Capital Destruction and Economic Growth: The Effects of Sherman's March, 1850–1920*

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Abstract

Using General Sherman's March through Georgia, South Carolina, and North Carolina during the Civil War, we study the effect of capital destruction on medium- and long-run local economic activity, and the role of financial markets in recovery. We show that the March's capital destruction led to a large contraction in agricultural investment, farming asset-prices, and manufacturing activity compared to neighboring counties. Elements of the decline in agriculture persisted through 1920. Exploiting variation in local access to antebellum 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). Further, 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 examine the long- and medium-run effects of General William Sherman's 1864-65 military march during the American Civil War as a laboratory to study the role of capital destruction on local economic activity.[1] We estimate that this shock 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.

Union General William Sherman's march from Atlanta, GA to Savannah, GA to Columbia, SC to Goldsboro, NC was one of the closing acts of the American Civil War. Though the war had been bloody, it had been fought mostly traditionally: army against army on battlefields with limited damage to civilian property or infrastructure. The march was different. In Sherman's view, 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 postbellum dollars, he claimed—to economic infrastructure (Trudeau 2008, p. 539)? And how long did the damage persist? To credibly identify Sherman's effect, we

[1] This setting allows us to estimate the overall impact of the destruction caused by Sherman, combining direct effects (e.g. the effect of having agricultural and manufacturing capital destroyed and having to rebuilt it) and indirect effects (e.g. any changes in future behavior or attitudes caused by the capital destruction).

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.[2] This allows us to difference out the overall effects of the war as well as the shifts in economic conditions across states.[3] 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 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. By 1870, the value of march-county farms was 20% lower than the value of non-march-county farms.[4] Consistent with this result, we also find 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

[2]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. We define any county with any area within the Sherman buffer as treated by Sherman, though in practice no results change if we limit treatment to counties with a higher share of area in the buffer. 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. 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).

[3]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.

[4]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.

tests, strengthening our causal interpretation. We show 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. We estimate a placebo test using plausible alternative march routes and vary our definition of treatment and control counties.

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 two main pieces of evidence in favor of this hypothesis, using newly digitized data on local bank and Southern credit-market conditions.[5] First, we find that the lack of provision of credit made the immediate recovery more difficult in the manufacturing sector. Formal credit markets were deeply impaired by the war everywhere but credit-dependant counties in the antebellum—those located closer to a bank branch or with firms tracked by the credit-rating agency Dun, Boyd, & Company—would have felt the finance disruption more. For these counties, we find much larger effects of Sherman on manufacturing activity, suggesting that credit drying up exacerbated the recovery for businesses that were more dependent on it. We also show that the negative effects of the march were much larger for industries more dependent on external finance (Rajan and Zingales 1998).

Second, we 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 local wealthy elites (Rajan and Ramcharan 2011; Jaremski and Fishback 2018; Martin 2016; McCurry 1997) and country stores (Atherton 1949; McCurry 1997; Sparks 1932; Bremer 2011) for financing between harvests. Consistent with this narrative, we find smaller effects of Sherman's march in counties with a larger share of wealthy individuals or in counties with antebellum country stores.

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

[5] In the appendix, we also show that potential alternative mechanisms suggested by the historical literature about the march—a demographic shift or the lack of infrastructure—do not explain the large effects of the march (see Appendix A.2).

activity recovered within twenty years from the march, improved land remained lower in Sherman counties until 1920. We draw two main conclusions from these disparate 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 boom 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 change the way economic activity is organized, changes that persist in the long run. In the case of postbellum Georgia, North Carolina, and South Carolina, the new institution was sharecropping and tenant farming. In particular, we document that the areas affected by the shock saw a shift in the 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.[6] 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. Relative to this literature, we examine how capital destruction affects business development across different sectors in a setting, the postbellum South, where agriculture was still an important share of the economy, as it likely is in many locations today where wartime capital destruction may be an issue. 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.[7] 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.

[6] 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 (Bianchi and Giorcelli 2018; Giorcelli 2018; Gregory 2017).

[7] One paper that does study industry-level effects of wartime capital destruction is Davis and Weinstein (2008), which uses data on manufacturing on 114 cities after the Japanese bombing to test for multiple equilibria in regional development.

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 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.[8]

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. 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

[8]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.

ex-slave labor force participation—generally dismissing capital destruction as a factor.[9] If capital destruction played any role in the stagnation of the postbellum Southern 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.[10]

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, General William Sherman, trained his sights on the destruction of the Confederate economy and infrastructure (Trudeau 2008, p.

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

[10] 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.

40). Sherman had just finished the successful Atlanta Campaign, a collection of skirmishes from Chattanooga, TN through northwest Georgia during the late spring and summer of 1864, which culminated in Atlanta's capture on September 2, 1864. 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 (US War Department 1901; Lee 1995). 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 to Columbia. The final drive took the army northeast before defeating the only Confederate army remaining in the Carolinas near Goldsboro, North Carolina.[11] In Table 1, we see that in 1860, the counties in Sherman's path—those counties inside 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

[11] 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).

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.

[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" and another 3,000 likely joined in on many occasions.[12] The men destroyed hundreds of businesses, homes, farms, railroad lines, and telegraph lines, and expropriated livestock and crops (US War Department 1901; Lee 1995).

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 "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

[12] 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.

larger battles, city sieges, and troop movements had taken place farther north or west.[13] 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. 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 private property owners whose capital had been destroyed during the war.[14] 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, 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, though these new rights were largely stripped when the planter class regained power (Naidu 2010). Along with constitutional amendments abol-

[13]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.

[14]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).

ishing slavery, guaranteeing citizenship, and granting the right to vote, Congress enacted several statutes enabling Reconstruction. The Freedmen's Bureau, a federal agency tasked by Congress with transitioning former slaves into freedom, started schools for both children and adults and provided 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.

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.[15] 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.

[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

[15] We include the counties of northwest Georgia in our control group. These counties were the stage for the Atlanta Campaign, traditional army versus army fighting between Sherman and Confederate armies led by Johnston and later Hood. During this conventional campaign, Sherman did not engage in the full-scale total war operation he implemented during the March we study. Infrastructure was not targeted, homes and barns and fences not destroyed, crops not "foraged" by Union troops. Thus, we think of these counties as plausibly untreated control counties. However, our results are robust to dropping all counties to the north and west of Fulton County (Atlanta).

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) and border data from Perlman (2015).

Because the 1870 census data were collected in a South that was still recovering from the Civil War, one might worry about data quality (Steckel 1991; Magnuson and King 1995; Reid 1995). In particular, it would be problematic if under-enumeration in march counties were more severe than in non-march counties. Heterogeneity in 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.[16] 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.[17]

To investigate the mechanisms behind any march and non-march differences, we augment the Haines (2010) data with five other data sets. First, we hand-collect bank-level entries in two Merchants & Bankers Registers, 1859 and 1864. 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. Second, we hand-collect firm-level records from Dun, Boyd, & Company's *The Mercantile Agency's 1860 Reference Book*. 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. See Brennecke (2016) for a detailed explanation of D&B in this era. We match the city of each firm to an

[16]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).

[17]Beyond this test, we think there are three reasons to believe that data quality is unlikely to explain our results. First, census enumerators were locally-hired and might be able to elicit accurate information from their neighbors. Second, few Reconstruction era land redistribution policies were ever enacted (Logan 2018); as early as 1870, we think Southerners would have approximately accurate beliefs about future land policy, policy beliefs that might affect census reporting. Third and most importantly, any reporting bias would only contaminate our findings if it were differential across Sherman and non-Sherman treated counties. As these counties were neighboring and in the same states and the same Reconstruction districts (districts were larger than states), contemporaries would likely be subject to the same potential land policies.

1860 county and calculate the number of credit-tracked firms in each county. Third, we draw data on external finance dependence from Nanda and Nicholas (2014) at the industry level. Fourth, we calculate the presence of high wealth individuals in each county from the IPUMS complete count censuses. Fifth, 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 Federal Census. We use these sources to proxy different types of local credit availability and credit demand before the march, allowing us to shed light on the role of credit in our results.

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.[18] 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.[19] We estimate the following specification:

[18]The value of farms encompasses both the direct value of farm and the value of other implements and machinery and buildings on the farm. Specifically, the value of farms is the sum of three concepts measured in the Census of Agriculture from 1850 to 1920: the value of farms (called the cash value of farms from 1850 to 1870 and the value of farmland/improvements starting in 1880), the value of farm implements and machinery, and the value of farm buildings (split out from the value of farms starting in 1900). The cash value of farms or the value of farmland/improvements is by far the largest component of this sum and our results are substantively the same if we just look at this outcome on its own.

[19]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 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.

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

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 in Georgia, North Carolina, and South Carolina. 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.[20]

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. 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.[21] As we discuss in the

[20] In Appendix Tables A.11 and A.12, we show that our results are robust to using Conley (1999) spatial standard errors. In Appendix A.5 we discuss the issue of spatial correlation in our empirical setting. We review the recent discussion in Kelly (2019) that false positives plague the historical persistence literature using spatial correlations and outline why the Kelly critique is not directly applicable to our empirical strategy.

[21] In the main analyses, the quartiles are constructed using 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. About half of the counties in the three Sherman states grew one variety of cotton (upland cotton) and the other half grew another variety (sea island). Our results are robust to including controls for cotton type interacted with cotton intensity interacted with year fixed effects. Our railroads variable is highly skewed and we simply

results section, the addition of these controls has little effect on our estimates, in particular in the medium-run.

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. 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.[22] Furthermore, we augment this specification with industry-group fixed effects $\delta_{g(i)}$ and state fixed effects δ_s.[23] 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.

split the sample at the first quartile rather than use all four quartiles. More discussion on the variable construction is provided in the data section.

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

[23] 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.[24]

[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. Of course, given the major changes wrought by the Civil War and emancipation, parallel pretrend tests have to be interpreted more carefully than usual. 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.

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

[24]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).

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. Across the various outcomes, we consistently find that the manufacturing sector contracted relatively more in Sherman counties in 1870. For instance, we find that capital declined 30% more in affected counties relative to the control group. However, while these results are always large in magnitude, they are quite imprecise, in particular when controls are included.

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. 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 a 40% lower establishment and employment growth relative to non-Sherman counties.[25] While this effect is large in absolute terms, there are two things to take into account. First, 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. Second, the

[25]Because of spillovers, our estimates could partially overstate the true effects. The intuition is simple: if different areas produce homogeneous goods, a contraction in destroyed areas may be partially compensated by an increase in neighboring areas. While this concern may be a confounding factor if we were interested in pinning down the exact magnitude of the Sherman effect, spillovers on their own would not be sufficient to explain the sign of our results. Furthermore, the donut-design discussed later also help addressing this issue.

elasticies implied by these estimates are consistent with other studies. If we consider the specification with 1860 controls, a one percent drop in capital corresponds to a 0.50 percent drop in employment, consistent with estimates of the elasticity of substitution between capital and labor commonly found in the literature (Chirinko 2008). Moreover, we found no difference in aggregate manufacturing growth before the Civil War, suggesting that the manufacturing effects were likely not the continuation of pre-march trends, though large postbellum institutional changes temper this test.[26]

[Table 4 about here.]

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 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

[26]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. 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. Prior to the march, 75% of the counties in the sample had at least one sawmill or lumber establishment. We find suggestive evidence of relative declines in lumber, albeit less precisely estimated than our main results, see Table A.2.

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.[27] 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. 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 248.9, 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 (Tables A.3 for agriculture and A.4 for manufacturing).

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.[28] Excluding paths through the three Sherman states, there are 852 paths.

[27] 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.

[28] 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. 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.

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 A.1. In five of six specifications, the Sherman t-statistic is smaller (more negative) than 95% of the placebo marches; in four of the six it is smaller than 98% of the placebos.[29] No placebo march has more negative t-statistics than we found for Sherman's march in all six specifications and only four of 852 placebo marches are smaller in three or more specifications.

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. Our 5-mile 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. In Figure A.2a, 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. However, as we use a broader definition of treatment, we tend to find smaller and less significant results. 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.

Our control-bandwidth choice is not driving our findings, as we show in Figure A.2b. We repeat the same procedure as above, but change the control to bandwidths of 25, 50, 75, 100, 125, and 150 miles. 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 the smaller sample size. We also show in Figure A.4 that the effects on manufacturing are also robust to alternative treatment and control bandwidths, though the estimates are a bit more stable when varying control distance rather than treatment definitions.

Finally, the magnitude of our main estimates could be inflated if economic activity is reallocated from destroyed counties to their neighbors. To address this concern, we implement a donut-design, removing control counties between 5 and X miles from the march, for $X \in \{10,20,25,50\}$. As we show in Figures A.5 and A.6 and find similar point estimates for

[29] In a corresponding analysis for the manufacturing growth results in Table 4, we find t-statistics as negative as the real Sherman's march less than 5% of the time in all specifications and in four of the eight specifications the real Sherman t-statistics are more negative than 99% of the placebos.

the 1870 Sherman effect for both agriculture and manufacturing.

