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The Effect of Military Campaigns on Political Identity: Evidence from Sherman's March

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Master Thesis

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Declaration of Authorship

I hereby proclaim that I wrote my master thesis on my own under the leadership of my supervisor and that the references include all resources and literature I have used.

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The Master Thesis Proposal

Author of the master thesis: **Martin Kosík**
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Title: **The Effect of Military Campaigns on Political Identity: Evidence from Sherman's March**

Research question and motivation
Causes and consequences of civil wars have received increased attention within economics in the past decade (Blattman and Miguel 2010). Study of the political and social effects of wartime violence is important for understanding why some societies spiral back into violence whereas others experience rapid recovery and sustained peace. The nascent literature on this topic shows that exposure to violence in war can increase cooperation and pro-social behavior, although this seems to be directed only to in-group members (Bauer et al. 2016). The evidence on the effect on political identity and behavior is much more limited and ambiguous. Blattman (2009) and Bellows and Miguel (2009) report higher community participation and political engagement among individuals directly exposed to war violence in northern Uganda and Sierra Leone, respectively. However, Adhvaryu and Fenske (2014) fail to find any significant effect of exposure to violent conflict in childhood on political beliefs or engagement using surveys of 17 countries in sub-Saharan Africa. Tangent literature within political science studies the long-term effects of repression instead of civil wars but the evidence is not clear here either. Lupu and Peisakhin (2017) show that repression increase political engagement, while Zhukov and Talibova (2018) find the opposite.
Contribution
My thesis would contribute new credible evidence to relatively small literature on the effect of military campaigns on political outcomes. Moreover, unlike most previous work which studies recent conflicts I could measure how persistent the potential effects are.

In addition, existing literature focuses mainly on civil wars in sub-Saharan Africa or other developing regions. These conflicts feature asymmetric and irregular warfare of insurgents against government. In contrast, the American Civil War was by and large fought in conventional manner and therefore my thesis could bring new perspective.

Methodology

The data will come from various sources. First, I plan to digitize the 1865 US War Department map of Sherman's march and combine it with important economic and social indicators from historical US censuses (including the share of population that were enslaved in 1860) that are typically used in the literature on this period (Acharya, Blackwell, and Sen 2016). These data would be complemented by panel of county-level results of presidential and congressional elections from 1840 to present (Clubb, Flanigan, and Zingale 2006). I would also use the survey of Confederate monuments and memorials conducted by the Southern Poverty Law Center (SPLC 2019). Finally, if the computational demands are not too high, I plan to download the names of all streets, roads, and schools in the relevant area using the OpenStreetMap database to measure what percentage are named after Confederate politicians and generals.

The identification strategy of the march's effect on outcomes for which antebellum county-level panel data is available (e.g. the Democratic party's vote share) will be based on a difference-in-differences design. The synthetic control method could be also applied as a robustness check in case of a possible violation of parallel trends assumption.

For the other outcome variables (e.g. the density of the Confederate memorials), I will follow Feigenbaum, Lee, and Mezzanotti (2018) and use the straight-line segment between the three main cities that the march targeted (Atlanta, Savannah, and Columbia) as an instrumental variable, as counties within this segment had a higher probability of exposure to the march but for a reason that is likely exogenous to later political outcomes.

Outline

1. Introduction
2. Literature Review
3. Historical Background
4. Identification Strategy

5. Results

6. Conclusion

References:

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Abstract

I use the military march of Union general William Sherman during the American Civil War to estimate the effects of wartime violence and destruction on post-war voting behavior and personal identity. First, I examine how the march influenced the support for the Democrats throughout the 19th and 20th centuries. Second, to proxy for the strength of Southern identity, I construct several variables from both historical and contemporary sources. These variables include the share of individuals likely named after famous Confederate generals, the relative frequency of streets likely named after Confederate figures, and the presence of Confederate monuments. The results show mostly small and statistically insignificant effects of the march on Democratic vote share. For some outcomes proxying for Southern identity, I find a significant positive effect; however, these results are not robust across different model specifications. Overall, the results suggest that Sherman's march did not have a transformative impact on the politics and personal identity in the US South.

Abstrakt

Používám vojenský pochod generála Unie Williama Shermana během Americké občanské války k estimaci efektu válečného násilí a destrukce na poválečné volební chování a osobní identitu. Zaprvé, zkoumám jak tento pochod ovlivnil podporu Demokratické strany během 19. a 20. století. Zadruhé, vytvořil jsem několik proměnných na základě jak historických tak současných zdrojů k tomu abych zachytil sílu Jižanské identity. Tyto proměnné zahrnují podíl osob, které jsou pravděpodobně pojmenované po generálech Konfederace, relativní frekvenci ulic pravděpodobně pojmenovaných po osobnostech Konfederace a přítomnost pomníků Konfederace. Výsledky ukazují převážně malé a statisticky nevýznamné efekty pochodu na volební podíl Demokratů. Pro některé závislé proměnné, které zachycují jižanskou identitu, nacházím signifikantní pozitivní efekty, nicméně tyto výsledky nejsou robustní napříč různými specifikacemi modelu. Celkově výsledky naznačují, že Shermanův pochod neměl transformativní dopad na politiku a osobní identitu v Jihu Spojených států.

Introduction

A growing number of empirical and theoretical studies have highlighted personal identity as an important determinant of economic and political behavior (Akerlof and Kranton, 2000; Benjamin et al., 2010; Shayo and Zussman, 2011; Cohn et al., 2015; Atkin, 2016; Grossman and Helpman, 2021). The overwhelming majority of the studies have treated personal identity as given and exogenous. However, this neglects the fact that individuals choose their personal identity and their identification with a particular group can change in response to external forces. A nascent strand of literature attempts to fill this gap by studying the process of identity formation and choice (Shayo, 2009; Atkin et al., 2021). Several empirical studies show that exposure to certain significant events (e.g., economic downturn), especially at an early age, can have long-lasting impact on preferences and personal identity (Yanagizawa-Drott and Madestam, 2011; Giuliano and Spilimbergo, 2014).

The focus of this thesis is the effect of wartime destruction on identity formation. Although the causes and consequences of wars have received increased attention within economics in the past decade (Blattman and Miguel, 2010), the impact of violence on personal identity have been studied relatively little. The existing literature has mostly focused on how exposure to violence in war influences cooperation and pro-social behavior (Bauer et al., 2016).

The potential effects of war on political behaviouŕ and identity could possibly explain some previous findings in the literature. Specifically, several studies have reported strong correlations between historical conflict and various aspects of institutions (Besley and Reynal-Querol, 2014; Ray and Esteban, 2017). This may seem difficult to reconcile with other studies which suggest that the destruction in war does not generate persistent poverty traps and the economy is able recover from these shocks relatively fast.[1] Given the recent literature showing high persistence of culture (Nunn and Wantchekon, 2011; Voigtländer and Voth, 2012; Alesina et al., 2013), it is possible that certain aspects of political identity and behaviour could be affected by historical conflict and influence the character of the present-day institutions even if the economy had fully recovered.

I estimate the long-run effects of the notoriously destructive military march

[1] See Miguel and Roland (2011) for the long-run economic effects of bombing Vietnam, Davis and Weinstein (2002) for the effects of Allied bombing of Japan during World War II, and Brakman et al. (2004) for the impact of destruction of German cities during World War II. All of these studies find no evidence of persistent poverty traps. A small exception is Feigenbaum et al. (2018) (which is further discussed in subsection 2.1).

of Union General William Sherman during the American Civil War on political outcomes. Specifically, I examine if the march influenced the vote share of the Democratic party in presidential elections (which prior to the 1960s was associated in the South with support for segregation and opposition to attempts to enforce civil rights by the federal government), the proportion of individuals sharing their first names with famous Confederate generals, the share of streets likely named after famous Confederate figures, the construction of Confederate monuments, and lynchings. I find small and mostly statistically insignificant effects, suggesting that the march did not substantially influence the politics or identity in affected areas of the US South.

This thesis also closely relates to Feigenbaum et al. (2018) who examined the economic effects of Sherman's march. They find that the value of farms partially recovered after 20 years, although lower agricultural investment lasted at least to the 1920s. Moreover, there was a large and persistent increase in land concentration in the counties hit by Sherman's march. In light of this, the small effects on voting outcomes I find in this thesis are somewhat surprising given that there is extensive literature emphasizing the importance of wealth inequality for politics (e.g., Acemoglu and Robinson, 2005).

The structure of the rest of the thesis is as follows. Section 1 reviews the relevant literature, section 2 summarizes the historical context of the study, specifically Sherman's march (subsection 2.1) and the political development in the US South after the Civil War (subsection 2.2). The sources and construction of the data used in this thesis are described in section 3. In section 4, I discuss the three approaches to identification in this context: selection on observables, instrumental variable, and difference-in-differences. The main results of these three methods are presented in section 5. In section 6, I assess the sensitivity of the main results using various robustness checks including alternative definitions of the treatment, the stability of coefficients after dropping some controls and the double machine learning method.

1 Literature Review

This paper relates to several different strands of literature. First is the empirical literature on the effects of wars on social and political preferences. Voors et al. (2012) study the impact of exposure to conflict on social, risk, and time preferences using data from a field experiment in rural Burundi. They show that individuals from villages that experienced greater levels of violence are more altruistic, more-risk seeking, and less patient (i.e., have higher discount rates). Cassar et al. (2013) examine the effect of exposure to conflict in the context of Tajikistan using survey and experimental data, and find that more violence-exposed individuals within the same localities exhibit lower willingness to participate in impersonal exchange and greater reliance on kinship-based norms in resolving disputes. Bauer et al. (2014) conducted social-choice experiments (Sharing and Envy Games) in conflict-affected areas in Georgia and Sierra Leone. The results show an increase in egalitarian choices of individuals with greater exposure to violence, however only towards their ingroup but not their outgroup. Furthermore, the effects are present only if an individual was exposed during a developmental window starting at around 8 years of age and ending at 20 years of age.

Cecchi et al. (2016) also study the impact of violence in Sierra Leone and find lower risk aversion and higher altruism of conflict-exposed individuals, confirming the results of the previous studies. In addition, they also observe that exposure to conflict leads to a greater willingness to compete toward the outgroup. Bauer et al. (2016) conduct a meta-analysis of 16 experimental studies from various settings on the effects of war on social preferences. The results provide fairly strong evidence that there are fairly large positive effects of war on prosociality of allocations in experimental games, social group participation, and community leadership. On the other hand, the effects on trust, voting, and interest in politics are much smaller and not significantly different from zero under the random effects model (in which the true effects are assumed to vary across the studies although they all are drawn from the same distribution). Bauer et al. (2018) show that former child soldiers forcibly recruited by the Lord's Resistance Army in northern Uganda engage more in the local community and are perceived as more trustworthy. Blattman (2009) and Bellows and Miguel (2009) report higher community participation and political engagement among individuals directly exposed to war violence in northern Uganda and Sierra Leone, respectively. On the other hand, Adhvaryu and Fenske (2014) fail to find any significant effect of exposure to violent conflict in childhood on political beliefs or engagement using surveys of 17 countries in sub-Saharan Africa.

Tangent literature within political science studies the long-term effects of repression instead of civil wars but the evidence is not clear here either. Lupu and Peisakhin (2017) show that repression increases political engagement, while Zhukov and Talibova (2018) find the opposite. The common challenge of the studies mentioned studies is potential non-random targeting of violence and non-random attrition of the sample. There are several different approaches to addressing this issue used in the literature. Some studies include various control variables to account for differences in the observable characteristics, while others employ an instrumental variable approach. Finally, in some cases, the authors argue that in the specific context they study the violence was indiscriminate and unlikely to be targeted (e.g., Bauer et al. (2014)). In this paper, I will use both approaches (selection on observables and instrumental variable) and compare their results to assess the robustness.

The second strand of literature relevant to this thesis reflects the growing interest within economics in understanding the impact and formation of personal identity (Shayo, 2020). Akerlof and Kranton (2000) incorporate identity or self-image considerations into a simple utility-maximization problem. In their model, a person is assigned a social identity, which is associated with certain prescriptions. An individual then might suffer a loss of utility if his or her behavior deviates from the prescriptions. Akerlof and Kranton (2000) show that the inclusion of identity can substantively change the predictions of models of gender discrimination, poverty, and division of labor within households. Shayo (2009) develops a more general model of social identity in which both the identity and behavior are determined endogenously, and applies it to the political economy of redistribution. In the model, individuals receive higher utility from identifying with a high-status group; however, they suffer a loss if they identify with a group whose average (prototypical) member is substantially different from them. The equilibrium is then defined as a steady state in which the behavior of an individual is consistent with his or her identity, the identities are consistent with the social environment, and the social environment is determined by the behavior of the individuals. Grossman and Helpman (2021) build a model in the spirit of Shayo (2009) to study the implication of identity for trade policies.

