"Let our ballots secure what our bullets have won:" Union Veterans and Voting for Radical Reconstruction

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Abstract

After the Civil War, Congressional Republicans used sweeping powers to expand and enforce civil rights for African Americans. This "second revolution" was surprising; Republicans needed to win the support of Northern voters that were widely opposed to racial equality before the war. I argue that wartime experiences of Union veterans caused them to support Republicans and their policies after the war. Using a difference-in-differences design, I find that county-level enlistment increased Republican voteshare, particularly in the vital elections between 1864 and 1868. Using individual data on military service and post-war partisanship, I show that as-if random exposure to company combat deaths caused soldiers to become more Republican. And ecological difference-in-differences show veterans became more supportive of African American suffrage. Historical evidence illustrates that wartime sacrifice, experiences with slavery, and contact with African Americans gave veterans new attitudes about civil rights and commitments to "win the peace".

Keywords: civil war; military service; voting; civil rights; Reconstruction; race

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Introduction

In 1869, the United States was in the midst of its "Second Revolution" (Foner 1988). In the preceding few years, a "Radical" Congress controlled by Republicans passed amendments ending slavery, creating rights and protections of national citizenship, and prohibiting the denial of suffrage on the basis of race. To secure these new rights, Congressional Republicans embarked on a massive expansion of federal powers: (*i*) preserving a legal state of war against much of the South until 1871; (*ii*) creating new federal agencies like the Freedmen's Bureau that usurped state powers to regulate labor and schooling (Downs 2015); and (*iii*) passing enforcement measures that allowed federal officials to supervise elections, try states or citizens for denying civil rights (Wang 1997), and suspend habeas corpus and deploy federal troops (Downs 2015). The "revolution" of Radical Reconstruction transformed the Constitution by redefining and expanding the rights of citizenship and enlarging federal power to enforce this change; and despite the eventual erosion and elimination of many Reconstruction policies, these laws meaningfully improved the lives of freed people (Stewart and Kitchens Forthcoming; Chacon and Jensen 2020; Logan 2018; Rogowski 2018), and secured the bedrock upon which the fight for equal civil rights over the following century-and-a-half would be built.

This revolution is all the more remarkable in that it was effected, by the North, through broadly free and fair elections. How did this happen? On one hand, these sweeping changes seem easily explained by theories of suffrage expansion. One common argument is that competition among economic elites leads rival factions to expand the vote to build winning coalitions (Ansell and Samuels 2015; Llavador and Oxoby 2005; Lizzeri and Persico 2004), while another is that electoral competition among parties drives efforts to make a more favorable electorate through expansion or contraction (Teele 2018; Capoccia and Ziblatt 2010). Both logics have been applied to Reconstruction. Moore (1967) claims that Republican support for suffrage was driven by economic conflicts between industrial capital in the North and an agricultural aristocracy in the South, while Valelly (2004) argues Republican leaders enfranchised African Americans to mitigate the electoral threat posed by Southern states re-joining the Union with all-white electorates. On the other hand, the competition narrative leaves much unexplained. To contemporaries, the dramatic extent of this revolution was not a foregone conclusion. While economic and partisan conflicts with the South were always clear, Republicans only came to embrace these radical reforms reluctantly and fitfully (Downs 2015; Wang 1997). Offsetting potential gains, parties that expand suffrage also risk alienating those who can already vote, and Republicans feared that expanding civil and political rights to African Americans would anger white voters and cost them elections.

These fears were not unfounded. From the perspective of antebellum politics, the project of Radical Reconstruction was shocking. Before the Civil War, Republican support for expansions to civil rights was limited. Contrary to Southern fears, most Republicans made tempered calls for the restriction and gradual elimination of slavery rather than immediate abolition. Moreover, for many in the party, opposition to slavery was paired with plans to expel African Americans from the republic rather than incorporate them as equal citizens (Foner 1979, 265–280). While some Republican activists put civil and political rights for African Americans to referenda in some states, these efforts were neither endorsed by Republican state platforms nor successful (Dykstra 1993; Bateman 2018). If the Republican party was, at best, ambivalent about civil rights, the wider public was often hostile. By 1860, white voters in several Northern states had strongly embraced *maintaining or expanding* a racially exclusive conception of citizenship (see, e.g. Dykstra 1993; Bateman 2018); as late as 1862, Illinoisans voted overwhelmingly to restrict blacks from entering the state and forbid them from voting (Allardice 2011, 101). This did not escape politicians' notice: even buoyed by victory over the Confederacy in 1865, many Republicans considered campaigning for African American suffrage and its enforcement to be "political suicide" (Wang 1997, 6).

Congressional Republicans in the years following the Civil War had to win over a public that was racially conservative and had little support for African American civil rights, while Democratic opponents unleashed a barrage of racial fear-mongering against them (Mendelberg 2001, Chapter 2). Racial conservatism is durable (Acharya, Blackwell and Sen

2016), and when Democrats pushed for civil rights during the "second" Reconstruction of the 1960s, racially conservative Southern whites rapidly and decisively abandoned the Democratic party (Kuziemko and Washington 2018). This raises the question: why didn't Northern voters turn against the Republican party during this moment of revolutionary change? The wartime experiences of nearly two million Union Army veterans caused them become more supportive of the Republican Party and its Reconstruction platform.

This argument starts from the premise that the Civil War fundamentally transformed American politics (Mayhew 2005). More generally, wars have been credited with driving state formation (Tilly 1990), democratization (Ferejohn and Rosenbluth 2017), and redistribution (Scheve and Stasavage 2012); and civil wars in particular have produced durable political re-alignments (Wood 2008) and expansions of state power into new policy areas (Paglayan 2017). One key mechanism by which wars produce these effects are through the experiences of participants, particularly combatants. War affects combatant's political attitudes and beliefs (Grossman, Manekin and Miodownik 2015; Koenig 2020; Jha and Wilkinson 2017; Parker 2009), endows them with new organizational skills and capacities for violence (Jha and Wilkinson 2012), and increases their political participation (Bellows and Miguel 2009; Blattman 2009).

Whether this is true of soldiers in the American Civil War is hotly debated. Historians have spilled oceans of ink documenting the political beliefs of Union soldiers. While most agree that these men joined not to defeat slavery but to preserve the Union, there are serious disagreements about whether and how soldiers' politics changed during the war. One group of historians argues that the wartime experiences of Union soldiers led them to enshrine the destruction of slavery as one the twin purposes of the war, alongside saving the Union (Manning 2007; Gannon 2011; Janney 2013). Yet, others claim that the war had little affect on soldiers' partisanship (White 2014) and that opposition to slavery was superficial and fleeting (Gallagher 2011). The electoral significance of veterans has received even less attention. In historical accounts of the pivotal Congressional elections of 1866, which enabled Republicans to pursue Radical Reconstruction over the veto of Andrew Johnson, veterans and veterans' organizations receive little to no mention (Foner 1988; Riddleberger 1979).

Yet, Union veterans made up a large share of the Northern electorate after the war, and there are plausible mechanisms by which their experiences may have caused them to back Republicans and their Reconstruction agenda. Consistent with evidence that combat reduces willingness to reconcile with the enemy (Grossman, Manekin and Miodownik 2015), soldiers' sacrifice and suffering in the war may have increased their desire to punish the South and ensure that they "won the peace" (Gannon 2011; Janney 2013). And, just as service in the Second World War to ostensibly protect freedom enabled political learning about civil rights among both African American (Parker 2009) and white soldiers (White 2016), Manning (2007) argues that Union soldiers' exposure to slavery both intensified moral outrage against it and led them to understand securing "Liberty" to be one of the twin purposes of the war. Finally, Union soldiers' wartime collaboration with enslaved and newly-freed African Americans against the Confederacy is similar to other contexts in which inter-group "contact" reduces racial prejudice (Mo and Conn 2018).

I substantiate my argument about the effects of military service by testing key implications of my argument against rival explanations and providing evidence for the causal mechanisms through a combination of quantitative and qualitative evidence. Using newly available individual data on nearly all Union Army soldiers and the full count 1860 US census, I estimate enlistment rates for counties in eight Northern states and townships in Iowa and Wisconsin. I use a continuous difference-in-differences design to identify the effect of enlistment rates on Republican vote-share. I find that higher enlistment rates caused substantial gains for Republicans after the war, particularly in the elections of 1864 through 1868, electing the Congresses that passed the Civil Rights Amendments and key civil rights legislation. Compared to 1860, Republican vote-share in 1866 increased 4 and 8.5 percentage points more in counties in the third and fourth quartiles of enlistment than in counties in the lowest quartile. I show that the parallel trends assumption is plausible and that these effects are robust to alternate specifications that address selection into enlistment and heterogeneous effects.

To demonstrate the plausibility of interpreting these ecological estimates as the effect of military service on the political allegiances of soldiers, I provide individual-level evidence for one of the mechanisms by which this might have happened: wartime loss. Data on individual partisanship for the 19th century is scarce, but I expand on the work of DeCanio (2007) by digitizing the self-reported partisanship of nearly thirty thousand people residing in nine Indiana counties in 1874. I combine this with data collected from military records on the war-time experiences of more than twenty thousand Union soldiers from these counties. Throughout the war, the Union Army deployed men into combat by regiment, which fought in a line, with companies (sub-units) arrayed end-to-end. Historical evidence and balance tests demonstrate that casualty rates for companies in the same regiment were plausibly as-if random. I exploit this natural experiment to show that, within the same regiments, soldiers in companies with greater casualties became more likely to identify as Republicans as opposed to Democrats after the war.

This is consistent with a story that soldiers supported Republicans not out of agreement with Radical Reconstruction policies but out of a refusal to back a Democratic party which had questioned the war effort and, in the South, was the party of former Confederates. However, I present quantitative and qualitative evidence that veterans also came to directly support civil rights expansions, even before Republicans endorsed the idea in state and national platforms. Using a difference-in-difference design, I show that townships in Iowa and Wisconsin with greater enlistment saw significantly larger increases in support for black suffrage in post-war state constitutional referenda. Ecological bounds show these were indeed effects on veterans. Finally, I present historical evidence about Union soldiers and veterans' organizations that show how veterans' wartime experience may have reduced their racial prejudice and led them to see securing "Liberty"—meaningfully ending slavery—as essential to winning the peace.

These results demonstrate the pivotal role veterans played in securing Radical Republican control over Reconstruction and provide novel quantitative evidence that decisively supports the conclusions of revisionist histories that Union soldiers came out of the war politically transformed (Manning 2007; Gannon 2011). In political science, this paper adds to a growing body of work on the political consequences of the American Civil War (Chacon and Jensen 2020; Logan 2018; Rogowski 2018; Stewart and Kitchens Forthcoming). In particular, this evidence adds contrast to work by Hall, Huff and Kuriwaki (2019) and Kalmoe (2020) who show that pre-war economic and political commitments drove enlistment in both the Confederate and Union armies. More broadly, these results contribute to understanding the political effects of civil wars on combatants and on changing attitudes about civil rights.

I proceed by describing the political context in which Republicans sought to win support for their Reconstruction policies. I then summarize the history of enlistment and service in the Union Army and debates about the political beliefs of soldiers, identifying three key mechanisms by which these men may have become more supportive of Republicans and their agenda. The following sections provide qualitative evidence for these mechanisms and for veterans' involvement in the key elections of 1866 and 1868, describe the data and design employed in statistical analyses, and report and interpret the results. I conclude by discussing the implications of this paper for understanding the political history of race in the United States, suffrage extensions, and the political legacies of civil wars.

Veterans and Politics

Background

At its formation, the Republican party brought together a disparate set of factions with conflicting reasons for opposing slavery. While some sought an immediate end to slavery and racial equality before the law, this was a minority position (Foner 1979). Moderate Republicans, including Lincoln, thought that, as slavery slowly died out, "the race problem" could be solved, not by making freed people equal citizens, but by their emigration to colonies.

And conservative Republicans flatly opposed any sort of legal equality for African Americans (Wang 1997). As abolitionist critics put it: the Republican Party, like the Democratic Party, was for "white men, not for all men" (Wang 1997).

After the war, Republican lawmakers were *reluctant* to adopt the key elements of Radical Reconstruction. On one hand, they feared the electoral repercussions. Even before the war, Democrats accused Republicans of plotting to undo the racial order. And when Lincoln issued the Emancipation Proclamation, it fueled further speculation. Democrats argued that Republicans would undo the natural supremacy of whites by setting African Americans as civic and "social" equals (implying inter-racial relationships) (Dykstra 1993), and that this would, by denying whites the right to determine locally who could vote and enjoy the protections of citizenship, "subjugate" white men to the interests of "the negro'' (Field 1982). When they debated the extent of the rights embedded in the 14th Amendment and the subsequent enforcement acts, Republicans explicitly voiced concerns about backlash from the Northern public. And they waited until after the convincing victory in the 1866 elections to start a gradual push to extend suffrage to African Americans (Wang 1997).

On the other, many Republican legislators simply did not support these laws to begin with. Historians argue that, by 1866, Republicans, and the Northern public more generally, came to embrace more radical reconstruction measures only after President Johnson's approach had yielded Southern statehouses run by former secessionists, the imposition of "Black Codes", and mass violence against freed people in Memphis and New Orleans (Foner 1988; Wang 1997). The electoral success of Republicans in 1866 has been attributed these events and to Andrew Johnson's stillborn efforts to build a moderate party out of conservative Republicans and War Democrats and his erratic whistle-stop tour in which he harangued hecklers and compared himself to Jesus (Riddleberger 1979). Union veterans are hardly mentioned in these accounts.

Argument

While historical explanations of the pivotal post-war elections do not attribute Republican success to Union Army veterans, there are three reasons I argue that they were an important political force. (1) The Union Army mobilized an unprecedented and unrivaled proportion of eligible voters for fighting. (2) Union veterans were politically active in the immediate aftermath of the war, and by all accounts, overwhelmingly in favor of the Republican Party and their Reconstruction platform. (3) This support was, in part, caused by veterans' wartime experiences.

More than 2 million men served in the Union Army during the Civil War. Not only was this the largest military mobilization in the United States to that time, but, proportionally, the largest mobilization in US history other than the Second World War. And, these veterans were likely to be politically pivotal as, because women were denied suffrage and most Southern states were denied representation in the first peace-time elections, they constituted an outsized share of the post-war electorate. While it is impossible to measure exactly what proportion of the electorate were veterans, we can estimate it using enlistment rates, mortality rates during the war, and the number of white male citizens in the post-war period. Given a series of conservative assumptions (see SA E.1)), by 1870, nearly 24 percent of eligible voters in the North were veterans.

Political mobilization

There is good reason to think that these former soldiers were politically active and that these activities were, on balance, in support of the Republican Party. Research on combatants in civil wars shows they have higher rates of political participation (Bellows and Miguel 2009; Blattman 2009). The same appears to be true for Union Army veterans. In the immediate aftermath of the war, a host of veterans organizations appeared. While many were organized around service in specific units, the largest took explicitly political positions on the issues of the war and Reconstruction (Dearing 1952, 80–123). During the elections of 1866 and

1868, veterans organizations held parades and mass meetings, attended conventions, and maintained campaign clubs (McConnell 1992; Dearing 1952). Notable groups included the Boys in Blue, the Soldiers' and Sailors' National Union League (SSNUL), Grand Army of the Republic (GAR), and the White Boys in Blue. Excepting the last, all of these major veterans organizations backed Radical Republicans.

Of these groups, the GAR was the largest, longest lasting, and most important Union veterans organization. Every Republican President from Grant to McKinley was a member, and, at its height in the 1890s, its membership reached 500,000 (which under-counts the number of Union veterans who ever were members) (McConnell 1992). Founders and leaders of the GAR understood backing Radical Republicans as a central role of the organization, and the thorough-going collaboration of Grand Army posts with Radical Republicans during the 1866 elections gave the organization a sharply political reputation (McConnell 1992; Dearing 1952).

While this worried some in the GAR who wanted to remain politically neutral, senior leaders understood the political support to protect war-time gains as the heart of the organization (McConnell 1992). And indeed, the GAR grew rapidly despite its public embrace of Radical Republican politics. And though the GAR publicly signalled partisan neutrality in 1867, John Logan (then national commander of the GAR and House Republican) was explicit in private communications: "The organization of the GAR has been and is being run in the interest of the Republican party" (Dearing 1952, 176). In 1868, Republicans coordinated veterans' organizations, including the GAR, across the country through a campaign organization they called the "Boys in Blue" (Dearing 1952). And both GAR leaders at the time and historians attribute the rapid atrophy of the organization in the years following 1868 to the loss of motivation once Republicans succeeded in achieving Radical Reconstruction over the objections of recalcitrant Southerners and Democrats (Dearing 1952; McConnell 1992).

Political Transformation?

But was veterans' support for Republicans caused by the war or the result of partisan selection into service? And more broadly, did their experiences transform the politics of veterans? This has been hotly contested by historians.

On one hand, there is broad consensus that enlistment in the Union Army was not driven by commitments to end slavery. Soldiers often explicitly averred that they were *not* abolitionists. Instead, historians note that men enlisting in the Union Army were motivated by a sense of honor, duty, and a commitment to preserving the United States as a beacon of democratic self-government (McPherson 1997; Manning 2007; Gallagher 2011). On the other, there is great disagreement about whether this changed over the course of the war.

In the past few decades, several historians have argued that Union soldiers came to see slavery as a central issue in the war, on par with preserving the Union. Both Manning (2007) and McPherson (1997) draw on letters and diaries to argue that Union soldiers came to see abolition as a necessity. Soldiers diagnosed slavery as the root cause of the war, recognized that the persistence of slavery aided their enemy, and concluded that only by ending slavery could the war be won. Beyond these instrumental reasons, Soldiers invoked their personal experiences with slavery to articulate newfound moral objection to slavery. And as casualties mounted, many soldiers came to see the war as God's punishment for the nation's sin of slavery. In this perspective, the Union could only be saved—in both a religious and political sense—and future war averted by eradicating slavery (Manning 2007). As further evidence of these changes, these authors point out that Union Army soldiers voting in the field supported Lincoln over his Democratic rival by a margin of 78 to 22, breaking toward Lincoln by 25 points more than voters at home (Winther 1944).

But, Gallagher (2011) disputes these conclusions. He claims there was great continuity in soldiers' views on slavery and race throughout the war. Soldiers did not view themselves as liberators of slaves; and, to the extent that soldiers came to oppose slavery, it was incidentally and narrowly, as a military necessity to achieve victory. White (2014) takes this argument farther, arguing that the 1864 soldier vote was indicative of neither growing support for emancipation nor Democrats becoming Republicans. Instead, he claims that due to intimidation of Democrats within the ranks, Democrats choosing not to re-enlist after the Emancipation Proclamation, and late-war recruits skewing Republican, the 1864 soldier vote reflects selection bias, not changing attitudes. And, as many soldiers expressed in their diaries: a vote for Lincoln was a rejection of negotiated peace, not an endorsement of emancipation or civil rights.

Recent scholarship lends credibility to this story of selection bias. While it appears that early enlistment was not driven by partisanship, by the end of the war, compared to all Northern white men, enlistees were on average more likely to have come from towns and counties that voted Republican in 1860 (Costa and Kahn 2008, 52–3). Similarly, Kalmoe (2020) finds that states and counties with higher pre-war Republican vote-share saw greater enlistment.

Despite these conflicting readings of war-time diaries, the *post-war* activities of veterans and their political organizations provide ample evidence that, at some point, large numbers of Union soldiers came to support both Republicans and securing emancipation. The historical record points to three interrelated mechanisms by which wartime experiences led this to happen.

Mechanisms of Transformation

Military service was a life-altering event for Union veterans (Costa and Kahn 2008). It removed them from their homes, embedded them in a hierarchical disciplinary organization, and exposed them to new people and places, even before they experienced combat. Recent work shows that smaller life changes such as moving, getting divorced, or being exposed to the *threat* of military service cause durable changes in attitudes and partisanship (Hobbs 2019; Erikson and Stoker 2011). Removed from their pre-war environs and thrust into new and intense experiences, soldiers were undoubtedly primed for political transformation.

Sacrifice Wartime suffering and sacrifice meant that Union soldiers had more at stake in ensuring that victory in the war was meaningful. Experiments in social psychology show that making sacrifices for a cause can intensify commitments to that cause. Military service (Koenig 2020) and combat experience in particular (Grossman, Manekin and Miodownik 2015) has been shown to produce intense antipathy against former enemies. These experiences of sacrifice and death strongly differentiated soldiers from civilians. While Faust (2008) argues that the scale of death in the Civil War also affected people at home, Marshall (2014) points out that the change in aggregate death rates during the war, even among young men, was not a radical departure from the high baseline mortality of the time. By contrast, soldiers personally experienced immense amounts of death, suffering, and terror.

One way veterans sought to make their sacrifices meaningful was by punishing Southern treason. Dearing (1952) recounts numerous cases in which Republican candidates and former military commanders alerted veterans to the threats posed by their former enemies, and called upon them to vote as if they were still an army on the field of battle. Between 1865 and 1868, veterans' organizations warned their members that the "late Confederate army of the South ... yesterday [was] using the bullet to overthrow the government, [and] to-day they are using the ballot to control it" (Dearing 1952, 150) and suggested that Democrats and Confederates were endangering "all the fruits of a glorious victory" (Dearing 1952, 117). Veterans' were called upon to win "another victory at the ballot box ... as decisive and more emphatic than that won on the tented field" to avert this "calamity" (Dearing 1952, 117).

Many political histories of this period characterize this rhetoric of "waving the bloody shirt" as a cynical campaign ploy by Republicans. This deeply misrepresents the context in which it occurred. In the immediate wake a war, for many veterans, the *de facto* re-imposition of slavery through the "Black Codes", the election of former Confederates to political office, and violent resistance by Southerners to Federal occupation and Reconstruction threatened to overturn the result of the war (Downs 2015; Wang 1997; Foner 1988). Even if soldiers had not come to believe in emancipation and civil rights for African Americans, they may have seen Radical Reconstruction as necessary to ensure the South remained defeated and did not rebel again.

Meaning of the War If the sole achievement of the war was merely restoring the Union, why were the Reconstruction policies of Andrew Johnson to bring a swift return to the *status quo ante bellum* anathema to veterans?

In addition to wanting to punish former Confederates for their treason, veterans made it clear that ending slavery was a central achievement of the war (Janney 2013; Gannon 2011). Historical evidence suggests that soldiers came to these conclusions through their experience of slavery. Prior to the war, few Union soldiers had first-hand experiences with slavery. When soldiers saw slavery in person, it elicited moral outrage for several reasons: many were convinced that slavery eroded the virtues and civic institutions necessary for republican government and that the persistence of slavery would lead to another war (Manning 2007). At the same time, to give meaning to their sacrifice, soldiers, like Lincoln, looked for Christian salvation of the Union in a “a new birth of freedom” (McConnell 1992; Hunt 2010; Gannon 2011; Janney 2013).

This is reflected in campaign slogans of veterans' groups during the crucial elections of 1866 and 1868. The veterans' newspaper *Soldiers' Friend* wrote: “you may ratify by your ballots the principles which you have manifested by your bullets”; the campaign song of the Boys in Blue makes explicit those principles: “We'll wipe treason out as we wiped slavery's stain; For traitors and slaves we've no place in our land”, “For God and the Union, for *Freedom* and Right / Let our ballots secure what our bullets have won” (Dearing 1952, 166). Veterans retained this interpretation of the war decades later: At “Blue and Gray” reunions, Union veterans never relinquished the moral supremacy of their cause, and they ardently disputed in textbooks, popular writing, and monuments Southern “Lost Cause” narratives of the war that denied the moral achievement of emancipation and the Civil Rights Amendments (McConnell 1992; Gannon 2011; Janney 2013).

Contact Finally, veterans' may also have changed their attitudes about civil rights through their interaction with African Americans during the war. Most white Northerners had limited or no interaction with African Americans, providing few examples to challenge racial stereotypes, but veterans serving in the South had many such opportunities. While there is limited evidence that "contact" *as such* reduces racial prejudice (Pettigrew et al. 2011), these effects are stronger when contact is prolonged, socially condoned, and toward a shared purpose (see, e.g. Mo and Conn 2018). The collaboration between Union soldiers and African Americans against the Confederacy seems to fit these conditions.

Soldiers who spent time in the South frequently met enslaved and freed African Americans. Soldiers presided over "contraband camps" (Hahn 2003), depended upon the labor of nearly two hundred thousand freed people working for the Union Army (McPherson 2008, 145), and received vital intelligence on Confederate troop movements and ambushes from African Americans who risked their lives to help (Hunt 2010). During the war, more than 180,000 African American men served in United State Colored Troops (USCT) combat units (McPherson 2008). White Union soldiers knew of or fought alongside African American regiments. Though some initially opposed the formation of the USCT (Manning 2007), Union veterans that fought alongside black units came to believe that by fighting and dying on the battlefield, African Americans had earned status as citizens (Hunt 2010; Gannon 2011).

These beliefs were persistent. For years after the war, veterans within the GAR demonstrated a commitment, albeit limited, to equality for African Americans. Unlike almost all other social organizations at the time and before the war, the GAR was racially integrated both nationally and within local posts. During Memorial Day ceremonies, white and black veterans paraded and attended church together (Gannon 2011). When posts denied admission to black veterans, they faced censure. And against prevailing segregation, black and white servicemen were buried together in GAR cemeteries. This inclusion was justified by appeals to the wartime service of African American soldiers. Yet, African American members held only symbolic offices (McConnell 1992), and pleas from African American posts in the South for the GAR to take a stand against lynching and Jim Crow went unanswered (Gannon 2011).