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 situation 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 relatively fast.[30]

Our analyses show that capital destruction can have a large impact in economic activity, even several years after the event. 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. Moreover, 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. 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

[30]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 Vietnam War (Miguel and Roland 2011) and the Japanese WWII reconstruction (Davis and Weinstein 2002), where cities are found to recover fully within 15 years. At the extreme, some papers find essentially no effect of large natural disasters, even in the very short run (Porcelli and Trezzi 2019). Bosker, Brakman, Garretsen, and Schramm (2008) is an exception in the literature, finding persistent effects of WWII on the distribution of West German city sizes.

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.

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.[31] Banks started to re-enter these states only in 1866 and even by 1870 only a handful of banks were operating.[32] 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 pre-war levels." Figure 3 provides suggestive evidence that access to 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)—and the default of the Confederate States on their debt obligations.[33]

[Figure 3 about here.]

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

[31] 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.

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

[33] Consistent with this scholarship, we generally find that counties more dependent on slavery in 1860 experienced a larger contraction in agricultural activity in the following decade. However, we do not find evidence that this postbellum effect of emancipation was relatively larger in Sherman counties.

2013; Koudijs and Salisbury 2016; Nier 2007). 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). 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 two pieces of empirical evidence that are consistent with this hypothesis.[34] First, 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. Second, 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 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 Credit in the Manufacturing Recovery

Jaremski (2014) documents the importance of a formal banking sector in encouraging the growth of manufacturing during the 19th century. 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

[34] In Appendix Sections A.2 and A.3, we show that three alternative channels affecting the slow relative recovery—a demographic shift after the war or the destruction of public infrastructure or selected out-migration from destroyed counties—are not supported in the data. More, our null results are consistent with the historical record of postbellum African-American migration and the rapid reconstruction of the Southern telegraph and railroad networks.

have—all else equal—suffered more than businesses that were already unbanked before 1860 if financial constraint was a key driver of the recovery.

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 than the median to a bank in 1859 (Figure A.3).[35] If access to credit helps explain the postbellum manufacturing recovery, we expect that counties more dependent on credit should suffer more with physical destruction. Our specification simply modifies the model presented in equation 2, interacting the march treatment with the indicator of finance access.

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 5, top panel). This suggests that Sherman's damage on manufacturing was stronger in counties that also experienced the disappearance of a local banking network. One concern with this analysis is that antebellum bank location is clearly not random and we discuss this threat in Appendix A.4.

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 three or more firms which appear to have had credit extended according to the 1860 Dun report (Figure A.3). 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 5. This regression also demonstrates that when places with more credit relationships external to the firm were damaged, recovery was slower.

Second, we find that the negative effects were much larger when looking at industries 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 depend 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 the industry-level using data collected in the 1920s for public and private firms by Nanda and Nicholas (2014). 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.[36] In the bottom

[35] 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. All distances are measured from county centroids. 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.

[36] Using the data from the published version of Nanda and Nicholas (2014), we identify industries that

panel of Table 5, 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. This evidence combined with our previous results based on cross-location differences are easily rationalized in a setting where financial frictions are important, but would be hard to square with alternative interpretations. Our credit market mechanisms are generally robust to the main tests discussed in the previous section, see Appendix A.4.

[Table 5 about here.]

Overall, these different tests highlight how financing considerations affected the recovery process in the manufacturing sector.[37] 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 landowners or local stores. In the next section, we investigate this type of credit in the recovery from Sherman's March.

6.2 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.

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

are more dependent on external finance by creating a dummy that is equal to one for the top tertile of industries in terms of the external dependence measure (i.e. top five industries). This approach is cleaner than using the continous variable, since we do not need to rely on the linearity assumption on the effect of external dependence. Since our industry composition is not coded exactly as in Nanda and Nicholas (2014), we hand-match the two data sets based on industry names and description.

[37] 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 3 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.

of country stores and furnishing merchants did not diminish after the war (Woodman 1999, p. 303-307). 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. 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).[38]

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. 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. Our hypothesis is that, if

[38]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.

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 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 6 only the average effect after the march.

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. 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 (see Appendix A.4).

We show that the effects of country stores are similar (bottom panel of Table 6). 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.[39] 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 fewer country stores. Since country stores were crucial to providing working capital financing to agriculture, this evidence is consistent with the view that lack of financing 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).

[39]While we do not observe the number of country stores after 1840, we can validate this measure by comparing it with a proxy measure in 1860, the number of managers of general stores found in the full-count 1860 Census (specifically, people reporting an occupation including the strings "store", "merchant", "grocer", or "good"). We find that the two variables have more than 90% correlation.

[Table 6 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. First, 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. Second, 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 4). 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 4 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 (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. 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 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, a system of tenancy emerged as the main labor and land market in the postbellum South (Shlomowitz 1979; Fite 1984).[40] 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. 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

[40] 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.

one explanation for the rise of tenancy and sharecropping in this period.[41] 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 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 a first step, we document that tenancy was mostly concentrated in smaller farms. According to the 1880 Census of Agriculture, 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 5).[42] 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. We find a similar distribution of size across tenant and owner-managed land in 1890.

[Figure 5 about here.]

[41] 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. 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.

[42] 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.

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 in Figure 6. Here, 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. That is, 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).[43]

[Figure 6 about here.]

As a last step, we examine whether this shift in farm size distribution led to any significant shift in land or real estate wealth inequality. Understanding the overall effect on inequality in land can provide useful information to evaluate the welfare impact of this change, but also this analysis can be used to provide indirect evidence that our previous result was not driven by differential trends in farm concentration across counties. We measure land inequality in two ways. First, 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.[44] Second, we draw on the complete count individual census data from 1850, 1860, and 1870, recently digitized and cleaned by IPUMS (Ruggles, Flood, Goeken, Grover, Meyer, Pacas, and Sobek 2017). Real estate wealth was enumerated in the 1850, 1860, and 1870 censuses and we calculate county-level Gini coefficients based on the distribution of real estate wealth among whites.[45]

[Table 7 about here.]

Using our usual specification, we find that farm size inequality increased substantially after Sherman's march in march counties relative to non-march counties (Table 7). Compared

[43]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). 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.

[44]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.

[45]We focus on whites because the enslaved population was not enumerated before emancipation and most newly freed African Americans had no wealth in 1870. We use real estate wealth rather than personal property because the latter was only collected in the 1860 and 1870 censuses and would prevent us from assessing pe-trends in our empirical analysis.

with the 1860 difference in inequality across march and non-march counties, the 1870–1890 differences were 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.[46] In the third and fourth columns of Table 7, 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.

We see the same increase in real estate wealth inequality following Sherman's march in columns 5 and 6 of Table 7. From the pretrends, we see that wealth inequality was not pre-trending differently in Sherman and non-Sherman counties, confirming our slave holding distribution precheck from columns 3 and 4. But the most important wealth inequality results are in the second row of columns 5 and 6 in Table 7. We see large increases in real estate wealth inequality in the Sherman counties after the march. Because wealth data is never again collected by the Federal Census after 1870, we can only describe these short run effects on wealth inequality, but they match quite closely our results on increased farm size concentration.

On top of providing useful evidence for our main analysis, we view these results on inequality in Table 7 as a relatively simple test of the Scheidel (2018) hypothesis that modern, industrial-scale war is one of the key "policy" levers to reduce inequality. This test of Scheidel's hypothesis appears to reject his claim; if anything, wartime destruction (Sherman's march) made inequality worse, contrary to Scheidel's conclusion that the Civil War dented

[46]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).

American inequality without leveling it.[47]

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."[48]

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 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.

[47] In his assessment of the American Civil War, Scheidel (2018) suggests that while the destruction and death of the US Civil War was industrial-scale and it did lead to major policy reforms, the war was not on the same scale as the World Wars in the twentieth century which significantly, in his telling, reduced inequality.

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

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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 (people per sq mile)	22.32	21.82	0.49	0.30
	(10.19)	(10.62)	(1.60)	(1.66)
Average Age	21.20	21.13	0.07	0.15
	(0.88)	(1.22)	(0.17)	(0.15)
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)	1496.19	1288.41	207.78*	165.94
	(748.63)	(831.55)	(123.63)	(127.45)
Percent Improved Farm Land	32.31	28.97	3.34	3.48
	(15.18)	(13.32)	(2.11)	(2.19)
Slaves per Farm	11.39	8.00	3.39**	1.65
	(7.32)	(9.07)	(1.32)	(1.30)
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)	666.87	535.69	131.18***	59.78
	(402.39)	(289.15)	(49.06)	(41.39)
Manufacturing				
Manufacturing Value Added (000s)	69.16	68.79	0.38	-4.23
	(97.40)	(110.46)	(16.35)	(16.57)
Manufacturing Establishments	36.90	26.51	10.39	12.88*
	(74.20)	(38.06)	(7.60)	(7.46)
Total Employment in Manufacturing	142.21	136.27	5.94	-0.75
	(204.63)	(184.11)	(28.89)	(28.50)
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)	2.56	1.39	1.17***	0.97**
	(2.77)	(2.32)	(0.37)	(0.38)
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. A plantation county is a county that is in the top quartile in share of slaveholdings 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 20 miles to either a canal or river based on canal and river data from Atack (2021). Railroad density also based on data from Atack (2021). 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, 1850-1890

	Log Value of Farms		Log Value of Livestock		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × 1850	0.044	0.013	0.037	0.020	0.067	0.027
	(0.059)	(0.085)	(0.033)	(0.042)	(0.044)	(0.062)
Sherman × 1870	−0.197**	−0.188**	−0.139***	−0.131**	−0.148**	−0.122*
	(0.077)	(0.081)	(0.050)	(0.060)	(0.061)	(0.067)
Sherman × 1880	−0.040	0.037	−0.033	0.015	−0.135***	−0.094**
	(0.059)	(0.055)	(0.037)	(0.032)	(0.046)	(0.044)
Sherman × 1890	−0.060	0.037	−0.086**	−0.031	−0.139***	−0.100**
	(0.075)	(0.070)	(0.043)	(0.041)	(0.048)	(0.044)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.853	0.880	0.865	0.883	0.811	0.824
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 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 information on variables is 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. 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 × 1850	−0.009	0.006	0.057	−0.025			0.038	−0.034
	(0.170)	(0.193)	(0.199)	(0.228)			(0.186)	(0.203)
Sherman × 1870	−0.246*	−0.098	−0.278*	−0.283*	−0.120	−0.086	−0.259*	−0.182
	(0.137)	(0.135)	(0.150)	(0.156)	(0.173)	(0.169)	(0.149)	(0.160)
Sherman × 1880	−0.055	0.041	−0.209	−0.130	−0.078	−0.001	−0.215	−0.016
	(0.180)	(0.171)	(0.158)	(0.169)	(0.169)	(0.165)	(0.168)	(0.153)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	847	847	847	847	641	641	847	847
Adjusted R^2	0.648	0.661	0.678	0.676	0.604	0.591	0.690	0.702
Clusters	224	224	224	224	224	224	224	224

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 information on variables is 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. We do not observe establishments in the 1850 county-level data (columns 5 and 6). 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	-0.513**	-0.516**	-0.378**	-0.459**	-0.640***	-0.905***	-0.318***	-0.379***
	(0.204)	(0.257)	(0.165)	(0.203)	(0.222)	(0.302)	(0.109)	(0.132)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.008	0.042	0.008	0.060	0.008	0.070	0.011	0.063
Clusters	201	201	201	201	201	201	201	201
	Change in Manufacturing Outcomes from 1860 to 1880							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.095	-0.050	0.220	0.168	-0.024	-0.014	0.011	-0.129
	(0.248)	(0.244)	(0.279)	(0.281)	(0.327)	(0.321)	(0.140)	(0.149)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.036	0.058	0.048	0.068	0.025	0.045	0.107	0.134
Clusters	201	201	201	201	201	201	201	201

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. More information on variables is 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: 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	-0.019	0.007	0.075	0.009	0.020	-0.208	-0.057	-0.101
	(0.222)	(0.341)	(0.175)	(0.285)	(0.217)	(0.403)	(0.075)	(0.161)
Bank County	0.954***	0.776***	0.739***	0.475**	1.019**	0.688*	0.647***	0.465***
	(0.340)	(0.288)	(0.263)	(0.206)	(0.454)	(0.364)	(0.174)	(0.136)
Sherman × Bank County	-0.902**	-0.853*	-0.828***	-0.787**	-1.206***	-1.174**	-0.477**	-0.447**
	(0.375)	(0.457)	(0.289)	(0.353)	(0.420)	(0.547)	(0.185)	(0.225)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.020	0.048	0.020	0.065	0.019	0.074	0.034	0.072
Clusters	201	201	201	201	201	201	201	201

	Dun, Boyd, and Company Status							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.285	-0.370	-0.157	-0.296	-0.263	-0.609**	-0.180*	-0.283**
	(0.205)	(0.242)	(0.151)	(0.183)	(0.212)	(0.271)	(0.093)	(0.115)
DB County	2.786**	2.127**	2.642***	1.995***	4.159**	3.402**	1.768**	1.283**
	(1.342)	(1.007)	(0.993)	(0.763)	(1.970)	(1.462)	(0.692)	(0.568)
Sherman × DB County	-2.245	-1.848	-2.187**	-2.024*	-3.792*	-3.654*	-1.353*	-1.213
	(1.513)	(1.615)	(1.090)	(1.055)	(2.146)	(2.141)	(0.769)	(0.757)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.042	0.055	0.061	0.081	0.070	0.099	0.066	0.083
Clusters	201	201	201	201	201	201	201	201

	External Finance Dependence by Industry							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.289	-0.287	-0.191	-0.276	-0.395*	-0.658**	-0.236**	-0.305**
	(0.236)	(0.282)	(0.182)	(0.218)	(0.220)	(0.300)	(0.108)	(0.137)
Sherman × High Fin. Dep.	-0.832**	-0.847**	-0.695**	-0.678**	-0.912**	-0.915**	-0.305*	-0.275
	(0.358)	(0.371)	(0.283)	(0.287)	(0.416)	(0.435)	(0.171)	(0.174)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.009	0.044	0.010	0.062	0.009	0.071	0.011	0.063
Clusters	201	201	201	201	201	201	201	201

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 information on variables is 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. We show these results are robust to our straight-line IV strategy in Table A.9.