In addition to the theoretical work discussed so far, there is a large empirical literature studying the impact of identity on behavior. First, there are several experimental studies that use priming to increase the salience of a certain identity. For example, Benjamin et al. (2010) test how Asian-American identity, which is hypothesized to be associated with patience, influences the implied discount rates in experimental decisions. They show that Asian-American subjects, whose ethnic identity was primed by asking them about languages spoken in their family, made more patient choices than Asian-American subjects not exposed to the prime. Cohn et al. (2014) and Cohn et al. (2015) conducted similar studies examining the impact of banking culture and criminal identity, respectively, on dishonesty. Second, several studies use observational data combined with plausibly exogenous variation in salience of identity to investigate its effects in a real-world setting. Shayo and Zussman (2011) exploit the effectively random assignment of cases to the judges of Israeli small claims courts to estimate the amount of ingroup bias. They find that the degree of ingroup bias is relatively small during periods of peace, but the bias rises sharply in the aftermath of terrorist attacks. Moreover, Shayo and Zussman (2017) show that the effects of ingroup bias persist for more than 3 years and appear to be driven by divergent dynamics in areas affected by terrorism rather than by personal exposure of the judges. Nevertheless, Fisman et al. (2020) provide evidence that personal exposure is also important. Specifically, they find that Hindu officers in an Indian bank that were exposed to religion-based communal violence during their youth lend more to Muslims, and these loans have a lower default rate, which is consistent with greater taste-based discrimination of violence-exposed officers.

More recently, the questions of identity formation and the choices made by individuals between various personal identities have received greater attention within the empirical literature. Atkin et al. (2021) exploit the differences in food prescriptions and taboos between various Indian ethnic and religious groups to estimate a structural model of personal identity choice fitted on food consumption data. Atkin et al. (2021) use this model to estimate the frequency of changes in identity and its implications for voting, health, and welfare. Eifert et al. (2010) find that individuals in Africa are more likely to identify themselves in ethnic terms (as opposed to religion, class/occupation, or gender) during periods of heightened political competition, which suggests that ethnic identities are used for political mobilization. Relatedly, regions in Namibia that were historically subject to indirect colonial rule, in which the traditional local chiefs were in charge of internal affairs (in contrast to directly ruled areas which were administered by centralized bureaucracies), exhibit greater salience of ethnicity today (McNamee, 2019). This is also consistent with evidence by Ali et al. (2019) who use regression discontinuity design to show that in sub-Saharan Africa citizens of anglophone countries are more likely to identify in ethnic terms in comparison to citizens of francophone countries. Whereas the British instituted an indirect rule in most of its African colonies, France encouraged language integration and centralization. Ananyev and Poyker (2019) use difference-in-differences to show that individuals in Mali more exposed to deterioration of state capacity (due to the insurgency caused by demise of Gaddafi) self-identify less with the nation state. Rohner et al. (2013) present evidence showing that individuals in regions of Uganda which experienced greater intensity of civil conflict exhibit lower generalized trust and identify more in ethnic terms. More recent empirical work also suggests that shared collective experience can build a sense of common identity. Depetris-Chauvin et al. (2020) show that individuals in sub-Saharan Africa are more likely to identify with their national identity (over ethnic identity) and are more likely to trust other ethnic groups in the days after their national football team won an important match. Moreover, the countries whose teams barely qualified into the Africa Cup of Nations exhibit less civil conflict in the following months than countries that did not. Yanagizawa-Drott and Madestam (2011) find that the individuals in the US who experienced rain-free Fourth of Julys as children are more likely to identify as Republicans and have higher voter turnout. Yanagizawa-Drott and Madestam (2011) explain this by rainfall leading to the cancellation of Fourth of July celebrations, which induce patriotic values associated with the Republican Party.

Third, this paper also contributes to the literature on the origins and evolution of identity and politics in the US South. Significant parts of this literature have emphasized the culture of the British immigrants that first settled the South (Fischer, 1989; Woodard, 2012). This work (together with e.g., Cobb, 2007 and Cooper and Knotts, 2017) is more qualitative in nature. Acharya et al. (2016b) examine the legacy of slavery and how it affects a wide range of contemporary political attitudes of the white population from partisanship to support for affirmative action. The influence of slavery on local levels of taxation and bureaucratic capacity is studied by Suryanarayan and White (2021). They find that counties with a higher slave population before the Civil War had higher levels of taxation during the Reconstruction (when there was a greater presence of the federal government to enforce rights of the former slaves) but lower levels of taxation after Reconstruction ended.

Naidu (2012) examines the effects of various laws that effectively disenfranchised the vast majority of black citizens passed by many Southern states in the late 19th century. The results show that the disenfranchisement laws lowered turnout increased the Democratic vote share and lead to lower investment into black schools (measured by the child-teacher ratio). Hornbeck and Naidu (2014) investigate the effect of the Great Mississippi Flood of 1927 on agricultural development. They find that flooded counties underwent greater mechanization and modernization of agriculture in the subsequent years. They interpret these results as the consequence of the large and persistent out-migration of the black population in the flooded counties, which induced landowners to invest in labor-saving technologies. Ager et al. (2019) use linked census data to document the persistence of the white Southern elite. Specifically, they show that even though the former Southern slaveholders lost a substantial fraction of their wealth after the Civil War, their sons and grandsons were able to recover from this shock and even surpass the educational and occupational attainment of sons and grandsons from families holding a similar amount of (non-slave) wealth prior to the Civil War. Ager (2013) finds that counties, where the planter elite owned more wealth before the Civil War had the worse economic performance in the subsequent post-war decades (and even after World War II). Ager (2013) interprets this as a consequence of the lower support for mass education by the planter elite. Finally, Feigenbaum et al. (2020) show that the institutions of the South became less coercive in response to a negative economic shock in the form of boll weevil infestation, with fewer lynchings and less construction of Confederate monuments. Nonetheless, the impact of violence and destruction caused by the Civil War on the political outcomes in the US South has not been yet studied quantitatively.

2 Historical Background

2.1 Sherman's March

While the military situation in the Eastern theater (mostly in Virginia) of the American Civil War had mostly remained in stalemate, during the fall of 1864, Union General William Sherman successfully concluded a series of military campaigns in Georgia by capturing Atlanta in September 1864. The campaigns featured, for the most part, conventional warfare consisting of a series of small and large clashes of the Union and Confederate armies (although the siege of Atlanta did lead to the destruction of infrastructure and industries and expulsion of some of the civilian population).

Sherman hoped that he could put pressure on the Confederate army fighting in Virginia by damaging the Southern economy and its warfare capacity (Trudeau, 2009). This would involve destroying critical infrastructure including railroads and telegraph lines, and also mills and cotton gins. Sherman considered several possible avenues for the campaign. First, he could march west towards Charleston or Savannah, where he would be in a good position to potentially threaten the Confederate armies of Robert E. Lee in Virginia (Marszalek, 2007, p. 295). Second, he could move his armies towards Albany, Georgia and liberate prisoners in Andersonville and destroy cotton plantations along the way (Marszalek, 2007, p. 295). Finally, he could march towards Pensacola, Florida, and combine forces with E. R. S. Canby's to capture Mobile (Marszalek, 2007, p. 295).

Sherman decided for the first option to march towards Savannah, as according to him it "would have a material effect upon ... [the] campaign in Virginia" (Marszalek, 2007, p. 295). However, this route left the Union Army without secured supply lines since it was operating deep in the Confederate territory. For this reason, the Union soldiers had to confiscate food from the local population, which made Sherman and his army especially unpopular among the Southern whites (Campbell, 2005). When planning the specific path his troops would take, Sherman used the statistics from the 1860 Census of Agriculture in order to target counties that were important for agricultural production (Trudeau, 2009, p. 538). After reaching Savannah, Sherman began moving his army north to capture Colombia, North Carolina and from Colombia towards Virginia in order to eventually support the Union's campaigns against Lee there. The destruction of infrastructure by Sherman's soldiers continued in this second part of the march as well. The last clashes of Sherman's troops against the Confederate Army took place near Goldsboro, North Carolina from March 19 to 21, 1865. Lee's army surrendered on April 9, which effectively ended the war as other Confederate generals also quickly surrendered.

The capital destruction caused by Sherman's march had a large negative effect on the local economy; however, most sectors experienced a fast recovery. Feigenbaum et al. (2018) study the economic impact of Sherman's march empirically using county-level US census data from 1850 to 1920. They show that the farms in counties on the path of Sherman's march had almost 20% lower value in 1870 than the farms in other counties in South and North Carolinas and Georgia. However in 1880, the value of farms was only 3% lower (furthermore this estimate is not statistically significant), which suggests a fast recovery. A similar pattern also holds for the value of livestock. In addition, there were no effects on the population size (not even in 1870), the share of African Americans, or the total length of the railroad in a county. In contrast, the effects on agricultural investment exhibit greater persistence. Specifically, the share of improved land in the march counties was around 15% lower from 1870 through 1920 with no sign of reversion to the mean. The growth of manufacturing employment, capital, and production in march-exposed counties also lagged behind in 1870, although it is not possible to assess the persistence due to limitations of the data. Finally, Feigenbaum et al. (2018) document large and persistent increases in the concentration of farmland in the march counties. They interpret this as a consequence of the lack of access to credit in some parts of the South. In particular, according to Feigenbaum et al. (2018), small farmers, who experienced negative shock due to capital destruction, were credit constrained and therefore had to sell some of their land to the wealthy landowners at lower prices, which led to a permanent increase in land concentration. This has important implications for this paper. Since the land concentration might have a direct effect on political outcomes, it is challenging to distinguish between the direct effect of Sherman's march and the effect via the increases in land concentration. Due to the data limitations, I will not be able to distinguish between these two channels. Therefore it is important to take this into account when interpreting the results.

2.2 Political Development in the South after the Civil War

After the Civil War, there were efforts by the federal government to substantially alter the social relations in the formerly Confederate states. This period is known as the Reconstruction era and is usually[2] dated from 1865 to 1877 (Foner, 2014). The most significant changes brought by Reconstruction were declared in three amendments to the US Constitution. The Thirteenth Amendment (ratified in 1865) abolished slavery, the Fourteenth Amendment (ratified in 1868) granted the former slaves citizenship and stipulated equal protection under the law, and the Fifteenth Amendment (ratified in 1870) prohibits discrimination in voting based on race or former enslavement. Furthermore, the US government operated the Freedmen's Bureau (from 1865 to 1872) which helped the former slaves to meet basic needs.[3]

The enfranchisement of former slaves radically altered the political landscape of the South during Reconstruction. African Americans became the key constituency of the Republican Party, which advocated both for their political rights and for greater redistribution towards them. Furthermore, black Americans gained representation (running virtually exclusively for the Republican Party) in both state and local governments. According to Foner (2014) (as quoted by Naidu, 2012) "In virtually every county with a sizable black population, blacks served in at least some local office during Reconstruction ... assumed such powerful offices as county supervisor and tax collector, especially in states where these posts were elective."

There was significant resistance to these changes by the parts of Southern society, which benefited from the pre-war social order. These views were chiefly represented by the Democratic Party, which advocated for greater autonomy of the Southern states so that the social changes could be rolled back. In a more extreme form, this opposition sometimes manifested itself in violence against freed people (in the forms of riots or lynchings). Nevertheless, despite the resistance, the Reconstruction policies were still enforced (albeit imperfectly) partly due to the substantial presence of federal troops in the South (which were remnants of the Union Army that operated in the South during the Civil War).

Progress made by Reconstruction started to be reversed during the 1870s

[2]Some historians (most notably Foner (2014)) consider Emancipation Proclamation, issued on January 1, 1863, to be the start of Reconstruction.

[3]For empirical investigation of the long-term effects of the Freedmen's Bureau see Rogowski (2018).

with gradual withdrawal of the federal troops and lesser commitment of the Northern Republicans to enforcing equality in the South (Foner, 2014). This process was completed by the Compromise of 1877, which was an unwritten agreement according to which the Democrats would award the presidency to Republican Rutherford Hayes in exchange for the federal government pulling the last troops out of the South and the Republican Party would refrain from interfering in the South's local affairs. During the 1890s Southern states started to pass laws aiming to effectively disenfranchise black voters. This was done by imposing restrictions on voting, such as poll taxes or literacy tests, that disproportionately affected black voters (and to some extent poor whites) to ensure formal compliance with the Fifteenth Amendment (Kousser, 1974). Furthermore, violence and intimidation were often used to reduce black political participation and impose white supremacy.[4] As a consequence, the Democrats overwhelmingly dominated the politics of the South in the subsequent decades. In addition, various laws (usually referred to by historians as Jim Crow laws) that mandated racial segregation in public schools, transportation, and public places were enacted during the end of the 19th and early 20th centuries.