Testing the Argument

While the balance of historical evidence indicates that war-time experiences, whether through sacrifice, coming to see emancipation as a war aim, or inter-racial contact, led veterans to come out of the war with greater support for Republicans and their Reconstruction agenda, there are obstacles to testing this argument.

First, the historical record of diaries, letters, and accounts of veterans mobilization is clearly open to some interpretation, and reasonable historians disagree.[1] Second, there are good reasons to be concerned about partisan selection into service. And third, there is very limited individual data on political behavior from the time with which to test the above claims.

I address these problems by triangulating evidence from statistical tests of three different implications of my argument:

1. Even if Republicans selected themselves into the Union Army at higher rates, the effects of enlistment could be found by looking at how support for Republicans changed after the war. If soldiers became more supportive of Republicans, then: *(H1) we should observe that, across the North, higher wartime enlistment rates caused communities to have greater post-war electoral gains for the Republican Party.*

2. Aggregate effects of enlistment rates do not by themselves confirm that it was *soldiers* who changed. These effects could also be consistent with greater gains among non-veterans in high-enlistment areas. But, if wartime experiences turned soldiers into

[1] However, the most compelling evidence that veterans backed Radicals in the elections of 1866 and 1868 comes from Dearing (1952), whose thesis was that veterans were hoodwinked and manipulated by conniving politicians into taking this position.

Republicans, we should observe these effects along the intensive margin: *(H2) soldiers with greater exposure to combat and death, compared to those with less exposure, should be more Republican after the war.*

3. Soldiers may have backed Republicans solely because they could not stomach voting for Democrats who had questioned the war effort, not out of support for Radical Reconstruction. But if soldiers did become more supportive of these policies: *(H3) we should observe that soldiers became more supportive of African American Civil Rights in referenda on that issue.*

Design

To test these implications, I employ three research designs.

Enlistment Rates and Republican Voteshare I identify the effect of wartime enlistment on votes for Republicans using a continuous difference-in-differences (DD) design (Angrist and Pischke 2008, 234–5). Here, the "treatment" is war-time enlistment rates in a county. The first difference compares counties before the war (when no one was enlisted) and after the war starts (when enlistment happened and the "treatment" occurred). Unlike binary DD estimators, all counties had *some* enlistment and enlistment rates are continuous. Thus, I estimate the effect of differing "intensities" of enlistment on the within-county change in support for Republicans. This estimator is given in equation 1.

$$\text{Republican voteshare}_{ie} = \alpha_i + \alpha_e + \beta\text{Enlistment Rate}_i * \text{Civil War}_e + \epsilon_i + \epsilon_y \qquad (1)$$

In this equation, subscript i denotes the county, y the year, and e the state-election. A state-election would be, for instance, the Massachusetts Congressional elections of 1860. Civil War_e is a dummy variable that is 1 if the election occurs in 1861 or later and 0 otherwise.

Enlistment Rate_i is the fraction of military-aged males in a county that served in the Civil War (between 0 and 1). The first difference (within counties) is imposed using county-level fixed effects (α_i). The second difference is obtained by the inclusion of state-election fixed effects (α_e), which imposes the assumption of parallel trends within states. As with regression estimators of any DD model, the treatment assignment, Enlistment Rate_i is constant within counties, and the post-treatment indicator, Civil War_e, is constant within state-elections (Angrist and Pischke 2008, 233–4). Thus, only the interaction remains in the equation and is captured by the parameter β. Errors are clustered by both county and year. I estimate this equation using elections between 1854 and 1880, the first year in which the Republican party contested elections, and the first Presidential election after the conventional date for the "end" of Reconstruction.

In SA A, I show that under very similar assumptions as the familiar binary DD estimator, this estimates the least squares linear approximation of the (potentially non-linear) average causal response function of Republican vote-share across different levels of enlistment. These assumptions are: (1) parallel trends in Republican vote-share among counties within the same state, across different levels of enlistment; and either (2a) within states, the effect of enlistment rates is not heterogeneous or that heterogeneity is independent of enlistment rates; *or* (2b) there is no confounding of the selection into enlistment rates. Below, I provide evidence of parallel trends and show that results remain robust in models that explicitly model heterogeneous effects and confounding.

I use this same design when estimating the effects of military service on voting for African American suffrage in state referenda. Because there are only two time-periods, I estimate the effect of enlistment on the difference in votes for suffrage, including state fixed effects. Additionally, because I interpret these as ecological regressions, the denominator for votes and enlistment rates are the number of men eligible to vote in the post-war referenda.

Combat Experience and Individual Partisanship To bolster these aggregate estimates, I also provide evidence of the effects of military service on individuals. It might seem natural to compare the partisanship of those who served versus those who did not, but this is fraught with problems. (i) Data on conditioning variables for this time-period are limited and noisy. Those who enlisted were likely different from those who remained home on both unobserved and poorly observed traits. (ii) While there was a "random" draft lottery during the war, the number of men actually drafted was quite small and uncovering the sample that was randomly assigned would be onerous. (iii) War-time service increased post-war mobility, likely producing differential attrition. Instead, I investigate the causal mechanisms by which military service affected those who served.

I identify the causal effect of exposure to combat casualties on the post-war partisanship of individual soldiers using a natural experiment. The war-time experiences of Union soldiers, including exposure to combat deaths, were determined by non-random selection at several levels. First, men who served chose when to enlist during the war, their term of enlistment (three-years, one-year, or less), the kind of unit (infantry, cavalry, or artillery), and which regiment (Costa and Kahn 2008, 52–7). Second, once in their units, soldiers could desert, seek transfers, and decide whether to re-enlist . Finally, Army commanders could assign units to different theaters of operation, front-line combat, or other less risky duties depending on their experience, morale, and reliability. At each stage, soldiers' partisanship may have informed their choices and the choices of their commanders (Kalmoe 2020).

I address this selection problem by exploiting variation in exposure to combat deaths among men who joined the same infantry regiments at the same time. The vast majority of men serving in the Union Army were in the infantry. The basic unit in which infantrymen were mobilized, maneuvered, and went into combat was the regiment. When first organized, regiments consisted of approximately 1000 men, but by the middle of the war, regimental strength was considerably lower (Hess 2015). Regiments were further subdivided into ten companies of equal size. Men in the same company were mustered in together and then trained, lived, worked, and fought directly alongside each other (Costa and Kahn 2008). While the deaths of any man in their regiment likely affected soldiers, deaths of men within the same company were undoubtedly more meaningful: they were likely to have known the fallen personally, possibly even before the war, and to have witnessed their deaths.

Variation in company-level combat casualties for men serving in the same infantry regiment was plausibly random. Men in the same regiment made the same selection decision on when and how they would serve, went the same places, and fought in the same battles. In battle, infantry regiments typically formed up in a line of men, two deep, horizontally arranged by company, approximately 140 yards across (see Figure B1) (Hess 2015). In the chaos and smoke of battle, which companies in that line received more casualties was effectively arbitrary.[2]

Nevertheless, the number of combat casualties seen by a soldier was also determined by how long they stayed with their unit. This could be affected by choices a soldier made to transfer, desert, or take actions that led to promotion, injury, or death; all of which could be driven by differences in partisanship. Thus, I construct an "intent-to-treat" measure of exposure to combat deaths.

$$\text{Company Casualties}_i = \sum_{j}^{n} KIA_j \cdot (i \neq j) \cdot (t_i^* \cap t_j \neq \emptyset) \tag{2}$$

I construct the company-level casualties treatment for soldier i, as follows. Soldiers j through $n-1$ are all the men who ever served in the company c to which soldier i was *first* assigned. t_j is the set containing all dates that soldier j served in company c, based on muster records. KIA_j is an indicator for whether soldier j died at the hands of the enemy: in battle, of battle wounds, or as a prisoner of war. To remove bias induced by the selection process determining whether soldier i stayed or left the company, t_i^* is the set

[2]For a more detailed justification for the plausibility of as-if random exposure, see Section B.

of dates soldier i *should* have served in the company based on the date of his muster and the term of enlistment. Thus, Company Casualties$_i$ is the number of men who soldier i was assigned to serve with who died as a result of combat.

I estimate the effect of company casualties using the following regression model, where i is a soldier, α_r is a fixed effect for men joining the same regiment in the same year, and ϵ_c is an error shared by men who joined the same company in the same year. In robustness checks, $\mathbf{X_i}$ is a set of covariates for soldier i drawn from their military records and the 1860 US Census.

$$\text{Partisanship 1874}_i = \alpha_r + \text{Company Casualties}_i + \mathbf{X_i} + \epsilon_c \tag{3}$$

While military records and the US Census provide many characteristics on which to check balance, they lack partisanship. To fill the gap, I trained and validated a machine-learning classifier that predicts the 1874 partisanship of soldiers based on fixed demographic characteristics of name, birth year, and birth place (See Section B.4). I show that companies with greater casualties are balanced in predicted partisanship and individual treatment is balanced on predicted partisanship, demographic features, education, household composition, and property ownership (See Section B.5).

Data

Enlistment Rates: I measure enlistment rates using a novel database of Civil War soldiers: the American Civil War Research Database (ACWRD). Drawing on unit histories, official records, and the reports of states Adjutant Generals, it links data on individual soldiers, military units, and engagements. For seven states (Illinois, Iowa, Wisconsin, Massachusetts, Vermont, Maine, and Connecticut), it is possible to link more than 90 percent of soldiers to their residence at the time of enlistment.[3] I then geocoded the residences, and matched these

[3]For Indiana, I construct county-level enlistment numbers from Adjutant General reports.

locations to counties and townships in 1860. I compute the county-level enlistment rate by dividing the number soldiers in a county by the number of military-aged males (between the ages of 10 and 39) present in the county in the 1860 Census. For a more detailed exposition and validation of this data, please see Section D.3 and Kalmoe (2020).

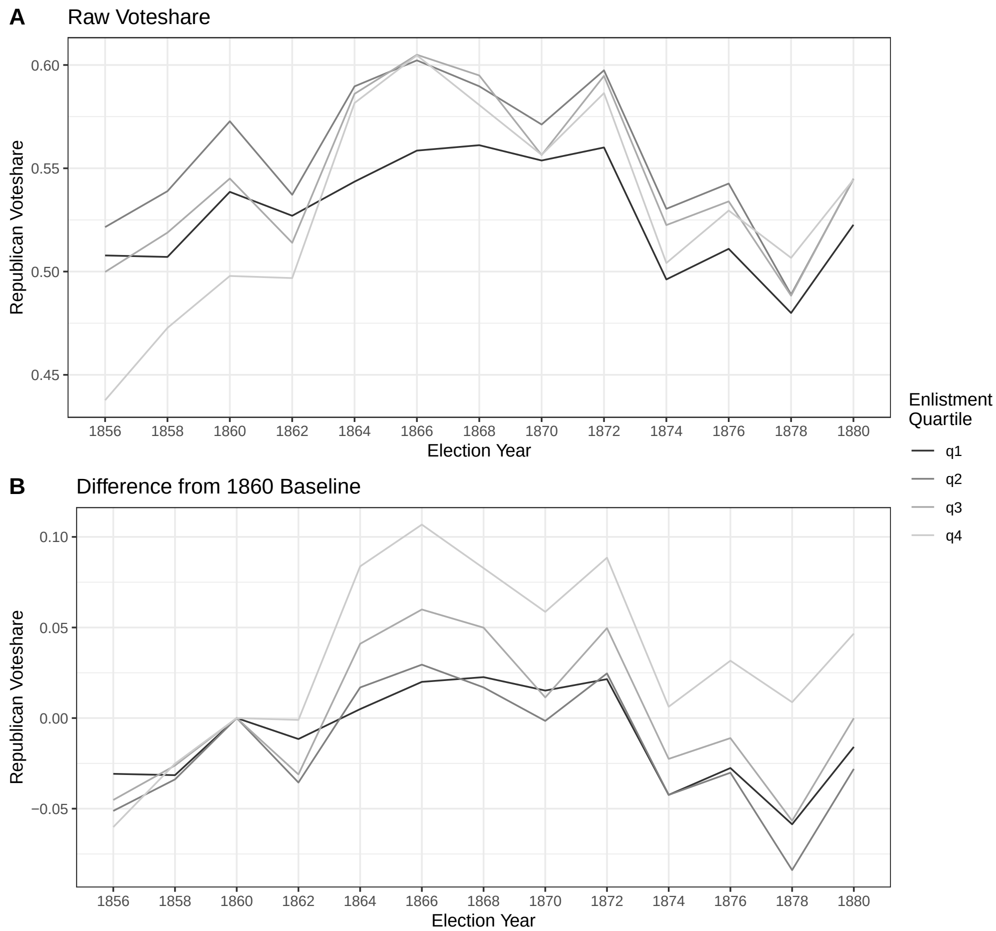
Elections: Republicans I measure support for the Republican party using Congressional and Presidential election returns. I collapse county-level returns from *United States Historical Election Returns, 1824-1968* to 1860 boundaries, using areal interpolation (ICPSR 1999), and then calculate Republican vote-share.

Elections: Black Suffrage I measure direct support for civil rights expansions using votes in state referenda to remove the word "white" from the qualifications for suffrage in the state constitutions in Iowa (1857 and 1868) and Wisconsin (1857 and 1865) (Dykstra 1993; McManus 1998). Other states held similar referenda, but either lack pre- and post-war votes or residences for soldiers. I draw on data compiled by Dykstra (1993) and McManus (1998) to calculate pro-suffrage votes in clusters of townships for which the boundaries were stable and election results are available in both pre- and post-war referenda. (See Section D.2)

Covariates: I collect a battery of economic, demographic, and political covariates for counties from the 1860 Census and (ICPSR 1999). Please see Section D.1 for a full list of these measures.

Indiana Soldiers: The effects of exposure to combat casualties for soldiers from Indiana use the following data. First, I measure post-war partisanship using county directories for nine counties in Indiana produced in 1874 (see Section B.3.1 and (DeCanio 2007)). These directories contain the names, ages, birthplaces, year of move into a county, and party affiliations of most adult men residing in those counties. I measure partisanship as: $Democrat = 1$, $Republican = 1$, and the partisan swing $(Republican - Democrat)/2$. Second, I used the ACWRD to identify 21301 soldiers from Indiana regiments who listed residences in those counties. I then linked these soldiers to the first company in which they served and calculated Company Casualties$_i$ as defined above. Third, I linked these soldiers to the 1860 Census using the `fastLink` algorithm (Enamorado, Fifield and Imai 2019), blocking on county of residence at enlistment and matching on name and birth year. This enables me to include pre-war covariates and focus my analyses on a set of 10358 soldiers that are "findable" pre-treatment. Finally, I linked the soldiers to the 1874 county directories, matching on county, name, and birth year. I was able to find 3914 soldiers after the war, 3264 of whom were located in the 1860 Census. For greater detail on these data and the matching procedures, see Section B.3.

Figure 1: Republican Voteshare Trends by Enlistment Quartile



Panel A shows the un-adjusted trend in county-level Republican voteshare averaged by within-state enlistment quartile. Trends start in 1856, the first year in which Republicans contested elections in all 8 states. Panel B shows the same data, subtracted from the quartile average in 1860.

Results

Enlistment Rates and Republican Gains

Did counties with higher enlistment rates vote more strongly for Republicans during Reconstruction? Table 1 reports the estimates of the difference-in-differences equation 1. Column (1) shows the main result: the shift towards Republicans was greater after the Civil War in places with more enlistment. The effect size suggests that a ten percentage point increase in enlistment would yield a 4.2 percentage point ($p < 0.001$) increase in Republican vote-share.[4] This estimate is robust to alternately excluding all counties in which Republicans ever failed to contest an election (Column 2) or including a dummy for county-elections which Republicans did not contest (Column 3), measuring enlistment as either total enlistment or surviving veterans (Table A2), and dropping individual states (not shown).

To interpret this as the causal effect of enlistment rates, we must believe two of three assumptions. First, counties with different rates of enlistment should have parallel trends before the war. Figure 1 shows the raw Republican voteshare in elections between 1856 and 1880 averaged by enlistment quartiles. Across the three election cycles prior to 1861, all four quartiles had parallel trends. Figure A1 formally tests the slope on enlistment rates and the difference between GOP vote-share in 1860 and every other election from 1854 to 1920. For pre-war elections, these slopes are not significantly different from 0. But once the war started, trends diverged: higher enlistment counties saw greater increases in Republican voteshare.

Second, even if the pre-war trends were parallel, it could be that there were other baseline differences between high and low enlistment counties that affected how they responded to the war (i.e., time-varying confounding). Figure A2 shows the within-state relationship between enlistment rates and 32 pre-war demographic, economic, and political covariates. Demographically, enlistment rates were higher in counties with smaller populations, fewer

[4] Within states, the SD of enlistment rate was 8.8 percent.

white people, more southern-born, and gender distributions skewed towards men. Economically, enlistment rates were higher where agricultural output was less and a greater share of men and women worked in manufacturing. Politically, enlistment was higher in places where Republican party and its antecedents had performed worse. To explicitly model potential time-varying confounding, I repeat the difference-in-differences analysis, including an interaction between all 32 covariates and the post-war indicator (see Figure A3 and Table A1). The results are substantively unchanged.

Third, and alternatively, even if trends are parallel and there is an absence of confounding, it could be that my estimates are biased because the effects of enlistment were heterogeneous across counties with different levels of enlistment. I address this concern by flexibly modelling heterogeneity in the DD effect of enlistment for each county, weighting cases based on similarity in pre-war covariates. The average partial effect of enlistment across all counties is substantively the same (See Section A.5).

Table 1: Difference-in-Difference Effect of Enlistment Rates on Republican Voteshare

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Enlist % · Postbellum	0.415***	0.353***	0.336***
	(0.098)	(0.068)	(0.057)
GOP no contest	included	dropped	dummy
County FE	X	X	X
State-Election FE	X	X	X
Observations	8,064	6,027	8,064
Note:	*p<0.05; **p<0.01; ***p<0.001		

Analyses include presidential and congressional election results in 384 counties in CT, MA, ME, IL, IN, IA, VT, and WI between 1854 and 1880. Standard errors clustered by county and election year. Counties with election cycles in which the GOP does not contest an election cycle either treated as a 0, the election is marked with a dummy, or the all observations for that county are dropped.

Combat Deaths and Individual Partisanship

Table 2 reports the effects of exposure to company-level combat deaths for soldiers who were linked to the 1860 Census. In baseline models (columns 1–3), which include only regiment fixed effects, I estimate that one additional casualty in a company[5] increases support for Republicans by 0.9 percentage points ($p = 0.022$), decreased support for Democrats by 1 percentage point ($p = 0.005$), and swung the margin toward Republicans by 1.9 percentage points ($p = 0.007$). These results remain virtually unchanged when including covariates drawn from enlistment records (columns 4–6) and when further adding Census covariates (columns 7–9).[6]

In additional analyses, I show that these results are robust to including all soldiers or only those located in the Census, using all plausible matches or only the best matches in 1874, and measuring the treatment as company combat deaths or combat death rates (See Section B.6)). Moreover, while the majority of soldiers cannot be located post-war, differences in attrition across levels of treatment are substantively small, unrelated to predicted pre-war partisanship, and treatment effects are unchanged when re-weighting soldiers based on their inverse probability of being found in 1874 (See Section B.7).

This provides strong support that wartime experiences of combat were a causal mechanism by which individual soldiers became more Republican. Consistent with this story, effects of combat deaths were larger for soldiers who were predicted Democrats (Figure B13). And since combat is one experience of the war that was unique to soldiers, these

[5]The SD of company combat deaths within regiments is 2.3.

[6]Army: number of men in company, date of enlistment, joined regiment at formation, rank at enlistment, year of birth, draftee or substitute, and county of residence. Census: predicted probability of being Democrat/Republican, attended school, illiterate, household head, # children in household, logged value of household real and personal estate, owned property, married, household size, # military-aged males in household, dummies for place of birth.

results support the plausibility of interpreting the DD effects of enlistment rates as causal effects of military service on veterans. To believe otherwise requires asserting that either (i) some other feature of military service dramatically suppressed soldiers' support for Republicans, or (ii) some experience unique to remaining at home during the war increased non-combatant support for Republicans by an even larger margin.

The estimated effects of combat deaths on partisanship are all the more remarkable given that (i) the dependent variable is measured with error, increasing standard errors; (ii) this identification strategy nets out variation in regimental combat experiences and exploits only "intended" exposure; and (iii) I find these effects in 1874. Not only was this nearly 10 years after the these veterans left the Army, 1874 was also a year of tremendous electoral losses for the Republican Party. War and Reconstruction issues were overshadowed by an economic crisis that divided the party and newspapers intensified their scrutiny of corruption allegations against Republicans nationally and Reconstruction state governments in the South (Barreyre 2015).

Voting for Suffrage

Did veterans also become more likely to back civil rights expansions? Table 3 reports the result of difference-in-differences and lagged-dependent variables ecological regressions. Both designs yield virtually identical results: a ten percentage point increase in enlistment implies an increase between 3.1 and 3.3 percentage points ($p < 0.001$). These results are substantively unaltered by conditioning on the fraction of people eligible to vote in the pre-war election and restricting the sample to townships with smaller population increases between 1860 and 1870. In Table C1, I restrict the analyses to Wisconsin. Even when controlling for the difference between Republican and pro-suffrage vote-share in 1857 and adding county fixed effects, townships with more enlistment saw greater increases in support for suffrage.

While ecological regression is notorious for its limitations, plausible conclusions can be reached with care (Section C). The central problem with ecological regression is that there

Table 2: Effect of Company Casualties on Post-war Partisanship

	Dependent variable:								
	Rep.	Dem. Baseline	Party Diff.	Rep.	Dem. Army Controls	Party Diff.	Rep.	Dem. All Controls	Party Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Company KIA	0.009*	−0.010**	0.010**	0.010*	−0.011**	0.010**	0.010*	−0.011**	0.010**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Regiment FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Army Controls	N	N	N	Y	Y	Y	Y	Y	Y
Census Controls	N	N	N	N	N	N	Y	Y	Y
Observations	3,549	3,549	3,549	3,113	3,113	3,113	3,113	3,113	3,113

Note: *p<0.05; **p<0.01; ***p<0.001

Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and their best match, if any, in the 1874 People's Guides. Baseline and control models, respectively, include data on 3106, 2757 individual soldiers, serving in 539, 507 companies, across 212, 200 regiments. Regiment fixed effects are for each group of soldiers who joined a regiment in the same year. Standard errors are clustered by company.

may be "contextual" effects: in our case, the change in support for suffrage among soldiers and civilians may be related to the fraction of people who enlisted. Because data is aggregated, we are unable to identify what this relationship is, potentially biasing our estimates. I place mathematical bounds on the size of contextual effects and on the effect of service on support for suffrage (Jiang et al. 2020). I show that for the effect on veterans to be non-positive, the contextual effects must be very large. And, I show that in one county and in townships in which pre-war support for suffrage was close to zero, the mathematical bounds on the difference-in-difference effect of military service on veterans' support for suffrage are greater than 0 (SA C.3).

Table 3: Effect of Enlistment on Support for Black Suffrage (Iowa and Wisconsin Township Returns)

	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Enlistment Rate	0.327***	0.312***	0.314***	0.226***	0.215***	0.222***
	(0.058)	(0.059)	(0.059)	(0.045)	(0.047)	(0.045)
Lagged DV	Y	N	N	Y	N	N
Differenced	N	Y	Y	N	Y	Y
Controls	N	N	Y	N	N	Y
Observations	545	545	545	471	471	471

Note: *p<0.05; **p<0.01; ***p<0.001

Enlistment rate is number of men serving over those eligible to vote in 1865 (WI) or 1868 (IA). Suffrage vote totals come from state constitutional referenda in stable clusters of townships in Iowa and Wisconsin. All models include state fixed effects. LDV slopes vary by state. Control variables include: (i) the fraction of pre-war eligible voters over post-war eligible voters. Townships are weighted by number of white men. The restricted sample includes only townships where the population eligible to vote in 1865/1868 changed by less than 50 percent between 1860 and 1870. Standard errors are robust.

Interpretation

Taken together, these three analyses, alongside the historical evidence, demonstrate: (i) Exposure to combat losses turned Union soldiers from Democrats into Republicans. (ii) The effects of enlistment rates show that these effects of military service substantially increased support for Republicans at the polls, particularly in vital elections over Reconstruction policy. (iii) Ecological estimates show that soldiers also came to support the civil rights policies directly. Here, I argue against several objections to this interpretation.

Selection?

Some might argue that selection into service could bias these results. But selection into service cannot explain the individual level effects. And to bias ecological inference using the difference-in-difference design, any story of confounding due to selection would have to operate through differential effects among veterans and civilians across different levels of enlistment. Upward bias, our concern, would result from the effects of military service being substantially smaller in low-enlistment areas than in high-enlistment areas. This could happen if the selection into service in low-enlistment areas more strongly skewed towards those with Republican partisanship or pro-suffrage attitudes. This would put a low ceiling on the number of servicemen in low-enlistment areas that could become Republican or pro-suffrage, leading the DD effects of service for these men to be near 0 or even negative. However, if this were true, then we should see no effect of enlistment in areas that had no pre-war support for either Republicans or suffrage. With no possibility of partisan selection, this particular bias should be absent. Yet, when restricting the analysis to townships in which Republicans and suffrage received support from less than 1 percent of eligible men in 1857, enlistment is still significant and positively associated with the change in support for suffrage (Figure C5).