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

Table 6: 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 × Post	-0.180**	-0.116*	-0.150***	-0.131***	-0.180***	-0.145***
	(0.071)	(0.070)	(0.045)	(0.046)	(0.052)	(0.052)
Sherman × Post × High Wealth 1850	0.214**	0.198*	0.174**	0.186***	0.048	0.050
	(0.097)	(0.103)	(0.069)	(0.062)	(0.093)	(0.096)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.862	0.880	0.879	0.884	0.823	0.828
Clusters	225	225	225	225	225	225

	Local Country Store Status					
	Log Farm Value		Log Livestock Value		Log Improved Acre Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.195***	-0.127*	-0.186***	-0.149***	-0.263***	-0.205***
	(0.067)	(0.066)	(0.044)	(0.044)	(0.058)	(0.053)
Sherman × Post × High Stores 1840	0.276**	0.260**	0.288***	0.287***	0.329***	0.287***
	(0.115)	(0.120)	(0.078)	(0.075)	(0.089)	(0.087)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
High Store × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,115	1,115	1,115	1,115	1,115	1,115
Adjusted R^2	0.856	0.878	0.870	0.883	0.817	0.826
Clusters	223	223	223	223	223	223

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 information on variables is 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. The dummy for High Stores is equal to one for those counties that are in the top quarter in terms of country stores in 1840. Two counties are removed from our sample in Panel B because they have missing values of country stores in 1840. Standard errors are clustered at the county level.

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

Table 7: Farm Concentration by Sherman March Exposure, 1850-1890

	Farm Size Gini		Slave Holding Gini		Real Estate Wealth Gini	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × 1850			0.002	0.003	-0.008	-0.002
			(0.006)	(0.005)	(0.006)	(0.008)
Sherman × 1870	0.022**	0.021*			0.037***	0.020**
	(0.011)	(0.012)			(0.009)	(0.009)
Sherman × 1880	0.061***	0.029***				
	(0.013)	(0.010)				
Sherman × 1890	0.057***	0.026***				
	(0.013)	(0.010)				
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	900	900	450	450	673	673
Adjusted R^2	0.488	0.629	0.629	0.514	0.664	0.716
Clusters	225	225	225	225	225	225

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 and 2 is constructed following Nunn (2008) and uses farm size data from the Census of Agriculture, 1850-1890. The gini measure in columns 3 and 4 is constructed using the same procedure, except using slaveholdings rather than farm size. The gini measure in columns 5 and 6 is constructed with the complete count census data for 1850, 1860, and 1870, the only three censuses with a question about real estate wealth. 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 information on variables is 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

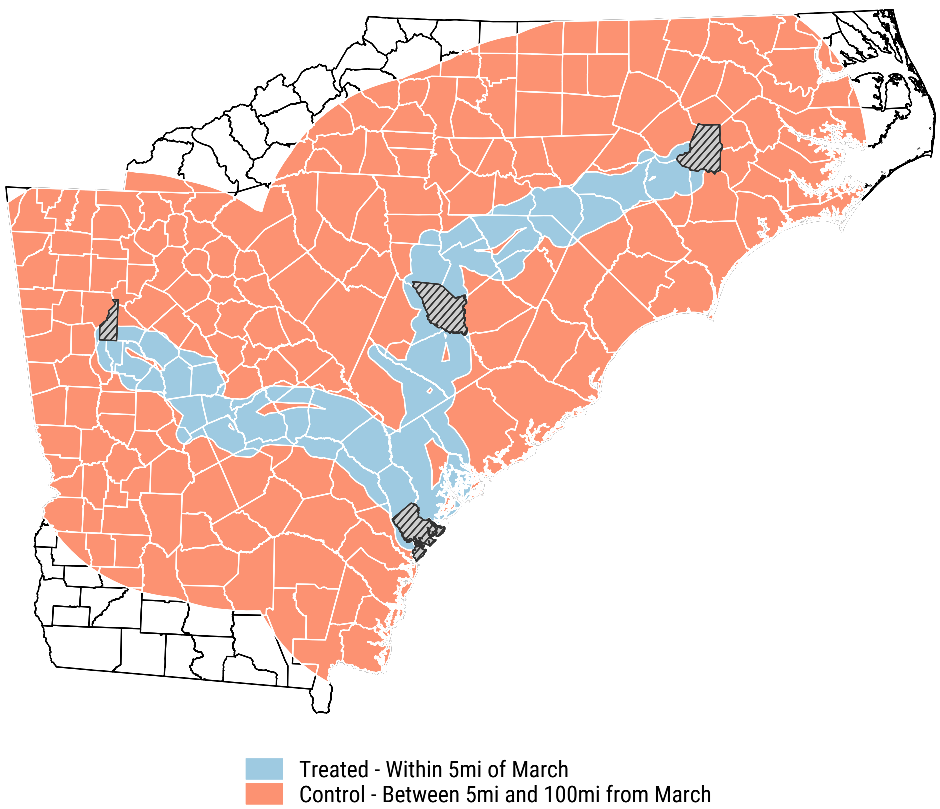


Figure 2: Sherman's March and 1860 County Boundaries. Based on the War Department Map in Figure 1. The vertex cities on the march are excluded from our analysis: Atlanta (captured September 2, 1864), Savannah, GA (December 10, 1864), Columbia, SC (February 17, 1865), and Goldsboro, NC (March 23, 1865).

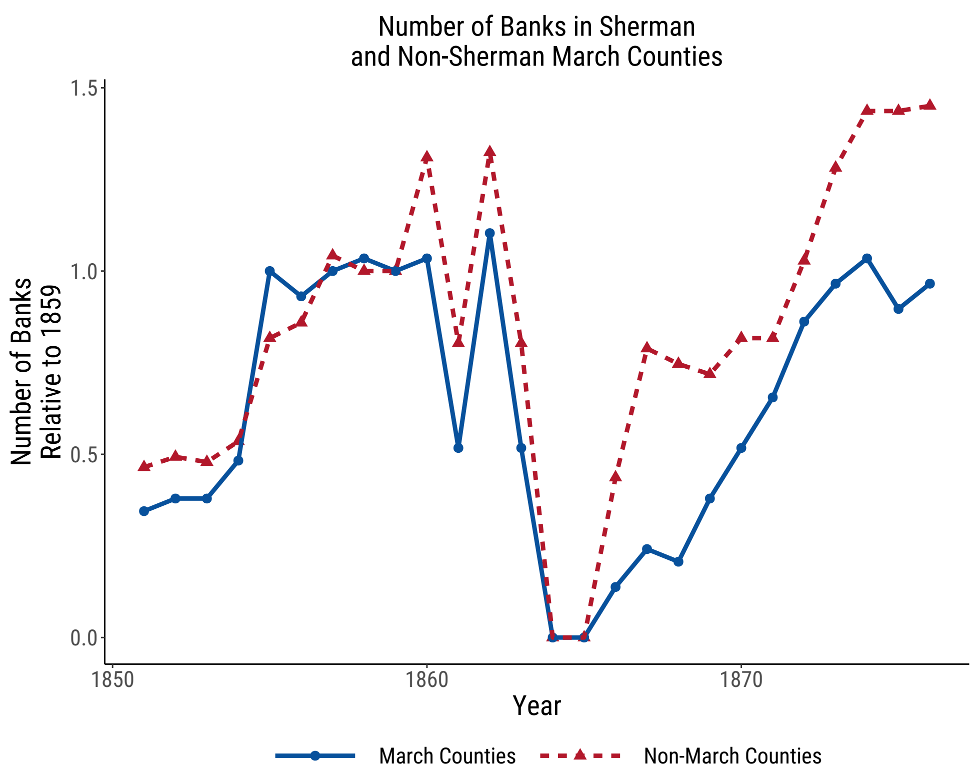
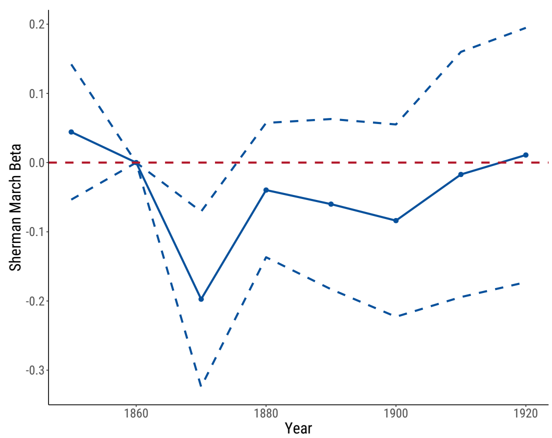
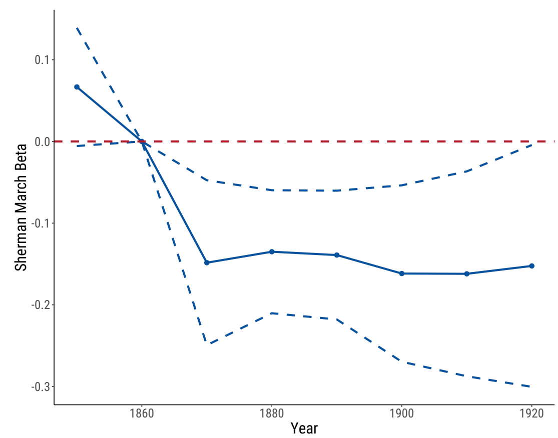


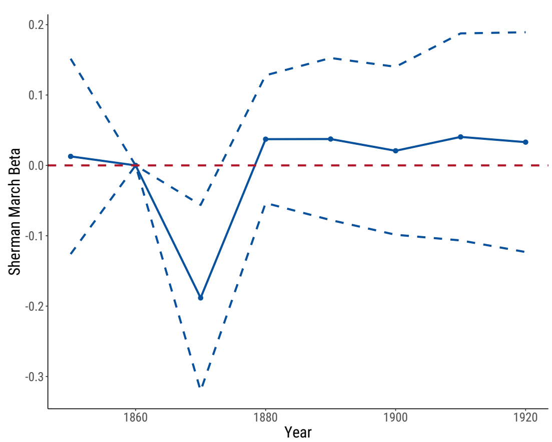
Figure 3: The number of banks in Sherman's march and non-march counties was comparable before the Civil War. After the war, banks may have recovered slightly faster in non-march counties.



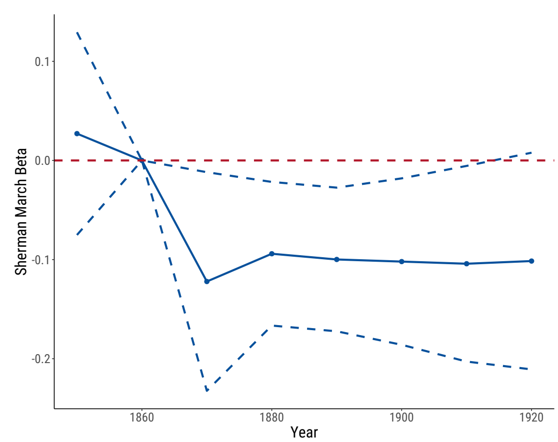
(a) Log Value of Farms



(b) Log Improved Acre Share



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



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

Figure 4: Difference in Log Value of Farms and Improved Acre Share, by Sherman March Exposure, 1850-1920, plotted with 90% confidence intervals

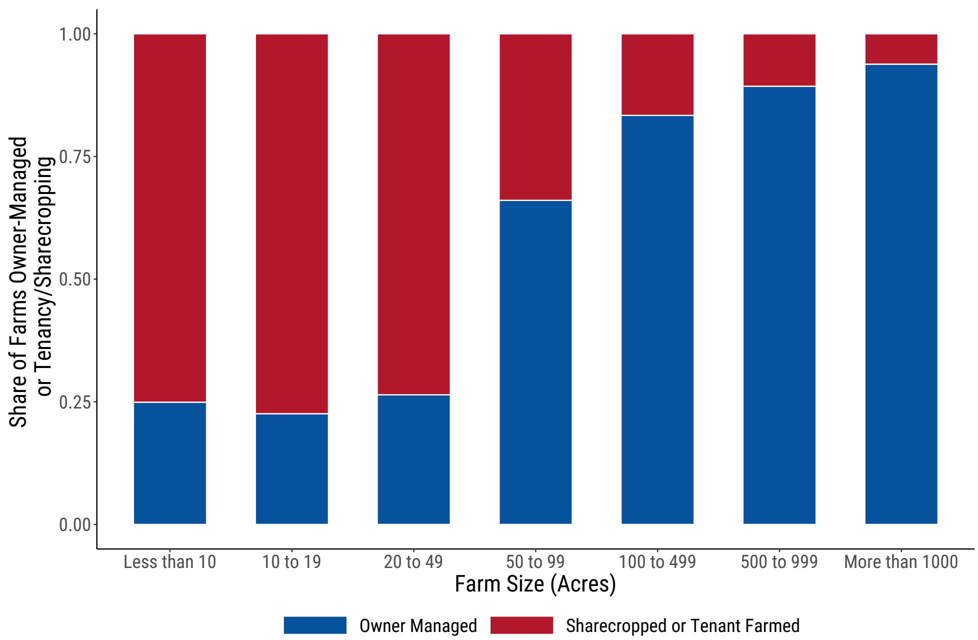


Figure 5: The vast majority of farms in Georgia, South Carolina, and North Carolina in 1880 that were smaller than 50 acres were managed by sharecroppers or tenant farmers. Larger farms—especially those larger than 100 acres—were almost all managed by their owners.