However, after World War II there started to be stronger disagreements between the Southern Democrats and the Democrats from northern and western states. This reflected the demographic shift within the Democratic coalition in the preceding years (Rae, 1994). Specifically, black voters in northern states largely left the Republican Party, since the Democrats' New Deal agenda aligned better with their economic interests (as opposed to pro-business policies advocated by the Republicans). As a consequence, some northern Democrats started to demand desegregation, which was strongly opposed by the Southern Democrats. When Harry Truman instituted desegregation in the military in 1948, some Southern Democrats founded the States' Rights Democratic Party (member of which were often called Dixiecrats), whose 1948 presidential candidate Strom Thurmond won 4 states. Nevertheless, after the 1948 elections, most Dixiecrats returned to the Democratic Party (Frederickson, 2001). A more permanent exodus of Dixiecrats took place in 1964 in response to Democratic President Lyndon B. Johnson's support for the Civil Rights Act of 1964, which had outlawed discrimination based on race and prohibited racial segregation in public schools, public accommodations, and public facilities. Most

[4] Jones et al. (2017) studies quantitatively the effect of violence on black political participation in the post-Reconstruction period. Their estimates suggest that exposure to lynching decreased local black voter turnout by around 2.5 percentage points

former Dixiecrats joined the Republican Party, whose 1964 presidential candidate, Barry Goldwater, opposed the Civil Rights Act on the grounds that it would violate the states' rights (Rae, 1994). This caused an important realignment in American politics. The 1964 presidential election was the first time a Republican candidate won the states of the Deep South (specifically, Louisiana, Mississippi, Georgia, Alabama and, South Carolina) for the first time since the end of Reconstruction. However, Johnson achieved a strong victory nonetheless, carrying 44 states (all states except Arizona, the home state of Goldwater, and the Deep South). Johnson used this strong mandate to pass the Voting Rights Act of 1965. This prohibited racial discrimination in voting and outlawed literacy tests and similar restrictions put in place during the Jim Crow period in the South. This led to dramatic increases in black voter registration in the Southern states.[5] In the 1968 presidential election, Nixon made an effort to gain the support of the Southern white voters, but this strategy was partly negated by the independent candidacy of segregationist George Wallace, former Democratic governor of Alabama, who won in the Deep South (Maxwell and Shields, 2021). Nevertheless, Nixon's Southern strategy did succeed in the 1972 election and the South became a Republican stronghold ever since. Several recent empirical studies[6] provide evidence that is consistent with qualitative narratives (e.g., of E. Black and M. Black, 2003), which have emphasized racial resentment as a main cause of the Southern realignment (as opposed to e.g., economic factors).

[5] Cascio and Washington (2014) employ a triple-differencing strategy to show that the removal of literacy tests at registration led to higher voter turnout and state transfers to counties with higher black populations.

[6] Kuziemko and Washington (2018) use a triple difference-in-differences strategy on individual-level Gallup and General Social Survey data to estimate the impact of racial attitudes on the partisan shifts. In particular, Kuziemko and Washington (2018) compare the changes in identification with the Democratic Party before and after spring of 1963 (the first difference), when the civil rights issues became salient, for individuals who reside in Southern states (the second difference) and were unwilling to vote for a black president (the third difference). The results show that the partisan shift of racially conservative Southern whites can explain the entire decline in the Democratic Party identification from 1958 to 1980.

3 Data

The data come from several different sources. First, I georeferenced the digitized 1865 US War Department map of Sherman's march[7] (shown in figure A1 in the appendix) and combined it with the historical boundaries of the counties in the US states of Georgia, North Carolina, and South Carolina as provided by Mullen and Bratt (2018). The resulting map of counties with lines indicating the movements of Sherman's army[8] is shown in figure 1. I follow Feigenbaum et al. (2018) and define the "Sherman" counties (i.e., those that were on the march's path) as all counties that intersect within 5 mile band around any of the army movement lines. Feigenbaum et al. (2018) justify this by referencing historical accounts, according to which the soldiers did not deviate far from the path of their armies.

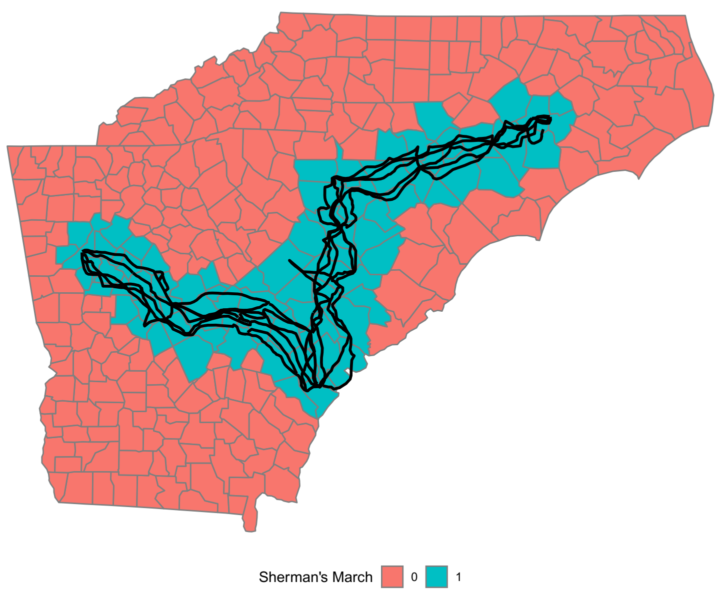

Figure 1: Map of US counties and Sherman March's Path

Note: The different lines represent the paths of the different armies participating in Sherman's march (specifically, the 14th. 15th, 17th, and 20th Army Corps and the Cavalry).

The most historical data on the county level were obtained from the rep-

[7]The scanned image of the map can be downloaded from `https://www.loc.gov/item/99447077/`

[8]Specifically, Sherman's army consisted of the 14th, 15th, 17th, and 20th Army Corps and the Calvary.

lication files of Acharya et al. (2016a) who collected a wide range of variables from various sources. The demographic (e.g., total population, the share of the population that are slaves) and agricultural (e.g., total farm value per acre of farmland, total acres of improved land, inequality in land ownership) characteristics of the counties are taken from the Census of Population and the Census of Agriculture, respectively (both in 1860). Acharya et al. (2016a) also assembled information from other studies into one unified dataset. For county-level vote shares of the Democratic Party for presidential elections from 1844 to 1968, they used the dataset by Clubb et al. (2006). They complemented it with contemporary public opinion data, pooling Cooperative Congressional Election Study (CCES) surveys (Ansolabehere and Schaffner, 2012) from the years 2006, 2008, 2009, 2010, and 2011 into one large dataset with over 157,000 respondents. The number of lynchings in the counties between 1882 and 1929 is based on the dataset created by Bailey et al. (2011). Finally, access to railways in a given county in 1860 was computed based on the GIS shapefiles by Atack (2015). An important challenge that faced Acharya et al. (2016a) when constructing this dataset spanning more than a century is that there were some changes to the county boundaries throughout the years. Acharya et al. (2016a) decided that they would use the modern county boundaries (as of 2000) and they would interpolate the 1860 census results onto these modern boundaries. In particular, Acharya et al. (2016a) use areal interpolation, in which a historical outcome for a modern county is constructed as a weighted average of the historical counties' outcomes where the weights correspond to the proportion of the historical counties' area that is contained in the modern county (the details are described in the appendix A of Acharya et al., 2016b).

The descriptive statistics for several variables are provided in table 1. The march and non-march counties do not appear to differ systematically in their vote share for the Democratic party prior to the Civil War. We also do not observe any significant differences in pre-war agricultural investment as measured by the acres of improved land in 1860. The total farm value per acre and land inequality (as measured by the Gini coefficient) appear to be fairly similar too. However, the counties on the march's path do have a significantly higher proportion of the population that were slaves in 1860. The spatial pattern of prevalence of slavery, which can be seen in figure A2 depicting the slave share in 1860 on a map, confirms this. This is consistent with the historical sources that emphasize that Sherman targeted areas with large plantations to undermine the Confederate economy (Trudeau, 2009). Since the legacy of slavery at the local level has been shown to have a persistent impact on various political outcomes and behavior (Acharya et al., 2016b), it will be necessary to control for this in the empirical analysis.

Another issue that table 1 reveals is the large fraction of missing data for the Democratic vote share for the earlier election years (especially prior to 1860). For the 1848 presidential election, the data are missing for 46.6% of all counties in the three states. The proportion of missing election results is somewhat lower for later years, with 30.5% missing data in 1860, 15.7% in 1827, and only 10.8% in 1900. One of the reasons is that South Carolina did not conduct a popular vote for presidential elections until 1860 (inclusive) since all of the Electors were appointed by the state legislature. In addition to this, there are more idiosyncratic factors that lead to the missingness of election results for some counties. The large fraction of missing data has important implications for the application of difference-in-differences, which are discussed in subsection 4.3.

Table 1: Summary statistics I

	Off march's path (N=229)	On march's path (N=76)	Overall (N=305)
Total population in 1860			
Mean (SD)	8230 (5920)	11300 (5640)	8990 (5990)
Median [Min, Max]	6910 [698, 40100]	10500 [2440, 31000]	7820 [698, 40100]
Share of slaves in 1860			
Mean (SD)	0.349 (0.201)	0.503 (0.157)	0.388 (0.202)
Median [Min, Max]	0.326 [0.0210, 0.850]	0.497 [0.197, 0.812]	0.394 [0.0210, 0.850]
Land inequality in 1860			
Mean (SD)	0.484 (0.0579)	0.461 (0.0563)	0.478 (0.0583)
Median [Min, Max]	0.485 [0.297, 0.642]	0.459 [0.314, 0.620]	0.478 [0.297, 0.642]
Acres of improv. land in 1860			
Mean (SD)	55200 (44800)	84400 (45300)	62500 (46600)
Median [Min, Max]	45200 [3110, 300000]	82600 [17300, 220000]	52800 [3110, 300000]
Farm value per acre in 1860			
Mean (SD)	23.5 (8.81)	24.8 (13.1)	23.8 (10.0)
Median [Min, Max]	21.8 [6.40, 97.2]	23.6 [6.25, 104]	22.2 [6.25, 104]
Railway access in 1860			
Mean (SD)	0.310 (0.464)	0.592 (0.495)	0.380 (0.486)
Median [Min, Max]	0 [0, 1.00]	1.00 [0, 1.00]	0 [0, 1.00]
Lynch rate (1882 to 1929)			
Mean (SD)	0.0121 (0.0179)	0.0118 (0.0142)	0.0120 (0.0170)
Median [Min, Max]	0.00527 [0, 0.101]	0.00698 [0, 0.0650]	0.00626 [0, 0.101]
Missing	6 (2.6%)	0 (0%)	6 (2.0%)
Dem. vote share in 1848			
Mean (SD)	44.3 (19.0)	46.9 (16.4)	45.0 (18.4)
Median [Min, Max]	44.2 [1.90, 90.3]	46.8 [4.20, 89.8]	45.7 [1.90, 90.3]
Missing	108 (47.2%)	34 (44.7%)	142 (46.6%)
Dem. vote share in 1860			
Mean (SD)	5.23 (8.49)	13.2 (15.7)	6.95 (10.9)
Median [Min, Max]	2.50 [0, 54.8]	7.35 [0, 70.2]	2.80 [0, 70.2]
Missing	63 (27.5%)	30 (39.5%)	93 (30.5%)
Dem. vote share in 1872			
Mean (SD)	52.4 (19.7)	48.5 (24.4)	51.4 (21.0)
Median [Min, Max]	50.3 [8.30, 100]	44.5 [6.10, 100]	49.5 [6.10, 100]
Missing	37 (16.2%)	11 (14.5%)	48 (15.7%)
Dem. vote share in 1900			
Mean (SD)	63.0 (17.6)	75.3 (17.5)	66.1 (18.4)
Median [Min, Max]	57.6 [20.0, 99.7]	74.1 [36.8, 99.9]	62.7 [20.0, 99.9]
Missing	26 (11.4%)	7 (9.2%)	33 (10.8%)

3.1 Data on Naming Patterns

Choice of a first name can be revealing about the personal identity of the parents. This has been used by several studies to measure the importance and expression of cultural or ethnic identity (e.g., Fryer and Levitt, 2004; Abramitzky et al., 2016; Fouka, 2020; Jurajda and Kovač, 2021). In our context, naming children after famous Confederate generals or politicians likely reflects a strong expression of a specific "Southern" identity. For this reason, I use individual-level census data with information on first names digitized by IPUMS (Integrated Public Use Microdata Series) project (IPUMS, 2020). For the 1880 census, a complete count with over 50 million individuals is available (Steven Ruggles et al., 2010). However, we are interested only in white individuals born after Sherman's march (who in 1880 were 15 years old or younger) which substantially reduces our sample size. Unfortunately, the full count data from IPUMS for other census years do not contain the first name. More detailed information (including the first name) is available only for a random sample of census records of a given year. Therefore I complement the 1880 census data with a 5% sample of the 1930 census. This has the advantage that the vast majority of individuals in this census were born after Sherman's march (individuals born prior would be more than 65 years old); however, at the cost of sampling only 5% of the population. Another issue with the 1880 census is that some counties could not be matched to our main dataset (due county boundary changes), which leads to missing data. The main variable measuring the frequency of Confederate names is constructed as the number of white individuals born after 1865 (inclusive) having Confederate first name in a given county divided by the total number of whites born after 1865 in a given county. The question then is which given names to classify as Confederate. If we counted all individuals who shared their first name with a famous Confederate general, we would likely misclassify many cases and capture spurious patterns. We could use the information on the middle names, although for most individuals only the initials of the middle name are available. I decided to apply a more conservative approach which labels names as Confederate only in cases where there can be a sufficient degree of certainty. First, I focus on those likely named after Robert Edward Lee, a commander of the Confederate Army, who became a cultural icon in the South after the Civil War. I classify all individuals with the first name "Robert" and the middle name initial "L" or "E" in the cases where the only middle name initial is available as having a Confederate name. If the full middle name is available, I require it to be "Lee", or a name starting with"Ed". Second, I also consider Confederate generals with very rare first names where the risk of misclassification is probably low. In particular, I classify as Confederate those with the first names of "Braxton" (after Braxton Bragg), "Jubal" (after Jubal Early), and "Stonewall" (after Thomas Jonathan "Stonewall" Jackson). The vast majority of the names I classify as Confederate are based on the Robert E. Lee match, which underscores his popularity in the South. Figure A3 in the appendix provides some validation for this approach. It shows the evolution of the share of Confederate names by the decade of birth using the full count of the 1880 census for Georgia and North and South Carolinas. We see that while before 1860, the share of Confederate names was negligible (less than 0.01 percent), it sharply increased to more than 0.15 percent during the decade from 1861 to 1870, and it persisted above 0.10 percent even during the following decade (from 1871 to 1880). This suggests that our classification method does capture the real historical changes and not only some spurious and random patterns. Nevertheless, since all of the Confederate figures I consider here were generals, it is possible that these names reflect certain admiration for the military rather than Southern identity.

