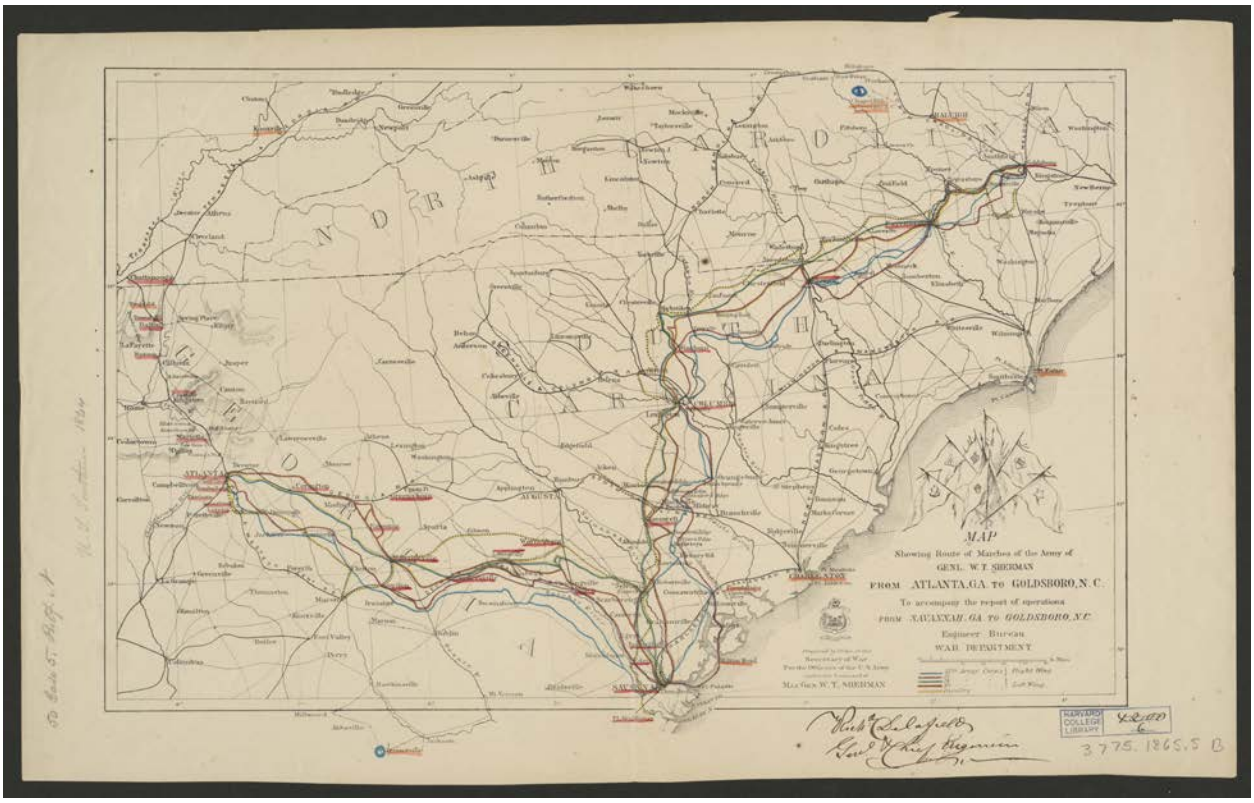


Figure 1: Sherman's March, War Department Map

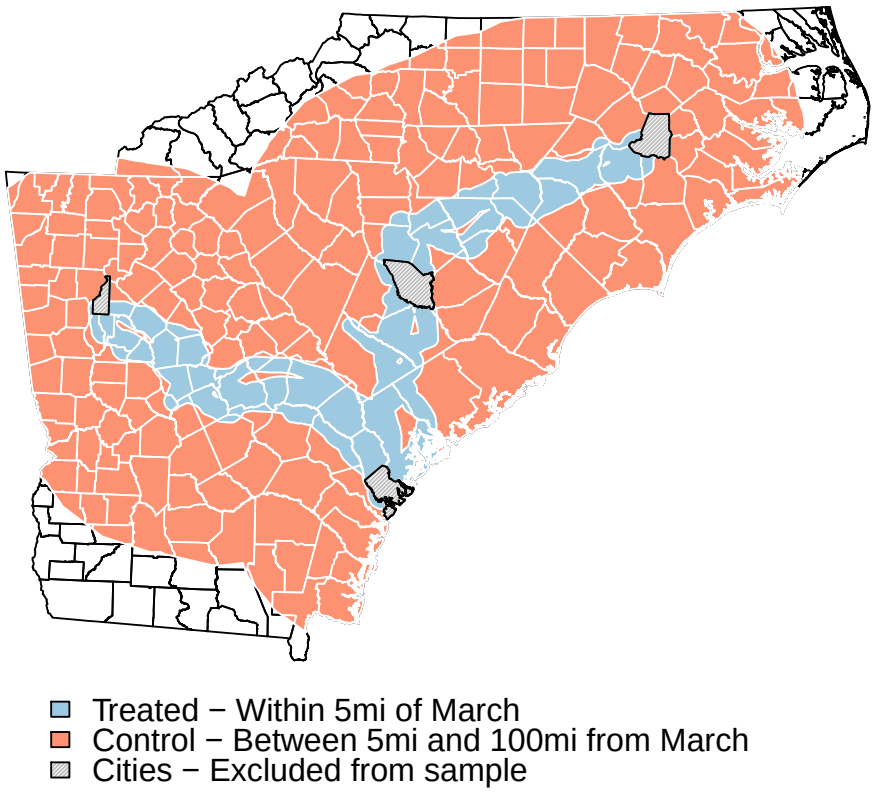


Figure 2: Sherman's March and 1860 County