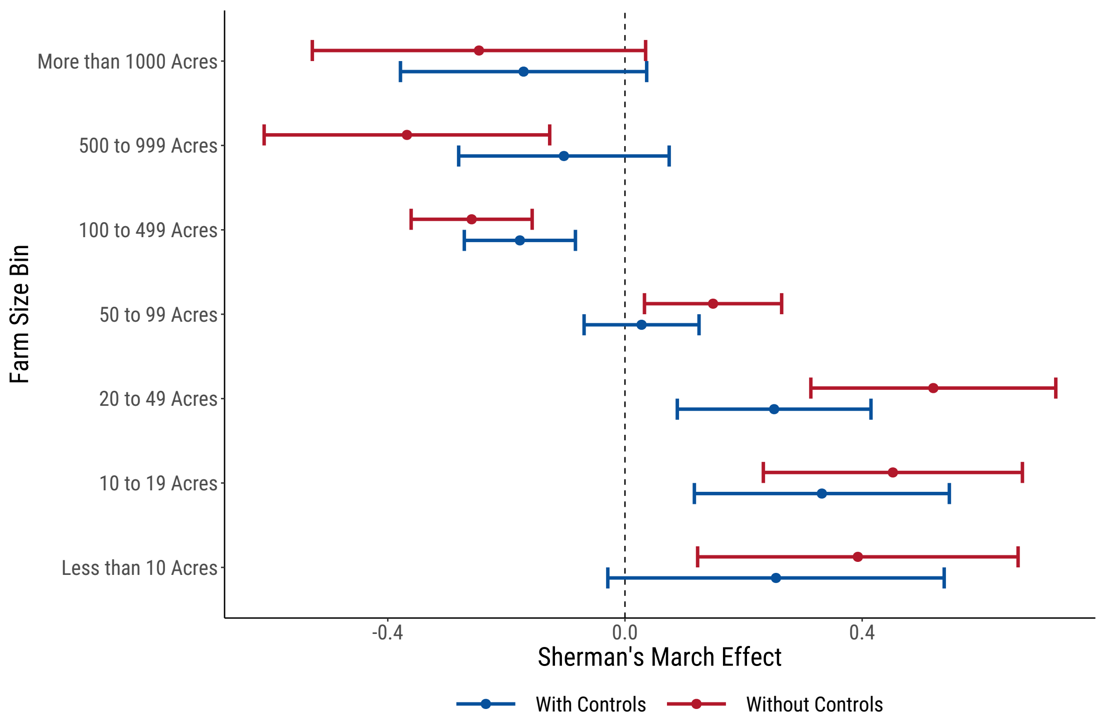


Figure 6: Sherman's March changed the distribution of effective farm sizes in the counties Sherman destroyed. Counties treated by Sherman had more small farms in the years after the Civil War, with growth in the number of farms under 50 acres. We plot coefficients from separate regressions with the log number of farms 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 and we plot 90% confidence intervals around the point estimates. For robustness, in Appendix Figure A.7, we present an alternative specification using the share of farms as the outcome.

A Appendix (For Online Publication)

A.1 Additional Tables and Figures

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

	March Counties	Non March Counties	Difference
Georgia	39.28	38.90	0.38
			(1.72)
North Carolina	38.91	38.49	0.43
			(9.96)
Total	39.25	38.84	0.41
			(1.87)

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	-0.360	-0.609	-0.414	-0.746*	-0.490*	-0.781**	-0.398*	-0.726**
	(0.682)	(0.869)	(0.339)	(0.451)	(0.282)	(0.394)	(0.204)	(0.331)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	181	181	181	181	181	181	181	181
Adjusted R^2	0.024	0.068	0.020	0.062	0.017	0.066	0.024	0.051
Clusters	181	181	181	181	181	181	181	181

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 information on variables is 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.

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

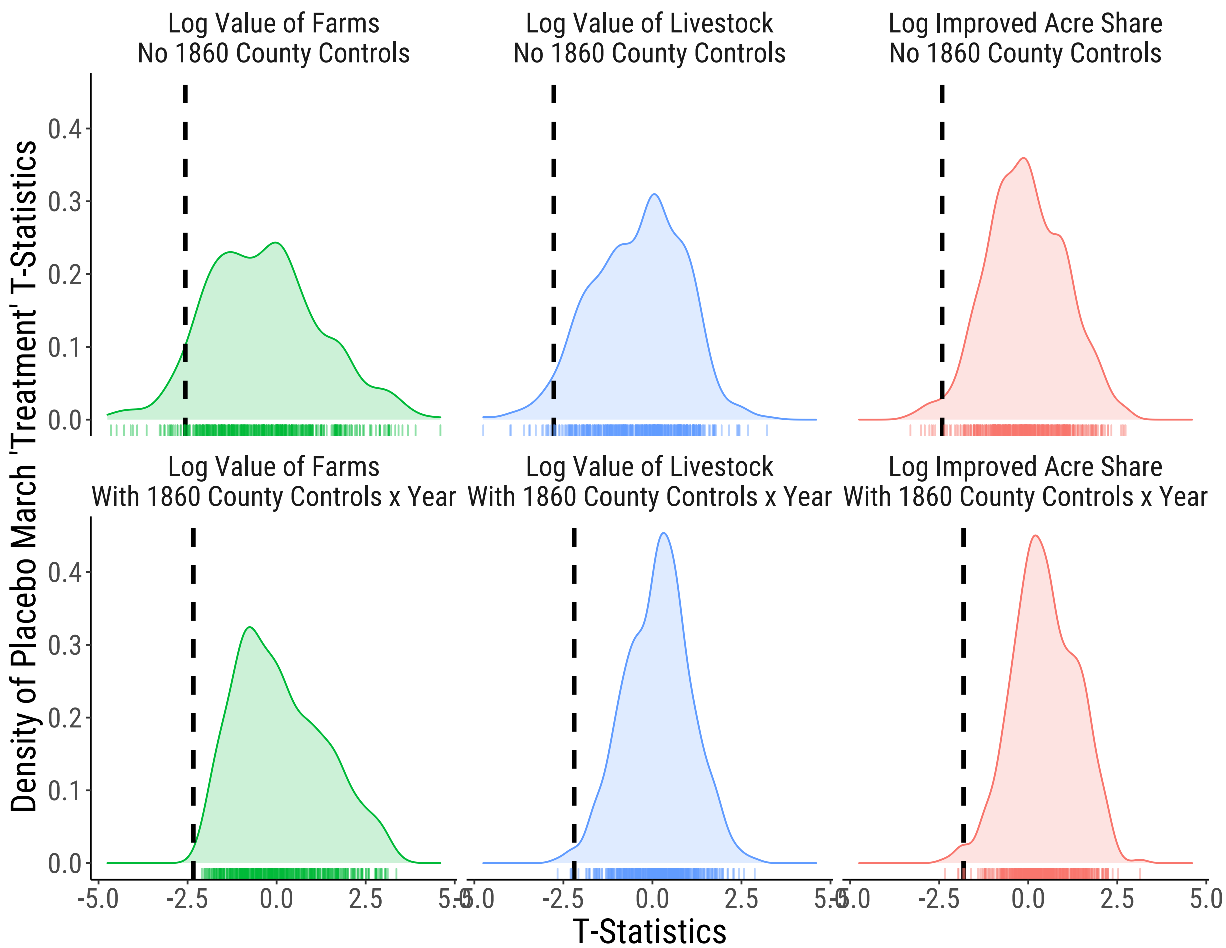
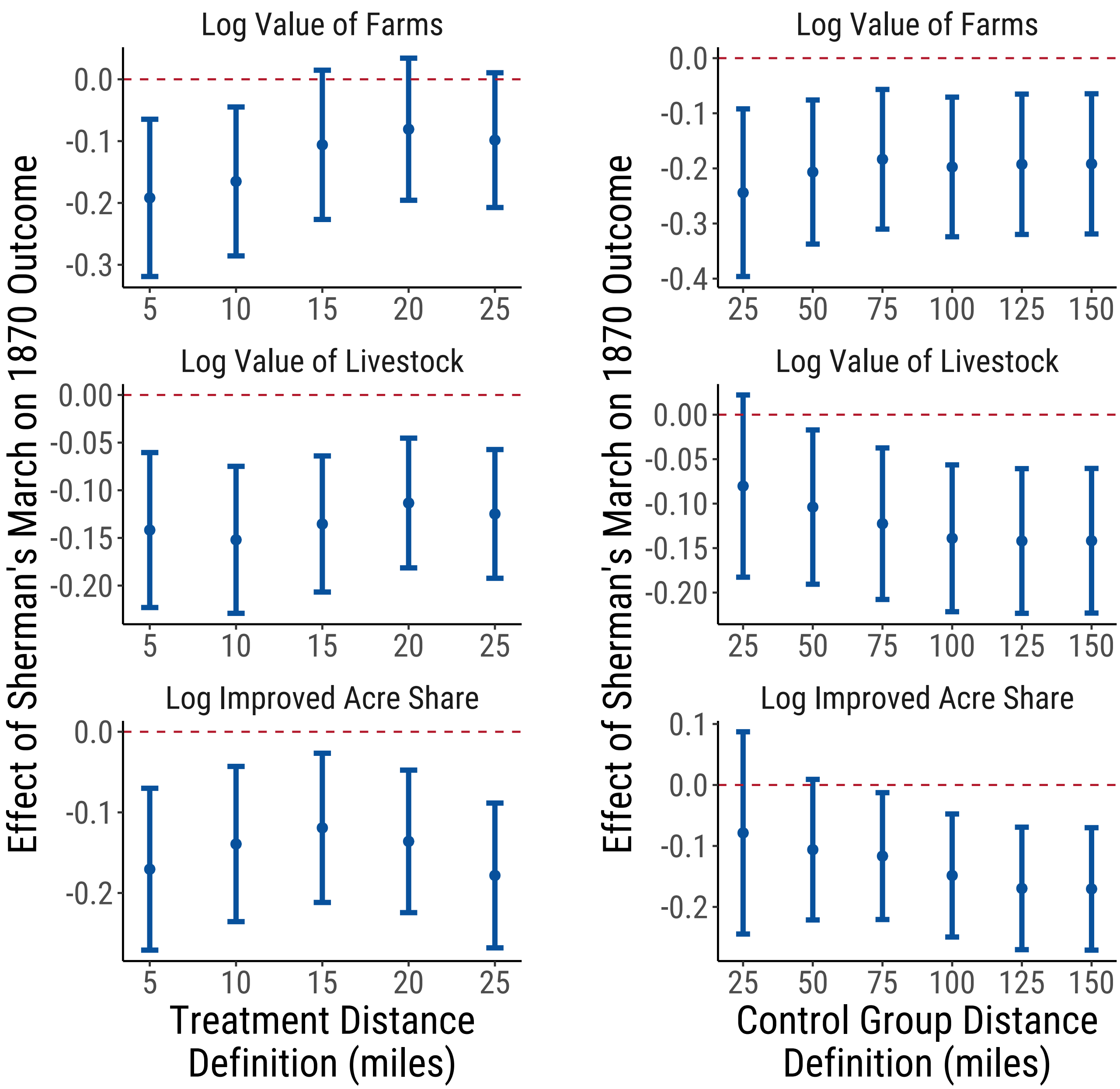


Figure A.1: Estimated effects of 852 placebo march paths between triples of Southern cities on 1870 outcomes. The t-statistics from Sherman's March are indicated with the dashed vertical line. In five of six specifications, fewer than 5% of the t-statistics from the placebo marches are as negative as the estimated Sherman effects. Placebo marches are built 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