In addition, I also examine possible effects on the presence of Confederate symbols in public places today. First, I use the database of Confederate monuments assembled by the Southern Poverty Law Center (SPLC, 2019), which includes the year of construction, longitude, and latitude of the monuments. Second, I downloaded the OpenStreetMap data files[9] for South Carolina, North Carolina, and Georgia to obtain the names and locations of all streets and roads in those states. In total the dataset contains 3,055,272 streets and roads (I will refer to both streets and roads as simply streets henceforth) in the three states, 963,159 of which are named (I dropped the unnamed streets from the analysis). I extracted a centroid from every named road and based on that I assigned the roads to the counties. The final variable measuring the relative frequency of Confederate streets was created as a ratio of the number of streets that contain a name of a famous Confederate figure[10] in a given county to the total number

[9]In particular, the files were downloaded from `http://download.geofabrik.de/north-america/us.html` on July 2, 2021 (the files are updated daily).

[10]Specifically, I define the street to be Confederate if its name contains any of the following (the full name of the Confederate figure it refers to is in the parenthesis): "Lee" (Robert E. Lee), "Davis" (Jefferson Davis), "Stonewall" (Stonewall Jackson), "Beauregard" (P.G.T. Beauregard), "Forrest" (Nathan Bedford Forrest), "Longstreet" (James Longstreet), "Hood" (John B. Hood), "Hampton" (Wade Hampton), "Kershaw" (Joseph Brevard Kershaw), "Rodes" (Robert E. Rodes), "Van Dorn" (Earl Van Dorn), "Maury" (Matthew Fon-

of streets in a given county. Since many street names contain only surname, it cannot be determined with certainty whether a particular street is named after a Confederate figure (e.g., Robert E. Lee) or only after someone with the same surname. Therefore for any classification method, there is a tradeoff between type I and type II errors. As a baseline, I consider a street to be Confederate if a surname of any Confederate figure appears in the street's name.[11] This more lenient approach will likely identify a greater number of true Confederate streets; however, at the cost of more false positives. Indeed, the more lenient method labels 5,961 out of 954,756 named streets as Confederate, whereas the more restrictive approach (using both first name and surname) classifies only 386 streets as Confederate. This suggests that that the more restrictive approach is too stringent for our application and therefore I will not use it for the analysis.

Table 2 shows the summary statistics for the variables mentioned above aggregated on the county level. First, we see that explicit Confederate first names are relatively rare. In an average county, only 0.359 percent of whites born after 1865 had Confederate first name in the 1880 census and only 0.244 in the 1930 census.[12] However, there is fairly large variation across the counties with the minimum Confederate first name in the 1930 census being 0 percent and the maximum being 1.43 percent. A similar pattern holds for the Confederate street names when the lenient method is used (i.e., matching only on surname). In that case, 0.707 percent of streets are classified as Confederate in an average county. In contrast, when the strict approach is used (i.e., matching on both first and last name) only 0.0475 percent of streets are labeled as Confederate in an average county. Furthermore, a median county has 0 percent of Confederate streets highlighting the strictness of this approach. Finally, Confederate monuments appear to be fairly prevalent with 66.9 percent of counties having at least one.

taine Maury), "Mosby" (John Mosby), "Ewell" (Richard Ewell),

[11] For this method, I exclude the following Confederate generals with very common surnames: Joseph E. Johnston, Albert Sidney Johnston, Nathan G. Evans, Bill Anderson.

[12] Nevertheless, it is important to keep in mind that for the 1880 census we are using full count data, whereas for the 1930 only 5 percent samples are used.

Table 2: Summary statistics II

	Off march's path (N=229)	On march's path (N=76)	Overall (N=305)
Conf. names share (%)-1880 census			
Mean (SD)	0.149 (0.152)	0.155 (0.117)	0.150 (0.144)
Median [Min, Max]	0.112 [0, 0.835]	0.137 [0, 0.533]	0.120 [0, 0.835]
Missing	33 (14.4%)	10 (13.2%)	43 (14.1%)
Conf. names share (%)-1930 census			
Mean (SD)	0.463 (0.658)	0.685 (0.579)	0.518 (0.646)
Median [Min, Max]	0.364 [0, 7.41]	0.602 [0, 3.03]	0.427 [0, 7.41]
Conf. monuments (dummy)			
Mean (SD)	0.633 (0.483)	0.776 (0.419)	0.669 (0.471)
Median [Min, Max]	1.00 [0, 1.00]	1.00 [0, 1.00]	1.00 [0, 1.00]
Conf. streets share (%)-lenient			
Mean (SD)	0.673 (0.568)	0.812 (0.534)	0.707 (0.562)
Median [Min, Max]	0.508 [0, 4.80]	0.695 [0, 3.50]	0.571 [0, 4.80]
Conf. streets share (%)-strict			
Mean (SD)	0.0449 (0.304)	0.0553 (0.286)	0.0475 (0.299)
Median [Min, Max]	0 [0, 3.43]	0 [0, 2.40]	0 [0, 3.43]

4 Identification Strategies

Before proceeding to the discussion of the identification strategies, I will describe the main outcome variables of interest. The outcomes of interest can be categorized into two broad classes, the ones relating to the election results and the ones relating to identity and symbolism. The main electoral outcome of this study is the vote share of the Democratic Party candidates in presidential elections from 1848 to 1968. As discussed in greater detail in subsection 2.2, prior to 1964 the Democratic Party in the South was associated with Confederate elites, who strongly opposed the abolition of slavery prior to the Civil war. After the Civil war, the Southern Democrats supported segregation in public places and, in effect, disenfranchisement of African Americans. After the Civil war, the Southern Democrats passed various "Jim Crow" laws in the state legislatures, which in effect disenfranchised African Americans and mandated racial segregation in public places. Therefore we can use support for the Democrats in the South as a measure of values associated with the Confederacy (such as opposition towards the involvement of the federal government and racial resentment). In addition to the support for the Democrats, I will also consider the vote shares of Strom Thurmond in 1948 and George Wallace in 1968, both prominent Southern segregationists (more on them in subsection 2.2). The advantage of using the vote share data is that the long panel of the presidential election results gives us an opportunity to examine the persistence of the potential effects of Sherman's march and enables us to apply panel data methods (such as difference-in-differences).

Nevertheless, there are several disadvantages too. First, the Democratic nominees across the different presidential elections had different agendas, and therefore the extent to which voting for the Democratic candidate reflected some kind of Southern identity might have changed. For this reason, it is important to keep in mind the historical context of the elections. Second, since we do not observe the individual voting intentions, we cannot determine how the composition of the voting population. In particular, we cannot distinguish whether lower vote share for the Democrats in a given county during the Reconstruction period was driven by the higher turnout of African Americans (who voted overwhelmingly for the Republicans) or by lower support for the Democrats among whites. This is especially relevant given that the share of blacks in the population was fairly large in some counties and given that the strength of implementation of the Jim Crow voting restrictions could vary across regions. These issues could be addressed by using individual-level data on voting intentions. Unfortunately, these are not available for presidential elections in the late 19th century or 20th century (or at least not with the sample sizes necessary for our purposes). However, we can at least use the CCES surveys for present-day political opinions (the general background on the CCES survey is described in greater detail in section 3). I will focus on three main outcomes: self-identification with the Democrats, self-identification with the Republicans, and the support for affirmative action policies. The CCES survey asks the respondents if they think of themselves as a Democrat, a Republican, or an independent based on which the respective county-level shares can be constructed. The support for affirmative action policies is measured as the share of respondents who strongly or weakly support "programs [that] give preference to racial minorities and to women in employment and college admissions in order to correct for discrimination" (Ansolabehere, 2013). For all these three outcomes, only the answers of white respondents are considered (which addresses the concerns about the spurious effects only due to the demographic composition).

The second class of outcomes is more relates to culture, identity, and symbolism. First, I consider the impact on the first names that the parents gave to their children and in particular, the relative frequency of first names that likely refer to famous Confederate politicians or generals obtained from the full count data of the 1880 census and the 5% sample of the 1930 census (more on the exact definition of the variables is in subsection 3.1). However, these "Confederate" first names were relatively rare and we might be worried that the 5% samples (and even the 1880 full count data since we are considering only those born after 1865) might be too small to capture the preference for Confederate first names with sufficient precision. Second, I examine how frequently are present-day streets named after Confederate figures using the OpenStreetMap data with over 900,000 named roads and streets. Nonetheless, despite the large size of the data, only 5,961 streets are classified as named after a famous Confederate general or politician (using the more lenient method). The third outcome in this class is an indicator variable for the presence of a Confederate monument in the county.

The last outcome I consider is the lynch rate, that is the number of lynchings from 1882 to 1930 per 10,000,000 residents (with the number of residents being measured by the 1920 census). Lynching became more common in the South after the Civil war as a mean, by which the Southern whites could maintain their social dominance.

Below I propose several different methods to identify the effect of Sherman's march. Each one relies on a somewhat different set of assumptions and has its own potential drawbacks. Therefore the range of the estimates across specifications might indicate how sensitive our conclusions are to different assumptions.

4.1 Selection on Observables with OLS

Arguably, the most straightforward identification strategy is to assume that, conditional on a set of observable (control) variables, Sherman's march was as-if randomly assigned. If we further assume the linear form for the control variables, we can write our specification as

$$y_{is} = \alpha + \beta \text{march}_{is} + \delta_s + x_{is}'\gamma + \epsilon_{is} \tag{1}$$

where y_{is} is an outcome for county i in state s, march_i is a dummy variable that equals one if county i was on the path of Sherman's march and zero otherwise, δ_s are state fixed effects, and x_i is a vector of control variables. Importantly, ϵ_{is} is assumed to be uncorrelated with march_{is} dummy.

As discussed in section 2, Sherman tended to target counties with large slave plantations according to historical sources. I will therefore include several variables from the 1860 census as controls to capture this. The first is the proportion of slaves in the total population in 1860. The second set of variables relates to local agriculture. These are: (i) the logarithm of the total value of farms per improved acre of farmland in the county, (ii) the logarithm of the total acres of improved farmland, and (iii) Gini coefficient of the farmland ownership, to measure inequality in the distribution of land (all from the 1860 census). To control for possible targeting of transport infrastructure, I add a dummy variable for access to railways in 1860. Finally, I also include the logarithm of the total population in 1860. Finally, the state fixed effects should capture the range of factors that have a uniform effect across counties within a given state such as some state laws or regulations.

There are several concerns with this approach. The main one is there could be some unobserved variables that influence both the probability of being on the march's path and the outcomes of interest. The absence of selection on unobservables cannot be tested from the data without imposing additional assumptions or availability of additional information (e.g., an instrument).