Selection by Racial Attitudes Even if veterans' support for suffrage and Republicans increased more than civilians, that difference might be due to something other than military service, such as ideology, partisan identity, party branding, and clientelism. If men who were pro-suffrage were more likely to become soldiers during the war, then, as the Republican Party endorsed extending suffrage they would have become more Republican. There is little evidence to support this: the Republican Party, from its beginning, brought together opponents of slavery. It is hard to imagine proponents of civil rights voting for Democrats in the elections just before the war. And consistent with this, in 1857 Wisconsin, pro-suffrage votes in nearly every township were strictly less than or equal to Republican votes.

A similar story might be that, among Republicans, soldiers with greater commitment to ending slavery and racial equality were more likely to enlist. Then, when the Republican Party began to adopt more Radical positions on race, moderate and conservative Republicans, who were less likely to have enlisted, defected from the party, while veterans remained steadfast. However, this is inconsistent with the evidence in Figure 1: Republican vote-shares in 1864 through 1868 *increased* over 1860, and increased *more* in places with higher enlistment.

Party Messaging Yet another claim might be that Republicans selected themselves into military service, and when the Republican party endorsed suffrage after the war, they followed party messaging. This seems unlikely for several reasons. First, I show above that soldiers *became* Republicans, even as the party became more Radical. Second, the Wisconsin referendum took place in November 1865, before Congressional Republicans endorsed suffrage and other Radical measures. In Wisconsin, the suffrage question divided the coalition of Republicans and War Democrats that governed the state; led by one of the state's Republican Senators, the Republican convention did not endorse suffrage; the leading Republican paper stated suffrage was a "minor issue" and support for it was not the "standard of party orthodoxy"; and, Lucius Fairchild, the Republican gubernatorial candidate, caused a stir by repeatedly refusing to take any public stance on the suffrage referendum (McManus 1998). Third, the positive effects of enlistment on suffrage are robust to conditioning on the pro-Republican/anti-suffrage vote-share in 1857. Finally, as Kuziemko and Washington (2018) show, in the "second Reconstruction", a shift in Democratic messaging in favor of Civil Rights led racially conservative party members to abandon the party, not change their racial attitudes.

Patronage not Policy? Support for the Republican party may have been driven by patronage to soldiers and officers that overrode racial prejudice and opposition to legal equality. Skocpol (1993) argues that, starting in the 1880s and 1890s, the first major social welfare program in the United States transferred vast sums of money to Union Army pensioners and was managed by Republican appointees as a vote-buying scheme. But in the era I discuss, only a tiny fraction of veterans received pensions, the amounts paid were smaller, and the partisan divide over pensions did not arise until decades later.

Why Did Veterans Change?

Was voting Republican the "only" choice? It could have been the case that Union veterans had no option but to vote Republican, despite repugnance toward African Americans and expanding civil rights, because Southern Democrats had led secession and formed the Confederate government while Northern "Copperheads" and "Peace" Democrats had opposed the war and pushed for a negotiated peace with "traitors" (White 2014). But this ignores several key points.

After the war, Democrats worked hard to embrace soldiers and the Union cause, while splitting it from the issue of civil rights. Many "War Democrats' backed the Army in its fight to save the Union and shared a "Union Party" ticket with Lincoln in 1864, but were key supporters of Andrew Johnson in the disputes with Republicans during Reconstruction. Believing that contact with freed people had *intensified* racism and opposition to civil rights among veterans, Democrats sought to win their support by accusing Republicans of pursuing "social equality" for African Americans, warning that the rights of white men were being eroded, and backing slates of ex-officers candidates in so-called "soldiers' parties,'' with names like the "Union Anti-Negro Suffrage Party" and platforms that praised white soldiers while denouncing civil rights reforms (Dearing 1952, 66)(Dykstra 1993; Field 1982). Like Republicans, Democrats worked with several veterans groups that were both patriotic and explicitly "anti-Negro'' (Dearing 1952). Nevertheless, these efforts reaped few rewards: "Soldiers' parties" failed to win many votes; And pro-Democrat veterans' groups were smaller and weaker than their Republican counterparts and quickly disappeared, while the GAR lasted for decades to come (Dykstra 1993; Field 1982; McConnell 1992; Gannon 2011).

Wartime Experiences While there is both quantitative and qualitative evidence for all three war-related mechanisms, it is stronger for the first two. The strongest evidence is for *sacrifice*: Not only did individual experiences of combat deaths increase support for Republicans; the effect of enlistment rates on Republican voteshare in 1866 was substantially higher in counties where the average soldier experienced more company- and regiment-level casualties (Figures A5, A6). There is also evidence that civil rights were part of the *meaning of the war*: in addition to finding that support for suffrage increased with enlistment; these effects were stronger in townships where soldiers experienced more company casualties that others serving in the same regiment (Figure C6). Finally, there is limited statistical evidence for *contact*: the effects of enlistment on suffrage were higher in townships where soldiers spent more time fighting alongside USCT (Figure C7).

Conclusion

Taken together, the evidence I present tells a clear story. Soldiers participating in the war became more supportive of Republicans and the policies of Radical Reconstruction. This was likely due to increased commitment to punishing their opponents and winning the peace that came from war-time sacrifice, an understanding acquired during the war that ending slavery was essential in itself and—for either pragmatic or moral reasons—for saving the Union, and changes in racial attitudes brought about through contact.

These conclusions make important contributions to our historical understanding. First, it provides new, compelling evidence that revisionist historians who argue the war transformed the political views of Union soldiers are correct (Manning 2007; McPherson 1997). Second, these results call for greater attention to the role of Union veterans in enabling the constitutional and legislative reforms of Congressional Reconstruction. Particularly illustrative of this is the Congressional elections of 1866, which emboldened Republicans to pursue equal suffrage laws and enabled them to override Johnson's veto. Republicans won their veto-proof supermajority by taking 147 out of 193 seats in the general election. The pivotal seats needed were won by margins between 4 and 5 points, which would have been secured by an increase in enlistment between 4.8 and 6 percentage points. Moreover, because enlistment was higher places where Republicans had performed poorly before the war, the biggest gains for Republicans were not wasted. Even the individual effects of company casualties were substantively large. Back-of-the-envelope calculations imply that decreasing or increasing combat deaths experienced by Indiana soldiers by one (within-regiment) standard deviation could have shifted the results of the 1874 Congressional Elections in Indiana (in reality, an 8-5 Democratic majority) to a 10-3 or a 7-6 Democratic majority (see SA E.2).

This paper also makes an important contribution to understanding the political effects of civil wars, and the American Civil War in particular. Recent work in political science emphasizes the partisan nature of enlistment; but consonant with broader research on civil wars, this paper shows that the war endogenously changed political views. More can be gleaned about the mechanisms by which civil war affects participants by expanding my analyses to a wider array of Union and Confederate soldiers. This paper examines soldiers in eight Northern states. Exploring heterogeneous effects of enlistment, especially in New York, Pennsylvania, Ohio, and the border states may be illuminating. Future research might exploit draft lotteries and military regulations on age cut-offs to identify the effects of serving in the Union or Confederate Army. And military records make it possible to explore the effects of exposure to slavery and inter-racial contact with freed-people. Finally, more work needs to be done to understand the effects of veterans' organizations and their political mobilization on veterans and their communities. This seems particularly important in explaining how effects of enlistment on Republican voting appear to persist into the 20th century (Figure A1).

Finally, this paper also makes a modest contribution to research on suffrage expansions more broadly and civil rights and race in the United States in particular. I find that, whatever economic or partisan incentives Republicans had for removing to race-based suffrage qualifications in the United States, their success also hinged on winning support from the white electorate. Racial biases are hard to move. And while, anecdotally, the war led some veterans reduced their prejudice against African Americans, the analyses in this paper provide no strong evidence for or against a change in racial prejudice. Instead, similar to white veterans of the Second World War (White 2016), I do show that wartime participation led Union soldiers to back the party and policies of civil rights, in spite of their racial biases.

Yet the *de facto* reversal of many of the gains of Reconstruction (Foner 1988) and the ultimate victory of the Southern "Lost Cause" narrative of the Civil War in the subsequent decades raises important questions about the depth and durability of Union veterans' commitment to civil rights. The results of this paper suggests that answering these questions may be pivotal in understanding why support for Reconstruction ebbed.

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Supplementary Appendix

A Enlistment Rates Difference-in-Differences: Design and Robustness

A.1 Design: Binary Diff-in-Diff

In a traditional difference-in-difference design, some units $i \in \{1 \dots n\}$ that are exposed to a treatment D at time $t = 1$ (but not at time $t = 0$). We want to estimate average effect of treatment on treated, $ATT = E[\tau_i]$ which is defined as:

$$E[\tau_i] = \frac{1}{n}\sum_{i=1}^{n}[Y(1)_{i1}|D_i = 1] - [Y(0)_{i1}|D_i = 1]$$

Here, $Y(D)$ indicates the potential outcome of Y for a specific value of treatment $D \in \{0, 1\}$. The subscript it indicates unit i at time t. $D_i = d$ indicates *actual* level of treatment received by unit i in time $t = 1$.

$E[\tau_i]$ cannot be directly estimated, because we only observe treated unit i in the post-treatment period 1 in its treated state $(Y(1)_{i1}|D_i = 1)$; what that treated unit would have done at time $t = 1$ were it untreated is counterfactual $(Y(0)_{i1}(D_i = 1))$.

The difference-in-difference estimator allows us to estimate the $ATT = E[\tau_i]$ by substituting observed potential outcomes for this counterfactual under a set of assumptions.

$$\tau = \{E[Y(1)_{i1}|D_i = 1] - E[Y(0)_{i0}|D_i = 1]\} - \{E[Y(0)_{i1}|D_i = 0] - E[Y(0)_{i0}|D_i = 0]\}$$

The difference-in-difference estimator allows us to use the *observed* potential outcomes of an *untreated* unit in the place of this counterfactual, by making a few assumptions about parallel trends between the treated and untreated units.

The parallel trends assumption is that the (counterfactual) change in $Y(0)$ for the *treated units* between time $t = 0$ to time $t = 1$ would have been the same as the observed change in $Y(0)$ in the *untreated* units over time.

$$E[Y(0)_{i1}|D_i = 1] - E[Y(0)_{i0}|D_i = 1] = E[Y(0)_{i1}|D_i = 0] - E[Y(0)_{i0}|D_i = 0]$$

In other words: the assumption is that the counterfactual change over time that treated units would have had *in the absence of treatment* is the same as the change over time that the untreated units had, factually, in the absence of treatment.

Thus, in the classical DID setup, there is a set of units $i \in \{1 \dots n\}$, some of which are treated and others untreated; and all units are observed in time $t = 0$ prior to the treatment and in time $t = 1$ after treatment. The DID estimator can be estimated using a difference in means, or using regression. In a regression context, this difference-in-difference is modeled as follows:

$$Y_{it} = \mu + \gamma T_t + \delta \cdot D_i + \tau \cdot D_i \cdot T_t + \epsilon_i$$

Here, μ is an intercept that captures the mean of Y for untreated units in the pre-period. D_i is an indicator for whether a unit receives treatment at time $t = 1$; it is *constant* within units as it indicates whether the unit *ever* receives treatment. In the regression, δ captures the difference in Y between the treated group and the untreated group, prior to treatment. T_t is an indicator for whether the Y_it is observed pre-treatment (0) or post-treatment (1). Thus, T_t is constant for all units observed at the same time. γ captures the change in Y among the untreated group from time 0 to time 1. Finally, τ captures the ATT, under the assumption of parallel trends: the difference in the observed change from pre- to post- in Y for the treated group versus the untreated group.

Angrist and Pischke note that if there are multiple treated and untreated units, μ can be substituted by separate intercepts for each individual unit μ_i. These μ_i capture the pre-treatment value of Y for each unit. If there are multiple pre- and post-treatment time periods, γ can be substituted by vector of intercepts γ_t for each specific time period. These capture the change in Y for the untreated units, relative to some omitted time period.

Notably, in such a regression, D_i and δ must be excluded because D_i is invariant within units i and thus not linearly independent of μ; similarly, T_t must be excluded because it is invariant within time periods t, and thus linearly dependent with γ_t. The resulting equation is:

$$Y_{it} = \mu_i + \gamma_t + \tau \cdot D_i \cdot T_t + \epsilon_i$$

$\hat{\tau}$ from this regression is an unbiased estimator of the ATT if the parallel trends assumptions hold.

A.2 Civil War Enlistment DID

In this paper, I apply this difference-in-difference design to examine the effect of enlistment rates in the Union Army on county-level voting for Republicans and Black Suffrage. Here, the treatment, D_i is the enlistment rate in the Union Army among military-aged males in county i. The the time variable T_t is an indicator for whether county i is observed before the Civil War (0), and thus before the county experienced the treatment, or after the start of the Civil War (1), when the county was affected by enlistment.

I include county-specific intercepts μ_i and state-year intercepts γ_{st}. This deviates from the classical model in that it assumes parallel trends **within states**, rather than nationwide.

This application of the DID differs in two key ways from the classical formulation.

1. Continuous Treatment: In the research design for this paper, treatment is the enlistment rate; it can take on values between 0 and 1. This contrasts with classical difference-in-difference approaches in which treatment is binary.

2. All units treated: In this design, all counties have some enlistment. Thus, there are no units that go "untreated" in the post-treatment period. Nevertheless, the level of treatment that counties receive after the war is different.

These two differences raise questions: Do the assumptions required for identification differ? And what is the effect that is identified?[1]

A.2.1 No Untreated Units

The first thing to note is that the identification in a difference-in-difference does not hinge on a set of units that is completely untreated. Consider the classic example in Card and Krueger (1994), that examines the effect of an increase in the minimum wage in New Jersey in 1992. They find the effect of this minimum wage increase in New Jersey using a DID in which Pennsylvania was the "untreated'' state.

Using this example, I illustrate how the central assumptions of the DID change slightly, even if all units are treated.

While people typically conceive of the treatment statuses being $D \in \{0, 1\}$, where 0 is untreated; it is equivalent to recast these treatment values as being $D \in \{\$4.25, \$5.05\}$. Thus, the difference-in-difference estimator becomes:

$$[Y(5.05)_{NJ\ 1} - Y(4.25)_{NJ\ 0}] - [Y(4.25)_{PA\ 1} - Y(4.25)_{PA\ 0}]$$

and the parallel trends assumption is that

$$E[Y(4.25)_{NJ\ 1}|D_i = 5.05] - E[Y(4.25)_{NJ\ 0}|D_i = 5.05] =$$
$$E[Y(4.25)_{PA\ 1}|D_i = 4.25] - E[Y(4.25)_{PA\ 0}|D_i = 4.25]$$

If we estimate this using differences in means, we get a result identical with a model in which treatment is binary, and if we used regression, the slope on τ would simply reflect this difference divided by $5.05 - 4.25$ to give the effect in terms of units of D. It does not matter that there is no unit without a minimum wage. Identification comes from a change in the level of the treatment.

However, if both units receive some, albeit different, treatment, the standard parallel trends assumption of the difference in difference requires an amendment to identify the effect. For instance, if Pennsylvania had changed its minimum wage to 4.75 in 1992, we could estimate this difference in difference:

$$[Y(5.05)_{NJ\ 1} - Y(4.25)_{NJ\ 0}] - [Y(4.75)_{PA\ 1} - Y(4.25)_{PA\ 0}]$$

[1] I am not here attempting to justify an ecological interpretation of these results. Though I should note: conceiving of this model as an ecological regression sidesteps the issues of continuous treatments and all units being exposed to treatment, while introducing other problems.

and get the effect of moving from a minimum wage of $4.75 to $5.05 if we assume that the change in employment New Jersey would have seen over time with a shift in a 4.75 minimum wage (as opposed to 5.05) is the same change that occurred in Pennsylvania.

$$E[Y(4.75)_{NJ\ 1}|D_{NJ} = 5.05] - E[Y(4.25)_{NJ\ 0}|D_{NJ} = 5.05] =$$

$$E[Y(4.75)_{PA\ 1}|D_{PA} = 4.75] - E[Y(4.25)_{PA\ 0}|D_{PA} = 4.75]$$

This assumption simply implies that the effect of going from 4.25 to 4.75 for New Jersey would have been the same as the effect for Pennsylvania and added to whatever shared trends affected both New Jersey and Pennsylvania. This is simply adding a second parallel trends assumption: the observed trend in the treated unit is the what the counterfactual trend the *untreated unit* would have had in the presence of treatment. That is to say: the effects of treatment are (on average) the same for treated and un-treated units. Under this second assumption, $\tau = ATE$.

Moveover, what is identified changes: when both units receive some treatment, we can only identify the effect of the net change in treatment between higher and lower treated units. In this example, we can only identify the effects of moving the minimum wage from 4.75 to 5.05; not from 4.25 to 5.05. In the context of this paper, we cannot find the effect of moving from 0 to 20 percent enlistment, because no counties had a rate of 0 enlistment.

This raises the next point: what do these assumptions mean and what is identified when the treatment is continuous?

A.2.2 Continuous Treatment

To understand what the parallel trends assumption means and what is identified, we need to expand the potential outcomes framework laid out above.

Rather than each unit in each time period having two potential outcomes (associated with treated or untreated), we can imagine that each unit i in time period t has a response schedule of potential outcomes: $Y(d)_{it}, d \in D$, where $Y(d)$ indicates the value Y would take if the continuous treatment D took on the value d.

We can represent this response schedule as $f_{it}(D)$: a function that tells us what the potential outcome of Y would be for unit i at time t for all possible values of D. This $f_{it}(D)$ could be non-linear and heterogeneous across i. The new effect we want to estimate is $\tau = E[f'_{i1}(D)]$: the average derivative of the response functions across all units i. As Angrist and Krueger (1999) point out, it is natural to view this average derivative of the response function as related to the conditional expectation function of Y as a function of D: $E[Y_i|D_i = d]$. It then makes it natural that we might estimate τ using least squares.

Just like the binary DID, we only observe $f_{i1}(D_i = d)$: the value of the response schedule for unit i for the actual value of treatment d that the unit takes. And because we do not observe the entire response function for unit i, $f'_{i1}(D)$ and $E[f'_{i1}(D)]$ are unknown.

Just as before, we can use a difference in differences to identify $E[f'_{i1}(D)]$, with some assumptions. The assumptions are easiest to see if we consider the average difference-in-differences in Republican voting between counties with an enlistment rate of d and counties with an enlistment rate of $d - \Delta$, were Δ is some arbitrary difference.

$$\{E[Y_{i1}(d)|D_i = d] - E[Y_{i0}(0)|D_i = d]\} - \{E[Y_{i1}(d-\Delta)|D_i = d-\Delta] - E[Y_{i0}(0)|D_i = d-\Delta]\}$$

This can be decomposed in terms of the average reponse function, as follows:

$$= E[f_{i1}(d) - f_{i1}(d-\Delta)|D_i = d] +$$

$$\{E[f_{i1}(d-\Delta) - f_{i0}(0)|D_i = d] - E[f_{i1}(d-\Delta) - f_{i0}(0)|D_i = d-\Delta]\}$$

This rearrangement gives us $E[f'_{i1}(d)]$ evaluated at d plus a bias term. This bias is the difference between the counterfactual trend units with enlistment rates of d would have had with enlistment rates of $d-\Delta$ and the actual trend units with enlistment rates of $d-\Delta$ had.

Angrist and Krueger (1999) note that assumption we have to make, in a regression context, is that conditional on covariates X, this bias term goes to zero. For the continuous difference-in-difference used in this paper, the assumption is that conditional on unit i fixed effects and state-year, dummies X_{it}:

$$E[f_{i1}(d-\Delta) - f_{i0}(0)|X_{it}, D_i = d] = E[f_{i1}(d-\Delta) - f_{i0}(0)|X_{it}, D_i = d-\Delta]\}$$

If this parallel trend assumption is correct: the unit fixed effects net out the baseline differences in the response functions: $f_{i0}(0)|D_i = d$ and $f_{i0}(0)|D_i = d-\Delta$, $f_{i1}(0)|D_i = d$ and $f_{i1}(0)|D_i = d-\Delta$. And the state-year fixed effects net out any changes in $f_{i1}(D) - f_{i0}(0)$ that are shared by units with different enlistment rates.

Note: this assumption still does not assume linearity in the response function $f_{i1}(D) - f_{i0}(0)$. And, this assumption does not require that the response functions are homogenous across units. It only requires that, within states, the response function of the change $f_{i1}(D) - f_{i0}(0)$ is, on average, the same across different rates of enlistment $D_i = d$. Thus, just as in the case where all units have some treatment, the key difference is that we must extend the parallel trends assumption: the average causal response functions of units with different levels of are parallel with each other.

When might this be violated?

1. These assumptions would be violented if units with different levels of D have, on average, different shifts in the average response function $f_{i1}(0) - f_{i0}(0)$ in the absence of treatment: pre-treatment trends are non-parallel. This is easy to test, as we can examine the pre-treatment period to assess this difference in average response function for several periods of time in which $D = 0$ for all units. This is true in the binary diff-in-diff

2. Additionally, these assumptions would be wrong if there were a confounder that is correlated with enlistment rates d that also determined Republican voteshare in counties in the post-war period (but not before). This is a kind of time-varying confounding,

and it would not be detected by assessing parallel trends pre-treatment. This, too, is a threat in a binary diff-in-diff.

3. Even if units have parallel trends over time in the absence of treatment, and there is no time-varying confounder, these assumption would be violated if units with different levels of treatment d have different causal response functions for $f_{i1}(D) - f_{i0}(0)|D_i = d, d \neq 0$ *on average*. This implies that the EFFECTS of treatment D — relative to unit baseline and shared trends over time —are heterogeneous. And that this heterogeneity is not independent of the levels of treatment. In order for this to induce bias, there must be both heterogeneous effects and confounding (the heterogenous effects are related to treatment assignment).

How does this parallel trend assumption differ from the binary case? The only key difference is that it assumes that, within states, the average causal response function of the change in Y from $t = 0$ to $t = 1$ is either: (i) identical for all counties i, in which case selection based on potential outcomes into d is not a problem; or (ii) the average causal response functions are heterogenous, but selection into d is independent of these heterogeneous effects.

A.3 Checking Parallel Trends

I first assess pre-war parallel trends by estimating the following model, using the same data as Table 1.

$$\text{Republican Voteshare}_{\text{ie}} = \alpha_i + \alpha_e + \sum_{y=1854;\neq 1860}^{1920} \beta_y \text{Enlistment Rate}_i * \text{Year}_y + \epsilon_i + \epsilon_y \quad (4)$$

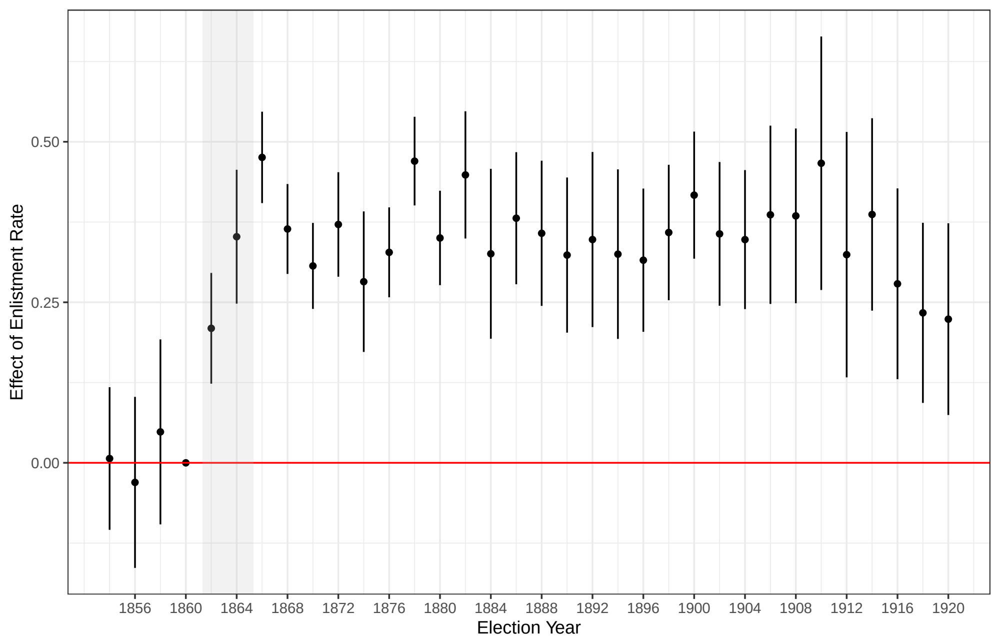
This estimates a separate slope on enlistment rate, for the difference between county Republican voteshare in each federal election between 1854 and 1920 and the 1860 federal elections. Figure A1, shows the results of these estimates. Prior to the Civil War, the over-time change in the average causal response functions of counties with different levels of enlistment is not different from 0.

One might be concerned that this lacks credibility: do we really believe that counties in, say the top and bottom quartiles, of enlistment share parallel trends. In the body of the paper, I check this in Figure 1. Even when we split counties into the quartiles of enlistment in their state (reflecting the same with-in state parallel trends I specify in the model), the trends are visibly parallel across quartiles. Statistical tests of these differences confirm that the change in Republican voteshare, relative to 1860, in counties with different quartiles of enlistment are not different from 0.