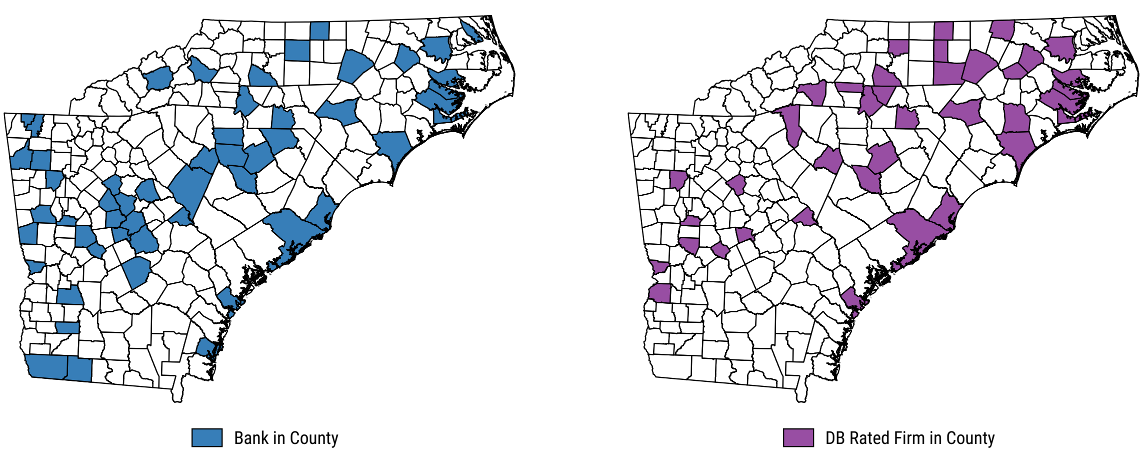
Figure A.2: Alternative treatment and control definitions, plotted with 90% confidence intervals

Table A.3: Instrumental Variables Robustness: Differences in Agricultural Outcomes Relative to 1860, 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 × 1850	-0.033	-0.076	-0.019	-0.038	0.004	-0.034
	(0.073)	(0.111)	(0.040)	(0.055)	(0.053)	(0.078)
Sherman × 1870	-0.193*	-0.186*	-0.134**	-0.145*	-0.150**	-0.157*
	(0.099)	(0.106)	(0.064)	(0.082)	(0.074)	(0.090)
Sherman × 1880	-0.030	0.042	0.018	0.052	-0.104*	-0.076
	(0.076)	(0.077)	(0.047)	(0.044)	(0.058)	(0.063)
Sherman × 1890	-0.025	0.081	-0.057	-0.010	-0.118**	-0.113**
	(0.100)	(0.096)	(0.052)	(0.054)	(0.059)	(0.056)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125

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 information on variables is 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.

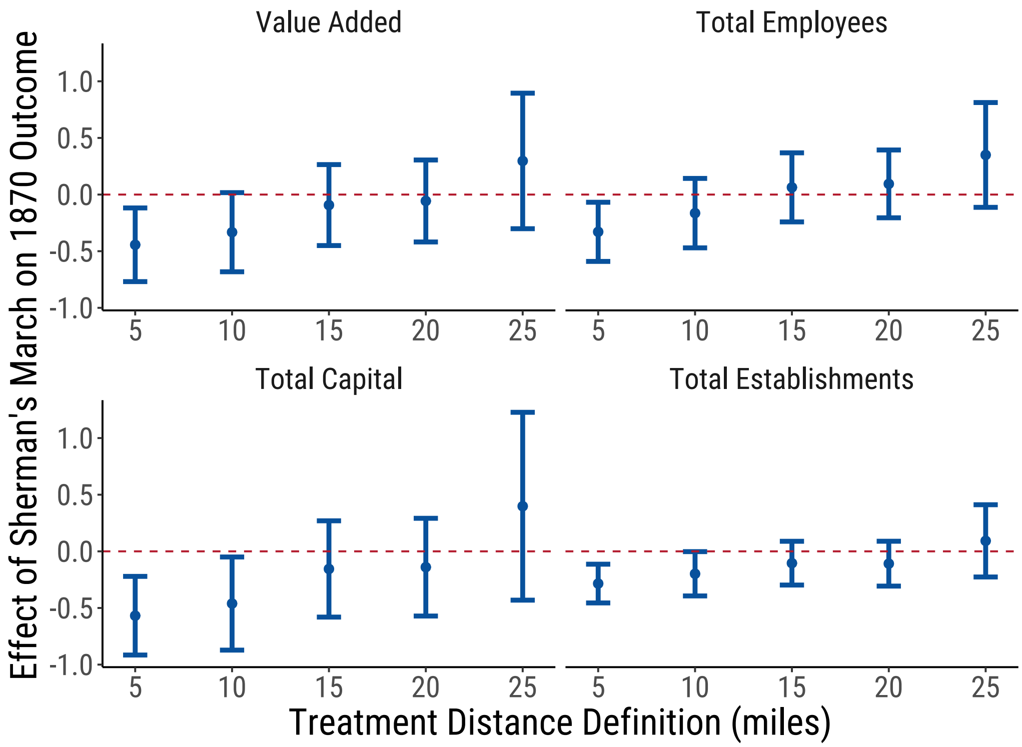
* $p<0.1$, ** $p<0.05$, *** $p<0.01$



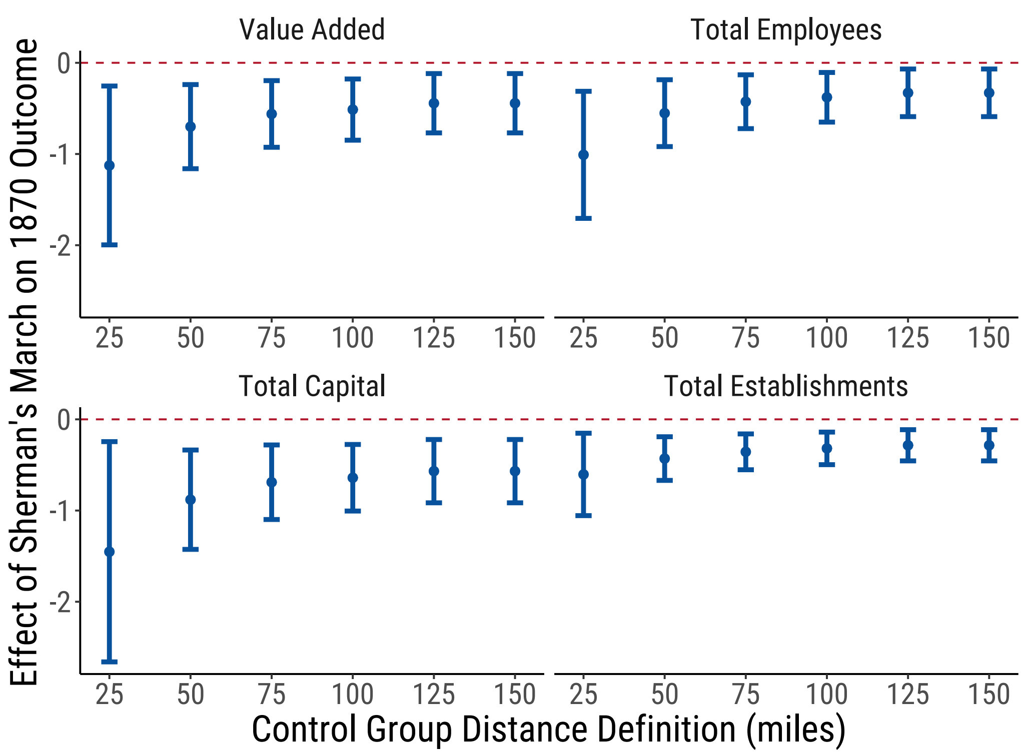
(a) Counties with banks in 1859 in Georgia, South Carolina, and North Carolina

(b) Counties with high demand for credit (based on Dun, Boyd, & Company data from 1860) in Georgia, South Carolina, and North Carolina

Figure A.3



(a) Sherman's effect across alternative treatment definitions



(b) Sherman's effect across alternative control definitions

Figure A.4: Alternative treatment and control definitions for manufacturing outcomes, plotted with 90% confidence intervals

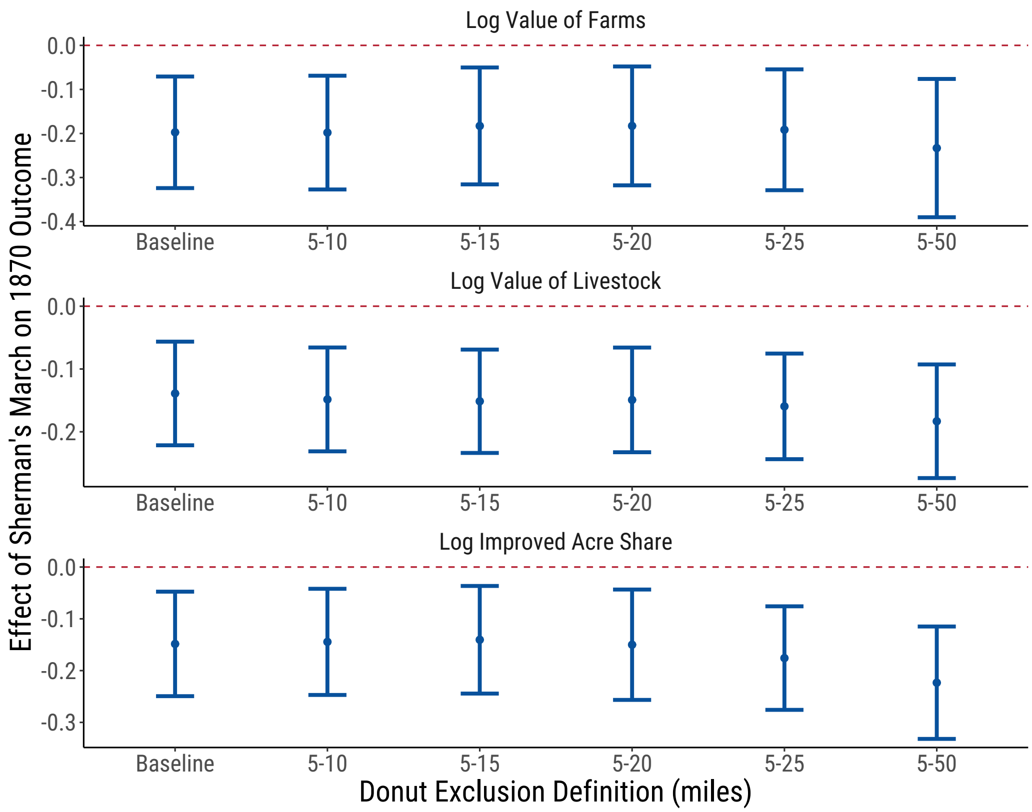


Figure A.5: Sherman's effect on agricultural across donut designs excluding counties neighboring Sherman counties, plotted with 90% confidence intervals

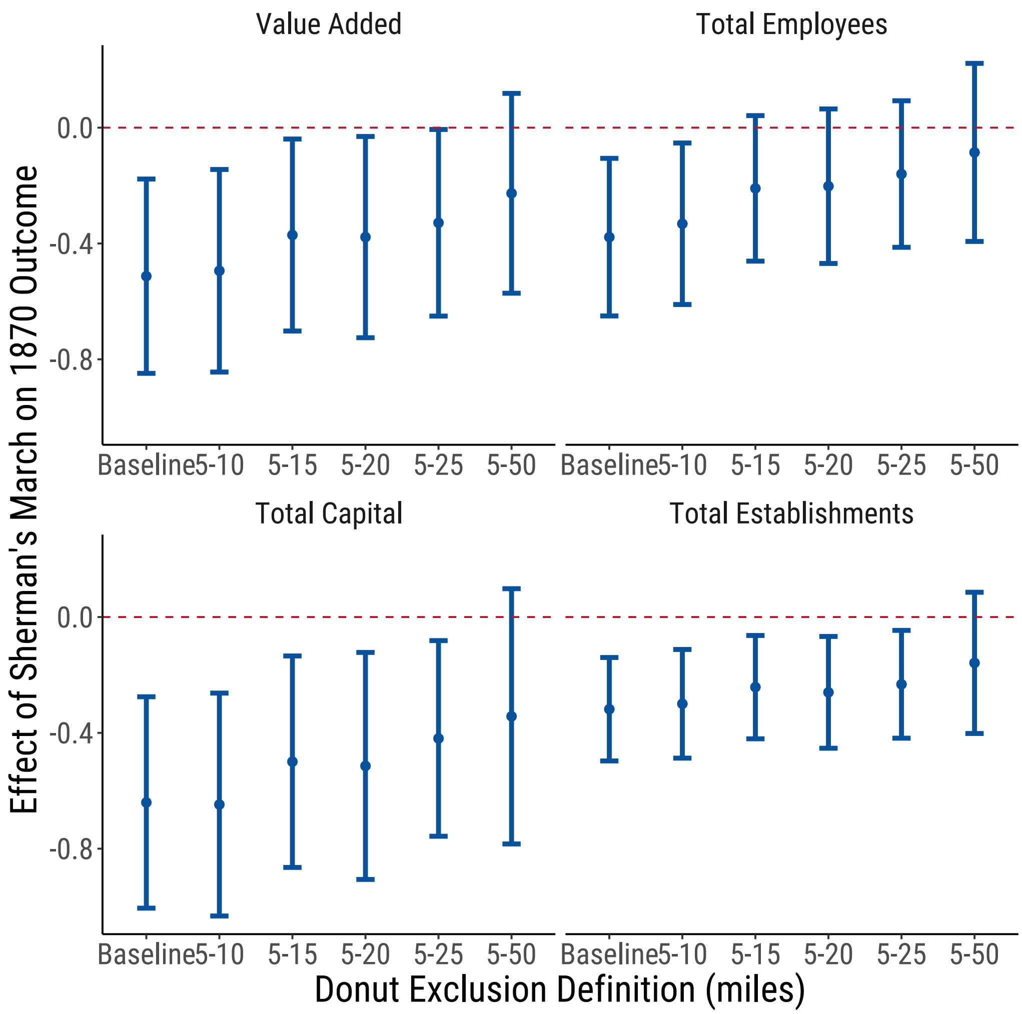


Figure A.6: Sherman's effect on manufcaturing across donut designs excluding counties neighboring Sherman counties, plotted with 90% confidence intervals