Nevertheless, there are methods that can probe the plausibility of this assumption under specific conditions. One popular approach in the applied work has been to include or remove certain control variables from the regression and observe how the coefficient on the treatment changes. If the coefficient changes only slightly, the researchers tend to view that as evidence against selection on unobservables, whereas substantial changes in the coefficient are perceived as a worrying sign. Several recent studies have formalized the assumptions behind this practice (Altonji et al., 2005; Oster, 2019). The necessary condition for these arguments to apply is that the bias arising due to the omission of the observed covariates is informative about the bias due to the unobserved covariates. Altonji et al. (2005) make the most optimistic assumption that the correlation of treatment and unobservables can be fully recovered from the correlation between treatment and observables (specifically, the unobservables have the same covariance with the treatment as the observables). Oster (2019) extends and generalizes this approach by taking into account the changes in R^2 to estimate how much variation in the dependent variable a given set of control explains. In essence, the coefficient stability after removing controls that lead to little changes in R^2 are not very informative about the degree of selection on unobservables since this might be only because the controls are not important in explaining the outcome. Based on this insight, Oster (2019) develops several methods to assess the robustness of the results. First, given a value of R^2 that would be achieved if both observables and unobservables were included in the regression[13] (denoted as $R^2_{\max}$), a researcher can calculate how much stronger the selection on unobservables relative to observables would have to be (denoted as δ) for the true effect of the treatment to be zero. The confidence for the deltas can be obtained using a bootstrap procedure. Second, Oster (2019) proposes a partial identification approach for the treatment effect that works the other way. That is, given a bounds δ and $R^2_{\max}$, a researcher can calculate the implied bounds for the treatment effect. Oster (2019) recommends that a sensible interval for δ is between zero (which would imply no selection on unobservables) and one (which would imply equally strong selection on observables and unobservables). For $R^2_{\max}$, Oster (2019) recommends the R^2 from the regression with all observed controls as a lower bound (which would imply no selection on unobservables) and one as an upper bound. Third, for $\delta = 1$, given a value for $R^2_{\max}$ Oster (2019) proposes a point estimator for the bias-adjusted treatment

[13]Note that in many applications, a natural value for $R^2_{\max}$ is one. However, in some settings, there might some idiosyncratic variation in the outcome which leads to $R^2_{\max}$ of less than one.

effect (under the assumptions mentioned above including the observables and unobservables sharing the same covariance properties with the treatment). In subsection 6.2, I apply these methods to the OLS regressions for the effect of Sherman's march to obtain a bias-adjusted estimate of the treatment effect and an estimate of δ.

Second, the linear function imposed for the effect of the control variables on the outcomes can be too restrictive. To address the second concern, I present the estimates for the effect of Sherman's march using Double Machine Learning (DML) approach by Chernozhukov et al. (2018), which allows for more flexible functional form, in section 6.3 as a robustness check.

As the default, I will use heteroskedasticity-robust standard errors (specifically, the HC2 variance estimator). However, there might a concern that these standard errors might be biased downwards since they do not account for the spatial correlation potentially present in the data. In the future work, I will also present the results using Conley (1999) variance estimators, which allows for the correlation of errors between counties whose distance is smaller than some pre-specified threshold to address this issue.

4.2 Instrumental Variable

If we suspect that the variable march_i is endogenous, then OLS is inconsistent. Luckily, we can still identify the effect of the march if a valid and relevant instrumental variable is available. I follow Feigenbaum et al. (2018) who proposes to use a straight line between the three main cities on the march's path (Atlanta - the starting point of the march, Savannah, and Columbia) as an instrument.[14] In particular, I draw a straight line from the centers of Atlanta to Savannah and from Savannah to Columbia and then I define that a county receives the instrument if its area intersects with a 10 km band around the straight lines (all other counties do not receive the instrument).

The underlying idea is the following: Sherman planned to march through Atlanta, Savannah, and Columbia; however, the counties between these cities were to some extent visited only because they happened to be on the way. This leads to the following two-equation model

$$y_{is} = \alpha_1 + \beta \text{march}_{is} + \delta_s + x_{is}'\gamma + \nu_{is} \tag{2}$$

[14] Straight-line instruments have been especially frequently in the literature on the effects of transportation infrastructure where the straight-lines are based on minimal distances between important cities (see e.g., Ghani et al., 2016; Banerjee et al., 2020).

$$\text{march}_{is} = \alpha_2 + \phi \text{line}_{is} + \delta_s + x'_{is}\psi + \eta_{is} \tag{3}$$

where line_{is} is equal to one if county i in state s intersects with a 10-km wide buffer around the straight line that connects Atlanta to Savannah and Savannah to Columbia, and zero otherwise. Other variables are defined as in specification 1. I will estimate the parameters using two stage least squares (2SLS).

There are two main conditions for a valid instrumental variable design: exogeneity and relevance. In terms of the model, exogeneity implies that conditional on the convariates there is no correlation between the error term in the first stage, η_{is}, and the endogenous variable, march_{is}. Exogeneity would be violated if being located on the line between major cities would have an effect on the outcomes of interest via other channels than changing the probability of being on Sherman's march path. Second, condition for identification is relevance, which means that the instrument have some non-zero effect on the endogenous variable. In terms the model, it implies that the coefficient ϕ is non-zero. Unlike exogeneity, relevance can be easily tested from data using standard methods.

However, even if both exogeneity and relevance are satisfied, the IV estimator could still be highly unreliable if the instrument is weak, i.e., is if the correlation between the endogenous variable and the instrument is very close to zero. Under weak instruments, the true distribution of the estimators can be highly non-normal the and the conventional asymptotic approximation fails (Andrews et al., 2019). There have been two main approaches in the literature to address this issue (see Andrews et al., 2019 for a review). The first is to detect the weakness of an instrument. A number of different tests have been proposed. The Stock and Yogo (2005) test is based on adjustment of critical values for the (non-robust) first stage F-statistic so that under the null hypothesis the instrument is weak (and not only irrelevant). The researcher only needs to specify what degree of instrument weakness is he or she willing to tolerate. This is done by selecting the minimum percentage of the worst-case bias that the IV estimators would suffer from under the null hypothesis due to the instrument weakness (Olea Montiel and Pflueger (2013) denote this as τ). Importantly, the Stock and Yogo (2005) test assumes homoskedasticity of the errors, which is likely not satisfied in many empirical settings. Olea Montiel and Pflueger (2013) propose a test based on scaling the non-robust F-statistic, which relaxes the homoskedasticity assumption. Conveniently, in the case of a single instrument the test of Olea Montiel and Pflueger (2013) is equivalent to using heteroskedasticity-robust F-

statistic with the Stock and Yogo (2005) critical values. This is the test I use in this paper to test for weakness of the instrument. The second approach is to use inference procedures that are valid regardless of regardless of whether the instrument is weak or not. For just-identified models (which is our case as well), there is strong recommendation to use the Anderson and Rubin, 1949 test and the confidence sets based on it due to the result of Moreira (2009) who showed that AR test is uniformly most powerful unbiased in these settings. However, the robustness of the AR tests might come at the cost of inefficiency when the instrument is strong (Andrews et al., 2019). Since I soundly reject weakness of the instrument used in this paper, I use the conventional confidence intervals (and not the AR confidence sets) as the default.

4.3 Difference-in-differences

The panel nature of the data on voting outcomes should in principle enable us to apply a difference-in-differences type design. However, there are several practical challenges. The most important is the missing data problem. For the 1860 presidential election (the last before the Civil war), the elections results are missing for 93 counties from 305 in the three states of interest, which is partly caused by the fact that South Carolina did not have a popular vote until 1872 (the Electors of the state were free to vote for whichever candidate they wanted). For elections prior to 1860, the missing rate is even higher. Keeping these problems in mind, I estimate the following specification using only the data from Georgia and North Carolina

$$v_{it} - v_{i1860} = \alpha_0 + \beta \text{march}_i + x_i' \gamma + \epsilon_i \tag{4}$$

where v_{it} is the vote share of the Democratic Party in county i in year t, and other variables are defined as above.

The identifying assumption is that in the absence of Sherman's march the Democratic vote shares in 1860 and 1872 would have evolved in parallel. The simplicity of this assumption can be considered to be an advantage of difference-in-differences as it is fairly easy to imagine under conditions it will be violated. On the other hand, parallel trends can be viewed as a strong assumption since it may be sensitive to the functional form of the outcome[15] (which in this context

[15] Note that this stands in contrast to selection on observables or instrumental variable strategies where the identification comes from the assumptions on the assignment of the treatment (or the instrument). Even if the linear models used for estimation under selection

means that the assumption is invariant to strictly monotonic transformations of the outcome). In particular, Roth and Sant'Anna (2021) show that the parallel trends assumption does not depend on the functional form of the outcome only if either (i) the treatment is as-if randomly assigned or (ii) the entire distribution of the potential outcomes for the untreated group can be inferred. If the treatment is as-if randomly assigned then other methods (e.g., a simple difference in means) can provide unbiased estimates and difference-in-differences may seem redundant. On the other hand, imposing the condition (ii) may not be justifiable in many cases. Since none of these conditions seems appropriate for the application in this paper, it is necessary to keep in mind that our parallel trends assumption will be sensitive to functional form.

Besides the potential violations of parallel trends, the high fraction of missingness of data for 1860 elections presents another set of challenges for this method. First, due to the absence of popular vote in South Carolina, we can only study the effects of Sherman's march in Georgia and North Carolina. However, there remains some missing data even when we entirely exclude South Carolina. The consequences of this for the analysis depend on the nature of the process that generates missingness. If the data is missing completely at random (in the best case), it will cause only the reduction in the statistical power of the estimators. However, if the missingness is correlated with some unobserved factors, it can even cause bias and inconsistency. For these reasons, I would consider difference-in-differences to be less credible in this setting than either instrumental variable or selection on observables designs.

on observables or in the instrumental variable design are misspecified, the OLS and the linear IV still recover a weighted average of the causal effects (Roth and Sant'Anna, 2021).

5 Main Results

5.1 Selection on Observables with OLS

First, let us consider the effect on the voting outcomes. I ran the same cross-sectional specification (1) separately for every presidential election from 1848 to 1964 (with a gap during and several years after the Civil War) with the vote share of the Democratic Party presidential candidate as the dependent variable.[16] In addition, I also included vote shares for two segregationist third party candidates: Strom Thurmond in 1948 and George Wallace in 1968 (more information on them is provide in subsection 2.2). The estimated coefficients on Sherman's march dummy from all of those specifications are plotted in figure 2. First, notice the estimates for elections prior to 1864. These can serve as placebo specifications of a certain kind, since we would not expect Sherman's march to have any effect on voting outcome before it took place. However, we see that the pre-march coefficients are, in fact, fairly large for some election years (in particular for 1852 and 1860). This might potentially indicate that our identifying assumptions are violated and invalidate the OLS results. On the other hand, there could be other explanations as well. Recall that there is much larger fraction of missing data for the elections before 1860, partly due to South Carolina not holding a popular vote. As a result, the confidence intervals for the effects in those years are much wider. Furthermore, none of the pre-1860 estimates are significant at 5 percent level (although 1860 is very close with the p-value of 0.59). Therefore the larger pre-1860 coefficients could only be a consequence of the estimates being noisier. If we change the focus and consider the period from 1872 to 1968, we see that the estimates for almost all the election years are small (usually between -2.5 and 2.5 percent) and include zero in their 95 percent confidence intervals. However, the vote share of Thurmond running under the States' Rights Democratic Party in 1948 is an exception that seems to stand out. The estimate implies that Thurmond received about 5 percentage point higher vote share which is fairly large compared to the overall county-mean of 26 that he received in the three states. This could be due to the strongly segregationist nature of his candidacy, which is something that was perhaps not the central issue during other election years. Interestingly, we observe only very small negative effects for Wallace in 1968 whose campaign was very similar in spirit to Thurmond's. One explanation could be that this is due to the

[16] For 1868 elections the data for almost all counties are missing and therefore I did not estimate the model for this.

Voting Rights Act of 1965 and its effect on black voter turnout. Until 1965, the election results largely reflected only the preferences of white voters, since most of blacks were subjects to many Jim Crow voting restriction. Therefore this could potentially explain the differences in the estimated coefficients for Thurmond and Wallace. Taken together, these results do not provide strong evidence that Sherman's march would boost support for Democratic presidential candidates in the subsequent presidential elections. Nevertheless, there seems to be some weaker evidence suggesting that strongly segregationist candidate Storm Thurmond did receive greater vote share in the march counties.