Boundaries. Based on the War Department Map in Figure 1. The vertex cities on the march are excluded from our analysis: Atlanta (captured September 2, 1864), Savannah, GA (December 10, 1864), Columbia, SC (February 17, 1865), and Goldsboro, NC (March 23, 1865).

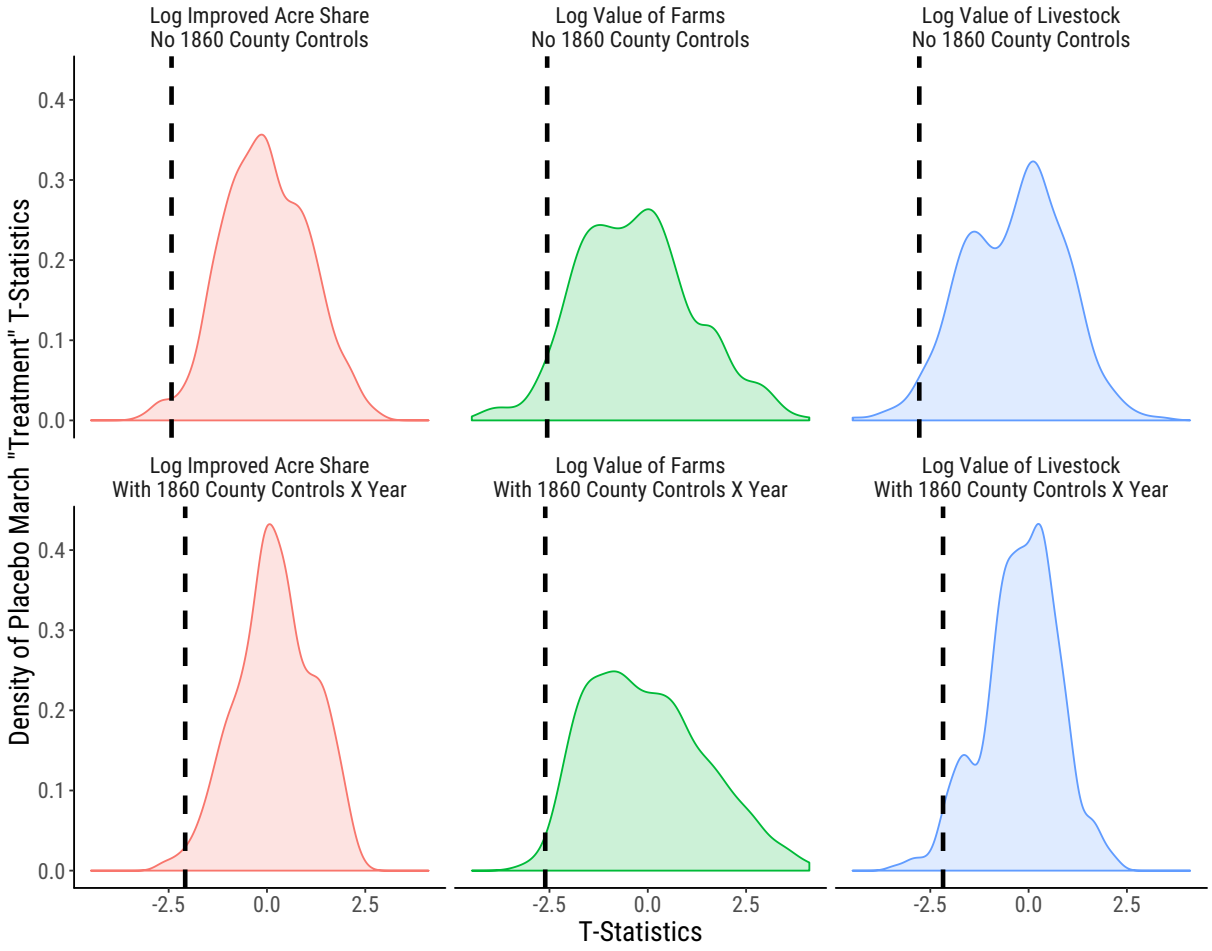
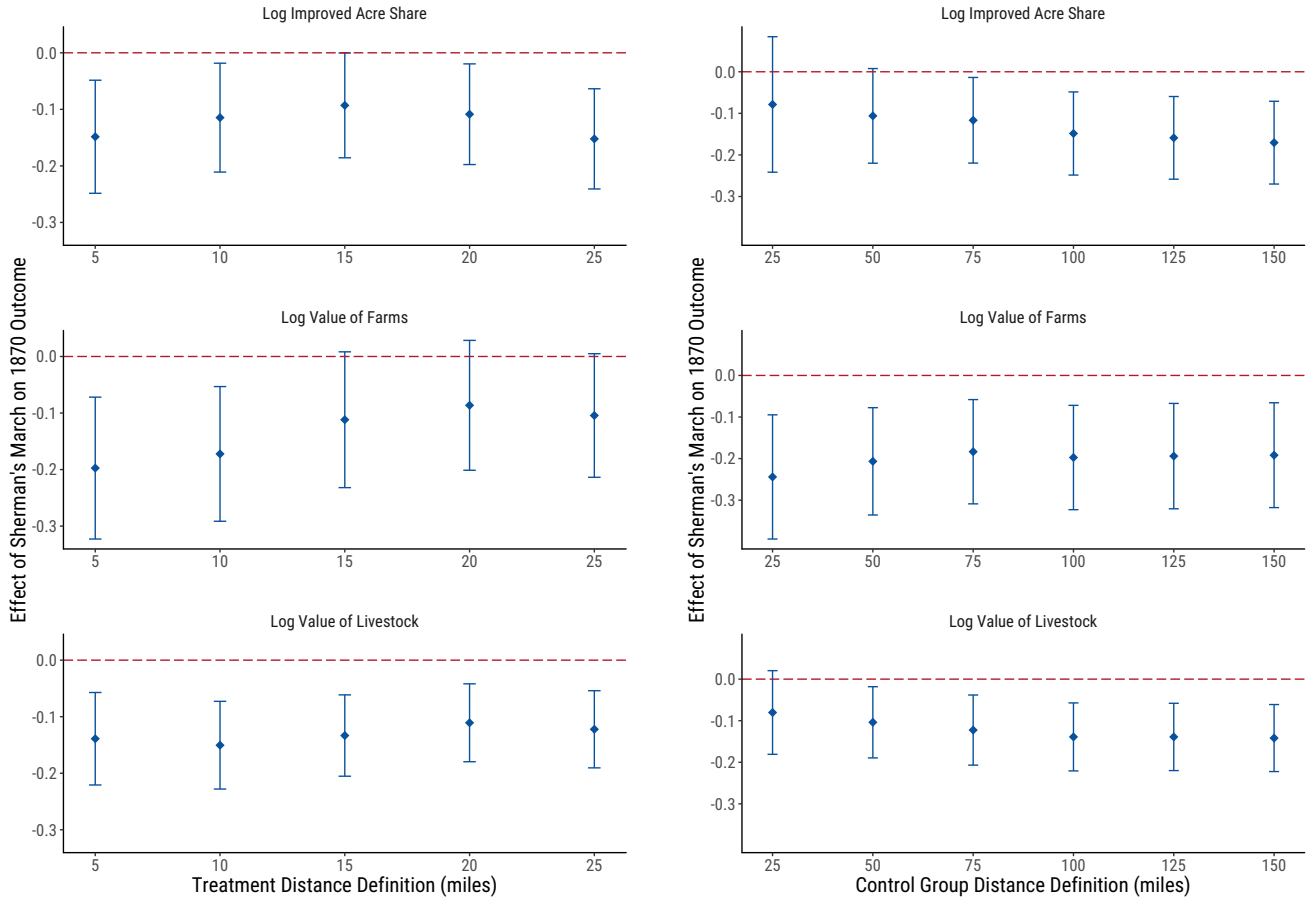