A.4 Addressing Time-Varying Confounding

Another concern might be that, even if there are parallel trends for counties with different levels of enlistment before the war, there may be some other attribute of counties with

Figure A1: Effect of veterans on Republican Vote-share in federal elections for each year between 1854 and 1920



This figure plots the year-specific effect of enlistment rates on Republican vote-share for federal elections with 1860 as the reference year. The model includes county and state-election fixed effects. Standard errors are clustered by county and year. Bars show 95 percent confidence intervals.

high enlistment that affected Republican voteshare once the war started. To address this possibility, I first explore the predictors of enlistment in Figure A2.

Figure A2: Predictors of County Enlistment Rates (Standardized Coefficients)

This figure plots the standardized coefficients from bivariates regressions of county enlistment rates on 32 different covariates, with state fixed effects. Bars show 95 percent confidence intervals.

As discussed in the paper, enlistment rates are significantly correlated with several economic, demographic, and political attributes of counties in 1860. These could shape how counties respond to the war. To address this possibility, I estimate a new model, including an interaction between each of these pre-war attributes and the post-war period (Table A1) or election year (Figure A3).

$$\text{Republican voteshare}_{ie} = \alpha_i + \alpha_e + \beta \text{Enlistment Rate}_i \cdot \text{Civil War}_e + \mathbf{X_i} \cdot \text{Civil War}_e + \epsilon_i + \epsilon_y \quad (5)$$

Table A1: Estimates of Difference-in-Difference Effect of Enlistment, including Covariate/-Time Interactions

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Enlist % · Postbellum	0.174***	0.164***	0.181***
	(0.035)	(0.046)	(0.039)
GOP no contest	included	dropped	dummy
County FE	X	X	X
State-Election FE	X	X	X
Covars · Postbellum	X	X	X
Observations	6,930	5,019	6,930
Note:	*p<0.05; **p<0.01; ***p<0.001		

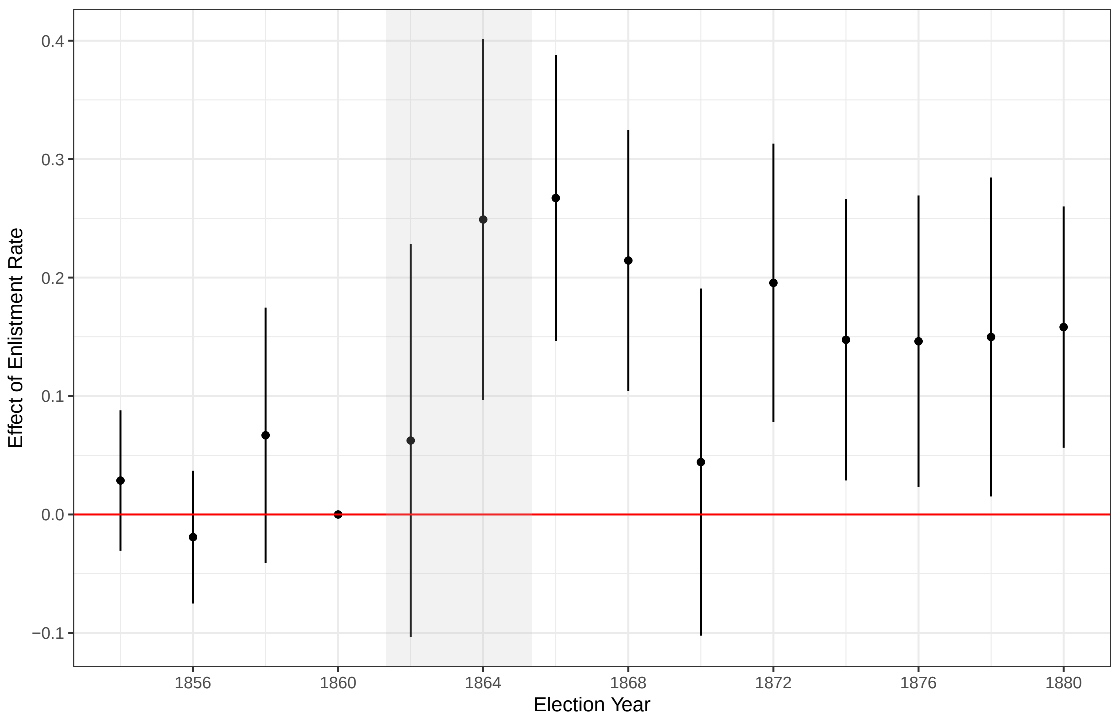
Data from Congressional and Presidential elections across 330 counties between 1854 and 1880. Standard errors clustered by county and election year. Counties with election cycles in which the GOP does not contest an election cycle either treated as a 0, the election is marked with a dummy, or the all observations for that county are dropped. The post-war indicator is interacted with 32 demographic, economic, and political covariates. Counties with missing data or extreme outliers in the covariates are dropped.

The results are substantively unchanged, even though we allow for possible time-varying confounding.

A.5 Addressing Heterogeneous Effects

Another possible source of trouble would be if the effects of enlistment varied across counties with different levels of enlistment. One way to address this problem would be to directly estimate heterogeneous effects in the enlistment DD model for counties that were otherwise similar to each other on baseline characteristics. One way to do this is to use *generalized random forests* (Athey, Tibshirani and Wager 2018). Athey, Tibshirani and Wager (2018) 's causal forests estimate the heterogeneous effects treatment W for case i. If X_i is a set of

Figure A3: Effect of veterans on Republican Vote-share in federal elections for each year between 1854 and 1920, with Covariate-Time interactions



This figure plots the year-specific effect of enlistment rates on Republican vote-share for federal elections with 1860 as the reference year. The model includes county and state-election fixed effects as well as interactions between baseline covariates and year. Standard errors are clustered by county and year. Bars show 95 percent confidence intervals.

covariates and W_i is a continuous or binary treatment, generalized random forests estimate $\theta(x)$, or the effect of W on Y for a set of values of X. This is done by generating weights that define the closeness of other observations to i within the space defined by X. These weights for the closeness of case j to case i are $\alpha_j(x_i)$. The sum of $\alpha_j(x_i)$ across all j is 1. Whereas many methods generate these weights using kernels which is subject to the curse of dimensionality, generalized random forests use random forests (repeated iterations of regression trees) to assign weights. In this case, θ is the locally weighted linear partial effect of W on Y.

$$\hat{\theta}(x_i) = \frac{\sum_{j=1}^{n} \alpha_j(x_i)(W_j - (\sum \alpha_j(x_i)W_j))(Y_j - (\sum \alpha_j(x_i)Y_j))}{\sum_{j=1}^{n} \alpha_j(x_i)(W_j - (\sum \alpha_j(x_i)W_j))^2}$$

These individual heterogeneous effects are then combined into average partial effect, with asymptotically unbiased standard errors. Using this method, I estimate the following difference-in-difference model for each election year y between 1854 and 1880. c indicates that I have centered the variable around the state mean, to impose the state-year fixed effects.

$$\text{GOP Voteshare}_{yc} - \text{GOP Voteshare}_{1860c} = \theta(x)\text{Enlistment Rate}_{ic} + \epsilon_i \tag{6}$$

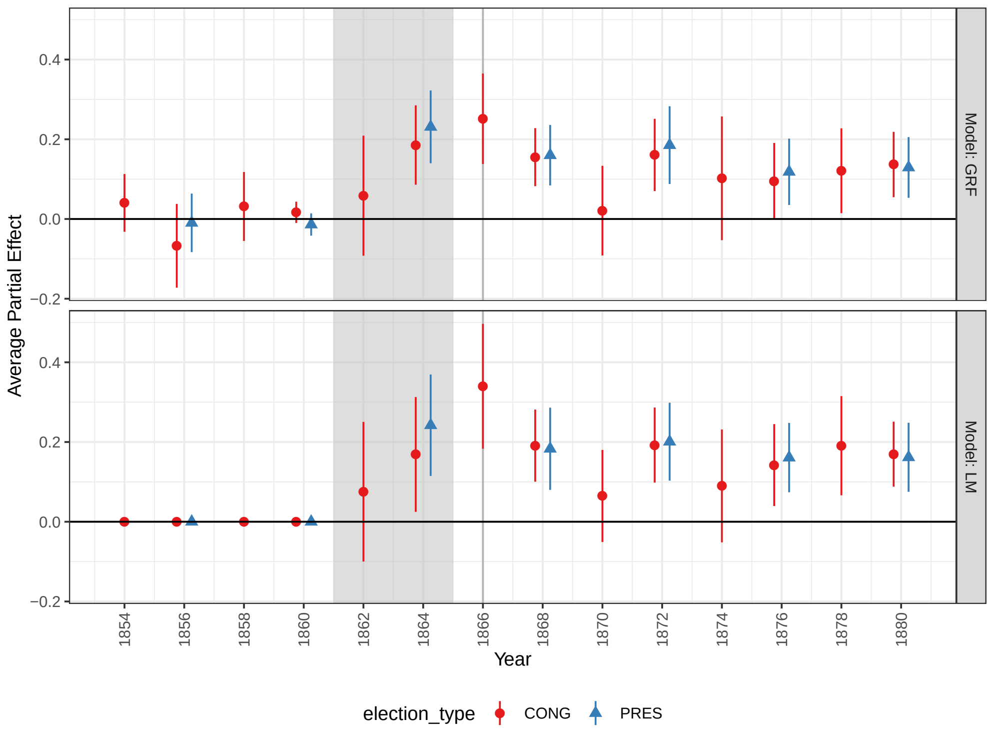
The variables used in weighting case similarity for computing heterogeneous effects are: (a) The performance of the Republican party in every federal election (Congressional and Presidential) from 1854 to 1860. For any county in which a given party did not compete in an election, I set their vote-share as 0 and add a dummy indicating the party did not compete in this county-election or that the data is missing. (b) All non-political covariates shown above. (c) Indicators for the state in which the county is located. The key assumption here is that the effects of enlistment are effectively constant or independent of enlistment for cases that are similar in covariates X.

Figure A4 shows that the results obtained using generalized random forests are substantively the same, even though we explicitly and flexibly model heterogeneous effects of enlistment.

A.6 Robustness

The effects are unchanged when using surviving veterans as a fraction of 1860 military aged males, instead of all enlistments: Table A2.

Figure A4: Average Partial Effect of Enlistment on GOP Voteshare: Diff-in-Diff



This reports the generalized random forests estimates of the average partial effect of enlistment for a difference-in-difference between Republican voteshare in each federal election between 1854 and 1880 and 1860. Standard errors are robust. Bars show 95 percent confidence intervals.

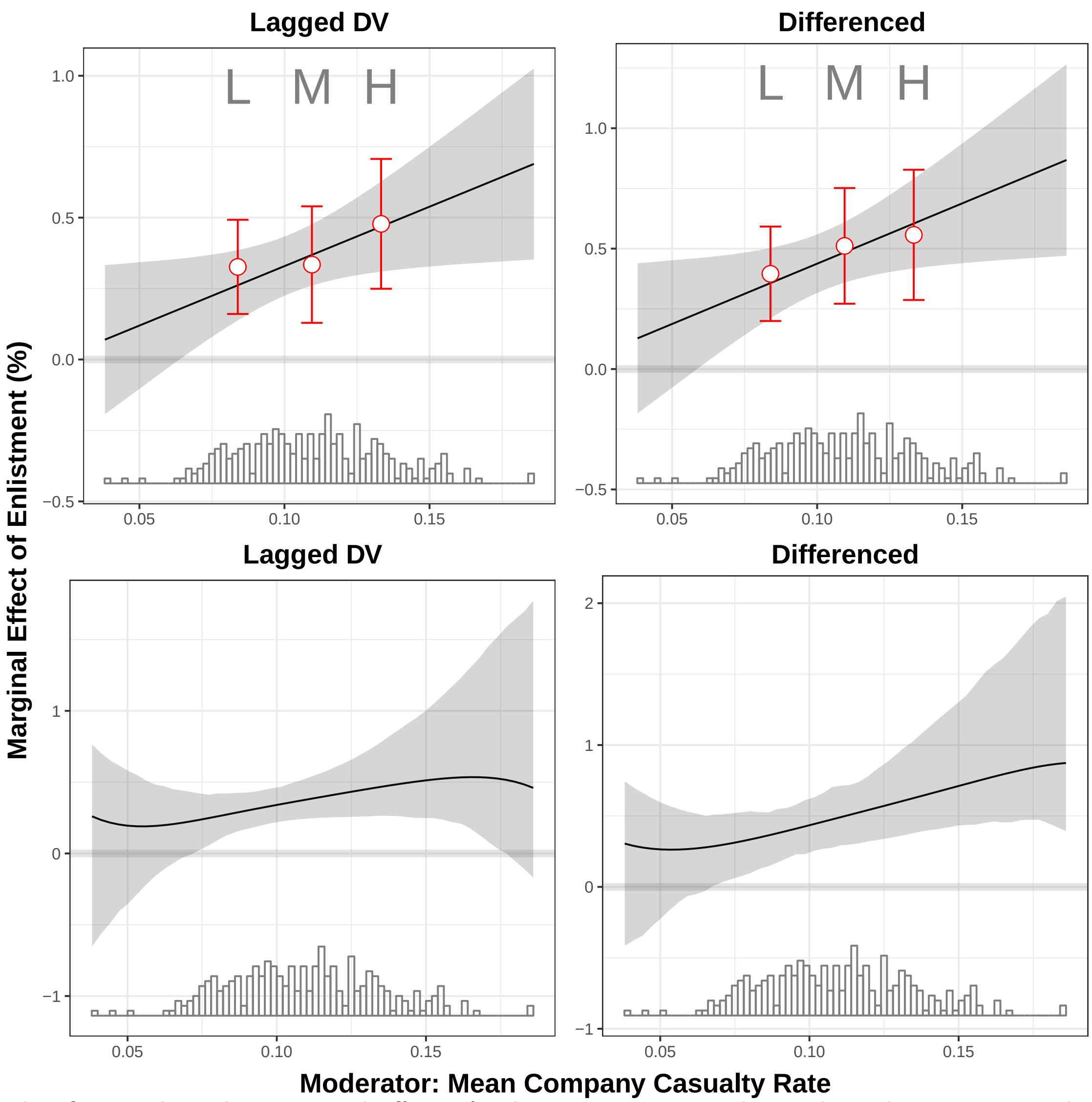
Table A2: Difference-in-Difference Effect of Enlistment Rates on Republican Voteshare: Survivors Only

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Survived % · Postbellum	0.414***	0.323***	0.322***
	(0.108)	(0.078)	(0.064)
GOP no contest	included	dropped	dummy
County FE	X	X	X
State-Election FE	X	X	X
Observations	6,153	4,473	6,153

Note: *p<0.05; **p<0.01; ***p<0.001

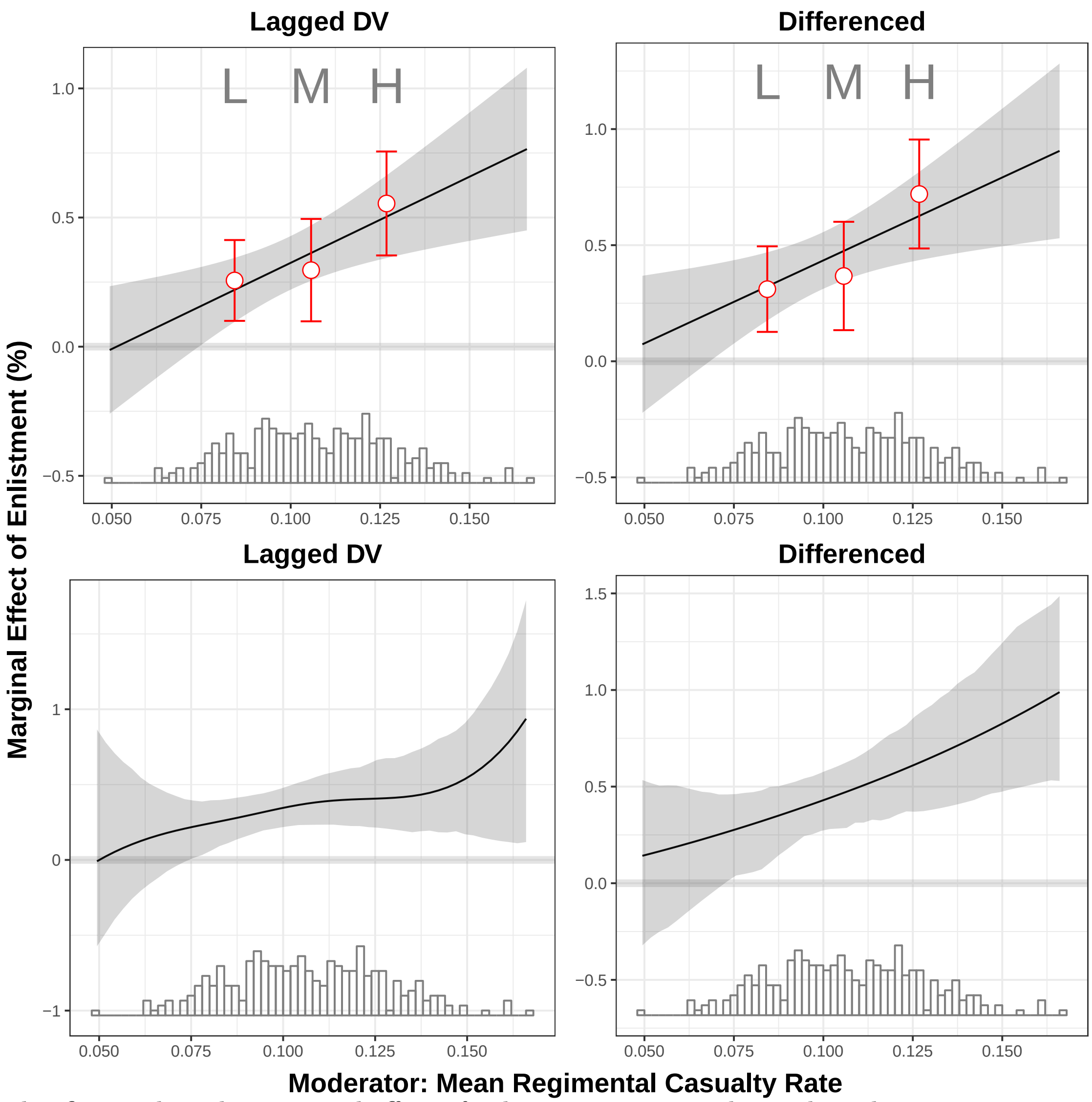
Data from Congressional and Presidential elections across 293 counties between 1854 and 1880. Standard errors clustered by county and election year. Counties with election cycles in which the GOP does not contest an election cycle either treated as a 0, the election is marked with a dummy, or the all observations for that county are dropped.

Figure A5: Marginal Effect of Enlistment Rates on Republican Voteshare Conditional on Company Casualty Rate)



This figure plots the marginal effect of enlistment rates conditional on the mean casualty rate for men in companies with county veterans on the difference between Republican Voteshare in 1864–1868 and 1854–1860.

Figure A6: Marginal Effect of Enlistment Rates on Republican Voteshare Conditional on Regimental Casualty Rate



This figure plots the marginal effect of enlistment rates conditional on the mean regiment-level casualty rate of county veterans on the difference between Republican Voteshare in 1864–1868 and 1854–1860.

B Company Casualties in Infantry Regiments: A Natural Experiment

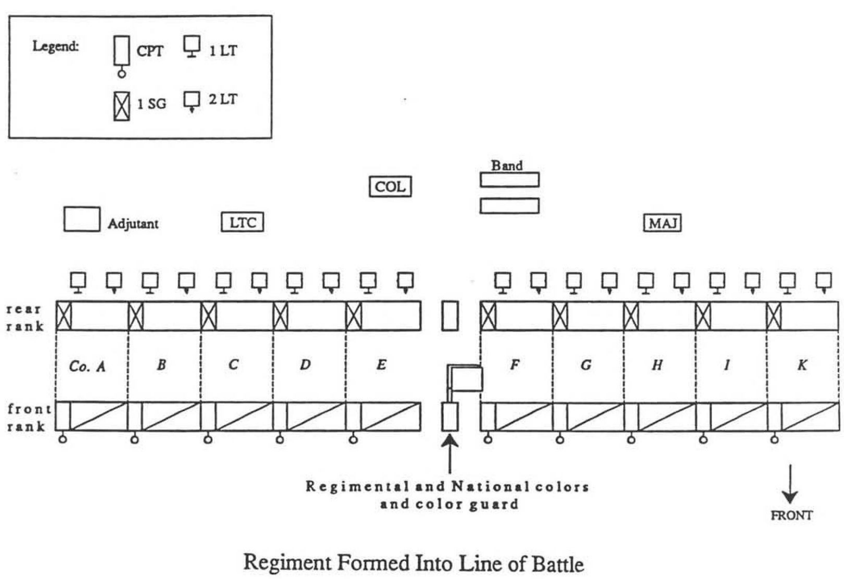
B.1 Company Exposure to Combat Fatalities

The vast majority of men serving in the Union Army served in infantry regiments. The basic unit in which infantrymen were mobilized, maneuvered, and went into combat was the regiment. While the Regular Army had independent companies of sharpshooters, volunteer regiments, which made up most of the army, operated as units. Regiments were further subdivided into ten companies of equal size, each commanded by a captain. Men in the same company were mustered in together and then trained, lived, worked, and fought directly alongside each other.

Men may have chosen when to enter military service, the branch of service, the term of their service, and even the specific regiment they joined. And Army commanders might decide based on regimental performance or perceived reliability to give certain regiments garrison duty or combat roles, or in battle, give more or less important or difficult objectives.

But, because regiments were the main unit around which military tactics of the time were designed, men in the same regiment went the same places, at the same times, and were located in the same place on the battlefield. In battle, infantry regiments typically formed up in a line of men, two deep, horizontally arranged by company, approximately 140 yards across (see Figure B1) (Hess 2015). In the chaos and smoke of battle, which companies in that line received more casualties was effectively arbitrary.

Figure B1: Regimental Battle Formation in the Civil War



This figure shows the standard battle formation of both Union and Confederate infantry regiments during the American Civil War. Ballard, Ted, and Billy Arthur, *Chancellorsville Staff Ride: Briefing Book*. Washington, DC: United States Army Center of Military History, 2002. Public Domain Image from Wikipedia

Because this logic applies only to infantry regiments, I exclude artillery and cavalry regiments from my analysis. This is because, artillery regiments deployed batteries to different places on the battlefield, and cavalry regiments, particularly early in the war, operated with detached companies.

Moreover, because the staff/headquarters company and regimental band were deployed behind the ten main companies, I exclude these from analysis. Similarly, regimental records often include "unassigned" troops, without a known company. I exclude these from analysis since we do not know which company they served with.

B.2 A Justification of As-If Random

Following Dunning (2012), I consider the information, incentives, and capabilities of soldiers, officers, and the enemy that might lead soldiers to experience more or fewer company casualties as a function of potential outcomes of partisanship. The evidence comes almost exclusively from (Hess 2015), which is the only contemporary academic historical work devoted to Civil War infantry tactics.

B.2.1 Soldiers

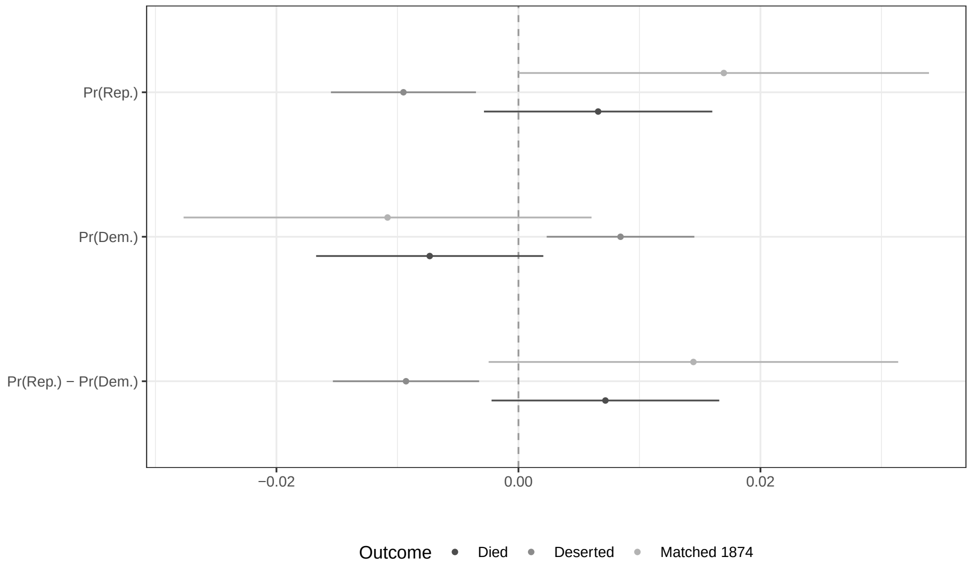
Information While soldiers could choose which company they joined when regiments were formed (Costa and Kahn 2008, 52–57), they could not have known which companies were more likely to receive casualties. (i) All companies stood together in a line across. (ii) While drill manuals specified which position companies should take in that line, soldiers entered the war unfamiliar with military drill instruction, and often the guidelines were ignored. This was true, because Army manuals called for companies to be arranged by the seniority of officers. This was irrelevant in almost all volunteer regiments, since all officers joined at the same time.