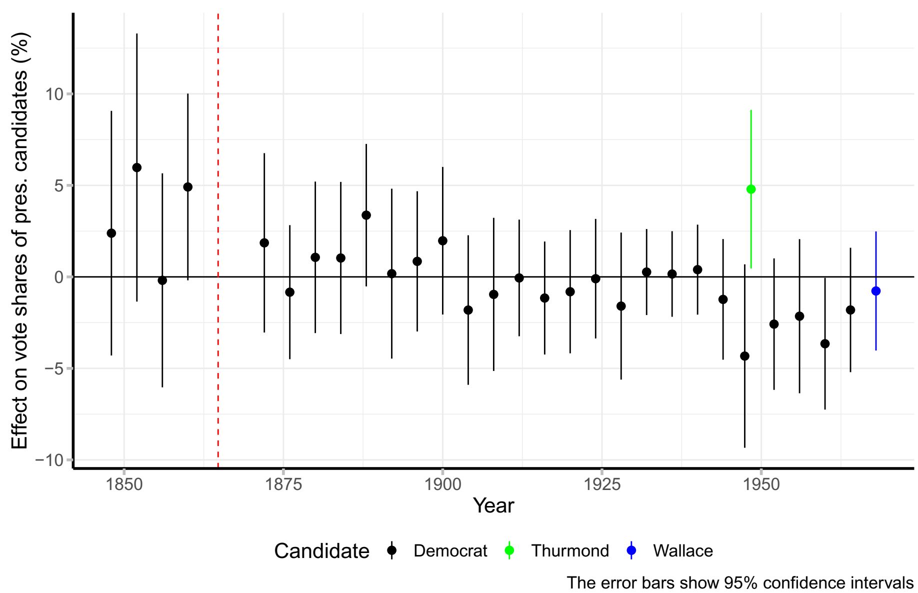

Figure 2: Democrats vote share - OLS - full controls

I also examine if there are long-term effects of Sherman's march for the present-day politics. I use pooled CCES survey data (which asked the respondents about their party identification and policy preference) aggregated at the county level. Table 3 displays the results. There is no significant effect of the march on the identification with either the Democrats or Republicans (among white respondents). There is slightly lower share of white respondents who support affirmative action policies although it is significant only at the 10 percent level.

Next, I consider the effect on other outcomes more related to identity using the same specification. All the results are provided in table 4. First, notice the

Table 3: CCES outcomes - OLS results

	Dem. share (%)	Rep. share (%)	Affirm. action sup. (%)
(Intercept)	96.497**	25.147	7.491
	(40.475)	(41.211)	(46.076)
Sherman's march	2.805	1.619	-5.321*
	(3.302)	(3.798)	(3.140)
Slave share	-6.015	11.186	-13.331
	(9.190)	(10.139)	(9.932)
Land inequality	-26.988	2.520	-3.903
	(38.055)	(34.615)	(40.535)
Log of acres of improved land	-10.642*	0.152	5.970
	(5.810)	(5.639)	(6.725)
Log of farm value per ac.	-5.272	-4.286	13.528**
	(6.085)	(6.773)	(5.408)
Railway access	2.806	-3.492	4.036
	(3.048)	(3.926)	(3.330)
Log of total population	8.029	4.143	-9.638
	(6.169)	(6.105)	(6.864)
Number of observations	281	281	281
Dep. var. mean	28.102	52.123	23.804
R^2	0.088	0.044	0.060

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC2 variance estimator are in the parentheses. State fixed effects are included in all specifications. Dem. (Rep.) share (in %) is defined as the proportion of respondents in a given county who identify as a Democrat (Republican). Affirm. action sup. us defined as the share of respondents (in %) in a given county who support affirmative action policies. All three dependent variables are based on answers in CCES surveys and the shares are calculated only among white respondents.

coefficients on the Sherman's march dummy for the share of Confederate names using the 1880 and 1930 censuses. While when the data from the 1880 census is used, the coefficient is very close to zero and not statistically significant, the coefficient estimated using the 1930 census samples is fairly large positive and significant at 5% level (it implies Sherman's march led to 0.209 percentage point increase in the share of Confederate names compared to the overall county-level mean of 0.518 percent). Second, we also observe a positive and significant effect on the present-day share of streets and roads named after a Confederate figure. Specifically, the estimated effect size is 0.146 which is fairly large in comparison with the overall mean of 0.707 percent. In contrast, the presence of any Confederate monuments in a county does not appear to be strongly influenced by Sherman's march or any other control variable in our model as none of the estimated coefficients is significantly different from zero even at 10 percent level. This might reflect that deeper historical factors included in this regression are not important predictors for decisions to build Confederate monuments and contemporaneous events were perhaps more relevant (i.e. people were building the monuments in reactions to the events at the time). Finally for the rate of lynching, Sherman's march appears to have a negative and borderline significant effect. On the other hand, the slave share in 1860 has a strong and positive effect and seems to be the most important predictor, which is consistent with the work of Acharya et al. (2016b) (although it could also be only a mechanical effect of lynchings being relatively more common in regions where the share of blacks was higher).

As robustness exercises, I apply several additional methods in section 6. First, I re-define the treatment to include counties that have greater distance from the march's path. Second, I use the methods by Oster (2019) to assess sensitivity to selection on unobservables. Third, I estimate the march's effect using the Double machine learning methods to account for the potential nonlinearities in the effect of the controls variables.

Table 4: Other outcomes - OLS results

	First names (1880)	First names (1930)	Street names	Monuments	Lynch rate
(Intercept)	-0.272	-0.083	1.044	-1.117	0.030
	(0.222)	(0.694)	(0.784)	(0.679)	(0.027)
Sherman's march	-0.006	0.209**	0.146**	0.032	-0.004*
	(0.020)	(0.081)	(0.069)	(0.061)	(0.002)
Slave share	-0.021	-0.104	-0.085	-0.013	0.023***
	(0.076)	(0.216)	(0.168)	(0.179)	(0.006)
Land inequality	0.069	-0.869	0.658	-0.272	-0.016
	(0.192)	(0.661)	(0.680)	(0.573)	(0.021)
Log of acres of improved land	0.081**	-0.012	0.201*	0.096	0.006
	(0.035)	(0.105)	(0.118)	(0.099)	(0.004)
Log of farm value per ac.	0.008	0.026	0.088	0.057	0.003
	(0.034)	(0.097)	(0.100)	(0.110)	(0.004)
Railway access	-0.018	-0.008	0.127**	0.017	-0.001
	(0.021)	(0.088)	(0.057)	(0.061)	(0.002)
Lof of total population	-0.053	0.126	-0.348***	0.075	-0.009**
	(0.038)	(0.105)	(0.134)	(0.104)	(0.004)
Number of observations	262	305	305	305	299
Dep. var. mean	0.150	0.518	0.707	0.669	0.012
R^2	0.059	0.048	0.082	0.144	0.261

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC2 variance estimator are in the parentheses. State fixed effects are included in all specifications. First names (1880) is the share (in %) of whites born after 1865 in a given county with first name that we classified as Confederate in 1880 census. First names (1930) is defined the same, only 1930 census is used. Street names is the share (in %) of streets and roads in a given that contain a surname of a Confederate figure in their name. Monuments is a an indicator variable that equals one if a Confederate monument was present in a county in 2019. Lynch rate is defined as the number of lynchings in a county from 1882 to 1929 per 10,000,000 residents.

5.2 Instrumental Variable

First, let us consider the first stage to assess the strength of the straight-line instrument. The estimated coefficients from the regression of Sherman's march dummy on the instrument and the other exogenous variables are shown in table 5. We see that the instrument has a large and significant effect on the probability of being on the march's path (in particular the instrument increase the probability by 75.6 percentage points) while the exogenous variables do not appear to have any significant effect. Nevertheless, I also apply the Montiel-Pflueger (2013) test for weakness of an instrument. Note that under the null hypothesis of this test the instrument is weak. The parameter τ of the test then sets the minimum bias that the weak instrument causes to the estimator under the null, expressed as a percentage of the worst case bias. Therefore higher τ means a more lenient test since greater bias is tolerated under the null. The results of the test are also provided in table 5. The test statistic (effective F-statistic) is 172.22, which is very high. For the test size (α) of 5 percent and the worst case bias percentage (τ) of 5 percent, the critical value is 37.42. This means that we can soundly reject the null hypothesis of the instrument weakness.

Figure 3 plots the coefficients on the march dummy from the instrumental variable specification, with the vote share of a given presidential candidate in the respective election year as the dependent variable. All the control variables are assumed to be exogenous (i.e., Sherman's march dummy is assumed to be the only endogenous variable). The overall results are similar to the OLS estimates in the their broad pattern. First, for some of the pre-1860 elections we observe relatively large estimates, but as in the OLS case they all have fairly wide confidence intervals and none of them is significant at the conventional levels. The coefficients between 1872 and 1952 tend to hover very close to zero. Somewhat surprisingly, after 1952 the coefficients decrease to around -10 percent and become significant. This could be related to increasing support for civil rights within some parts of the Democratic Party but this is only a speculation. In comparison with the OLS results, the estimated effect for Thurmond's vote share in 1948 is slightly lower and no longer significant although it still has the same sign. Moreover, we do not see any significant effects on the vote share of Wallace in 1968 either.

The results testing for the potential impact on present-day party identification and policy opinion using the CCES data are provided in table A2 in the appendix. As one would expect based on the lack of any strong short-term or

Table 5: IV - first stage

	Sherman's march
(Intercept)	-0.757*
	(0.435)
Straight-line instrument	0.756***
	(0.050)
Slave share	0.152
	(0.099)
Land inequality	0.201
	(0.445)
Log of acres of improved land	0.082
	(0.065)
Log of farm value per ac.	0.040
	(0.062)
Railway access	0.012
	(0.044)
Log of total population	-0.032
	(0.069)
R^2	0.475
N	305
Montiel-Pflueger (2013) weak. inst. test	
Confidence level (α)	5%
Effective F-statistic	172.222
Critical value for $\tau = 5\%$ (2SLS)	37.418

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC2 variance estimator are in the parentheses. State fixed effects are included. τ sets the minimum bias that the weak instrument leads to under the null hypothesis, expressed as a percentage of the the worst case bias (higher τ imply a more lenient test since greater bias is tolerated under the null).

medium-term voting outcomes shown above, the estimates for the effects on the identification with either the Democratic or the Republican party and on the support for affirmative action policies all cannot rejected the null of zero effect not even at the 10 percent level.

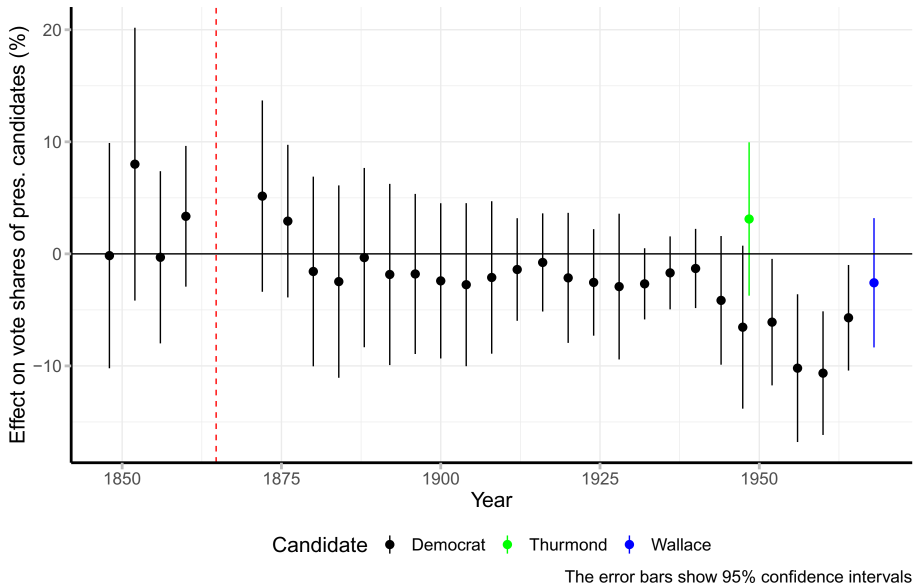

Figure 3: Democrats vote share - IV results

The coefficients from the second stage for the effects on the other outcomes are presented in table A1. The overall results seem to be rather different from the OLS estimates. In particular, the point estimate of the march's effect for Confederate first names using 1880 census is almost the same as when OLS is used (i.e., very small and not significant). However, the 2SLS estimate with the 1930 data is much smaller than the corresponding OLS estimate (0.074 vs. 0.209). Similar pattern holds for the effect on Confederate street names where the 2SLS estimate is less than half of its OLS equivalent (0.064 vs. 0.146) and is no longer statistically different from zero at the conventional significance levels. The estimated effect for the presence of any Confederate monuments is actually higher when 2SLS is used, nevertheless its size is still very small and not significant. Finally, the point estimates for the lynch rate are almost the same for both methods, only the 2SLS coefficient has a wider confidence interval.

Table 6: Other outcomes - IV - second stage

	First names (1880)	First names (1930)	Street names	Monuments	Lynch rate
(Intercept)	-0.272	-0.181	0.984	-1.095	0.029
	(0.223)	(0.690)	(0.785)	(0.675)	(0.027)
Sherman march	-0.006	0.074	0.064	0.061	-0.004
	(0.033)	(0.134)	(0.120)	(0.102)	(0.004)
Slave share	-0.021	-0.062	-0.060	-0.022	0.023***
	(0.077)	(0.223)	(0.172)	(0.180)	(0.006)
Land inequality	0.069	-0.835	0.679	-0.280	-0.016
	(0.192)	(0.668)	(0.683)	(0.571)	(0.021)
Log of acres of improved land	0.081**	0.001	0.209*	0.093	0.006
	(0.035)	(0.107)	(0.118)	(0.098)	(0.004)
Log of farm value per ac.	0.008	0.017	0.083	0.059	0.003
	(0.034)	(0.099)	(0.100)	(0.111)	(0.004)
Railway access	-0.018	0.001	0.132**	0.015	-0.001
	(0.021)	(0.090)	(0.057)	(0.061)	(0.002)
Log of total population	-0.053	0.125	-0.349***	0.076	-0.009**
	(0.038)	(0.106)	(0.133)	(0.104)	(0.004)
Number of observations	262	305	305	305	299
Dep. var. mean	0.150	0.518	0.707	0.669	0.012
R^2	0.059	0.041	0.079	0.143	0.261

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC variance estimator are in the parentheses. State fixed effects are included in all specifications. First names (1880) is the share (in %) of whites born after 1865 in a given county with first name that we classified as Confederate in 1880 census. First names (1930) is defined the same, only 1930 census is used. Street names is the share (in %) of streets and roads in a given that contain a surname of a Confederate figure in their name. Monuments is a an indicator variable that equals one if a Confederate monument was present in a county in 2019. Lynch rate is defined as the number of lynchings in a county from 1882 to 1929 per 10,000,000 residents.