Figure 3: Estimated effects of 852 placebo march paths between triples of Southern cities on 1870 outcomes. The t-statistics from Sherman's March are indicated with the dashed vertical line. Fewer than 5% of the t-statistics from the placebo marches are as negative as the estimated Sherman effects. Placebo marches are build by connecting three Southern cities (defined as counties with more than 2000 urban residents in 1860) by paths between 100 and 300 miles, mimicking the sizes and distances between Atlanta, Savannah, and Columbia where Sherman actually marched. The t-statistics presented are from a regression replicating Equation 1 for each placebo march.



(a) Sherman's effect across alternative treatment definitions

(b) Sherman's effect across alternative control definitions

Figure 4: Alternative treatment and control definitions

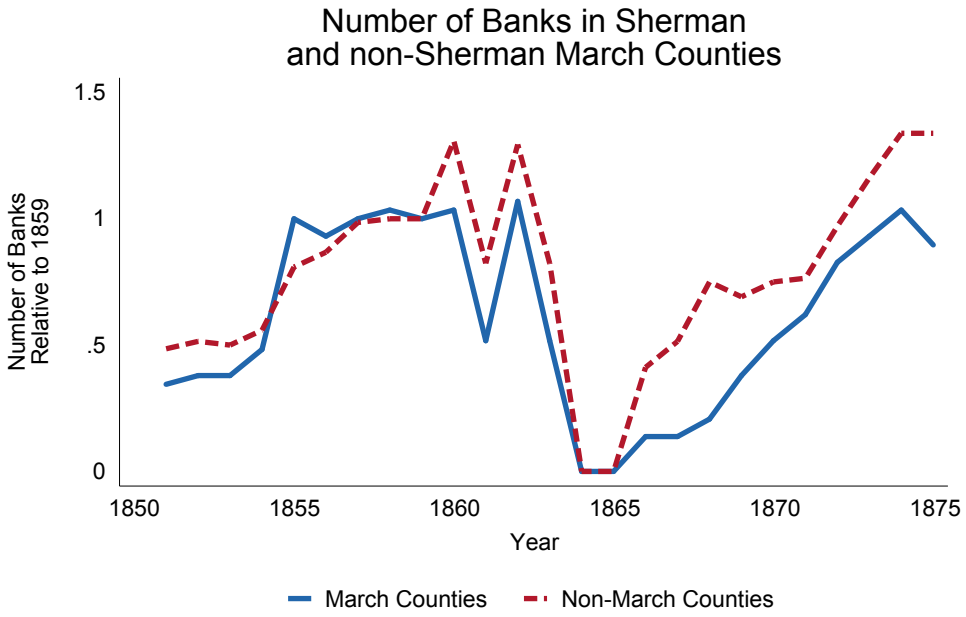
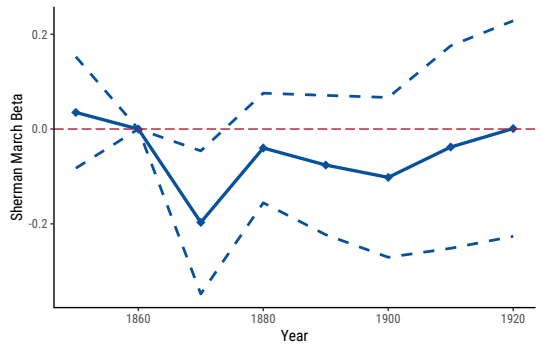
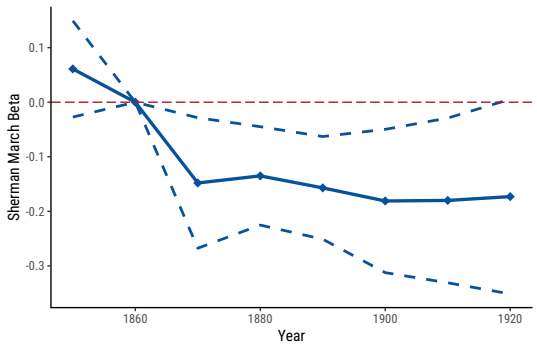


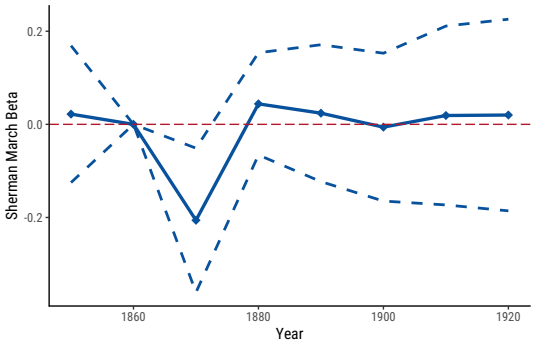
Figure 5: The number of banks in Sherman's march and non-march counties was comparable before the Civil War. After the war, banks may have recovered slightly faster in non-march counties.



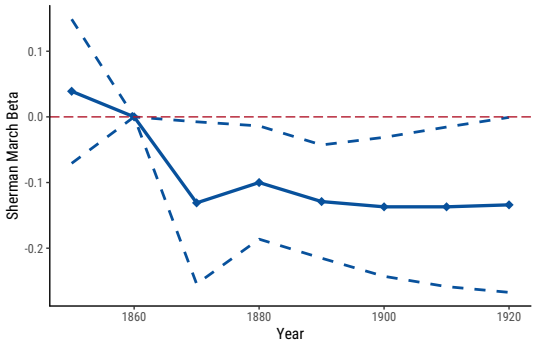
(a) Log Value of Farms



(b) Log Improved Acre Share



(c) Log Value of Farms with 1860 County Controls X Year



(d) Log Improved Acre Share with 1860 County Controls X Year

Figure 6: Difference in Log Value of Farms and Improved Acre Share, by Sherman March Exposure, 1850-1920