On the other hand, once in a regiment, soldiers could have known that tasks such as skirmishing (fighting in loose formation in advance of the main line) or serving in the color guard (protecting the flag bearers of the regiment) or color company (immediately to the right of the color guard) might be more dangerous. And soldiers may have known that their regiment was soon to enter battle, as well as whether some battles might be more dangerous.

Incentives In general, soldiers may have incentives to desert or use caution to avoid death. Conversely, soldiers may have wanted to seek glory or fight for the cause, leading them to take greater risks. And it is plausible that these incentives varied by partisanship. Republicans may have been more supportive of the cause and thus more motivated to take greater personal risks and fight harder, while Democrats may have been more likely to want to simply make it through the war alive. This is borne out in a raw comparison of predicted Democrats versus predicted Republicans: predicted Republicans were more likely to die, while predicted Democrats were more likely to desert (Figure B2.

Capacities Despite some information, soldiers had limited capacity to select themselves into treatment.

Figure B2: Differential Attrition by Predict Partisanship (Bivariate))



This figure shows the change in attrition, death, or desertion (related to attrition) associated with a 2 SD change in the predicted probability of Republican or Democratic partisanship in a bivariate regression model.

Before combat (i) Soldiers could work to be chosen for the color guard, which protected the regimental colors. Because the colors were essential for guiding the line of soldiers into battle and were source of morale, the enemy frequently targeted the colors. However, the members of the color guard were detached from their companies and were composed of corporals and sergeants. Excluding men of these ranks from analysis would address this form of selection.

(ii) Soldiers could not *choose* to join a company in advance based on its proximity to the colors. These companies might receive more casualties (though see below). These positions were often dictated by the company's ability to drill, which Hess notes was often a function of how skilled and motivated the captain was to drill his men. The companies best at drill would up on the flanks or in the center, near the color guard. This was to ensure that the regiment kept good formation in battle. While soldiers might attempt to shirk at drill to avoid being the color company, it seems unlikely that this would affect mortality. First, good performance at drill was also understood to be essential to for the regiment to survive under fire; even for reasons of survival, soldiers had reason to be competent at drill. Second, it is unlikely that individuals could conspire to get an entire company to be worse at drill. Third, even if they could get better at drill, it is implausible that this could be done with such precision that it would lead them to be placed on the flanks (potentially less exposure) vs. the center (more exposure). Moreover, the company directly to the left of the color guard was also likely in more danger. Yet this position was not dictated by drill capacity.

(iii) Finally, soldiers could choose to desert rather than fight. This is a more serious problem of selection. If we simply were to use the actual "Exit" date from a company, soldiers who either deserted or died would determine their extent of exposure to combat casualties. And this could induce selection bias. But, I address this problem by constructing an intent-to-treat exposure to combat casualties.

During Combat Soldiers had limited capacities to alter their exposure to death during combat. Linear combat (fighting in lines) provided few opportunities for individuals or companies to take independent action. As discussed below, the whole point of having men fight in lines was to increase the effectiveness of massed rifle fire by largely unskilled marksmen under conditions of low visibility and limited means of command and control. Armies positioned officers and non-commissioned officers to keep men from lagging. And they practiced drill in order to keep their lines in good order. While men could elect not to fire, or fail in their aim due to fear, there were few opportunities to run or hide. In Hess's review of combat reports, he records very few mentions of companies operating independently at all. As discussed below, these instances were the result of battlefield idiosyncrasies.

Similarly, there were few opportunities for soldiers and companies to fight *harder*. While there are many accounts of units fighting to their last, which could induce selection bias, these accounts detail regiments, because companies simply did not operate independently. At the individual level, it could be that Republican soldiers took more personal risks. This appears to be the case, but even if this was true: (1) we don't include individuals in their own treatment variable and (2) if so, this could lead to attrition that reduces the number of Republicans observed after the war, because the most Republican men in a company might end up dying, increasing the "treatment'' for less Republican men who, because they survived, were located after the war. This would bias my estimates downward.

B.2.2 Commanding Officers

Information Officers likely knew, to some extent, the partisanship of their companies. They might infer this from where units were raised, the captains of those units, or the ethnic or occupational composition of the regiment. Or, more simply, given the vigorous political discourse within the Union Army, through conversation and debate (McPherson 1997). Based on this, officers may have been more suspicious of units that were less reliable.

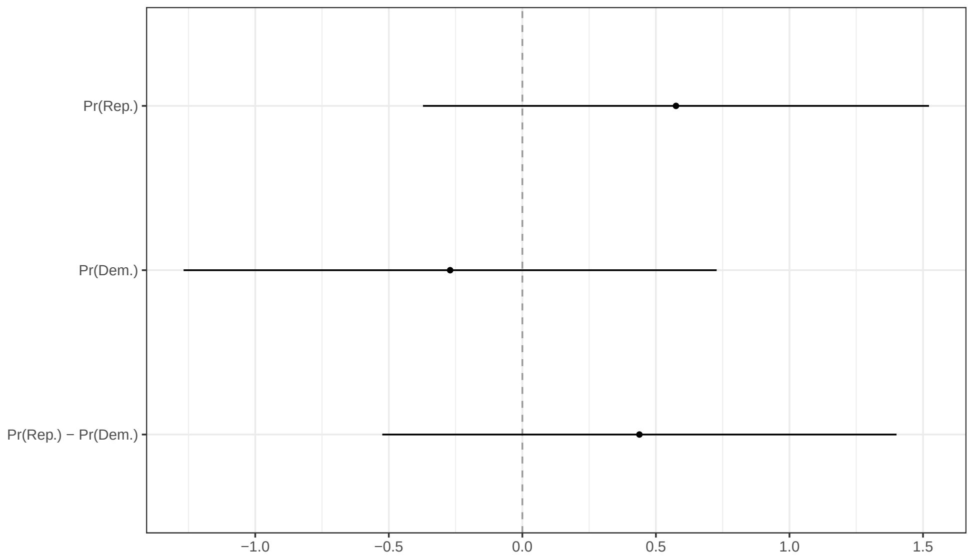
Incentives There were three major considerations in how officers deployed companies in combat. **Keeping Unit Formation:** Army manuals suggested putting veteran companies on the ends and near the colors, but in an war where most regiments were of volunteers raised at the same time, this advice was rarely followed. Instead, regimental commanders debated whether to put the most well-drilled marchers on the ends of the line and in the center to help keep the regiment's line from falling apart.

Another consideration was **terrain**. As a unit marched into combat, companies could become detached to the side or the rear of the regiment if they came upon obstacles. The affected companies moved to the rear of the rest of the regiment and retook their place in line when the obstacle was passed.

Finally, regimental tactics gave companies distinct orders based on **battlefield exigencies**. While most of the time, companies moved together in a line, deviations from this reflected emergencies. Sometimes companies were deployed as skirmishers, in loose formation, in front of a regiment. While this became more common over time, Hess notes that it was more common to use entire regiments as skirmishers for a brigade. Thus, it is unclear whether there was selection into skirmishing duty by partisanship. And it is also unclear whether skirmishers were in more or less danger, given that they were more spread out and encouraged to make use of cover. In other times, the left or right wings of regiments were used separately. This was done if the regiment were accidentally split in battle or when facing a flanking maneuver from the enemy. Again, it is hard to imagine how partisanship of companies would be used to inform how these decisions were made in the heat of battle.

If there were some selection, we'd expect companies with different baseline partisanship to have different combat casualties. In Figure B3, I report the within-regiment relationship between company casualty rates and average partisanship of men in those companies. There are no significant differences in casualty rates by company average partisanship, and the estimates are substantively small: within regiments, a 2 SD increase in mean Republican partisanship is associated with a 0.25 or smaller SD difference in casualties.

Figure B3: Differential Attrition by Predict Partisanship (Bivariate))



This figure the relationship between a 2 SD change in mean company predicted partisanship and the number of combat casualties in a company, conditioning on regimental fixed effects and weighting companies by the number of men use to calculate mean company partisanship.

Capacity Other than choosing which companies would occupy the flanks and the center, officers could not position companies in line based on their partisanship. Fighting was done as a regiment. And when companies were detached, the reasons had to do with unexpected circumstances generated by chaos on the battlefield. One reason regiments fought together in cohesive lines was that it was difficult on a smoky battlefield, with rifle and artillery fire, to give specific directives to different companies. This is why regiments practiced drill together, in order to accomplish carefully choreographed maneuvers to be used in combat in order to fight more effectively and save their own lives in an attack.

When Hess discusses case studies of tactical errors that led to greater casualties, these occurred due to errors made by brigade or division commanders sending regiments into the wrong place, or when whole regiments found themselves in the wrong place on the battlefield.

B.2.3 Enemy

Finally, decisions made by the enemy about how to fire on regiments might create non-random company casualties.

Information Confederate units would be able to see color guards, since these were intended to be visible on the battlefield. It is also possible that they could distinguish officers.

Incentives Battle reports indicate that both armies sought to kill officers and color guards to both demoralize units and eliminate leadership to reduce the fighting effectiveness of the enemy.

Capacity While there was capacity, despite smoke, for Confederate units to see and target color bearers, and perhaps officers, this is mitigated by a few other considerations. First, officers were dispersed throughout the regiment (Captains and lieutenants were attached to each company). Second, while color bearers were targeted more, it is also the case that regiments were trained to fire against the enemy lines, not specifically at individuals. The primary reason that both armies kept using these linear tactics, despite the advent of the rifled musket, was that in order to maximize firepower, they wanted to mass rifle fire against enemy units. This was to increase the efficacy of rifle fire. Men in both armies lacked sufficient training to shoot accurately, the armies could not afford to expend munitions training them to shoot more accurately, and without smokeless powder, battlefields had limited visibility. This limited the effectiveness of individual firearms and prioritized large volumes of fire from large groups of men simultaneously.

B.2.4 Color Guards

Based on the preceding the discussion, the biggest potential source of selection effects in company-level casualties is related to color guards and color companies. Color guards, selected from corporals and sergeants of other companies, were at the center of the line. These men attracted more fire from the enemy. To their right, was the color company, often chosen for its ability to perform marching and drill well. Color guards may have been more likely to be Republican, given that it was a dangerous position. Color companies may have been more likely to be Republican, since good marching order might have been related to enthusiasm for the war.

It is impossible to reconstruct the membership of the color guard or color companies. The composition of the color guard changed and is not usually recorded on company rosters in the data available. Companies chosen to be color companies changed in regiments throughout the war: regiments often held drill competitions between companies to choose which were best.

While we cannot easily test whether color companies or selection into the color guard led to bias, a couple points suggest this is less of a problem.

(1) If members of the color guard were more likely to be Republican, then the remaining men in their companies would be exposed to more company deaths, if color guard members

were more likely to die. But this would produce selection where the remainder of the company was, on average, less likely to be Republican, more likely to have more combat death exposure, and more likely to be observed after the war. This differential attrition would bias the effects I estimate downward.

(2) Companies with better marching skills might be chosen to be the color company. But better marching companies were also chosen to be on the left and right flank, far away from the color guard. And the company to the left of the color guard was not chosen based on its skills. Thus, even if companies that were more Republican on average were better motivated at drill, they could not easily determine whether they ended up in the center or on the flanks. And companies with poorer marching skills might still end up near the color guard. Moreover, the survival of a regiment in the face of the enemy depended on effective drill. Being able to complete complex choreographed maneuvers under fire often made the difference between holding off an enemy attack or being flanked and suffering great losses. Thus, efficacy in drill may have been motivated by enthusiaism for the war or enthusiasm to make it home alive.

For these reasons, I consider it implausible that soldiers could have selected into different levels of company casualties based on partisanship.

B.3 Data

In the following sections, I describe the data used in the analysis of this natural experiment and how it was collected.

B.3.1 Post-war Partisanship

This analysis is only possible because there are data on post-war partisanship for individuals who served in the war. I draw on the *People's Guides* of nine Indiana counties, published in 1874.[2] These were published by Cline and McHaffie. These guides report the history of the county and township, and include a directory of people residing there. An example of one page is located in Figure B4. While not fully exhaustive, they include a large fraction of people in these counties. For each person, their name, occupation, location of residence, birth place and year, date of settlement in the county, religious affiliation, and political partisanship is listed. A sample from these guides was previously collected and analyzed by (**?**DeCanio and Smidt 2013). Similar guides exist for 6 counties in Illinois. These are harder to digitize and contain less data on which to match individuals (no birth year), so I did not prioritize this data collection.

To collect the names, birth years, birth places, and partisanship of individuals listed in these guides, I did the following. (1) I downloaded all pages of the guides from archive.org. (2) I converted each page into a gray-scale image. (3) I passed each gray-scale image to Google Cloud Vision to detect the text on each page. (4) Using custom R and Python

[2]Bartholomew, Boone, Hamilton, Henry, Hendricks, Johnson, Montgomery, Morgan, and Vermillion Counties

Figure B4: Example Page from 1874 People's Guide

Apel, Chas.; carpenter and builder; Columbus. Born in Prussia 1842; settled in B. C. 1865. Rep. Lutheran.

Aikens, David; farmer; 2 m s Columbus. Born in Va. 1835; settled in B. C. 1860. Dem. Protestant.

Aikens, James; at leisure; 2 m s Columbus. Born in Va. 1828; settled in B. C. 1867. Rep. Protestant.

ABBETT, W. A.; justice of the peace and farmer; 3 m s w Columbus. Born in B. C. 1832. Indpt. Methodist.

Abbett, O. P.; farmer; 2 m s w Columbus. Born in B. C. 1834. Indpt. Methodist.

Abbett, Henry; cooper and farmer; 2 m w Columbus. Born in B. C. 1832. Dem. Christian Union.

Abbett, Washington; farmer; 2½ m w Columbus. Born in Ky. 1827.

Arwin, John S.; physician and surgeon; Columbus. Born in Tenn. 1824; settled in B. C. 1868. Dem. Protestant.

Anderson, S. F.; 4 m n Columbus. Born in B. C. 1833. Rep. United Brethren.

ARNHOLT, WM.; farmer; 4 m s e Columbus. Born in Germany 1846; settled in B. C. 1868. Dem. Lutheran.

Arnold, Thomas; farmer; 3½ m n w Columbus. Dem. Chris.

Anthony, Joseph; farmer; 3 m n Columbus. Born in Tenn. 1830; settled in B. C. 1849. Rep. Protestant.

Armet, Charles; farmer; 2½ m n w Columbus.

Akins, C. E.; collector of Singer Sewing Manufacturing Co.; Columbus. Born in Ohio 1846; settled in B. C. 1873. Rep. Christian.

Adams, D. W.; druggist; Columbus.

scripts, I combined letters into words, words into rows, and rows into biographical entries using the x-y coordinates of text on the page.

Then, I used regular expressions to extract the names, birth places, birth years, years of settlement, and partisanship of each individual. I then had workers on Amazon Turk correct errors in machine transcription for each biographical entry. While some minor errors likely remain, I have key biographical details for 27169 people across these 9 counties.

B.3.2 Union Soldiers from Nine Indiana Counties

To find the effects of wartime experiences on post-war partisanship, I needed a baseline sample of soldiers to track over time. While some soldiers undoubtedly left their county of residence, a comparison of soldiers from the same county seems more plausible than to include soldiers who later moved into these counties. Moreover, by considering, say, all Union Soldiers or all Union Soldiers from Indiana, I was increasing the risk of making incorrect matches.

Based on records from the Indiana Adjutant General, 21056 men enlisted in the Union Army from these nine counties. However, a large fraction of Indiana soldiers in the ACWRD lack places of residence. To link individual soldiers to these nine counties I did the following: (1) I linked each listed place of residence given by Indiana soldiers in the ACWRD to a list of all place names (county, township, city/village, post-office) reported in the 1860 and 1870 Census in Indiana. In some cases, the same place name appears in multiple counties. And for some soldiers, no place of residence is listed. (2) For soldiers whose residence uniquely matches one of the 9 counties, they were linked to that county. (3) If soldiers' places of residence matched to multiple counties in that list of 9, they were linked (if possible) to the one county to which the majority of men in his company were uniquely matched. Otherwise, they were considered as possible residents of multiple counties (4) For soldiers with no residence listed, they were assigned to one of the 9 counties if the majority of the men in their company came from that company.

Table B.3.2 shows the number of enlistees recorded in each county by the Adjutant General of Indiana and the number of individual soldiers I match to each county (weighted by 1 over the number of counties to which they match).

For these set of Union soldiers, I have the following variables: rank at enlistment, term of enlistment, type of regiment (infantry, artillery, cavalry), date of enlistment, regiment of service (and muster in/muster out dates), company of service, how this person entered the regiment (volunteer, drafted, substitute), how they left the regiment, and exposure to casualties as described in the paper.

B.3.3 Linking Soldiers to 1874 People's Guides

I link Union Soldiers to the 1874 People's Guides using a deterministic matching procedures. While Enamorado, Fifield and Imai (2019) argues that there are gains to be made by using an automatic and probabilistic method, applying the fastLink algorithm to this data yielded poor results. The primary reason for this are the few characteristics on which to match and a substantial number of cases in which first names.

Table B1: Number of Union Army Servicemen by County

County	**Soldiers (Adjutant General)**	**Soldiers (ACWRD)**
Bartholomew	2813	2490
Boone	2442	3196
Hamilton	2272	2756
Hendricks	2416	2747
Henry	2549	2633
Johnson	2033	1500
Montgomery	2850	2903
Morgan	2114	2046
Vermillion	1567	1029

My deterministic matching procedure uses the following attributes: county of residence, first name, last name, and birth year.

For each soldier who *survived the war*, I implement these steps to identify a set of matches: 1. I identify a set of people in the 1874 People's Guides who (a) reside in the same county the soldier resided in at enlistment and (b) are reported as moving to that county before 1866 (since soldiers may have returned home as late as that). 2. I clean first names by linking reported names, which may be misspelled or abbreviated to full names, using a crosswalk created by Abramitzky et al. (2019). 3. I then generate potential matches under the following conditions:

- If the first name and last name match exactly
- If first name matches exactly and lastnames match exactly using the metaphone sound code or have a distance of less than 0.1 on a Jaro-Winkler score.
- First names match exactly on metaphone sound code or have a Jaro-Winkler distance is less than 0.1 and last name matches exactly
- For soldiers with only a first initial listed, I consider as matches those with the same first initial and an exact match on the last name.

Within these, I consider as the best matches those that are closer on all name matching metrics and have the closest year of birth.

This procedure generates matches for 4888 Union soldiers. On average, each soldier is matched to 1.35 people post-war, with a max of 14. When restricting to best matches, the average number of matches is 1.06, with a max of 12. In cases where soldiers have more than one match, I weight them in subsequent analyses with $1/m$ where m is the number of matches.

B.3.4 Linking Soldiers to the 1860 Census

In addition to linking soldiers to the 1874 People's Guides, I also link them to the 1860 US Census. I do this for three reasons. (1) Soldiers may vary in how easily they can be located, in general, in historical records. This may have to do with how consistent they were in providing biographical details, and it may reflect whether soldiers are in fact from the county I match them to using the procedure used above. Linking soldiers to the 1860 US Census provides confirmation that soldiers were in fact from the county in question and provide a pre-treatment measure of "findability".

(2) In addition to military enlistment details, this provides additional demographic characteristics on which to test balance and use as conditioning variables. (3) Most importantly, linking to the US Census permits me to generate a measure of predicted partisanship for these soldiers, even if they are not located in the 1874 People's Guides. This permits conditioning on predicted partisanship and checking for differential attrition by baseline partisanship.

I link soldiers using the FastLink algorithm, blocking on county of residence (Enamorado, Fifield and Im 2019). I generate matches between soldiers and people in the Census using on cleaned first names, last name, birth year, and the metaphone sound codes of the first and last name. I kept matches in which the posterior probability of a correct match exceeded 0.8.

In using this procedure, I match 12099 soldiers to the 1860 Census. The mean number of matches per soldier is 1.39, and the maximum number is 16. When using Census data for soldiers, I take an average of the data for each Census match, weighted by the match probability given by fastLink.

B.4 Predicting Partisanship

In order to examine imbalance on partisanship by treatment and to investigate the possibility of differential attrition by partisanship, I need to have a pre-treatment measure of partisanship for soldiers. While it is not possible to find individual partisan affiliation for soldiers in this period of time, the next best option, if imperfect, is to generate predicted partisanship based on demographic attributes of soldiers.

To do this, I use demographic data available in the 1874 to predict partisanship. Because I need to generate these predictions for soldiers who don't appear in the 1874 People's Guides and these predictions shouldn't suffer from post-treatment bias, the demographic details need to exist in both the 1874 guides and the 1860 Census. I use names, birth years, and places of birth.

I trained a machine learning algorithm using these variables to predict partisanship in 1874. I then use this model to predict the partisanship of soldiers in 1860. In what follows, I describe this process in more detail and provide validation that the predict partisanship measure meaningfully captures variation in partisanship.

B.4.1 Training Data

To generate predictions of partisanship, I use all people listed in the 1874 People's Guides who were *not matched to any soldiers*. I then created the following features corresponding to these people:

- The first name of the person as listed in the guide/census
- The last name of the person as listed in the guide/census

- The cleaned first name of the person, using the name variant/abbreviation crosswalk created by (Abramitzky et al. 2019).
- The metaphone sound encoding of the first name as listed in the guide/census
- The metaphone sound encoding o the last name as listed in the guide/census
- The birth year listed in the guide/census
- The place of birth listed in the guide/census. Here, I used state of birth in the US or country of birth (collapsing many regions in Germany/Ireland to the country).
- Partisanship, collapsed to: Republican, Democrat, Other, or None. The vast majority of people are in Republican or Democrat. The most common “other’’ is the Grange.

Note that the county of residence or township and occupation, although also in the census, are not included. These may be affected by war-time experience, and so I do not include them as features to predict partisanship.

B.4.2 Machine Learning Classifier

I then used the `fastText` algorithm to classify individuals' partisanship. This algorithm is used for text classification and, rather than using a bag of words to classify texts, represents words and subsets of characters in that word in a lower dimensional space, and then uses these word representations to classify documents. I used this approach, because name spellings are too numerous to include as binary-encoded features and I would be predicting with new datasets that might even contain new new variants. FastText permits making predictions even with names/words that have not been seen in the training data.

To train the algorithm, I split this data into 5 different training/testing groups and trained the model on each group. Each time, I used fastText's auto-tune functions to select the optimal model parameters, as chosen by performance on the test group.

B.4.3 Validating Predictions

To validate the performance of these models for four sets of data. (1) I generated predictions for each model on the “test’’ group (not used in training, only in choosing model parameters). (2) I also check the performance of taking the average of predictions from all five models (bagging) for the people in the 1874 guides that were matched to soldiers, and not used in the training at all (“validation’’ group). (3) I averaged the predictions from the five models for all men of voting age in residing in the nine Indiana counties in 1860, using data from the 1860 Census. This is the “aggregate census’’ group. (4) For analyses in the paper, I generate latent partisanship for soldiers by (a) averaging predicted probabilities of partisanship from all five models for people in the Census and (b) averaging these scores across all matches a soldier has in the Census, weighting by the match probability derived using fastLink. This is the “individual census’’ group.

Predicting Partisanship in 1874 I assess the predictive power of these classifiers in two ways.

ROC/PR Curves The ROC curve plots the trade-off between false positives rates and true positive rates, given different probability thresholds for making a binary classification. A poor classifier performs about as good as or worse than random guessing (which would have the performance of the diagonal line). The PR curve plots the trade-off between precision (the fraction of positive classifications that are true positives) and recall (the fraction of the true positives that are correctly identified by the classifier). Here, we assess the performance of the curve against the performance of the "no-skill" classifier of guessing every case to be a match. The more the area under the PR or AUC curve, and the more these curves are higher than the "no-skill" curves, the greater the predictive power of the classifier.

Predicted Probability vs. Actual Probability I also plot the (smoothed) probability of being a Republican/Democrat across predicted probabilities of being a Republican/Democrat. This should be nearly perfectly correlated, if the classifier performs well.

Performance in the "test" set In figure B5, I show that the ROC and PR curves for the classifier in the "test" set, while imperfect, substantially outperforms the naive classifier for both Republicans and Democrats. And in figure B6, I show the predicted probability of being a Republican and Democrat versus the actual probability. Again, while the classifier tends to over-predict at the high and low end, there is clearly a strong relationship between predicted and actual probability of Partisanship.

Performance in the "validation" set In figure B7, I show the ROC and PR curves for this classifier in the "validation" data: the 1874 biographical records linked to Union Soldiers. These were never used in training the classifier. Despite that, and despite there being substantial differences in the fraction who are Republican vs Democrat (soldiers are more Republican and non-soldiers), the classifier still outperforms the naive "no-skill" classifier. And again, the predicted probability of partisanship is strongly related with actual probability of partisanship (Figure B8). Like the test set, though, the relationship isn't one to one. It should be noted that that the classifier does a worse job at predicting Democrats among veterans: veterans predicted with to be Democrats with nearly 100 percent probability are only actually Democrats less than 70 percent of the time. This is likely reflecting the effects I find that people were converted to being Republican.

Performance in the "aggregate census" set This provides evidence that the classifier does a fairly good job predicting partisanship in 1874 for people whose demographic data was measured in 1874. But does it do a good job predicting partisanship during or before the Civil War, when selection into service and, potentially, combat experiences took place?