5.3 Difference-in-differences

The results of difference-in-differences for specification (4) are provided in table 7. When no controls are included, the effect of Sherman's march on the percentage point change in the Democratic vote share between 1860 and 1872 is estimated to be -3.099. When our standard set of controls are included, the estimated effect reverses sign to 1.966 percentage points. However, note that the standard errors in both cases are larger than the absolute value of the estimates and therefore they are hardly significant at any conventional level. Similar description applies to the difference in the Democratic vote share between 1860 and 1900. Here too are the estimates very small (1.037 percentage points without controls and -2.381 percentage points with controls) and not significant. Overall, we find little evidence that Sherman's march would have any effects on the electoral support for the Democrats. Nevertheless, it is important to consider that the results could be affected by the missingness of large fraction of observation (specifically, all counties of South Carolina had to be excluded since South Carolina did not conduct popular vote through 1860). This not only reduced statistical power but also changes the estimand if the there is heterogeneity in the match's effect across states.

It is interesting to compare how the effect of pre-war prevalence of slavery changes in times, even though it is not the main focus of this paper. In particular, while the coefficient on the share of population that were slaves in 1860 is almost -40 percentage point for the difference in Democratic vote shares between 1860 and 1872, the same coefficient is over 28 percentage points for vote share difference between 1860 and 1900 (both are highly significant). The most compelling explanation for this seems to be the end of Reconstruction and the gradual rise of Jim Crow. That is, counties with higher share of slaves in 1860 had larger black population who voted overwhelmingly for Republicans in 1872 elections. However by 1900, most blacks were effectively disenfranchised due to various voter-restriction laws (see e.g., Naidu, 2012).

Table 7: Democratic vote share difference (in percentage points)

	1860-1872		1860-1900	
	(1)	(2)	(3)	(4)
Intercept	48.438***	94.562**	55.181***	64.830**
	(1.482)	(37.318)	(1.236)	(30.560)
Sherman's march	-3.099	1.966	1.037	-2.381
	(3.590)	(3.464)	(3.334)	(3.604)
Slave share		-39.513***		28.553***
		(7.940)		(7.952)
Land inequality		-9.646		-19.086
		(25.380)		(23.193)
Log of acres of improved land		11.420**		-1.567
		(4.877)		(4.059)
Log of farm value per acre		6.458		3.563
		(4.644)		(3.985)
Railway access		-4.080		1.098
		(2.677)		(2.755)
Log of total population		-18.683***		-0.351
		(5.353)		(4.246)
North Carolina dummy		-8.573***		-3.580
		(2.531)		(2.569)
Number of observations	210	210	212	212
Dep. var. mean	47.759	47.759	55.406	55.406
R^2	0.004	0.296	0.001	0.139

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.).

The standard errors based on HC2 variance estimator are in the parentheses.

6 Robustness checks

6.1 Different Measure of Exposure to Sherman' march

In our baseline specifications, a county was defined to be exposed to Sherman's march if it intersected with a 5 mile band around the path of any of Sherman's armies. This was chosen to reflect that the deviations of the individual soldiers from the main path of their armies were rather limited. Nevertheless, the 5 mile band cutoff is somewhat arbitrary and therefore I present the results for different cutoff sizes (specifically, for the 10, 20, and 50 mile bands).

The estimates of the baseline OLS specification for the effect on other outcomes with varying band lengths are provided in table A3 in the appendix. If we compare it with the results for the 5 mile band in table 4, we see that the estimates that use the 10 and 20 mile bands are broadly similar. In particular, the effects on the share of Confederate first names (from the 1930 census), the share of Confederate street names and the lynch rate are all significant at at least the 10 percent level with the same sign and comparable effect sizes. However, the results start to diverge for the 50 mile band at which point none of the estimates are significant at the conventional levels anymore.

6.2 Sensitivity to Selection on Unobservables

As discussed in greater detail in subsection, the methods by Oster (2019) can be used to assess the sensitivity of the results to selection on unobsevables and obtain bounds on the treatment effect under the assumption that the selection on observables is informative about selection on unobservables. I apply these method the OLS regression results presented in subsection 5.1. I estimate two kinds of parameters. First one is bias-adjusted treatment effect. This based on two very strong assumptions that the selection on unobservables and observables are equally important (i.e., that $\delta = 1$) and that have the same covariance properties with the treatment. Second parameter (denoted as δ) gives us by how much the relative strength of selection on observables compared to unobservables would have to be to fully explain away the estimated treatment effect (i.e., so that the true treatment effect is zero).

The estimates of both of these parameters together with their 95% confidence intervals based on a bootstrap procedure are provided in table 8. We see that for virtually dependent variables the confidence intervals on the estimates of both the bias-adjusted treatment effect and δ are too wide to be informative.

This could be due to the relatively small sample size and large error variance even after the observed controls are included.

Table 8: Sensitivity to unobservables using Oster (2019) methods

	Bias-adj. treament effect			Strength of sel. on unob. (δ)		
	Est.	95% CI (l.)	95% CI (u.)	Est.	95% CI (l.)	95% CI (u.)
Democrats' share in 1872	10.291	-2.826	23.407	-0.072	-3.843	3.700
Democrats' share in 1900	-8.768	-16.976	-0.560	0.286	-0.230	0.803
Thurmond's share in 1948	-0.284	-5.838	5.270	1.030	0.140	1.919
Conf. first names share-1880 census	-0.670	-51.851	50.511	-0.036	-5.187	5.116
Conf. first names share-1930 census	0.335	-612.692	613.363	0.165	-6.418	6.749
Conf. streets share	42.748	-2416.738	2502.235	0.255	-32.577	33.087
Conf. monument dummy	-1.339	-2.590	-0.087	0.050	-0.129	0.229
Lynch rate	-0.026	-0.070	0.018	-27.523	-1700.354	1645.308

The table shows the mean and the confidence intervals of bootstrapped estimated of the bias-adjusted treatment effect and relative strength of selection on unobservables compared to observables under zero treatment effect (δ). For each specification. 1000 bootstrapped samples were generated. First names (1880) is the share (in %) of whites born after 1865 in a given county with first name that we classified as Confederate in 1880 census. First names (1930) is defined the same, only 1930 census is used. Street names is the share (in %) of streets and roads in a given that contain a surname of a Confederate figure in their name. Monuments is a an indicator variable that equals one if a Confederate monument was present in a county in 2019. Lynch rate is defined as the number of lynchings in a county from 1882 to 1929 per 10,000,000 residents.

6.3 Selection on Observables using DML

Even if we assume selection on observables, imposing linear functional form on the effect of control variables might be too restrictive. We can relax assumption with regard to linearity of control variables and instead consider the following partially linear model

$$y_i = \beta \text{march}_i + g_0(x_i) + \varepsilon_i \tag{5}$$

$$\text{march}_i = m_0(x_i) + \nu_i \tag{6}$$

where $\mathbb{E}[\varepsilon_i|\text{march}_i, x_i] = 0$ and $\mathbb{E}[\nu_i|x_i] = 0$. We include the same set of control variables as in the fully linear case. The double/debiased machine learning (DML) method by Chernozhukov et al. (2018) consists of estimating g_0 and m_0 using flexible machine learning models. That is, first we estimate $m_0(x_i)$ and obtain the residuals $\hat{\nu}_i = \text{march}_i - \hat{m}(x_i)$ where $\hat{m}(x_i)$ is the machine learning prediction fitted "out-of-sample" (cross-fitting). Second, the estimate of $g_0(x_i)$ is obtained, again, using cross-fitting. Finally, the parameter of interest, β is estimated as

$$\hat{\beta} = \left(\sum_{i \in I} \hat{\nu}_i \text{march}_i \right)^{-1} \sum_{i \in I} \hat{\nu}_i \left(y_i - \hat{g}(x_i) \right)$$

The regularisation bias in the estimation of β is addressed by using Neyman orthogonal moments. Chernozhukov et al. (2018) show that the DML estimator of β is approximately unbiased and normally distributed. However, note that the DML requires the ML prediction $\hat{m}(X_i)$ and $\hat{g}(X_i)$ to converge uniformly to their true values at least at the rate $N^{-\frac{1}{4}}$.

The R package by Bach et al. (2021) was used to implement the DML. The predictions for $g_0(x_i)$ and $m_0(x_i)$ were obtained by cross-fitting a random forest with 500 trees, 4 randomly sampled control variables, maximum tree depth of 5, and minimum node size of 2. The results for the voting outcomes are provided in figure A4. The overall pattern is very similar to the OLS results, suggesting that potential non-linearities in the effect of control variables do not have a large impact for the estimated coefficient on the Sherman march dummy. Nevertheless, the confidence intervals seem slightly shorter, which could be due to the lower residual variance of the DML.

6.4 Instrumental variable using DML

Similarly to the selection on observables case, the DML method can be used to estimate partially linear instrumental variable models as well. Here again, the main advantage is to allow for non-linear effect both in the first stage (i.e., for the regression of the exogenous variables on the instrument) and in the second stage (i.e., for the regression of the exogenous control variables on the outcome). I will consider the following semiparametric instrumental variable model

$$y_i = \beta \text{march}_i + g_0(x_i) + \varepsilon_i \tag{7}$$

$$\text{line}_i = m_0(x_i) + \nu_i \tag{8}$$

where $\mathbb{E}[\varepsilon_i|\text{line}_i, x_i] = 0$ and $\mathbb{E}[\nu_i|x_i] = 0$. Again all the other variables are defined as in specifications 2 and 3. Note that I included two state dummy variables in the control vector x_i. To obtain predictions for $g_0(x_i)$ and $m_0(x_i)$, I again fitted random forest models with the same hyperparameters as in subsection 6.3 (i.e., 500 trees, minimum node size of 2, etc.).

The estimates and the confidence intervals for the effect of Sherman's march on the Democratic vote share in presidential elections throughout the years are plotted in figure A5 in the appendix. If we compare it with the results in figure 3, there is relatively little difference between the two sets of estimates (although, again, the confidence intervals are slightly shorter in some cases), which suggests that potential non-linearities in the effects of controls do not have strong influence on the IV estimates.

7 Conclusion

In this thesis, I estimated the long-term effects of a destructive military campaign, Sherman's march through Georgia, South and North Carolina, on subsequent political outcomes and identity. I considered a wide range of outcomes including voting in presidential elections, the share of individuals likely named after famous Confederate generals, the presence of a Confederate monument, and the relative frequency of lynchings. Overall, the results do not seem to support the view that Sherman's march had a transformative effect or even that it significantly boosted Southern resentment as some of the historical literature would argue (e.g., Campbell, 2005).

Nevertheless, there are several ways in which this work could be further extended. First, additional data sources could be collected. For example, if the IPUMS project makes available the information on names of individuals for the full count census data (for years other than 1880), it could be used to update the estimates in this paper. Second, one important limitation of this paper is that the dataset used is rather small (only 305 counties for the three states) and in some cases the data are missing, which leads to lower statistical power. This is due to the fact that for most of the variables necessary for the analysis, the county is the smallest level of aggregation. The census datasets usually do contain identifiers for enumeration districts, which offer much greater granularity, but most of the enumeration district maps have not been georeferenced yet and doing so would require substantial effort.