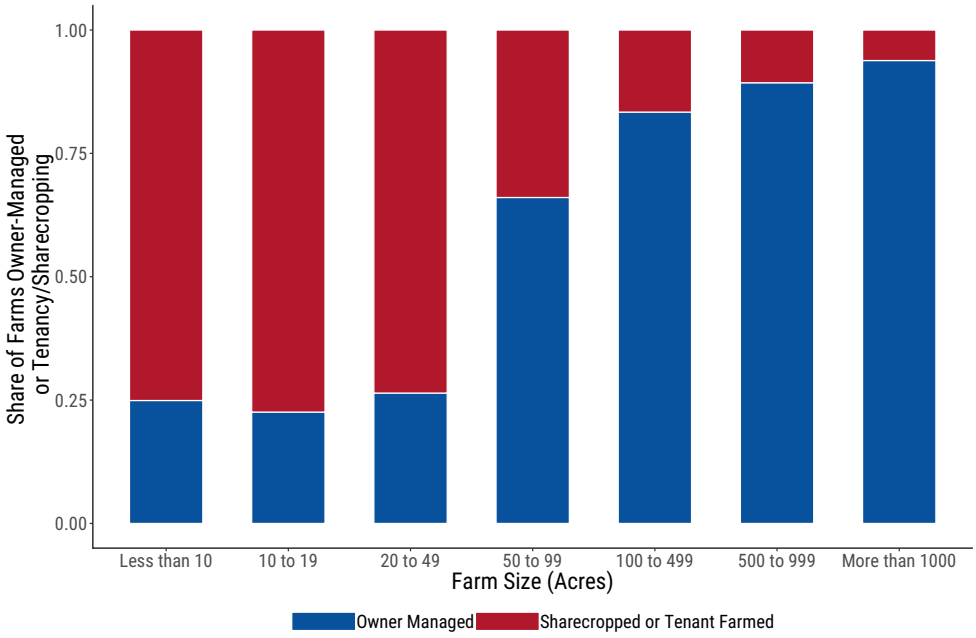
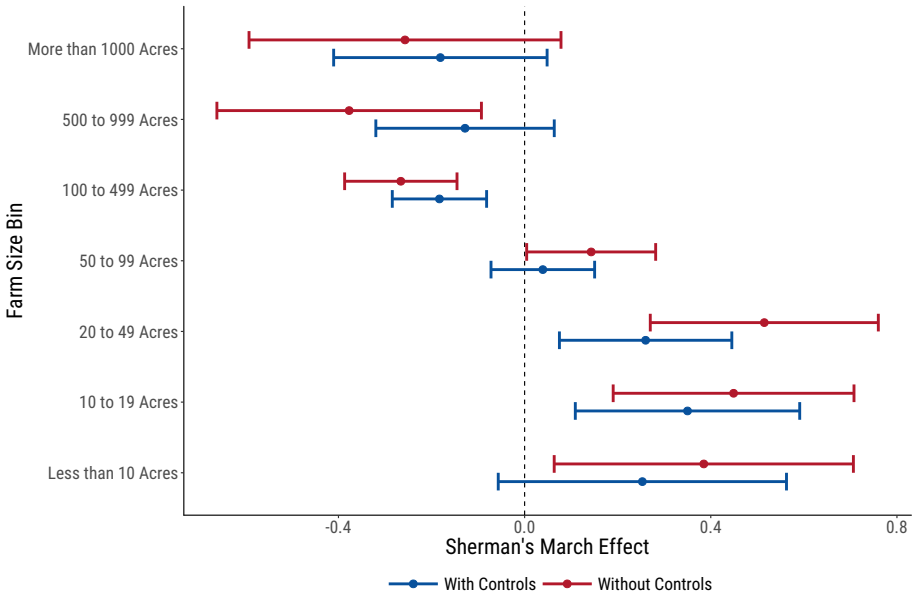
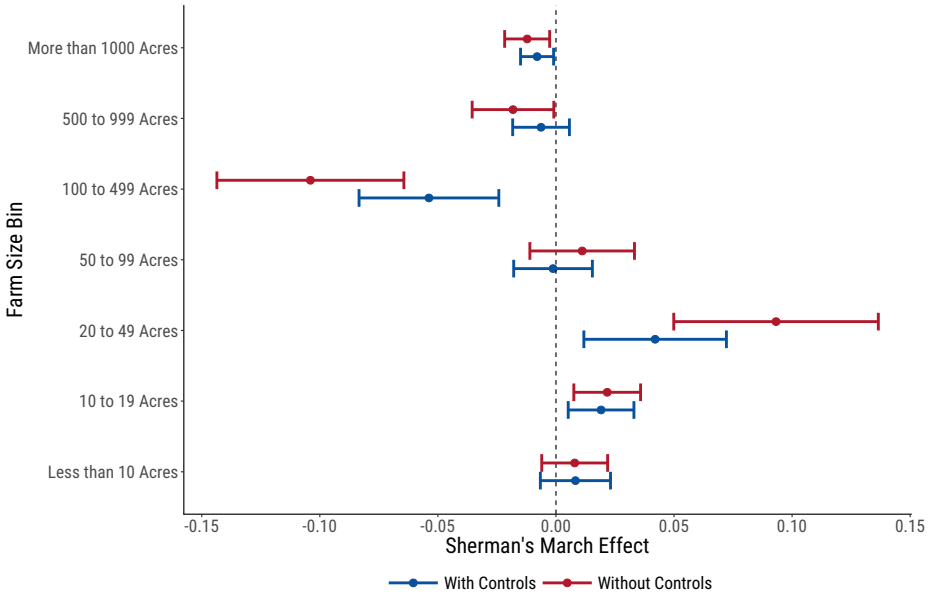


Figure 7: The vast majority of farms in Georgia, South Carolina, and North Carolina in 1880 that were smaller than 50 acres were managed by sharecroppers or tenant farmers. Larger farms—especially those larger than 100 acres—were almost all managed by their owners.



(a) Log Number of Farms by Size



(b) Share of Farms by Size

Figure 8: Sherman's March changed the distribution of effective farm sizes in the counties Sherman destroyed. Counties treated by Sherman had more small farms in the years after the Civil War, with growth in the share and number of farms under 50 acres. We plot coefficients from separate regressions with the log number of farms (a) and the share of farms (b) of each size as the outcomes as in equation 1. We observe the distribution of farms in 1860, 1870, 1880, and 1890. The coefficient plotted is $Sherman \times Post$. All specifications include county fixed effects and state by year fixed effects. The estimates from the models with controls include the same controls as the main specification in the paper. Standard errors are clustered at county-level.