To assess this, I aggregate the predicted probabilities of Democratic and Republican partisanship for men eligible to vote in the 1860 elections and living the 9 Indiana counties, according the 1860 Census. I then take the average Republican probability, average Democratic probability, and the difference of these two for all men in each county (and in townships in 3 counties).

In figure B9, I show the correlation between the predicted partisanship of these 9 counties in 1860 and the actual 1860 vote share for Democrats and Republicans. While, again, the

Figure B5: AUC/PR Curves for "Test" Set

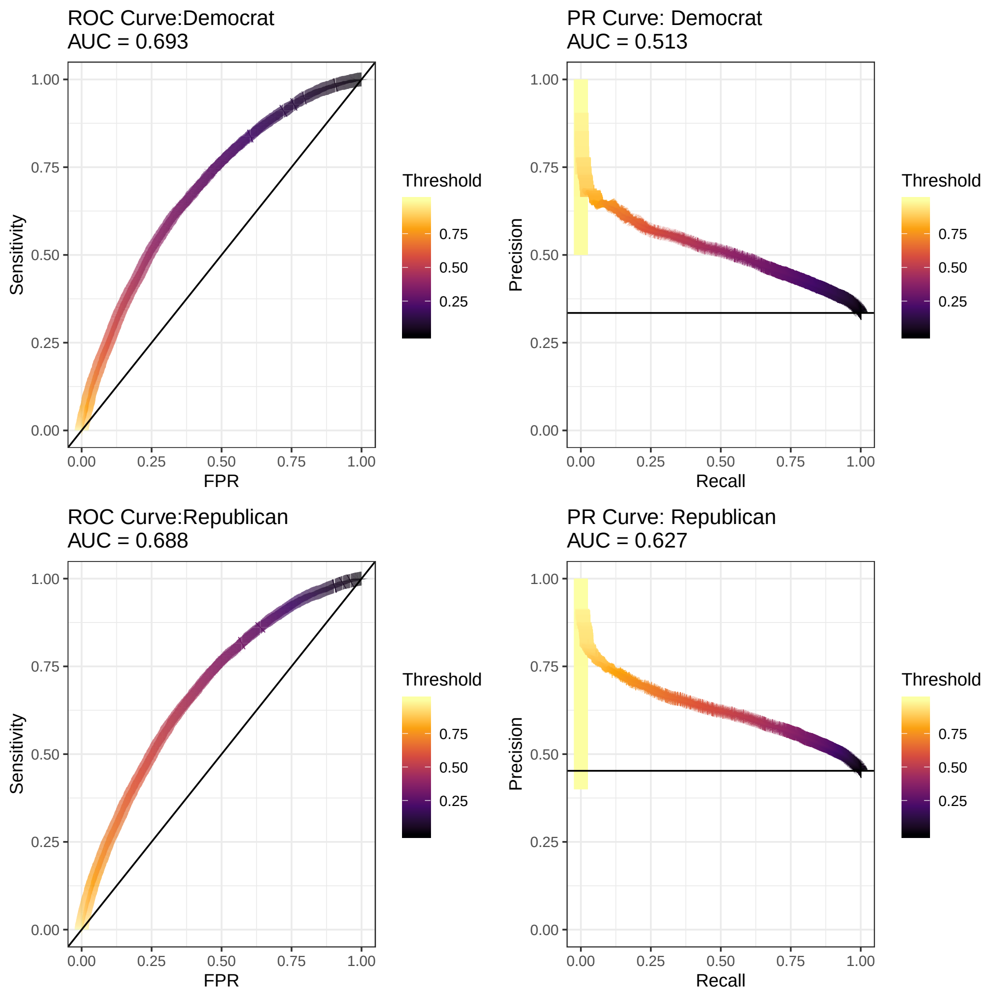
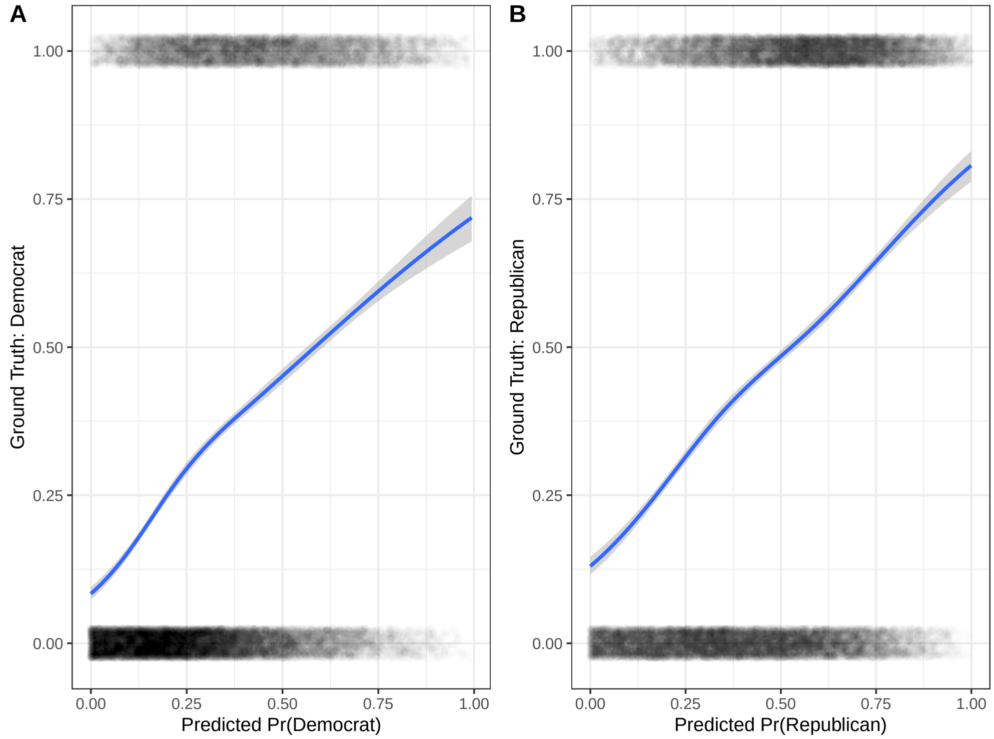


Figure B6: Actual Probability of Partisanship vs. Predicted Probability of Partisanship: "Test" Set



predicted probability does not predict voteshare at a one-to-one level, the correlation is nearly 1. This is remarkable given that the classifier was not provided any county labels.[3]

I repeat the same procedure using township level returns in Hendricks, Henry, and Morgan counties. These results are reported in Figure B10. Again, while the relationship is not one-to-one, predicted partisanship of townships in 1860 is correlated with the difference in Democratic and Republican vote share at nearly 0.8. Clearly, the model generates predictions of partisanship that correspond to real pre-war differences.

Performance in the "individual census" set Finally, I evaluate whether the specific measure I use in the paper for the latent partisanship of individual soldiers (averaging predicted partisanship across all census matches for that soldier) is meaningfully predictive. In Table B2, I show that predicted partisanship significantly predicted actual 1874 partisanship for the set of soldiers who are linked to both the census and to 1874. Moreover, I also find that predicted partisanship significantly predicts features of military service that correlate with partisanship. For example, in the 1874-linked sample, substitutes are more likely to be Democrats. I find that this is also true using predicted partisanship. Similarly, I find that predicted Democrats are more likely to desert and predicted Republicans are more likely to

[3]The correlation between predicted partisanship and voteshare is weaker for all Indiana counties, but still highly significant.

Figure B7: AUC/PR Curves for “Validation” Set

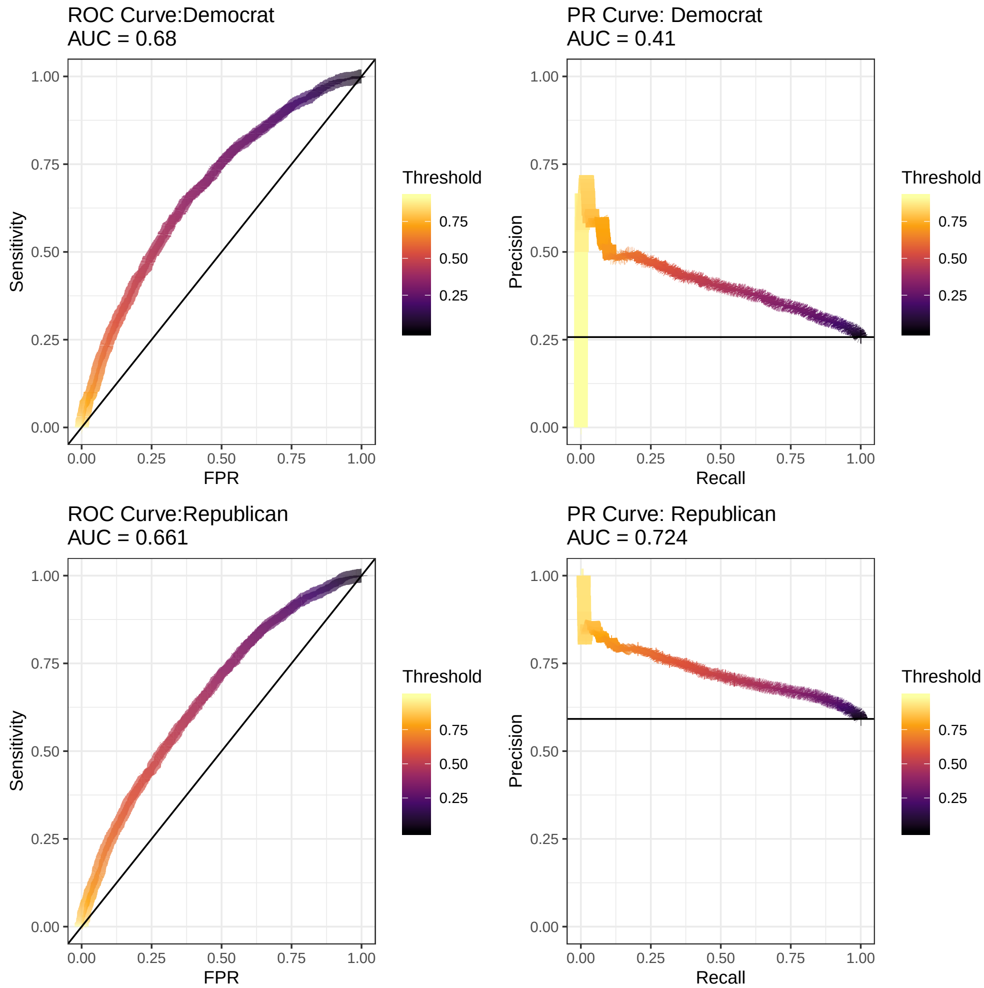
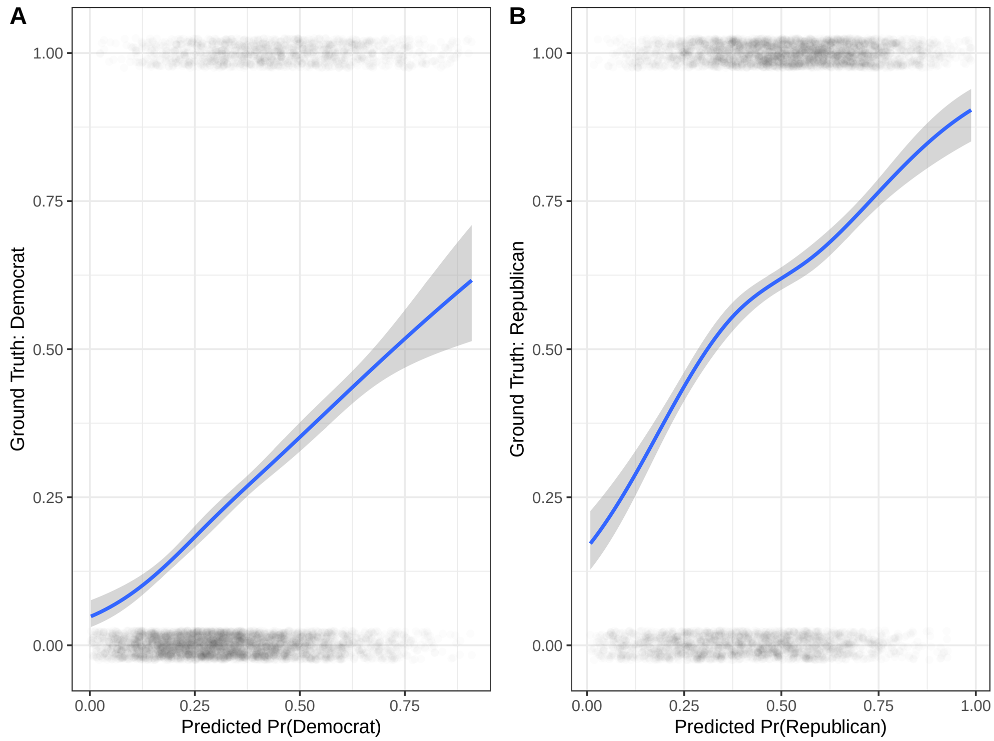


Figure B8: Actual Probability of Partisanship vs. Predicted Probability of Partisanship: "Validation" Set



die (Figure B2).

B.5 Balance Tests

In this section, I report the results of balance tests on pre-war covariates. Figure B11 reports the relationship between company casualties and average partisanship, net regiment fixed effects. Figure B3 shows the relationship between a 2 SD change in each pre-war covariate and the change in company casualties, using the design with regiment fixed effects. There is balance across nearly all variables. The exception is age and status as household head. People who are household heads or older had slightly lower exposure to company casualties. This imbalance is unconcerning for two reasons. (1) The within-regiment relationship between age and household head and post-war partisanship is precisely estimated and 0. (2) This likely reflects the fact that younger men (less likely to be heads of household) are less risk averse and thus either take actions that might endanger themselves or their company. Moreover, these changes are substantively small. The SD of within-regimental variation in company casualties is 2.3. A 2 SD change in age at enlistment is related to less than a tenth of a SD change in company casualties.

While there appear to be large differences by draft and substitute status, this is because there is nearly no variance within regiment fixed effects and draft/substitute status.

Figure B9: Predicted vs. Actual 1860 Voteshare for 9 Indiana Counties

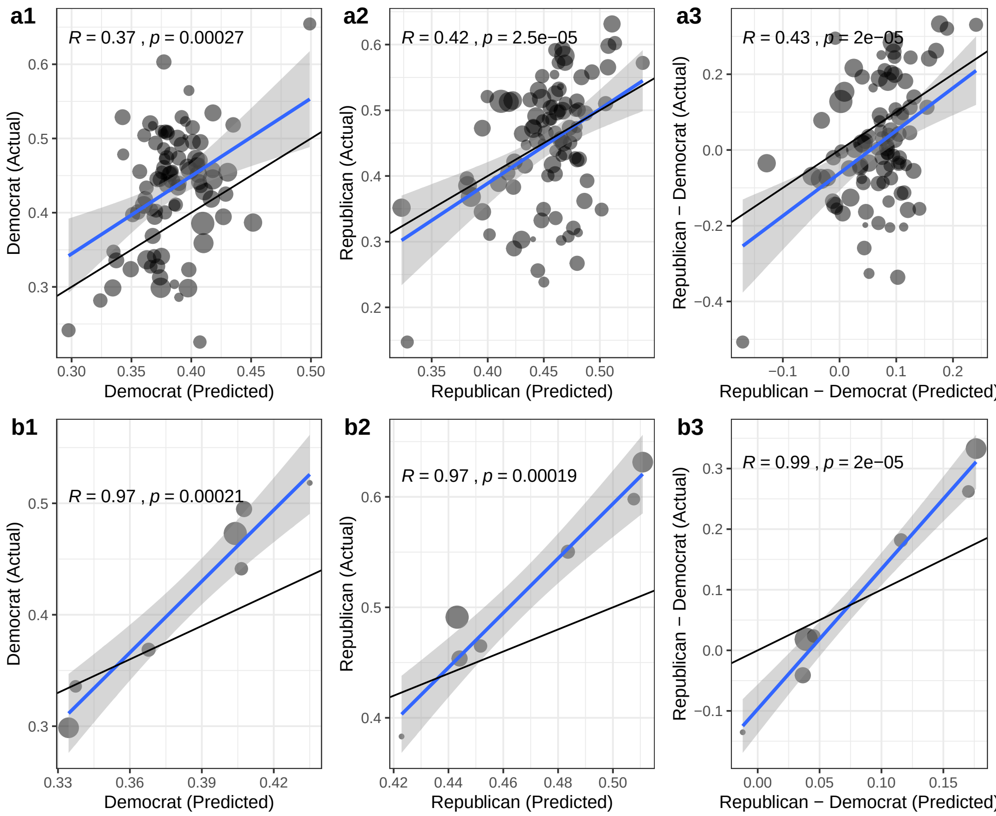


Figure B10: Predicted vs. Actual 1860 Voteshare for Townships in Hendricks, Henry, and Morgan Counties

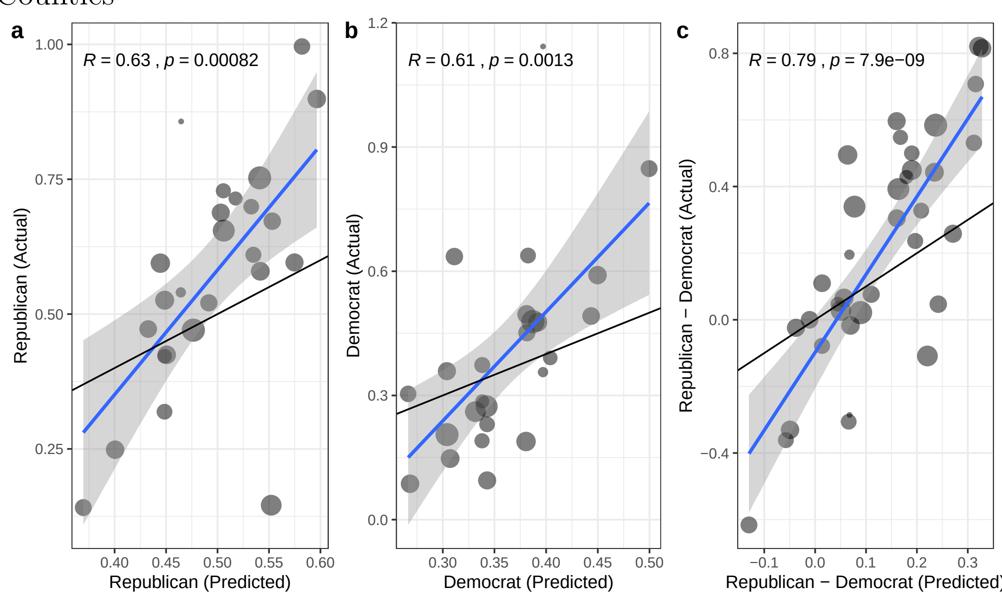


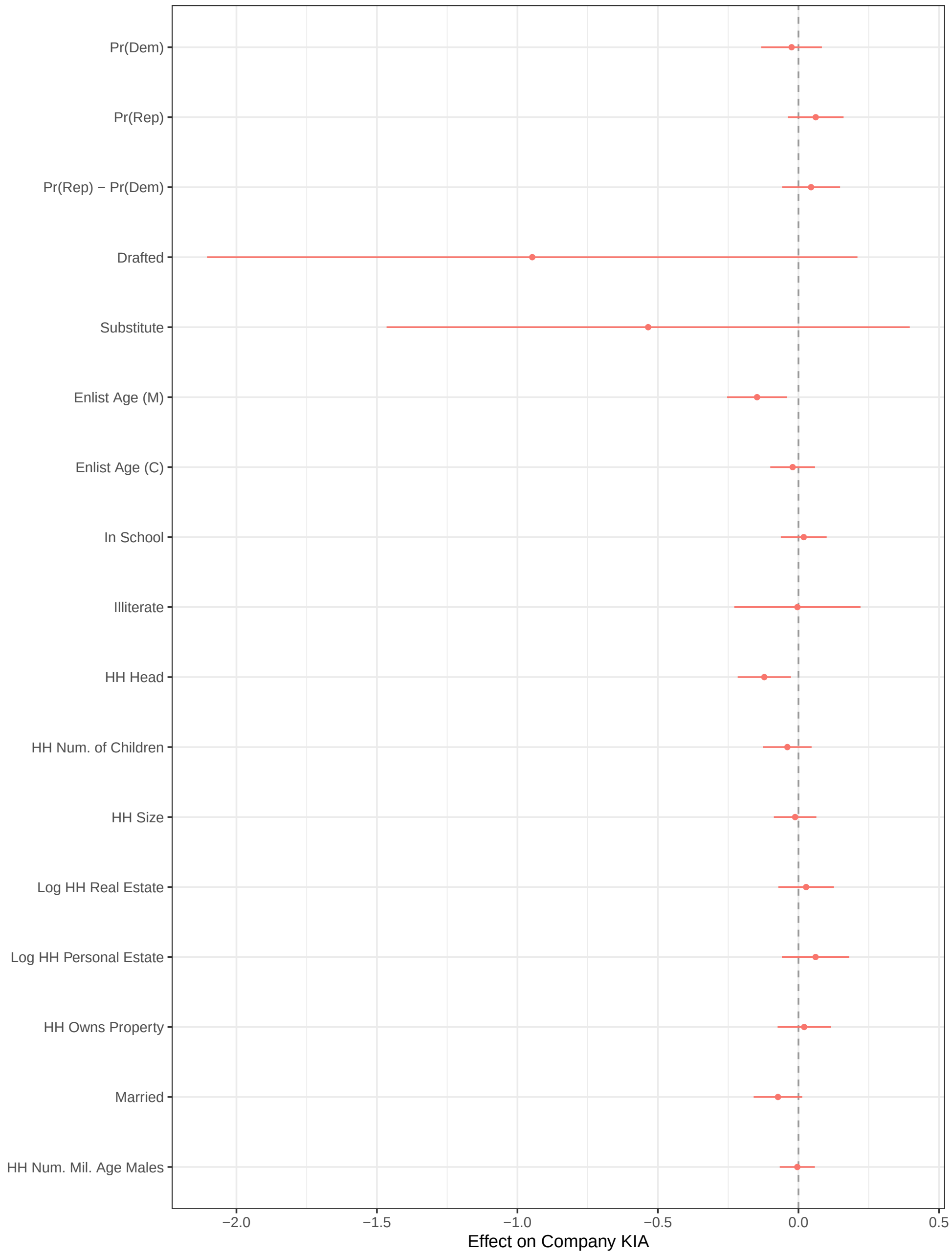
Table B2: Indiana Veterans: Predicting Partisanship with Latent Partisanship

	Dependent variable:					
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	All Matches			Best Matches		
	(1)	(2)	(3)	(4)	(5)	(6)
Rep. Prob.	0.410***			0.430***		
	(0.039)			(0.041)		
Dem. Prob.		0.372***			0.384***	
		(0.037)			(0.038)	
Diff Prob.			0.211***			0.221***
			(0.018)			(0.018)
Observations	5,958	5,958	5,958	4,518	4,518	4,518

Note: *p<0.05; **p<0.01; ***p<0.001

Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and 1874 People's Guides. Restricted sample includes only the best 1874 matches. Individuals are weighted by 1 over the number of matches. Latent partisanship is predicted probability of being Republican or Democrat based on the person's name, birth year, and birth place listed in the 1860 census. Standard errors are clustered by individual.

Figure B11: Balance on pre-war attributes across levels of treatment



This plots the standardized effect of a 2 SD change in pre-war attributes on company casualties, conditional on regiment fixed effects.

B.6 Robustness

Tables B3, B4, B5 show that the results are robust to considering different samples: using the best or all matches in 1874, and using all soldiers or only those found in the 1860 census.

Table B3: Effect of Company Casualties on Post-war Partisanship (Census-Linked, All Matches)

	Dependent variable:							
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.	Rep.	D
		Baseline			Army Controls			All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Company KIA	0.007*	−0.008**	0.008**	0.008*	−0.009**	0.008**	0.008*	−0
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0
Regiment FE	Y	Y	Y	Y	Y	Y	Y	
Army Controls	N	N	N	Y	Y	Y	Y	
Census Controls	N	N	N	N	N	N	Y	
Observations	4,676	4,676	4,676	4,171	4,171	4,171	4,171	4

Note: *p<0.05; **

Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and their best 1874 People's Guides. Baseline and control models, respectively, include data on 3135, 2786 individual sol 509 companies, across 212, 200 regiments. Regiment fixed effects includes a dummy for each group of s regiment in the same year. Standard errors are clustered by company.

Figure B12 shows the distribution of t statistics on company casualties across different specifications: including all soldiers (where partisanship for those without a match is 0), only non-attriters; Census matched/All soldiers; Best post-war match or all post-war matches; No covariates, army covariates, or army and census covariates; measuring treatment as company casualties or casualty rate.

B.7 Attrition

Only 33 percent of soldiers found in the 1860 Census were matched to the 1874 People's Guides and only 23 percent of soldiers over-all were matched. While this is not far off from other attempts to link historical records (see, e.g. Abramitzky et al. 2019), there are reasons to be concerned about attrition bias. It is necessary to assess whether there is any relationship between attrition and potential outcomes, any relationship between attrition and treatment, and any interaction between treatment, partisanship, and attrition. In this section, I investigate these possibilities and then directly assess the robustness of my results to the use of IPW adjustments, assuming Missingness Independent of Potential Outcomes conditional on covariates.