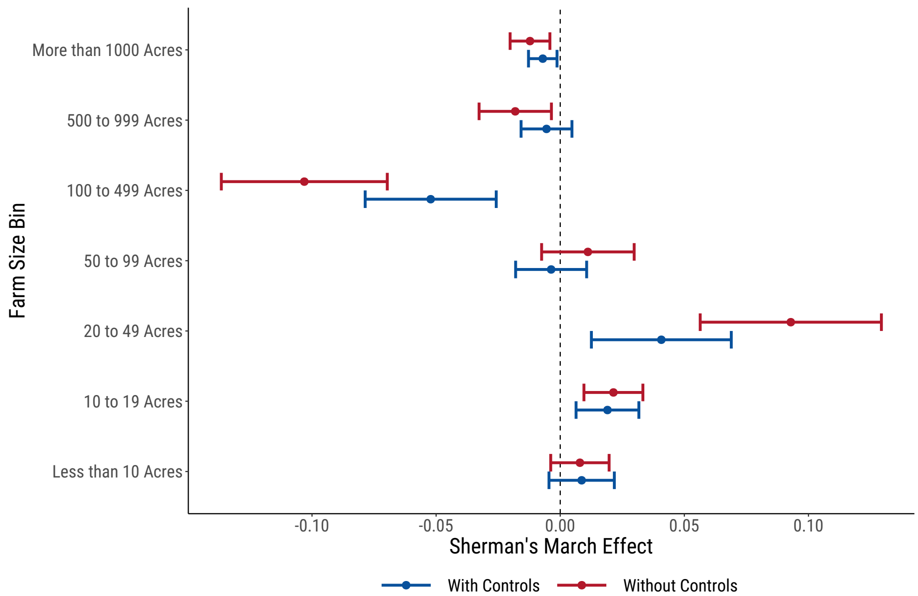


Figure A.7: Sherman's March changed the distribution of effective farm sizes in the counties Sherman destroyed. In Figure 6, we showed effects on the number of farms, here we show the share of farms. Counties treated by Sherman had more small farms in the years after the Civil War, with growth in the share and number of farms under 50 acres. We plot coefficients from separate regressions with the share of farms 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 and we plot 90% confidence intervals around the point estimates.

Table A.4: Instrumental Variables Robustness: Change in Manufacturing Outcomes, by Sherman March Exposure, 1860-1870

	Change in Manufacturing Outcomes from 1860 to 1870							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.655***	-0.819**	-0.412**	-0.577**	-0.655**	-1.095***	-0.338***	-0.434**
	(0.239)	(0.341)	(0.198)	(0.262)	(0.254)	(0.391)	(0.123)	(0.171)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
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. More information on variables is 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.2 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).[49] Between Atlanta and Savannah alone, Sherman claimed to have destroyed 310 miles of track (Trudeau 2008, p. 533). These claims may 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 A.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. Demographics do not appear to explain much of the economic effects of the march. Looking across both total population and the African American population, 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

[49] 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).

Table A.5: Demographic and Infrastructure Outcomes, by Sherman March Exposure, 1850-19890

	Log Population		Log Black Population		IHS Railroad Miles	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × 1850	0.054*	0.050	0.049	0.068	0.003	0.005*
	(0.028)	(0.038)	(0.045)	(0.059)	(0.002)	(0.003)
Sherman × 1870	-0.024	-0.030	-0.003	0.011	-0.002	-0.002
	(0.019)	(0.022)	(0.032)	(0.037)	(0.001)	(0.002)
Sherman × 1880	0.021	0.016	0.057	0.044	-0.004	-0.005
	(0.023)	(0.025)	(0.040)	(0.045)	(0.003)	(0.004)
Sherman × 1890	-0.027	-0.018	-0.006	-0.019	-0.002	-0.003
	(0.039)	(0.037)	(0.059)	(0.058)	(0.005)	(0.005)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 RR Controls × Year	No	Yes	No	Yes	No	No
1860 Population Control × Year	No	No	No	Yes	No	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.936	0.943	0.937	0.939	0.700	0.704
Clusters	225	225	225	225	225	225

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. We avoid including the 1860 control that corresponds to the outcome in columns 2 and 6. 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 information on variables is 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. Railroad miles are measured as miles of track per county square mile with railroad data from Atack (2021); we use the inverse hyperbolic sine transformation to avoid excluding counties with no railroad track.

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

not different across march and non-march counties.

These results suggest that differences in either demographic composition or infrastructure cannot 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. In Appendix A.3, we investigate whether or not our null results on overall migration are hiding selection in *who* leaves Sherman counties. Combined with the restuls of Table A.5, we are more confident that demographic changes are not a mechanism that can explain our estimated Sherman effects on agriculture or manufacturing.

A.3 No Selected Out-Migration from Sherman Treated Counties: Evidence from Census-Linked Sample of Individuals

In Subsection A.2, we presented evidence that the effects of Sherman's march on the treated counties were not driven—or magnified or dulled—by changes in county-level demographics.

However, at the county-level, our analysis was focused on total population and the racial composition of counties. In this appendix section, we present more detailed evidence on selective migration, using a linked sample of people matched across censuses. In a differences in differences analysis, comparing Sherman treated counties with untreated counties and people linked from 1860 to 1870 (treated by the march and the Civil War) with people linked 1850 to 1860 (pre-treatment), we find no strong evidence that people fled the Sherman counties in general or that selected types of people—young or old, farmers or non-farmers, wealthy or not wealthy—were more or less likely to move out of the Sherman-treated counties.[50] We do see some evidence that farmers were slightly more likely to move out of the Sherman treated counties than their non-farming counterparts but these effects are relatively small (2 to 4 points on base out-migration rates of 25 to 50%, depending on the exact outcome and specification) and could be direct results of the finance-exacerbated declines in agriculture in the Sherman affected counties.

A.3.1 Linking the Sample

To estimate the (differential) migration effects of Sherman's march, we link the 0 to 65 year-old white male population of Georgia, North Carolina, and South Carolina in 1860 ahead to 1870, as well as a pre-treatment linked sample of those same states in 1850 ahead to 1860. We construct these links using the links created by the Census Linking Project.[51] Our final sample is 281,362 individuals.[52]

We focus on the white population for two reasons. First, linking rates are notoriously low for blacks in any period. Second, the enslaved population was not enumerated by name before the Civil War. So there are vastly more enumerated African Americans in 1870 than in 1860 or 1850 and the resulting samples would be tiny and potentially selected.

A.3.2 Empirical Strategy and Results

With the linked sample, we implement a simple difference in difference, comparing migration choices of people in Sherman counties in 1850 to people in Sherman counties in 1860, thus differencing out any county-level fixed effects. We include control for state by year fixed effects, individual controls (specifically fixed effects in age in the base year), and (in some

[50] Our goal in this paper is to understand location effects and so we do not analyze the effects of Sherman's march on the individuals we track in this section. We leave analysis of the effects on people exposed to Sherman or not on occupation scores, indvidual wealth measures, marriage, fertility, occupation choice, and other outcomes for future work. For discussion of the potential intergenerational wealth effects of the Civil War and the march, see Ager, Boustan, and Eriksson (2019).

[51] For a more general review of automated linking procedures, see Abramitzky, Boustan, Eriksson, Feigenbaum, and Pérez (2019).

[52] The regression samples are smaller as we restrict to our baseline Sherman treated and control counties, omitting the vertex cities and counties farther than 100 miles from Sherman's march.

specifications) county controls in 1860 × year fixed effects, mirroring our main specfication in the paper. The migration choices are measures 10 years later in the linked censuses. We construct three primary migration outcomes: did an individual migrate out of their baseline county?; did an individual migrate out of their baseline state?; did an individual leave the three Sherman states (GA, NC, and SC)? Following Bailey, Cole, Henderson, and Massey (2017), we generate inverse propensity weights (IPW) to reweight our sample and account for selection in linking rates. The weights are based on first and last name commonness, first and last name length, the presence of middle initials in the census record, and third-order polynomials in age and year of birth.

We have four main findings in our linked sample. First, overall, whites in the Sherman treated counties were no more or less likely to move along any of our migration outcomes, as we show in Table A.6. While the estimated coefficients on Sherman × Post are positive, indicating slightly higher rates of out-migration for individuals in the Sherman treated counties in 1860 (followed to 1870) relative to those counties 1850 to 1860, the estimates are all statistically insignificant and economically quite small. This accords with our previous results that overall net population flows out of Sherman counties are statistically similar to non Sherman counties.

Second, there are no dramatic patterns in selective migration by age, as we show in Figure A.8. To show this, we replace the one Sherman × Post coefficient with six Sherman × Post × Age Bin coefficients, saturating the age-space in the baseline year with 10 year age bins (people aged 50 to 65 are included all in the final bin). We include fixed effects for each age bin, as well as interactions of age bins with county fixed effects and with year fixed effects. People over the age of 50, though they out-migrate at lower rates overall, are a bit more likely to be pushed to move by Sherman treatment but these relative differences are quite small and we find it unlikely that our main results are driven by more people over 50 (at baseline; they are over 60 in the following census) fleeing the Sherman-treated counties from 1860 to 1870.

Third, we see farmers are more likely to leave Sherman treated counties than non-farmers, though the magnitude of the Sherman effect is only a few percentage points (Table A.7). We see this whether we measures farmers as people who reported farmer as an occupation in the or were coded by IPUMS in the agriculture industry or as living on a farm, all measured in the base year. This suggests that there was some selective migration and it is consistent with the negative effects on agricultural land value and investment we document. The increase in out-migration by farmers could be a direct result of the finance-exacerbated decline in agriculture in the Sherman treated counties.

Table A.6: No Differential Out-Migration From Sherman's March Counties, Census-Linked Samples from 1850-1860 and 1860-1870

	All Whites, 0-65 in Base Year					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	0.010	-0.010	0.003	0.003	-0.002	0.001
	(0.021)	(0.024)	(0.011)	(0.011)	(0.009)	(0.009)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	261,786	261,786	261,786	261,786	261,786	261,786
Adjusted R^2	0.087	0.089	0.064	0.065	0.059	0.061
Clusters	225	225	225	225	225	225
	All Whites, 20-50 in Base Year					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.003	-0.024	-0.012	-0.010	-0.019	-0.014
	(0.022)	(0.025)	(0.015)	(0.017)	(0.012)	(0.012)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	88,545	88,545	88,545	88,545	88,545	88,545
Adjusted R^2	0.085	0.088	0.061	0.063	0.058	0.059
Clusters	225	225	225	225	225	225

Note: Standard errors clustered by county. Sample includes only white men. Following Bailey, Cole, Henderson, and Massey (2017), we generate inverse propensity weights to reweight our sample and account for selection in linking rates.