A Appendix

A.1 Additional Tables and Figures

Table A.1: Link Rate from Marriages to 1870 Census (%)

	March Counties	Non March Counties	Difference
Georgia	37.80	39.10	-1.30
			(2.28)
North Carolina	38.91	38.66	0.26
			(8.99)
Total	37.92	39.13	-1.21
			(2.28)

Notes: Lists of the names of grooms in Georgia and North Carolina were collected from state marriage records between 1868 and 1872. The grooms were then matched by first and last name to the complete 1870 census schedule. The link rate reports the share of grooms successfully matched using a variant of the automated linking procedure described in Feigenbaum (2016). Match rates are comparable to other linking projects using census data in this era. The Georgia and North Carolina Marriage Records are from FamilySearch.org.

Table A.2: Change in Lumber Manufacturing Outcomes from 1860 to 1870, by Sherman March Exposure

	Lumber Industry							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.958	-0.926	-0.521	-0.533	-0.502*	-0.599*	-0.493*	-0.479*
	(0.910)	(0.950)	(0.402)	(0.453)	(0.285)	(0.318)	(0.263)	(0.272)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	180	180	180	180	180	180	180	180
Clusters	180	180	180	180	180	180	180	180
Adjusted R^2	0.022	0.116	0.017	0.068	0.018	0.089	0.018	0.030

Each column is a separate county level regression of the percentage change between 1860 and 1870 in the column indicated lumber manufacturing outcome on an indicator equal to one if the county is within five miles of Sherman's march plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. More info on variables are in the text where we discuss the main specification. The sample is all lumber industries in counties within 100 miles of the march. Standard errors are clustered at the county level.

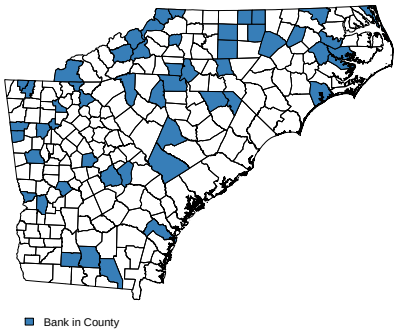
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.3: IV: Agricultural Outcomes, by Sherman March Exposure, 1850-1890

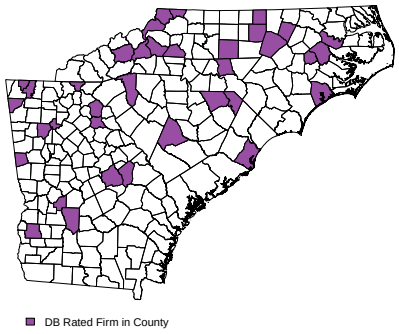
	Log Value of Farms		Log Value of Livestock		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x 1850	-0.037	-0.056	-0.024	-0.031	0.003	-0.030
	(0.073)	(0.097)	(0.040)	(0.052)	(0.054)	(0.069)
Sherman x 1870	-0.193*	-0.194*	-0.134**	-0.129*	-0.150**	-0.156**
	(0.099)	(0.099)	(0.064)	(0.076)	(0.073)	(0.079)
Sherman x 1880	-0.030	0.070	0.018	0.060	-0.104*	-0.071
	(0.076)	(0.075)	(0.047)	(0.040)	(0.058)	(0.059)
Sherman x 1890	-0.046	0.077	-0.079	-0.031	-0.142**	-0.135**
	(0.100)	(0.100)	(0.056)	(0.059)	(0.060)	(0.058)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	1123	1123	1123	1123	1123	1123
Clusters	225	225	225	225	225	225

Each column is a separate county-year level regression of the indicated agricultural outcome on an indicator equal to one if the county is within five miles of Sherman's march interacted with decadal indicators, plus the noted fixed effects and controls, where the Sherman's march indicator is instrumented with an indicator for within 15 miles of a straight-line path between the four march vertices: Atlanta, GA, Savannah, GA, Columbia, SC, and Goldsboro, NC. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