Table B4: Effect of Company Casualties on Post-war Partisanship (Best Matches)

	Dependent variable:					
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	Baseline			Army Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Company KIA	0.007*	−0.008**	0.007**	0.007*	−0.010***	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Regiment FE	Y	Y	Y	Y	Y	Y
Army Controls	N	N	N	Y	Y	Y
Census Controls	N	N	N	N	N	N
Observations	4,635	4,635	4,635	4,123	4,123	4,123

Note: *p<0.05; **p<0.01; ***p<0.001

Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and their best match, if any, in the 1874 People's Guides. Baseline and control models, respectively, include data on 3880, 3477 individual soldiers, serving in 617, 587 companies, across 234, 223 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Standard errors are clustered by company.

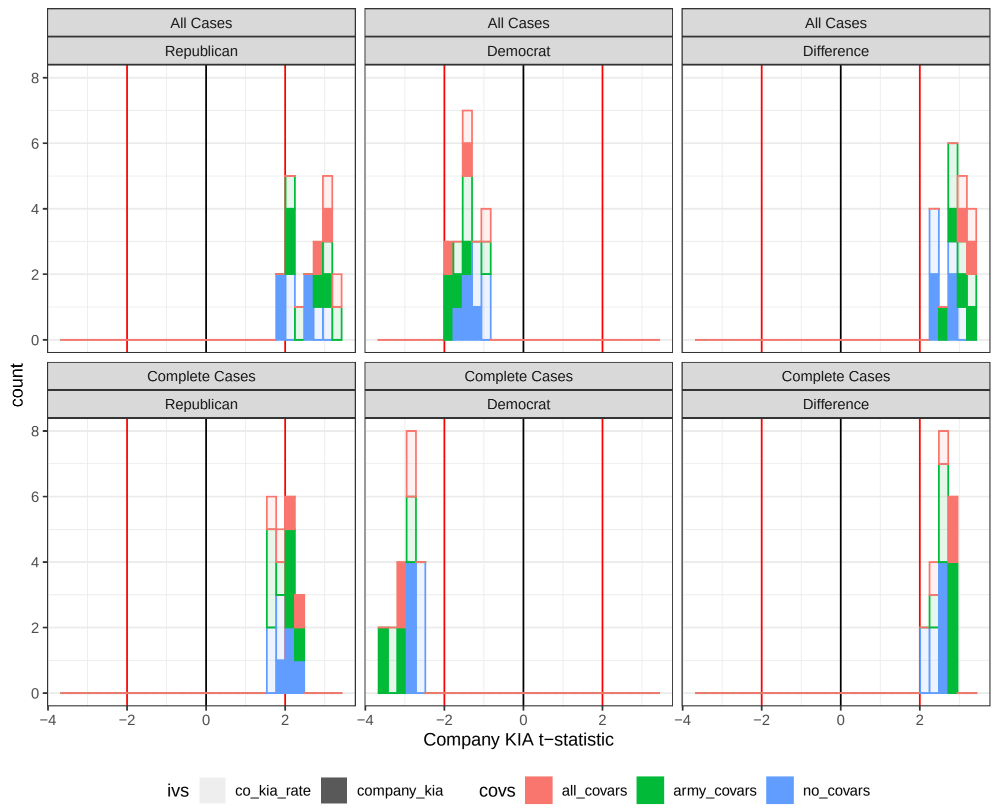
Table B5: Effect of Company Casualties on Post-war Partisanship (All Matches)

	Dependent variable:					
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	Baseline			Army Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Company KIA	0.006*	−0.007**	0.006*	0.006*	−0.008***	0.007**
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Regiment FE	Y	Y	Y	Y	Y	Y
Army Controls	N	N	N	Y	Y	Y
Census Controls	N	N	N	N	N	N
Observations	5,968	5,968	5,968	5,384	5,384	5,384

Note: *p<0.05; **p<0.01; ***p<0.001

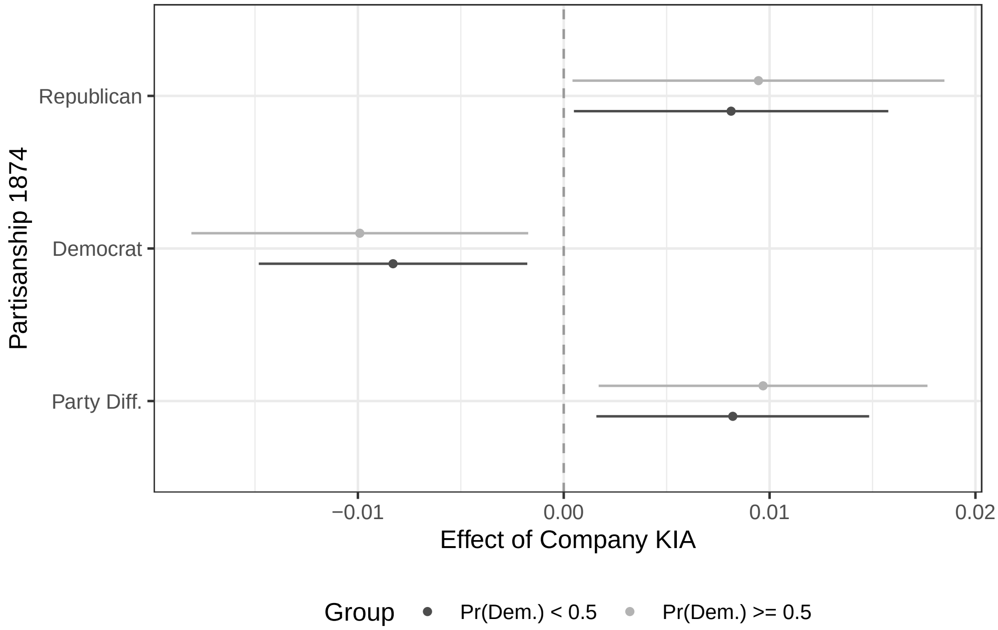
Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and their best match, if any, in the 1874 People's Guides. Baseline and control models, respectively, include data on 3905, 3502 individual soldiers, serving in 618, 588 companies, across 234, 223 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Standard errors are clustered by company.

Figure B12: Robustness of Company Casualties Results to alternate specifications



This reports t statistics for analyses of the company casualty natural experiment using different samples, covariates, and measures of the treatment.

Figure B13: Effects of Company Casualties By Predicted Partisanship



This shows the results from Table 2, columns 1–3, broken down for soldiers who were latent Democrats (Pr(Democrat) > 0.5), vs. not. While these differences are not significant, estimated effects are consistently larger for predicted Democrats.

B.7.1 Sources of Attrition

I start by examining what explains variation in attrition. In the analysis sample (infantry soldiers matched to the 1860 census), I compare modes of entering and exiting service and find that, other than death, the only significant predictor of attrition was desertion. Deserters were about 5 ppt less likely to appear post-war.

In unadjusted comparisons (no regiment fixed effects), partisanship is related to attrition (Figure B2). Latent Republicanism increases the probability that a person is observed after the war, decreases the chance that a person deserts, and is positively but not significantly related to dying. Democrats, by contrast, were more likely to desert.

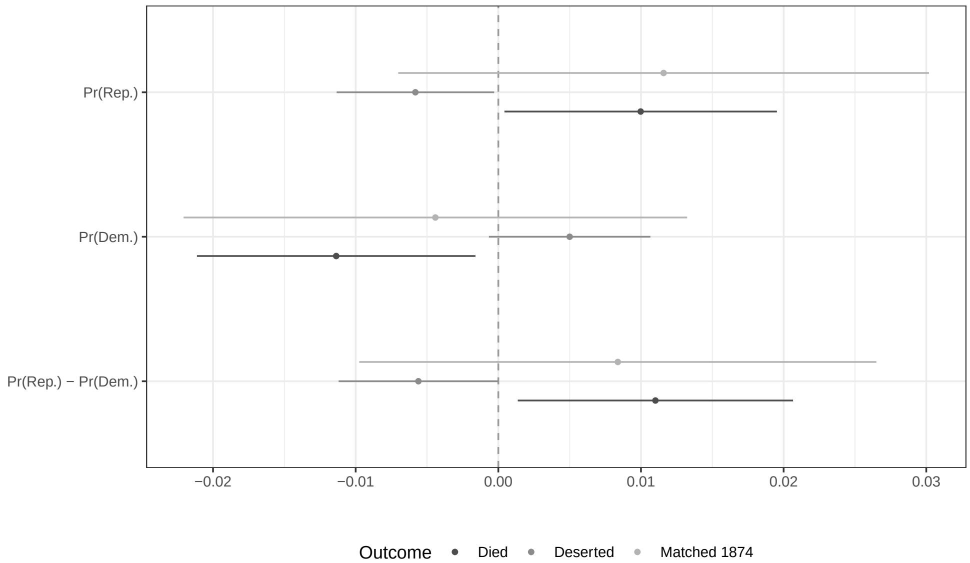
Within regiments, partisanship is not significantly related to overall attrition (Figure B14). Latent Republicanism increases the probability that a person died and decreases their chance of dying. People who were more likely to be Democrats were more likely to deserts and less likely to die.

B.7.2 Does treatment predict attrition?

Attrition bias could arise if treatment is related to attrition and relationship between treatment and attrition is related to potential outcomes.

Using the design employed for analyzing the natural experiment (regiment fixed effects), do company-level casualties predict attrition? Figure B15 shows that neither death nor desertion (the two main sources of attrition) are related to treatment. However, a 2 SD increase in company casualties is positively (though not significantly) related to being found

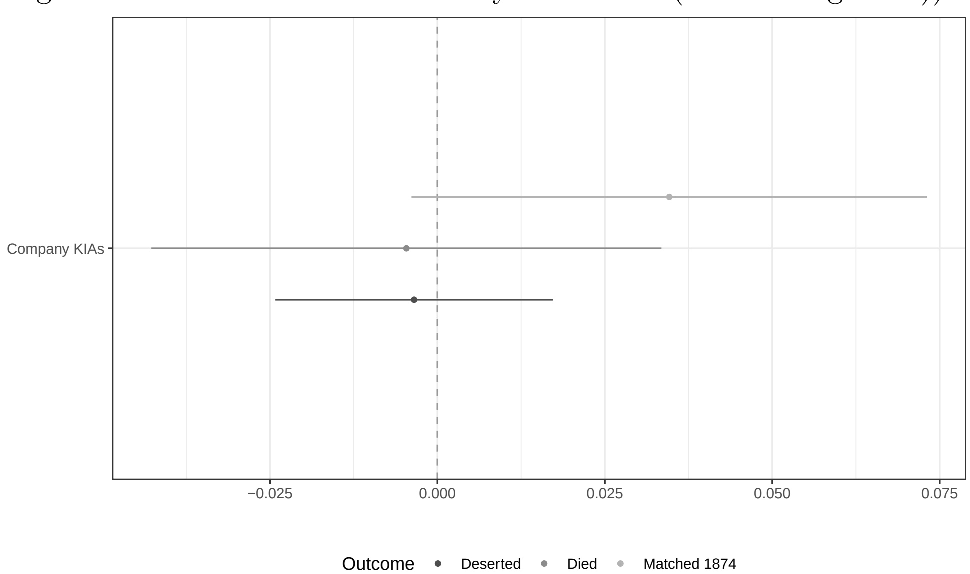
Figure B14: Differential Attrition by Predicted Partisanship (Within Regiment))



This figure shows the change in attrition, death, or desertion (related to attrition) associated with a 2 SD change in the predicted probability of Republican or Democratic partisanship, using the within regiment design.

after the war (an increase of about 2.5 ppt). And Figure B11, above, shows that treatment is balanced across baseline partisanship.

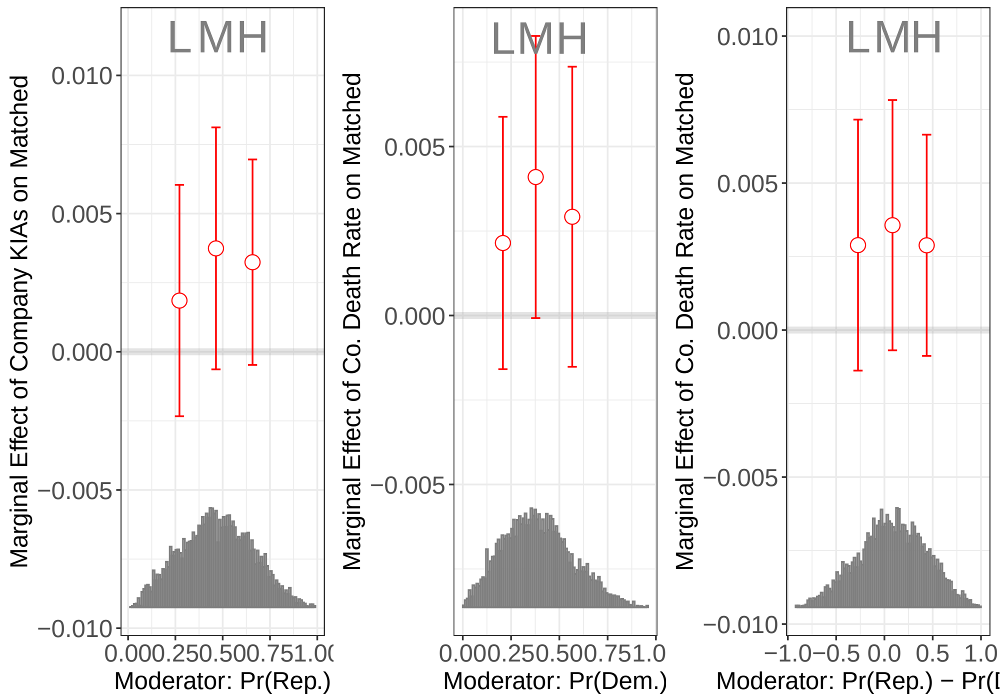
Figure B15: Differential Attrition by Treatment (Within Regiment))



This figure shows the change in attrition, death, or desertion (related to attrition) associated with a 2 SD change in company casualties, using the within regiment design.

Though not significant, I further test whether there is any relationship between the effect of treatment on attrition and latent partisanship. Figure B16 shows binned interaction effects between the combat casualties and predicted partisanship, obtained using the `interflex` package in R. There are no significant differences between any of the marginal effects of company casualties at high, medium, or low levels of Democratic, Republican, or difference in predicted partisanship.

Figure B16: Differential Attrition by Treatment (Within Regiment) and Partisanship



This figure shows the change in attrition associated with an increase in company casualties within regiments, marginally across different levels of predicted probabiltiy of Republican and Democratic partisanship

This is reassuring, as it does not provide any clear evidence of attrition by partisanship. If anything, the positive relationship between company combat deaths and attrition may reflect long-term trauma. On average, soldiers who were wounded or disabled were about 3.8 ppt more likely to be found after the war. This is likely because they needed help from friends and family to cope with these injuries. Something similar might be happening with those who had more intense combat experiences.

Nevertheless, out of an abundance of caution, I also replicate the main analyses using IPW estimators to address any possible attrition bias.

B.7.3 MIPO conditional on covariates

If there is differential attrition, one way to address it is to estimate the effect of combat casualties, where Missingness is Independent of Potential Outcomes, conditional on covariates. The recommended way of addressing this is to weight complete cases as by:

$$\frac{1}{\pi(R_i|Z_i, X_i)}$$

Where R_i is an indicator that case i is observed post-war; Z_i is level of treatment, and X_i is a vector of covariates (Gerber and Green 2012). This is an inverse probability weight estimator. A few issues arise when using inverse probability weights. First, these weights may perform poorly if there is a lack of positivity: there are sets of covariates for which missingness is near 0 or 100 percent. One way to investigate this is to look at overlap in covariates between the matched sample and the full sample of soldiers. While there is imbalance in the distribution of covariates (both raw (Figure B17), and centered within regiments (Figure B18)), the distributions clearly overlap.

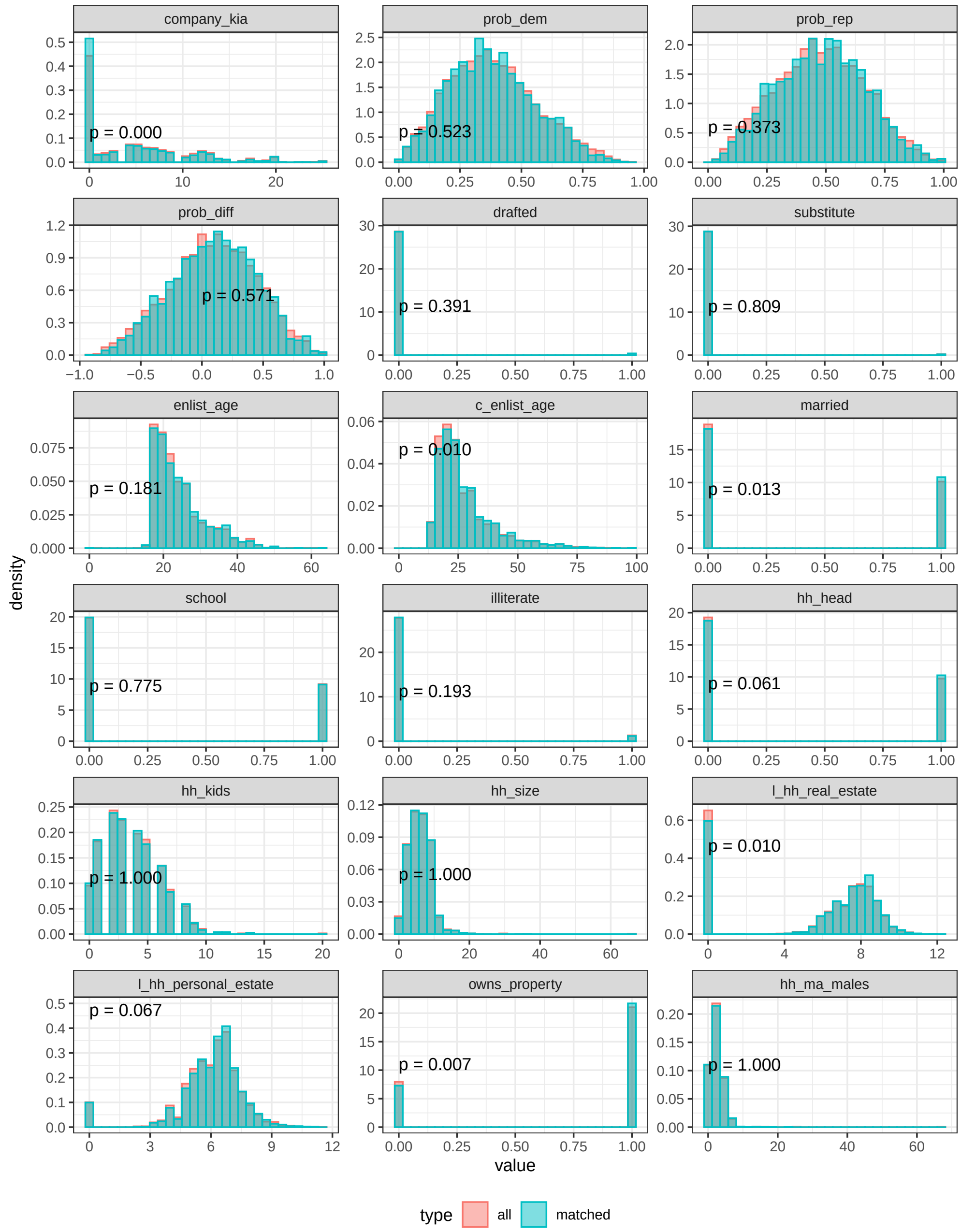
Second, inverse probability weights depend on estimating a propensity score model. If the propensity score model is incorrectly specified, bias may persist. To flexibly estimate the propensity that soldiers were matched after the war, I use generalized regression forests proposed by Athey, Tibshirani and Wager (2018). This permits both non-linearities and interactions between covariates in estimating the propensity score. In this model, I estimate propensity to be matched as a function of the treatment and a vector of covariates.[4] I include all of these variables in as both differences from the regiment mean and as the regimental mean.

Third, propensity scores do not ensure balance on covariates (in either the first or higher moment). Thus, I also estimate propensity scores for being matched using covariate balancing propensity scores that both predict propensity but also are constrained to maximize balance on covariates (Imai and Ratkovic 2014). I employ the CBPS method for the ATE, which returns identical weights to those defined above. Here, I constrain balance on all of the same covariates listed above, interactions between the treatment variable and every other covariate, and the square of every variable, including treatment. I consider two different models: one in which I include the covariates, interactions, and squared terms both centered on regiment means and at the regiment mean (CBPS 1); and for only the covariates centered on regimental means (CBPS 2). Figures B19 and B20 show the improvement in balance on post-war missingness achieved.

Figure ? shows the results of replicating the analysis in the body of the paper (Table 2, columns 1–3) using no weights, regression forest weights, and the CBPS weights. The results are virtually identical.

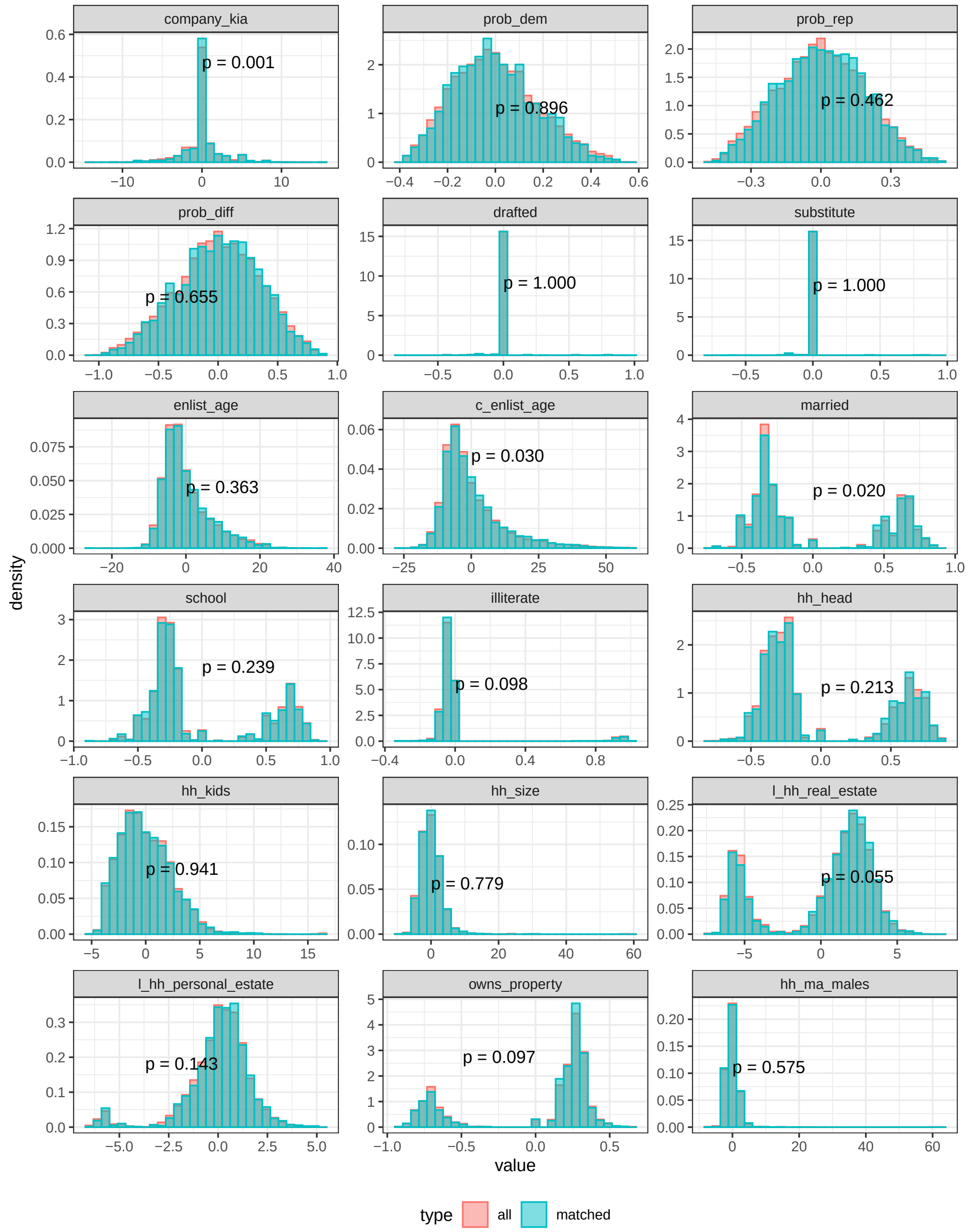
[4]Combat casualties, other deaths, company casualty rate, other death rate, number of men in company, date of muster into company, date regiment was organized, rank at enlistment, pr(Democrat), pr(Republican), pr(No party), drafted, substitute, Age (census), attended school in 1860, illiterate in 1860, HH head in 1860, children in HH in 1860, logged HH real estate value, logged HH personal estate value, HH owns property, married in 1860, HH size in 1860, military aged males in HH, birth place, county of residence in 1860.

Figure B17: Overlap on (raw) covariates for the full sample and those matched to 1874 People's guides.