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

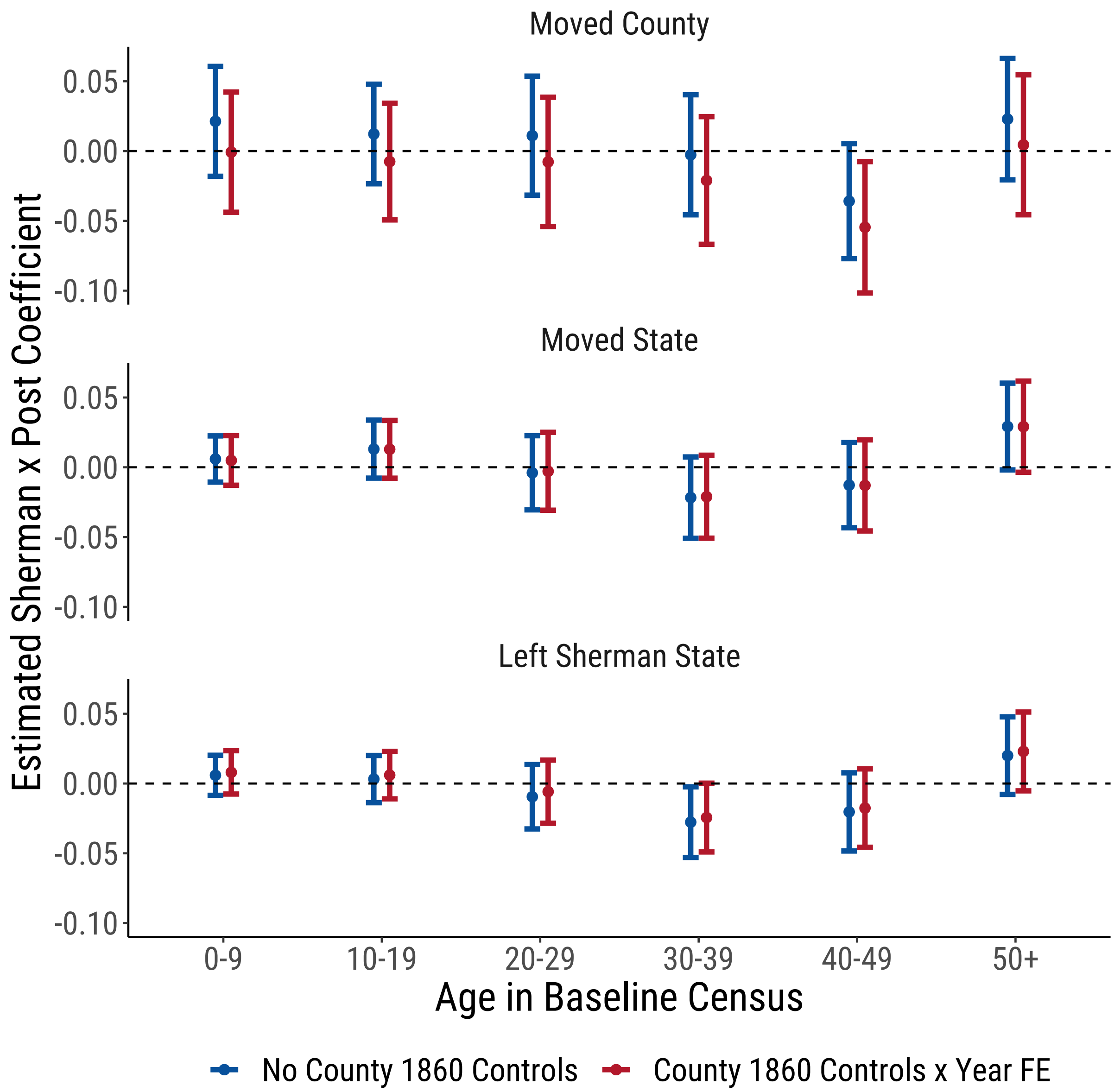


Figure A.8: Estimated effects of Shermans March × Post × Age Bin in Individual Census Linked Analysis, plotted with 90% confidence intervals.

Table A.7: Small Differential Out-Migration by Farmers From Sherman's March Counties, Census-Linked Samples from 1850-1860 and 1860-1870

	Farmers by Occupation					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.032	-0.053**	-0.035**	-0.032*	-0.039**	-0.032**
	(0.024)	(0.026)	(0.018)	(0.019)	(0.016)	(0.014)
Sherman × Post × Farmer Occupation	0.039*	0.034*	0.042**	0.036**	0.038**	0.033**
	(0.020)	(0.019)	(0.017)	(0.016)	(0.016)	(0.016)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,017	103,017	103,017	103,017	103,017	103,017
Adjusted R^2	0.103	0.106	0.069	0.071	0.065	0.066
Clusters	225	225	225	225	225	225

	Agriculture by Industry					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.030	-0.054**	-0.029*	-0.027	-0.031**	-0.024*
	(0.024)	(0.027)	(0.017)	(0.019)	(0.015)	(0.014)
Sherman × Post × Agriculture Industry	0.040**	0.037**	0.033**	0.028*	0.026*	0.022
	(0.019)	(0.018)	(0.016)	(0.015)	(0.015)	(0.015)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,017	103,017	103,017	103,017	103,017	103,017
Adjusted R^2	0.102	0.105	0.068	0.070	0.064	0.066
Clusters	225	225	225	225	225	225

	Lives on Farm by Residence Status					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.033	-0.056**	-0.039**	-0.035*	-0.040**	-0.031*
	(0.025)	(0.028)	(0.019)	(0.021)	(0.017)	(0.016)
Sherman × Post × Lives on Farm	0.041*	0.037*	0.043**	0.037*	0.036*	0.030
	(0.021)	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,017	103,017	103,017	103,017	103,017	103,017
Adjusted R^2	0.103	0.105	0.070	0.071	0.065	0.067
Clusters	225	225	225	225	225	225

Note: Standard errors clustered by county. Sample includes only white men, aged 20 or more in the base year. Following Bailey, Cole, Henderson, and Massey (2017), we generate inverse propensity weights to reweight our sample and account for selection in linking rates.

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

Fourth and finally, we find little evidence of a differential Sherman effect when splitting our sample by two other important socio-economic measures in the base year: marital status and wealth status. Married and single men move at different rates (not surprisingly, single men move more) but not by Sherman treatment status, as we show in the top panel of Table A.8. We find similar and null results when we cut by occupation score among the non-farmer population: high and low SES men are not affected by Sherman differentially (not reported here). Finally, when we compare men with and without real estate wealth in the base year (we use real estate wealth because personal property was not recorded in the 1850 Census), there is no evidence of differential Sherman effects. We show this in the bottom panel of Table A.8 where we split at zero versus non-zero wealth because the median man in our sample has zero wealth.

A.4 Robustness for the Role of Credit Markets in the Extent of Capital Devastation

This appendix subsection discusses additional robustness tests related to Section 6.

One concern with the analysis presented in Table 5 is that antebellum bank location is clearly not random. Endogenous bank location would be a problem if locations with banks systematically experienced negative shocks in 1860s, independent of Sherman-caused capital destruction. We develop a placebo test to rule out this possibility. We draw on the the placebo marches described in Section 5.2.2 and study whether the closeness to antebellum (1859) banks predicts any negative relative effects in counties that could be exposed to similar (non-Sherman destruction) shocks as our Sherman counties, again studying counties located on the paths between southern cities. To do this, we collected data from the Census of Manufactures with industry by county level data for 1860, 1870 and 1880 for the non-Sherman southern states and created a set of placebo Marches, as previously described. In Figure A.9 (similar to Figure A.1), we plot the distribution of t-statistics on the bank and Sherman treatment interaction. For all four outcomes, we find placebo marches rarely yield as large and negative t-statistics as the true Sherman's march, doing so less than 5% of the time in all specifications and less than 1% of the time in four specifications.

Our credit market mechanisms are generally robust to the main tests discussed in reference to the main agricultural and manufacturing results. First, we use our straight-line IV strategy in Table A.9, replicating the results in Table 5. Generally, the results are similar though statistically a bit weaker, as was the case when we applied the IV in to the main results. Second, we show that our manufacturing growth effects are robust to alternative treatment and control bandwidths, though we lose statistical precision as treatment grows too wide (and encompasses too many control counties). See Figure A.10a and A.10b for the

Table A.8: No Differential Out-Migration From Sherman's March Counties, Census-Linked Samples from 1850-1860 and 1860-1870

	Individuals with Non-Zero Real Estate Wealth in Base Year					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.002	-0.022	-0.013	-0.010	-0.019	-0.011
	(0.022)	(0.026)	(0.017)	(0.019)	(0.014)	(0.015)
Sherman × Post × Non-0 Wealth	0.001	-0.003	0.010	0.005	0.009	0.004
	(0.016)	(0.016)	(0.014)	(0.014)	(0.012)	(0.012)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,017	103,017	103,017	103,017	103,017	103,017
Adjusted R^2	0.111	0.114	0.072	0.074	0.066	0.068
Clusters	225	225	225	225	225	225
	By Marital Status in Base Year					
	Moved County		Moved State		Left Sherman State	
	(1)	(2)	(3)	(4)	(5)	(6)
Sherman × Post	-0.012	-0.032	-0.025	-0.024	-0.025	-0.020
	(0.026)	(0.030)	(0.019)	(0.022)	(0.017)	(0.017)
Sherman × Post × Married	0.017	0.011	0.027*	0.025	0.018	0.016
	(0.020)	(0.020)	(0.016)	(0.017)	(0.015)	(0.015)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year FE	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	103,017	103,017	103,017	103,017	103,017	103,017
Adjusted R^2	0.093	0.096	0.063	0.064	0.060	0.061
Clusters	225	225	225	225	225	225

Note: Standard errors clustered by county. Sample includes only white men, aged 20 or more in the base year. Following Bailey, Cole, Henderson, and Massey (2017), we generate inverse propensity weights to reweight our sample and account for selection in linking rates.

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

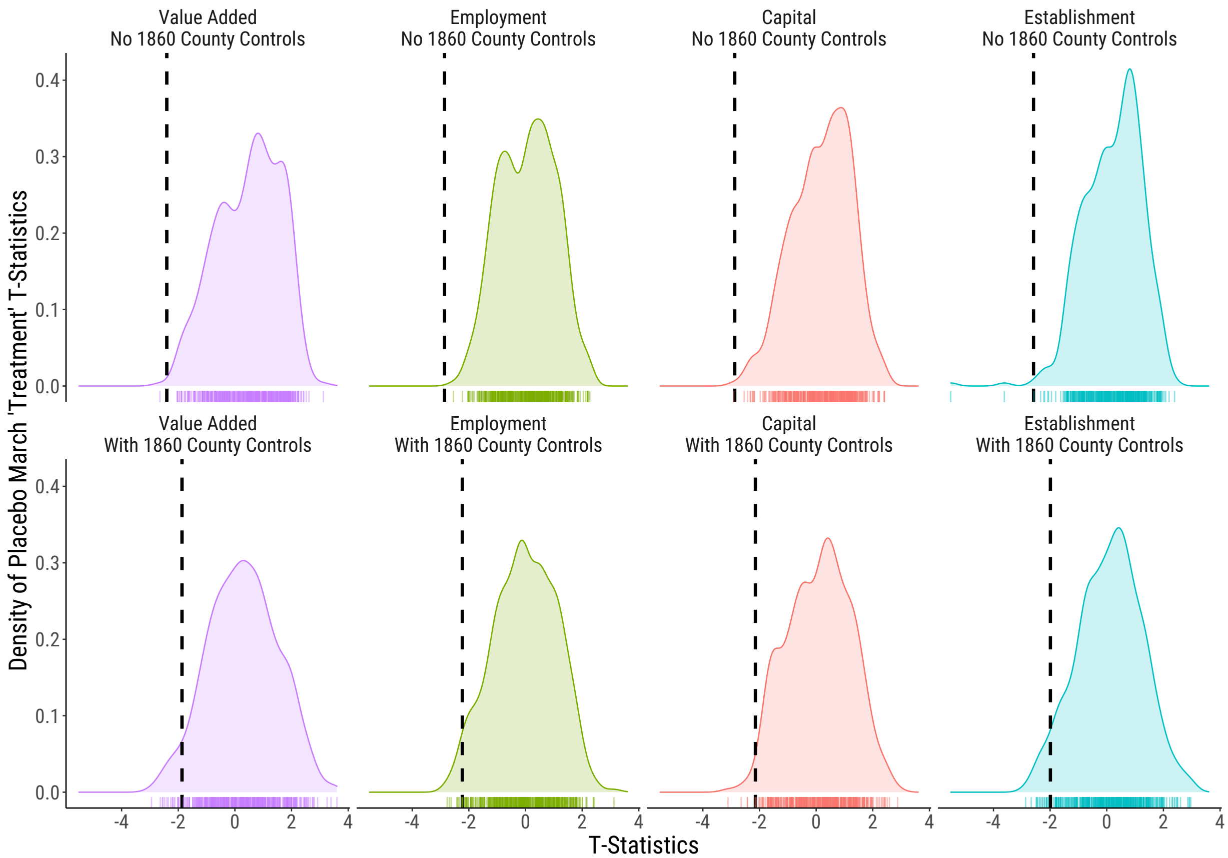


Figure A.9: Estimated effects of Shermans March × Bank County in 852 placebo march paths between triples of Southern cities on growth rates in Census of Manufacturing data from 1860 to 1870. The t-statistics from Sherman's March are indicated with the dashed vertical line. For all four outcomes, fewer than 5% of the t-statistics from the placebo marches are as negative as the estimated Sherman effects. Placebo marches are built 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 2 for each placebo march.

application of this approach to the bank interaction.

Finally, for the agricultural results, we find that using a more extreme definition of wealthy individual—top 2%—provides very similar results in the top panel of Table A.10. In the bottom panel of Table A.10, 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. 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.

A.5 Spatially Adjusted Standard Errors

Spatial correlation is a key concern for analyses that exploit location-based heterogeneity in treatment. To deal with concern, we follow the recent empirical literature and we re-estimate our main results using Conley standard errors, which account for correlation across areas—counties in our setting—that are located close to each other (Conley 1999, 2008). Following Kelly (2019)'s warning to make the geographical cutoff radius sufficiently large, we allow errors to be correlated up to 100km in our setting.[53] In practice, for our standard model we implement Conley standard errors using the package that is developed by Thiemo Fetzer, which allows to allow for spatial correlation along a smooth running variable—in our case distance—and temporal correlation.[54] One caveat of this package is that we need to specify the lags of temporal correlation that are allowed in the model: to allow the maximum flexibility, we allow correlation of up to five lags in our time dimension across censuses. When we study the effects of Sherman's march on growth rates in manufacturing using our collapsed difference-in-difference, in which we only exploit cross-sectional variation, we use the standard code developed in Hsiang (2010).