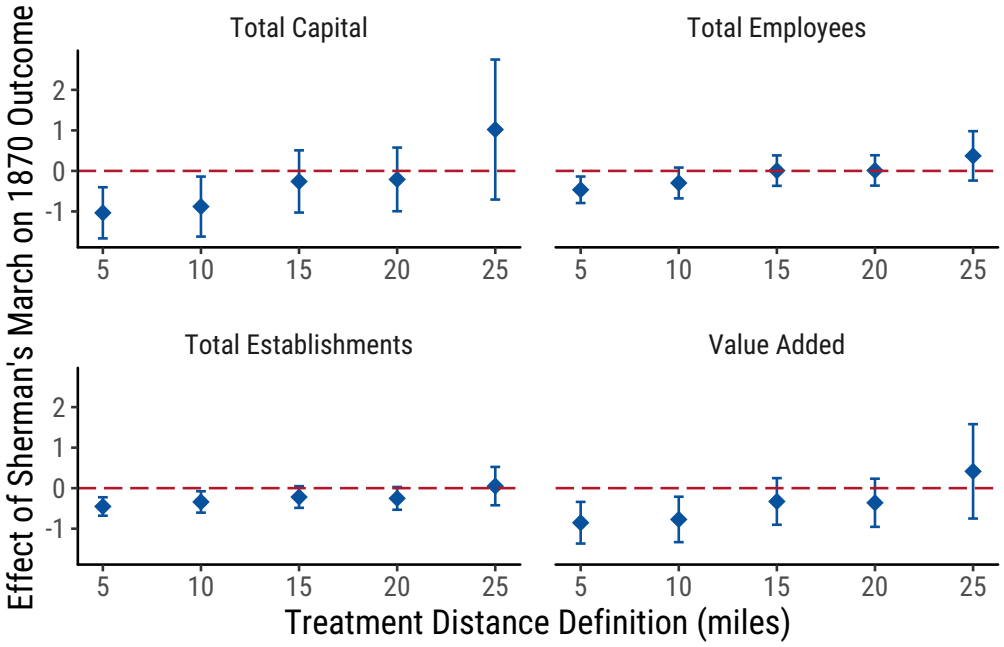


(a) Counties with banks in 1859 in Georgia, South Carolina, and North Carolina

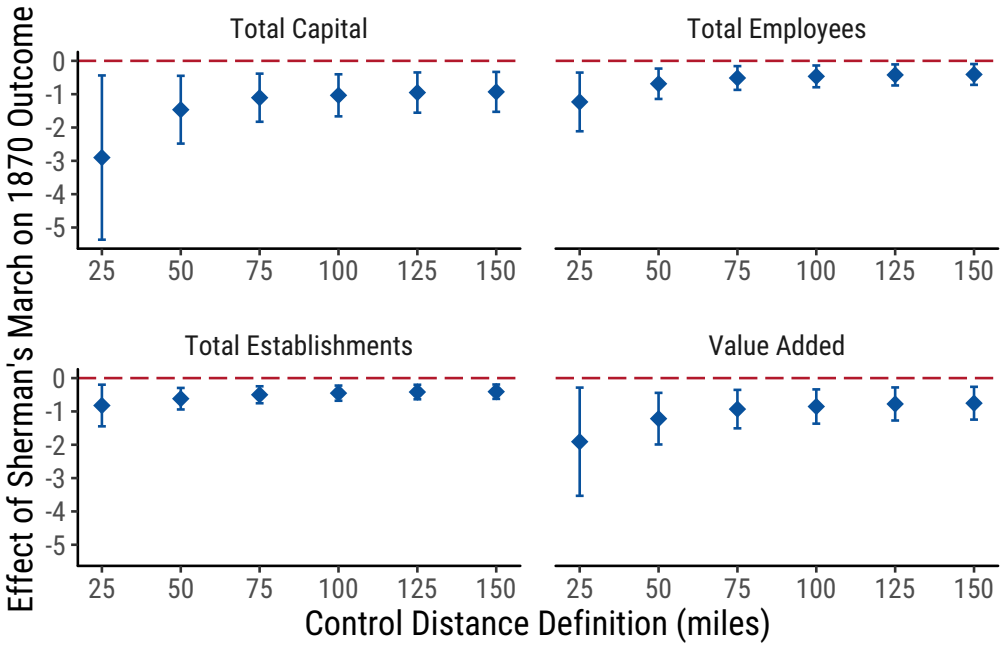


(b) Counties with Dun, Boyd, & Company rated firms in 1860 in Georgia, South Carolina, and North Carolina

Figure A.1



(a) Sherman's effect across alternative treatment definitions



(b) Sherman's effect across alternative control definitions

Figure A.2: Alternative treatment and control definitions for manufacturing outcomes

Table A.4: IV: Change in Manufacturing Outcomes from 1860 to 1870, by Sherman March Exposure

	Growth, 1860-1870							
	Establishments		Capital		Employment		Value Added	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.991***	-1.702***	-0.520**	-1.027***	-1.022**	-2.173***	-0.485***	-0.745***
	(0.373)	(0.633)	(0.241)	(0.348)	(0.428)	(0.830)	(0.152)	(0.235)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201

Each column is a separate county-industry level regression of the indicated manufacturing outcome on an indicator equal to one if the county is within five miles of Sherman's march plus the noted fixed effects, where the Sherman's march indicator is instrumented with an indicator for within 15 miles of a straight-line path between four march vertices: Atlanta, GA, Savannah, GA, Columbia, SC, and Goldsboro, NC. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.5: Agricultural Outcomes, by High Wealth, Country Stores, and Sherman March Exposure, 1850-1890