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Appendix

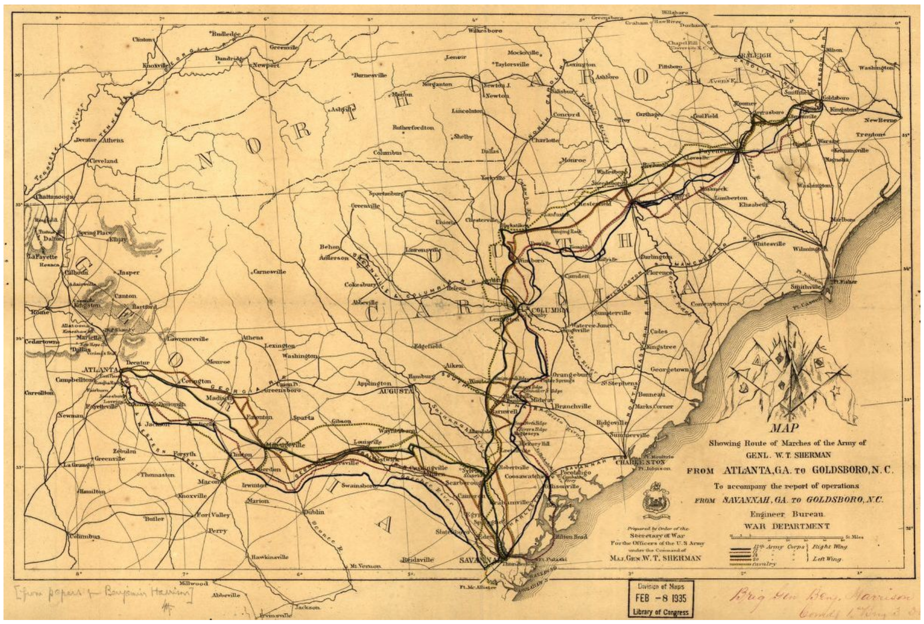

Figure A1: Map of Sherman's March, War Department

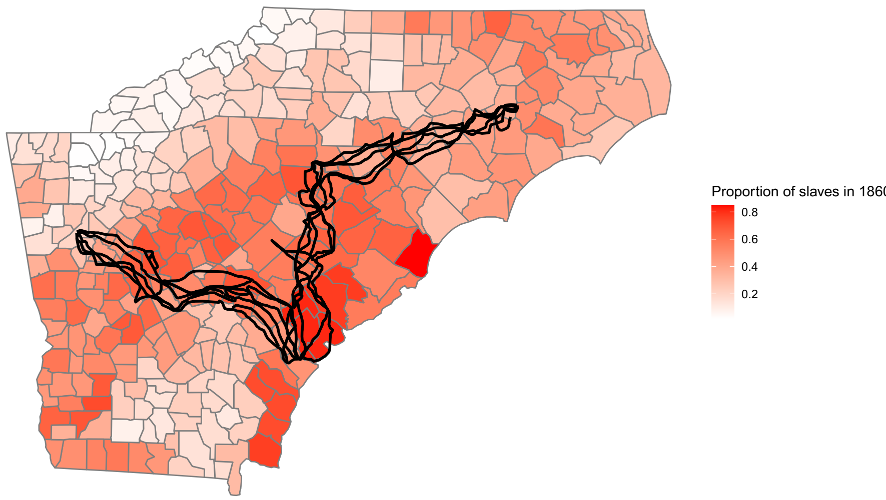

Figure A2: Map of slave share of population across counties in 1860
Note: The different lines represent the paths of the different armies participating in Sherman's march (specifically, the 14th. 15th, 17th, and 20th Army Corps and the Cavalry).

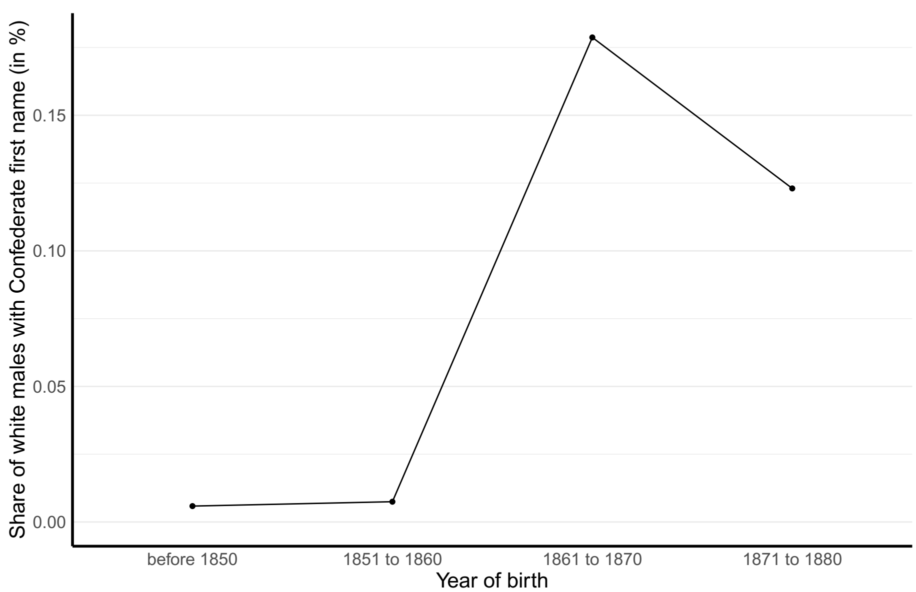

Figure A3: Share of Confederate first names by decade of birth
Note: The source of the data is the full count of 1880 census. Only data from Georgia, South Carolina, and North Carolina were used.

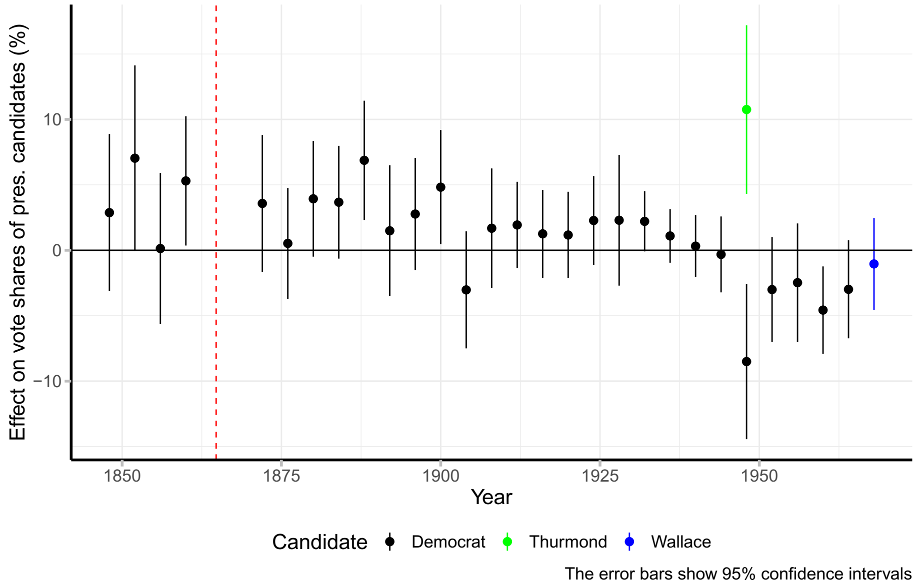

Figure A4: Democrats vote share - Partially linear model using DML

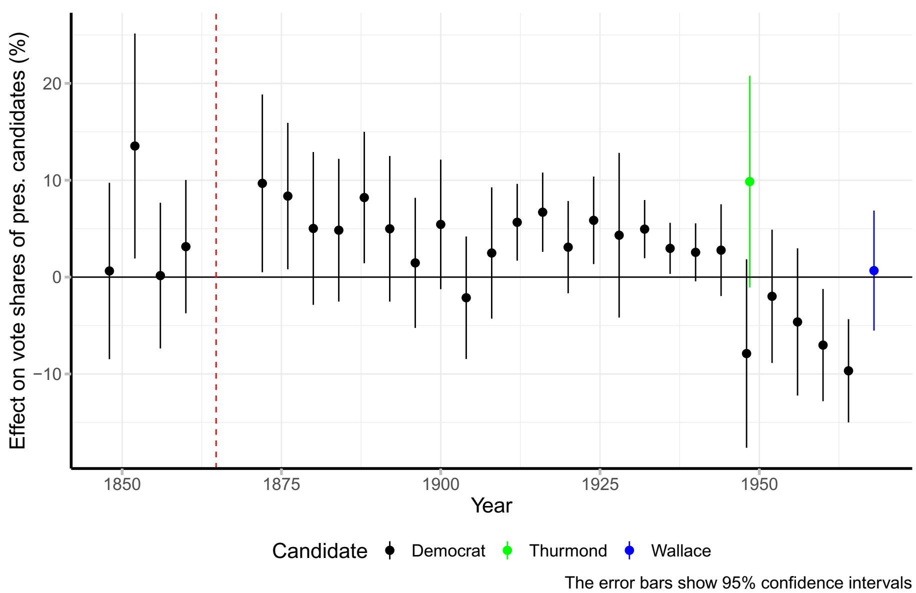

Figure A5: Democrats vote share - Partially linear IV using DML

Table A1: Other outcomes - IV - second stage

	First names (1880)	First names (1930)	Street names	Monuments	Lynch rate
(Intercept)	-0.272	-0.181	0.984	-1.095	0.029
	(0.223)	(0.690)	(0.785)	(0.675)	(0.027)
Sherman march	-0.006	0.074	0.064	0.061	-0.004
	(0.033)	(0.134)	(0.120)	(0.102)	(0.004)
Slave share	-0.021	-0.062	-0.060	-0.022	0.023***
	(0.077)	(0.223)	(0.172)	(0.180)	(0.006)
Land inequality	0.069	-0.835	0.679	-0.280	-0.016
	(0.192)	(0.668)	(0.683)	(0.571)	(0.021)
Log of acres of improved land	0.081**	0.001	0.209*	0.093	0.006
	(0.035)	(0.107)	(0.118)	(0.098)	(0.004)
Log of farm value per ac.	0.008	0.017	0.083	0.059	0.003
	(0.034)	(0.099)	(0.100)	(0.111)	(0.004)
Railway access	-0.018	0.001	0.132**	0.015	-0.001
	(0.021)	(0.090)	(0.057)	(0.061)	(0.002)
Log of total population	-0.053	0.125	-0.349***	0.076	-0.009**
	(0.038)	(0.106)	(0.133)	(0.104)	(0.004)
Number of observations	262	305	305	305	299
Dep. var. mean	0.150	0.518	0.707	0.669	0.012
R^2	0.059	0.041	0.079	0.143	0.261

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC variance estimator are in the parentheses. State fixed effects are included in all specifications. First names (1880) is the share (in %) of whites born after 1865 in a given county with first name that we classified as Confederate in 1880 census. First names (1930) is defined the same, only 1930 census is used. Street names is the share (in %) of streets and roads in a given that contain a surname of a Confederate figure in their name. Monuments is a an indicator variable that equals one if a Confederate monument was present in a county in 2019. Lynch rate is defined as the number of lynchings in a county from 1882 to 1929 per 10,000,000 residents.

Table A2: CCES outcomes - IV results

	Dem. share (%)	Rep. share (%)	Affirm. action sup. (%)
(Intercept)	97.097**	28.124	7.643
	(40.817)	(41.571)	(46.502)
Sherman's march	3.549	5.317	-5.132
	(5.315)	(6.391)	(4.644)
Slave share	-6.230	10.120	-13.385
	(9.068)	(10.104)	(9.724)
Land inequality	-27.317	0.886	-3.986
	(38.293)	(34.927)	(40.966)
Log of acres of improved land	-10.705*	-0.158	5.954
	(5.863)	(5.680)	(6.788)
Log of farm value per ac.	-5.223	-4.043	13.540**
	(6.098)	(6.740)	(5.395)
Railway access	2.757	-3.738	4.023
	(3.058)	(3.898)	(3.278)
Log of total population	8.027	4.131	-9.638
	(6.163)	(6.102)	(6.859)
Number of observations	281	281	281
Dep. var. mean	28.102	52.123	23.804
R^2	0.088	0.041	0.060

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

All the control variables show their value as of 1860 (i.e. slave share in 1860 etc.). The standard errors based on HC2 variance estimator are in the parentheses. State fixed effects are included in all specifications. Dem. (Rep.) share (in %) is defined as the proportion of respondents in a given county who identify as a Democrat (Republican). Affirm. action sup. us defined as the share of respondents (in %) in a given county who support affirmative action policies. All three dependent variables are based on answers in CCES surveys and the shares are calculated only among white respondents.

Table A3: Other outcomes - OLS - different treatment definitions

	First names (1880)	First names (1930)	Street names	Monuments	Lynch rate
Sherman's march (10 miles)	-0.017	0.210***	0.126*	0.018	0.126*
	(0.020)	(0.080)	(0.065)	(0.059)	(0.065)
Sherman's march (20 miles)	0.004	0.186**	0.134*	-0.004	0.134*
	(0.020)	(0.082)	(0.079)	(0.057)	(0.079)
Sherman's march (50 miles)	0.007	-0.042	0.055	0.080	0.055
	(0.022)	(0.103)	(0.078)	(0.061)	(0.078)

[a] The standard set of controls was included in all specifications. The standard errors based on HC2 variance estimator are in the parentheses. First names (1880) is the share (in %) of whites born after 1865 in a given county with first name that we classified as Confederate in 1880 census. First names (1930) is defined the same, only 1930 census is used. Street names is the share (in %) of streets and roads in a given that contain a surname of a Confederate figure in their name. Monuments is a an indicator variable that equals one if a Confederate monument was present in a county in 2019. Lynch rate is defined as the number of lynchings in a county from 1882 to 1929 per 10,000,000 residents.