The results are reported in Table A.11 for the main agriculture results and Table A.12 for manufacturing. To facilitate the comparison with the main analyses, we report in these tables both the original clustered standard errors from the main results and the new Conley-

[53]Our empirical setting is conceptually related to the cricism in Kelly (2019) but different in a key way. Kelly (2019) argues that many persistence papers are plagued by false-positives. If two independent random white noise variables over some xy-space are both spatially correlated, there is a good chance of finding incredibly strong correlations after sampling points (cities) in the plane. For papers investigating the effects in the very long run that compare some spatial independent variable historically to some spatial dependent variable today, this could generate spurious results that appear to be very statistically powerful and robust. However, the Kelly (2019) critique is not directly applied to a difference-in-difference strategy like we use. In our difference-in-difference, we have variation within units across time---we would not be identified if all we had was cross-sectional variation in treatment at some lagged period given our county fixed effects. Still, spatial autocorrelation is potentially an issue, and this is why we implement Conley's errors as discussed in this appendix section.

[54]We thank Professor Thiemo Fetzer for providing the code and documentation online. His files are accessible at: http://www.trfetzer.com/conley-spatial-hac-errors-with-fixed-effects/

Table A.9: Instrumental Variables Robustness: Change in Manufacturing Outcomes from 1860 to 1870, by Sherman March Exposure and Finance Access

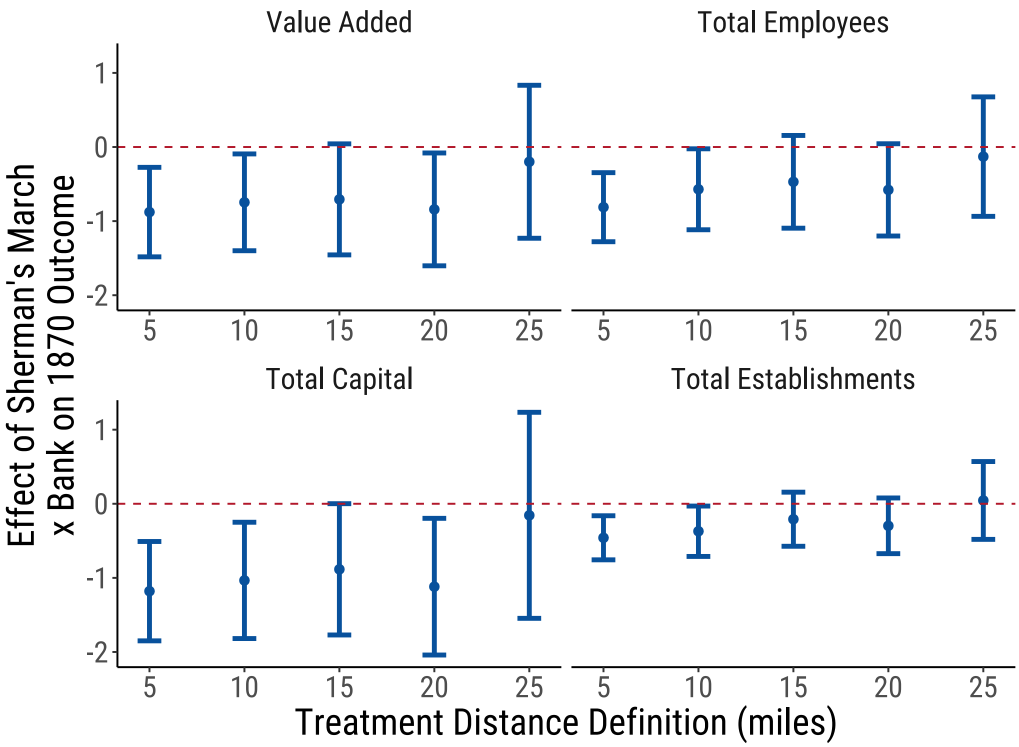
	Bank Status							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	0.147	0.097	0.215	0.093	0.233	-0.023	-0.022	-0.092
	(0.309)	(0.442)	(0.252)	(0.360)	(0.307)	(0.509)	(0.108)	(0.213)
Sherman × Bank	-1.416***	-1.442**	-1.110***	-1.083**	-1.573***	-1.747**	-0.545**	-0.503*
	(0.451)	(0.615)	(0.362)	(0.454)	(0.510)	(0.714)	(0.220)	(0.279)
Bank County	1.068***	0.893***	0.801***	0.536**	1.100**	0.805**	0.662***	0.476***
	(0.352)	(0.311)	(0.270)	(0.219)	(0.467)	(0.390)	(0.178)	(0.142)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Clusters	201	201	201	201	201	201	201	201

	Dun, Boyd, and Company Status							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.285	-0.531	-0.086	-0.342	-0.145	-0.687*	-0.137	-0.293*
	(0.260)	(0.325)	(0.200)	(0.239)	(0.285)	(0.352)	(0.117)	(0.150)
Sherman × DB	-5.643	-5.709	-4.925	-4.615*	-7.726	-8.049*	-2.992	-2.750
	(4.266)	(3.547)	(3.412)	(2.577)	(5.614)	(4.788)	(2.013)	(1.707)
DB County	3.574**	2.932**	3.280**	2.533**	5.076**	4.315**	2.150**	1.602**
	(1.716)	(1.374)	(1.273)	(1.007)	(2.424)	(1.926)	(0.869)	(0.724)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Clusters	201	201	201	201	201	201	201	201

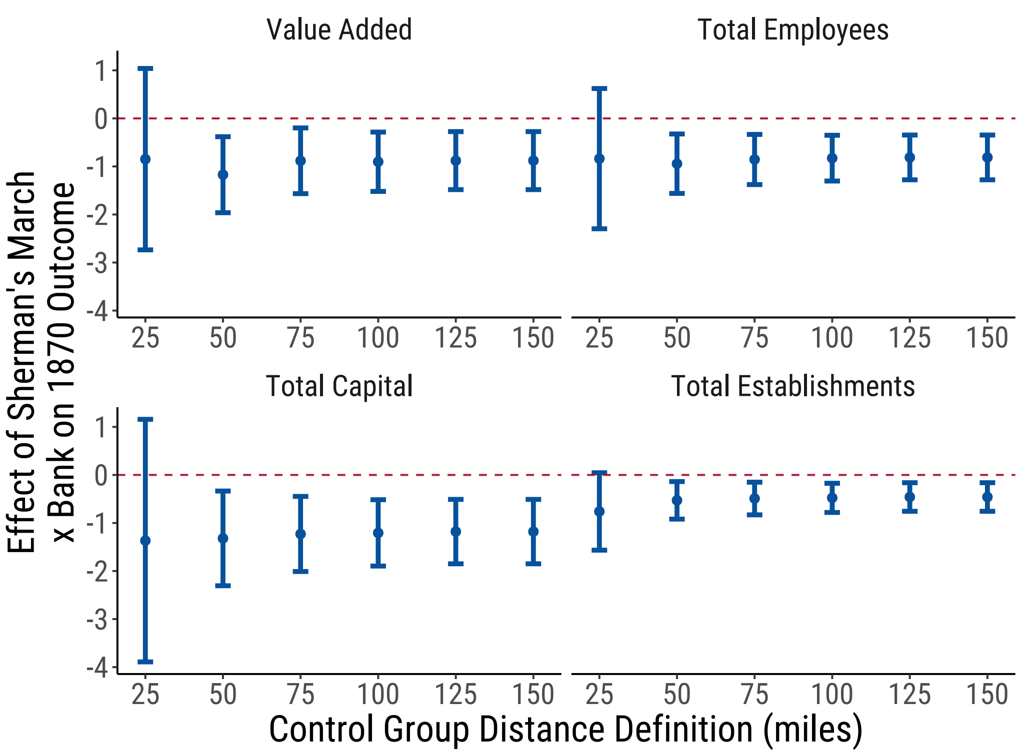
	External Finance Dependence by Industry							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.455	-0.610*	-0.202	-0.365	-0.376	-0.803**	-0.276**	-0.376**
	(0.285)	(0.365)	(0.228)	(0.285)	(0.272)	(0.393)	(0.126)	(0.179)
Sherman × High Fin. Dep.	-0.748*	-0.781*	-0.786**	-0.790**	-1.045**	-1.089**	-0.234	-0.215
	(0.404)	(0.425)	(0.320)	(0.329)	(0.463)	(0.490)	(0.192)	(0.197)
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Clusters	201	201	201	201	201	201	201	201

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. We instrument for Shermans's march exposure with an indicator for counties 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. More information on variables is 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.

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



(a) Sherman's effect across alternative treatment definitions



(b) Sherman's effect across alternative control definitions

Figure A.10: Alternative treatment and control definitions for manufacturing outcomes. Here, we plot the coefficient on the interaction of Sherman's March with the Bank County indicator, the main coefficient of interest in Panel A of Table 5 with 90% confidence intervals.

Table A.10: Robustness to Alternative Wealth Measures: Agricultural Outcomes, by High Wealth 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 × Post	-0.133*	-0.086	-0.135***	-0.124***	-0.159***	-0.138***
	(0.070)	(0.068)	(0.045)	(0.043)	(0.046)	(0.048)
Sherman × Post × High Wealth 2%	0.154	0.129	0.202***	0.192***	0.116	0.090
	(0.095)	(0.105)	(0.071)	(0.067)	(0.090)	(0.093)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.861	0.881	0.877	0.885	0.830	0.833
Clusters	225	225	225	225	225	225

	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 × Post	-0.176***	-0.115*	-0.136***	-0.100**	-0.168***	-0.149***
	(0.065)	(0.061)	(0.044)	(0.040)	(0.043)	(0.048)
Sherman × Post × High Wealth 5% (White)	0.277***	0.205*	0.187***	0.117*	0.109	0.089
	(0.105)	(0.107)	(0.071)	(0.071)	(0.099)	(0.093)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
High Wealth × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.864	0.885	0.877	0.885	0.828	0.834
Clusters	225	225	225	225	225	225

Robustness to Table 6. 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 information on variables is 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.11: Conley Spatial Standard Errors Robustness: Differences in Agricultural Outcomes Relative to 1860, 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 × 1850	0.044	0.013	0.037	0.020	0.067	0.027
	(0.059)	(0.085)	(0.033)	(0.042)	(0.044)	(0.062)
	[0.079]	[0.080]	[0.051]	[0.043]	[0.061]	[0.061]
Sherman × 1870	−0.197**	−0.188**	−0.139***	−0.131**	−0.148**	−0.122*
	(0.077)	(0.081)	(0.050)	(0.060)	(0.061)	(0.067)
	[0.097]	[0.079]	[0.068]	[0.062]	[0.066]	[0.062]
Sherman × 1880	−0.040	0.037	−0.033	0.015	−0.135***	−0.094**
	(0.059)	(0.055)	(0.037)	(0.032)	(0.046)	(0.044)
	[0.077]	[0.055]	[0.048]	[0.035]	[0.048]	[0.042]
Sherman × 1890	−0.060	0.037	−0.086**	−0.031	−0.139***	−0.100**
	(0.075)	(0.070)	(0.043)	(0.041)	(0.048)	(0.044)
	[0.089]	[0.059]	[0.052]	[0.040]	[0.053]	[0.041]
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
1860 County Controls × Year	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,125	1,125	1,125	1,125	1,125	1,125
Adjusted R^2	0.853	0.880	0.865	0.883	0.811	0.824
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 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, 1850-1890. Standard errors clustered at the county level reported in parentheses. Conley (1999) spatially adjusted standard errors reported in brackets with a geographical cutoff radius of 100km.

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

based standard errors. Our key conclusion is that the substantive interpretation does not change when we adjust for spatial correlation. Among all the coefficients, we find that the new and old standard errors are generally very close in magnitude. The standard errors are also not consistently smaller or larger in either calculation. Altogether, these tests confirm that our results are robust to concerns of spatial correlation in our setting.

Table A.12: Conley Spatial Standard Errors Robustness: Change in Manufacturing Outcomes, by Sherman March Exposure, 1860-1870

	Change in Manufacturing Outcomes from 1860 to 1870							
	Value Added		Employment		Capital		Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sherman	-0.513**	-0.516**	-0.378**	-0.459**	-0.640***	-0.905***	-0.318***	-0.379***
	(0.204)	(0.257)	(0.165)	(0.203)	(0.222)	(0.302)	(0.109)	(0.132)
	[0.202]	[0.242]	[0.156]	[0.187]	[0.184]	[0.248]	[0.089]	[0.108]
1860 County Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,404	1,404	1,404	1,404	1,404	1,404	1,404	1,404
Adjusted R^2	0.008	0.042	0.008	0.060	0.008	0.070	0.011	0.063
Clusters	201	201	201	201	201	201	201	201

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. More information on variables is 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 clustered at the county level reported in parentheses. Conley (1999) spatially adjusted standard errors reported in brackets with a geographical cutoff radius of 100km.

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