	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x 1850	0.023	0.040	0.022	0.027	0.030	-0.004
	(0.075)	(0.096)	(0.037)	(0.048)	(0.046)	(0.065)
Sherman x 1870	-0.314***	-0.282***	-0.168**	-0.171**	-0.146**	-0.138**
	(0.102)	(0.102)	(0.071)	(0.079)	(0.059)	(0.070)
Sherman x 1880	-0.073	-0.009	-0.062	-0.050	-0.140***	-0.112**
	(0.077)	(0.069)	(0.042)	(0.038)	(0.047)	(0.049)
Sherman x 1890	-0.093	-0.031	-0.151***	-0.151***	-0.154***	-0.171***
	(0.099)	(0.095)	(0.055)	(0.055)	(0.057)	(0.054)
Sherman x High Wealth 1850 x 1850	-0.017	-0.048	0.015	0.007	0.094	0.111
	(0.101)	(0.109)	(0.074)	(0.075)	(0.101)	(0.109)
Sherman x High Wealth 1850 x 1870	0.324**	0.236*	0.130	0.115	0.070	0.035
	(0.131)	(0.140)	(0.106)	(0.103)	(0.151)	(0.183)
Sherman x High Wealth 1850 x 1880	0.143	0.151	0.173***	0.190***	0.082	0.053
	(0.110)	(0.106)	(0.067)	(0.058)	(0.091)	(0.091)
Sherman x High Wealth 1850 x 1890	0.134	0.143	0.240***	0.282***	0.077	0.129
	(0.131)	(0.135)	(0.086)	(0.082)	(0.087)	(0.089)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	1123	1123	1123	1123	1123	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.029	0.009	0.028	0.009	0.014	0.003

Each column is a separate county-year level regression of the indicated agricultural outcome on an indicator equal to one if the county is within five miles of Sherman's march, interacted with decades dummy and a dummy for high density of High Wealth Individuals in 1850, plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march and all decades, 1850-1920, as discussed in the paper. The dummy for High Wealth is equal to one for those counties that are in the top quarter in terms of share individuals in the top 5% of the overall wealth distribution in 1850. The standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.6: Agricultural Outcomes, by High Wealth, Country Stores, and Sherman March Exposure, 1850-1890

	Antebellum Local Wealth Density: Share in Top 2%					
	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x Post	-0.194***	-0.114*	-0.146***	-0.110***	-0.154***	-0.146***
	(0.073)	(0.065)	(0.049)	(0.040)	(0.046)	(0.044)
High Wealth 2% x Post x Sherman	0.257**	0.195*	0.158**	0.142**	0.095	0.089
	(0.113)	(0.109)	(0.080)	(0.065)	(0.100)	(0.093)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	898	1123	898	1123	898	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.056	0.013	0.033	0.004	0.025	0.001
	Antebellum Local Wealth Density: Share of Whites in Top 5%					
	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman x Post	-0.176**	-0.090	-0.152***	-0.119***	-0.144***	-0.123**
	(0.081)	(0.075)	(0.054)	(0.044)	(0.050)	(0.052)
High Wealth(White Share) x Post x Sherman	0.216**	0.142	0.183**	0.167***	0.068	0.042
	(0.100)	(0.098)	(0.075)	(0.063)	(0.100)	(0.104)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls X Year	No	Yes	No	Yes	No	Yes
Observations	898	1123	898	1123	898	1123
Clusters	225	225	225	225	225	225
Adjusted R^2	0.059	0.019	0.032	0.010	0.013	0.001

Each column is a separate county-year level regression of the indicated agricultural outcome on an indicator equal to one if the county is within five miles of Sherman's march, interacted with an indicators for post 1860 decades and a dummy for high density of High Wealth Individuals in 1850, plus the noted fixed effects and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. In general, to flexibly control for these characteristics of the county in 1860, we divide the sample in four quartiles along each of these characteristics and then add them in the regression interacted with time dummies. More info on variables are in the text where we discuss the main specification. The sample is all counties within 100 miles of the march and all decades, 1850-1890, as discussed in the paper. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.7: Change in Manufacturing Outcomes from 1860 to 1870, by Sherman March Exposure and Financial Access

	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.113	-0.341	0.000	-0.207	-0.087	-0.609	-0.110	-0.206
	(0.318)	(0.441)	(0.184)	(0.246)	(0.341)	(0.520)	(0.081)	(0.159)
High Bank Drop	2.699**	2.435**	1.779***	1.323***	3.411**	2.570*	1.565***	1.236***
	(1.080)	(0.948)	(0.556)	(0.463)	(1.649)	(1.333)	(0.427)	(0.372)
Sherman x High Bank Drop	-2.920***	-3.100**	-1.903***	-1.892***	-3.702**	-4.224**	-1.435***	-1.425***
	(1.110)	(1.369)	(0.582)	(0.659)	(1.669)	(2.120)	(0.442)	(0.519)
Industry Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1404	1404	1404	1404	1404	1404	1404	1404
Clusters	201	201	201	201	201	201	201	201
Adjusted R^2	0.029	0.056	0.042	0.070	0.035	0.078	0.059	0.073

Each column is a separate county-industry-year level regression of the change from 1860 to 1870 in the indicated manufacturing outcome on the displayed interaction terms, fixed effects, and controls. The 1860 county controls include size of the county, measured in squared miles; population; size of the agricultural output; and intensity in cotton production; average slave ownership per farm; share of manufacturing employment; plantation county; and railroad miles. More info on variables are in the text where we discuss the main specification. High Bank Drop is a dummy which is equal to one for counties in the bottom half in terms of bank growth over 1859-1870. The sample is all counties within 100 miles of the march. Standard errors are clustered at the county level.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$