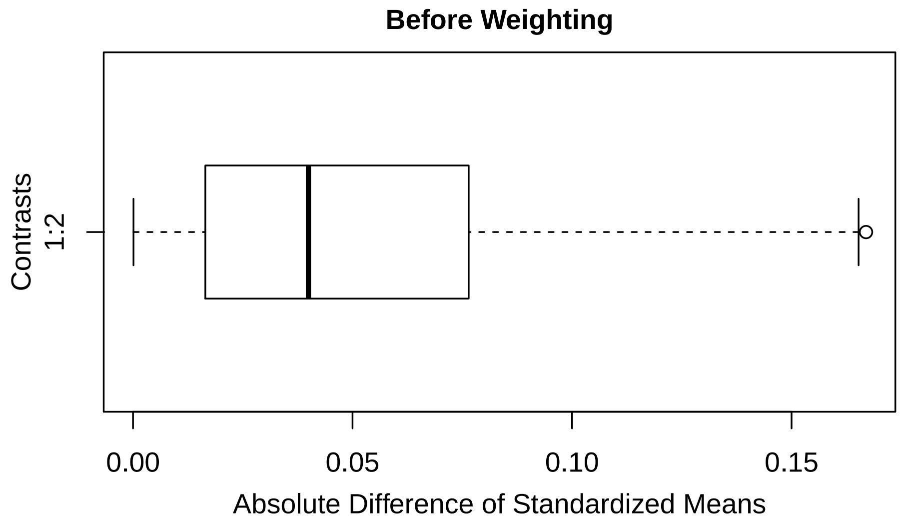
This figure shows overlap in the distribution of covariate values for the full sample of soldiers and those matched to the 1874 Guides. p values are for differences in means for binary variables, and for KS tests for continuous variables.

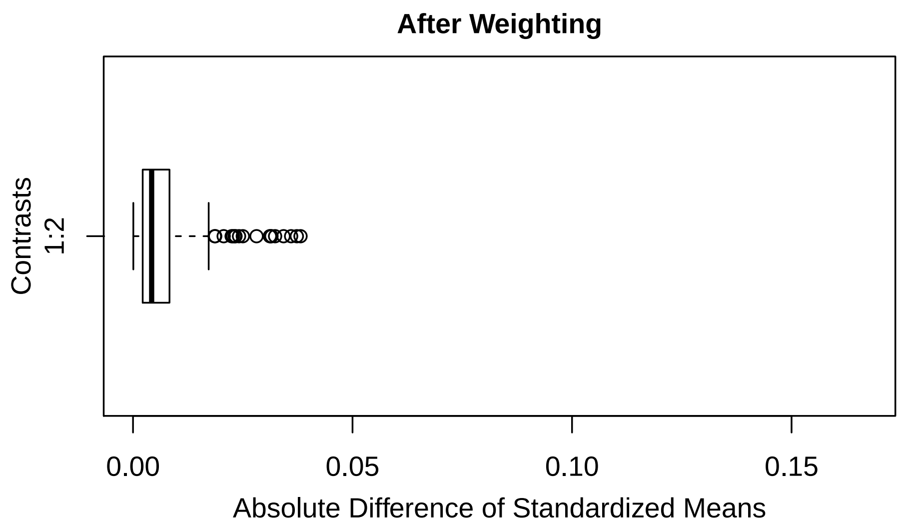
Figure B18: Overlap on (regiment-mean-centered) covariates for the full sample and those matched to 1874 People's guides.



This figure shows overlap in the distribution of covariate values, centered on the regiment mean, for the full sample of soldiers and those matched to the 1874 Guides. p values are for differences in means for binary variables, and for KS tests for continuous variables.

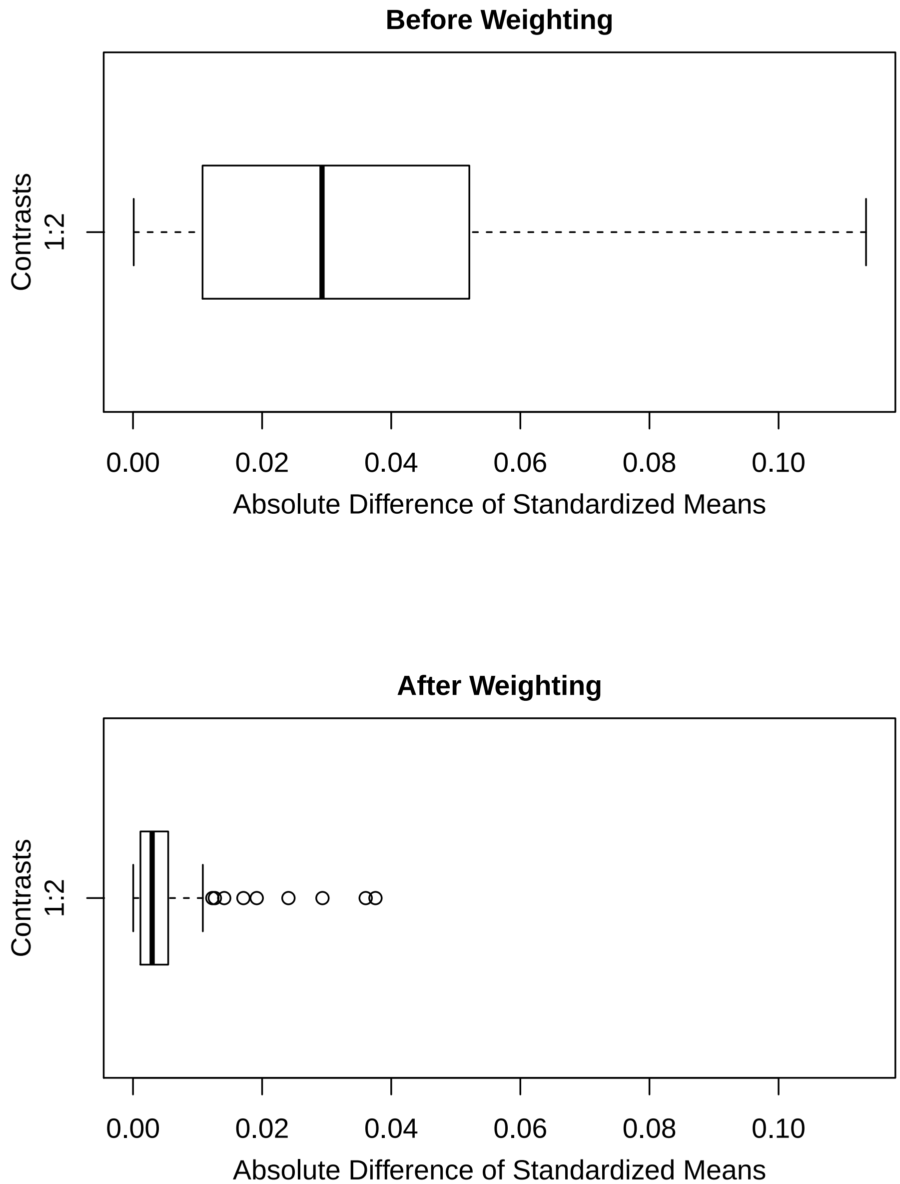
Figure B19: Balance between matched and un-matched soldiers before and after weighting using CBPS (option 1)





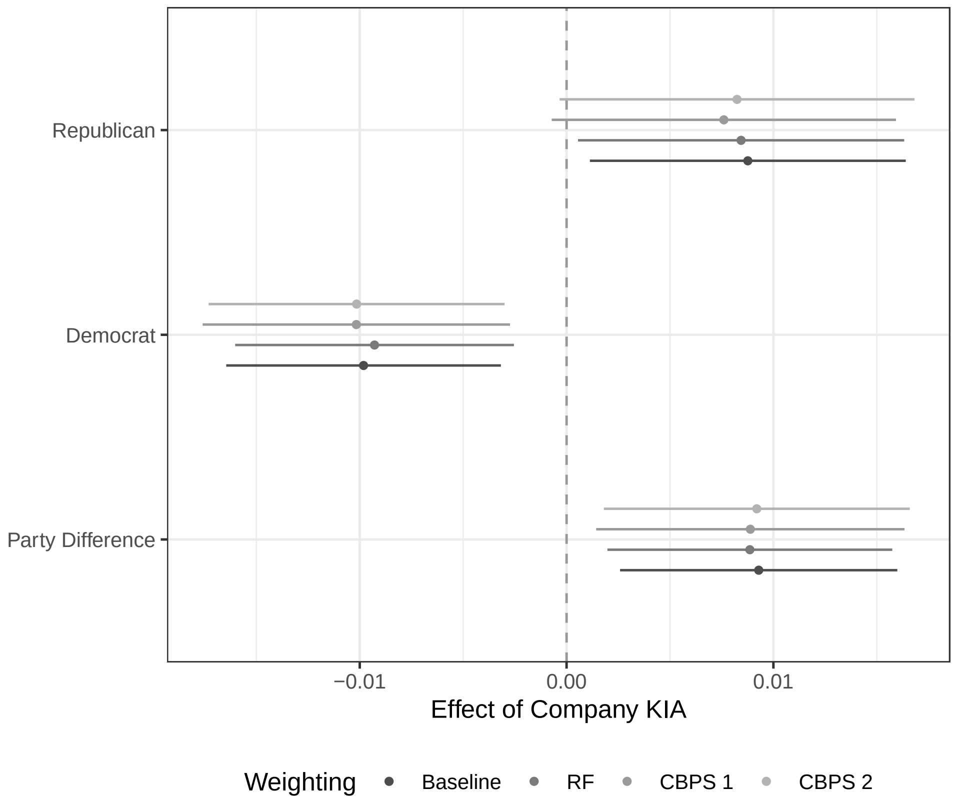
This figure shows balance between matched and unmatched soldiers before and after applying inverse probability weights generated using CBPS balancing on within- and between- regiment differences in treatment, covariates, interactions between treatment and covariates, and squared terms

Figure B20: Balance between matched and un-matched soldiers before and after weighting using CBPS (option 2)



This figure shows balance between matched and unmatched soldiers before and after applying inverse probability weights gforenerated using CBPS balancing on differences from within-regiment means of treatment, covariates, interactions between treatment and covariates, and squared terms

Figure B21: Robustness of Effects of Company Casualties to Weighting to address Attrition



This shows a replication of the analyses in Table 2, columns 1–3, using no weights, regression forest weights, and the CBPS weights.

C Veterans and Suffrage

In this section, I discuss the plausibility of drawing ecological inferences about the effect of military service on veterans' support for African American suffrage, using a difference-in-difference ecological regression.

C.1 Ecological Diff-in-Diff

In the conventional ecological "accounting identity'', pre-war suffrage support in a township i, S_{i0}, can be expressed as a function of (a) the fraction of men who will enlist, X_i, (b) the fraction of enlistees who supported suffrage at $t = 0$, β^e_{i0}, and (c) the fraction of non-enlistees who supported suffrage at $t = 0$, β^n_{i0}.

$$S_{i0} = X_i\beta^e_{i0} + (1 - X_i)\beta^n_{i0}$$

Assuming that β^e_{i0} and β^n_{i0} are independent of X_i, Goodman's ecological regression $S_{i0} = \alpha + X_i\beta + \epsilon_i$ returns an unbiased estimate of $\beta = E(\beta^e_{i0}) - E(\beta^n_{i0})$.

If we extend this to a second time period, time = 1, then:

$$S_{i1} = X_i\beta^e_{i1} + (1 - X_i)\beta^n_{i1}$$

And the difference Δ in support for suffrage can be expressed as a function of the change in support among enlistees, and change in support among non-enlistees.

$$S_{i\Delta} = X_i(\beta^e_{i1} - \beta^e_{i0}) + (1 - X_i)(\beta^n_{i1} - \beta^n_{i0})$$

And, here, $S_{i\Delta} = \alpha + X_i\beta + \epsilon_i$ then returns an unbiased estimate of $\beta = (\beta^e_{i1} - \beta^e_{i0}) - (\beta^n_{i1} - \beta^n_{i0})$ if $E(\beta^e_{i1} - \beta^e_{i0})$ and $E(\beta^n_{i1} - \beta^n_{i0})$ are independent of X. However, there is no requirement that β^e_{i1}, β^e_{i0}, β^n_{i0}, or β^n_{i1} are independent of X.

For further proof of this, the R code included below simulates this process, allowing there to be contextual differences in the cross-sectional support for suffrage among enlistees and non-enlistees. The simulations show that the ecological diff-in-diff regression is an unbiased estimator if the *shifts* in support for suffrage among enlistees/non-enlistees are uniform across X or independent of X.

```
require(magrittr)
require(data.table)

sim_ei_dd = function(n = 100) {
out = list()
#Fraction veterans, x
x = rnorm(n) %>% plogis()

#Fraction veteran support in time 0, that varies with x
#veteran support at x = 0
v00 = rnorm(1)
#contextual effect for veterans
```

```
v10 = rnorm(1)*x
#error
e_v0 = rnorm(1)
#Fraction veteran support suffrage
B_vi_0 = (v00 + v10 * x + e_v0) %>% plogis

#Fraction civilian support in time 0, that varies with x
#civilian support at x = 0
c00 = rnorm(1)
#contextual effect for civilian
c10 = rnorm(1)*x
#error
e_c0 = rnorm(1)
#Fraction civilian support suffrage
B_ci_0 = (c00 + c10 * x + e_c0) %>% plogis

#Suffrage at time 0:
s0 = x*B_vi_0 + (1-x)*B_ci_0

#Select a single uniform t1 - t0 shift for civilians, veterans,
#among set of possible shifts
diff_c_u = runif(1, 0 - min(B_ci_0), 1 - max(B_ci_0))
diff_v_u = runif(1, 0 - min(B_vi_0), 1 - max(B_vi_0))

#Select t1-t0 shifts for civilians, veterans
#independent of x, among shifts possible for all units
diff_c_i = runif(n, 0 - min(B_ci_0), 1 - max(B_ci_0))
diff_v_i = runif(n, 0 - min(B_vi_0), 1 - max(B_vi_0))

#Select different t1-t0 shifts for civilians, veterans
#among possible shifts for each unit
diff_c_alt = runif(n, 0 - B_ci_0, 1 - B_ci_0)
diff_v_alt = runif(n, 0 - B_vi_0, 1 - B_vi_0)

#Get suffrage vote at time 1
s1_u = x*(B_vi_0 + diff_v_u ) + (1-x)*(B_ci_0 + diff_c_u)
s1_i = x*(B_vi_0 + diff_v_i ) + (1-x)*(B_ci_0 + diff_c_i)
s1_alt = x*(B_vi_0 + diff_v_alt ) + (1-x)*(B_ci_0 + diff_c_alt)

#Truth
out$att_u = mean(diff_v_u - diff_c_u)
out$att_i = mean(diff_v_i - diff_c_i)
out$att_alt =mean(diff_v_alt - diff_c_alt)

#Truth: correlation between B_vi_0, B_ci_0 and x
```

```
out$beta_vx_0 = lm(B_vi_0 ~ x) %>% coef %>% .[2]
out$beta_cx_0 = lm(B_ci_0 ~ x) %>% coef %>% .[2]

#Truth: correlation between B_vi_0, B_ci_0 and x
out$beta_vx_diff_i = lm(diff_v_i ~ x) %>% coef %>% .[2]
out$beta_cx_diff_i = lm(diff_c_i ~ x) %>% coef %>% .[2]
out$beta_vx_diff_alt = lm(diff_v_alt ~ x) %>% coef %>% .[2]
out$beta_cx_diff_alt = lm(diff_c_alt ~ x) %>% coef %>% .[2]

#Estimate Ecological Regression
out$att_hat_u = lm(s1_u - s0 ~ x) %>% coef %>% .[2]
out$att_hat_i = lm(s1_i - s0 ~ x) %>% coef %>% .[2]
out$att_hat_alt = lm(s1_alt - s0 ~ x) %>% coef %>% .[2]
return(as.data.table(out))
}

sim_data = lapply(1:10000, function(x) sim_ei_dd(100)) %>% rbindlist

#Average bias of ecological DD regression, with uniform shift
sim_data[, att_hat_u - att_u] %>% mean
#Histogram of bias in ecological DD regression, with uniform shift
sim_data[, att_hat_u - att_u] %>%
hist(xlab = "Estimator Bias",
main = "Bias of Ecological DD Regression\nw/ Uniform shifts")

#Average bias of ecological DD regression, with shifts independent of x
sim_data[, att_hat_i - att_i] %>% mean
#Histogram of bias in ecological DD regression, with uniform shift
sim_data[, att_hat_i - att_i] %>%
hist(xlab = "Estimator Bias",
main = "Bias of Ecological DD Regression\nw/ shifts independent of x")

#Bias in ecological DD regression as function of contextual effect
#between x and civilian shift
sim_data[, list(beta_cx_diff_i, att_hat_i - att_i)] %>%
plot(xlab = "Slope on x and civilian contextual shift",
ylab = "Estimator Bias")

#Bias in ecological DD regression as function of contextual effect
#between x and veteran shift
sim_data[, list(beta_vx_diff_i, att_hat_i - att_i)] %>%
plot(xlab = "Slope on x and civilian contextual shift",
ylab = "Estimator Bias")
```

C.2 Contextual Effects: Conditioning

I address the possibility that there are contextual effects, where the shifts in support for suffrage are related to X, in two ways. First, I condition on a set of covariates W that are plausibly related to X and to the shift in support for suffrage over time. If, after conditioning on W, $E(\beta^e_{i1} - \beta^e_{i0})$ and $E(\beta^n_{i1} - \beta^n_{i0})$ are independent of X, then the ecological difference-in-difference estimator is unbiased.

For the suffrage referenda in Wisconsin townships, it is possible for me to condition on county fixed effects, the difference between Republican support and suffrage support in 1857, and the change in the fraction of people eligible to vote between 1857 and 1865. If, within counties, or conditional on the gap between Republican and suffrage support, in 1857, enlistment rates are independent of the shift in support for enlistees and non-enlistees ($E(\beta^e_{i1} - \beta^e_{i0})$ and $E(\beta^n_{i1} - \beta^n_{i0})$), then the estimated slope on enlistment rates are an unbiased estimator of the difference-in-difference ATT for enlistment.[5] The results of these analyses are reported in Table C1. Even in the most restrictive specification, I find that veterans increased support for suffrage by 10 ppt.

C.3 Contextual Effects: Bounding

The second approach to dealing with contextual effects is to place mathematical bounds on their size, and then use that to derive the implied bounds for the ATT of service on enlistees. This approach essentially admits that we may not be able to condition away contextual effects, and instead seeks to find a region of identification within which the contextual effects must be. This is more believable, because it is plausible that there are contextual effects: the changes in support for suffrage among enlistees and non-enlistees may vary with the fraction of men who enlisted. Jiang et al. (2020) show that, if we assume that these contextual effects are linear,

$$E(\beta^e_{it}|X_i) = e_{0t} + e_{1t}X_i$$

$$E(\beta^n_{it}|X_i) = n_{0t} + n_{1t}X_i$$

then, we can partially identify the following model:

$$E(S_{it}|X_i) = n_{0t} + (e_{0t} - n_{0t} + n_{1t})X_i + (e_{1t} - n_{1t})X_i^2)$$

I apply the bounds that Jiang et al. (2020) derive to bound the ATT on enlistees' support for suffrage. Importantly, these bounds, because they use more information than the Duncan-Davis bounds, are usually narrower. Where L_{nt} and U_{nt} are the lower and upper bound for support among non-enlistees in time t, and L_{et} and U_{et} are the lower and upper bound for support among enlistees in time t, derived using the partial identification of the linear-contextual effects ecological regression proposed by (Jiang et al. 2020).

$$ATT_L = (L_{e1} - U_{e0}) - (U_{n1} - Ln0)$$

[5] Also assuming parallel trends for enlistees and non-enlistees.

Table C1: Effect of Enlistment on Support for Black Suffrage (Wisconsin Township Returns)

	Dependent variable:									
	(Yes/Elig.) Full Sample					(Yes/Elig.) Restricted Sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enlistment Rate	0.211***	0.185***	0.192***	0.150***	0.114***	0.180***	0.148***	0.160***	0.109***	0.084**
	(0.031)	(0.030)	(0.036)	(0.032)	(0.031)	(0.031)	(0.028)	(0.037)	(0.030)	(0.030)
Lagged DV	Y	Y	N	N	N	Y	Y	N	N	N
Differenced	N	N	Y	Y	Y	N	N	Y	Y	Y
Controls	N	Y	N	Y	Y	N	Y	N	Y	Y
County FE	N	N	N	N	Y	N	N	N	N	Y
Observations	362	362	362	362	362	321	321	321	321	321

Note: $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Enlistment rate is number of men serving over those eligible to vote in 1865. Suffrage vote totals come from state constitutional referenda returns in stable clusters of townships/counties in Wisconsin. Control variables include: (i) the fraction eligible to vote in 1857 over those eligible to vote in 1865; and (ii) The fraction voting for the Republican gubernatorial candidate in 1857 minus the fraction voting for suffrage in 1857. Townships are weighted by number of white men. The restricted sample includes only townships where the population eligible to vote in 1865 changed by less than 50 percent between 1860 and 1870. Standard errors are robust.

$$ATT_U = (U_{e1} - L_{e0}) - (L_{n1} - Un0)$$

Using this approach (and excluding outliers, as the authors suggest), the bounds for the ATT on veterans in Wisconsin are $-0.272, 0.596$, which includes the estimated effect, 0.29. Figure C1 shows the size of the contextual effects needed for the true effect on veterans to be less than 0. For veterans and civilians, it plots the $t_1 - t_0$ changes in support for suffrage at the high and low end of enlistment rates that are possible given the bounds on linear contextual effects. For each point on the graph, it also uses a diverging color scale to show what the true ATT would be, for contextual effects of that magnitude.

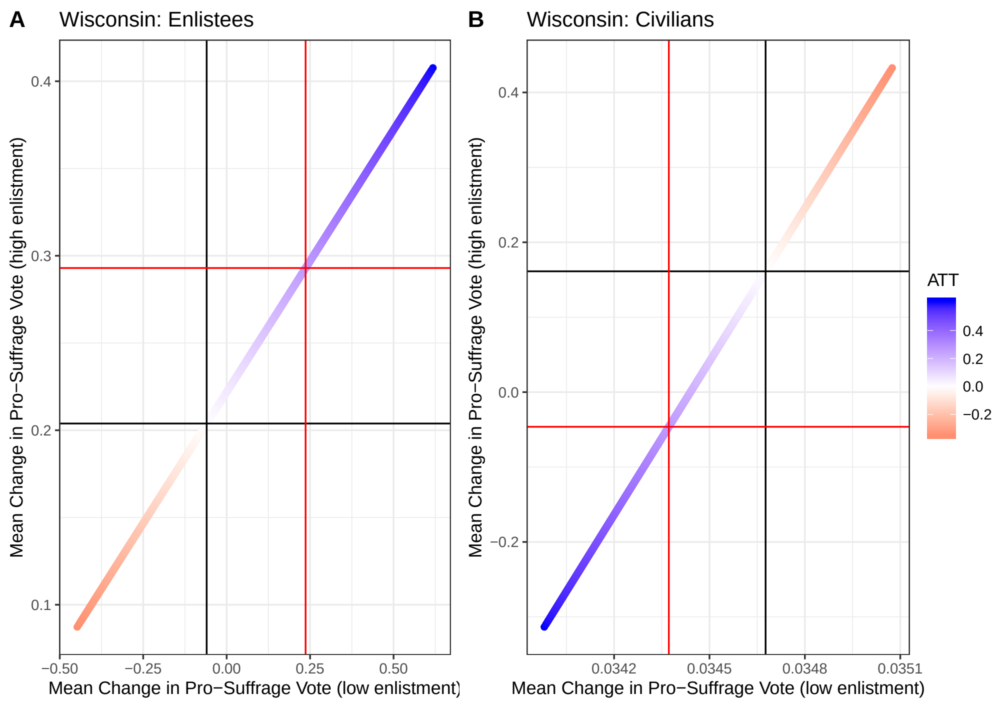
For there to be no effect on veterans, we'd have to believe that veterans' support for suffrage in low-enlistment townships dropped by 6 ppt or more, while veterans' support in high-enlistment townships *increased* by 20 ppt. These are strongly heterogeneous effects: substantially more than the 6.9 ppt shift toward suffrage state-wide and larger in absolute magnitude than the shifts toward suffrage in more than 96 percent of townships, and 1.9 SD larger than the average overtime change within townships. If the absolute difference in over-time change in support for suffrage among veterans in high and low enlistment townships were any smaller (than 26 ppt), then the ATT must be above 0.

Moreover, if we calculate the Duncan Davis bounds for the change in support for suffrage among veterans and non-veterans and plot them against the enlistment rate, we can investigate whether these bounds include or exclude the contextual effects required for the ATT to be 0 or less. Figure C2 plots these bounds for veterans and indicates whether these bounds are *above*, *below*, or *include* the minimum contextual effect for which the $ATT \geq 0$. At the low end of enlistment, the bounds for several townships show that the change in support among veterans *must* have been higher the minimum contextual effect for which the $ATT \geq 0$, while there are only only a few townships for which the bounds are below the minimum. Figure C3 plots the bounds for non-veterans and indicates whether these bounds are *above*, *below*, or *include* the maximum contextual effect for which the $ATT \geq 0$. While at low levels of enlistment, many townships are bounded above and below the contextual effect, at higher levels of enlistment, many more townships are bounded *below* the maximum contextual effect for non-veterans. Both of these points suggest that the contextual effects are likely not sufficiently large for the $ATT \leq 0$.

Another approach to addressing the issue of bounds is to look at the bounds for subsets of the data. I consider two natural subsets of the data. First, I consider bounds for townships in the same county, since these are natural geographic units which might share many features. I find that for Grant County, Wisconsin, the ATT is bounded by $0.03, 0.57$. No other counties are bounded above or below 0.

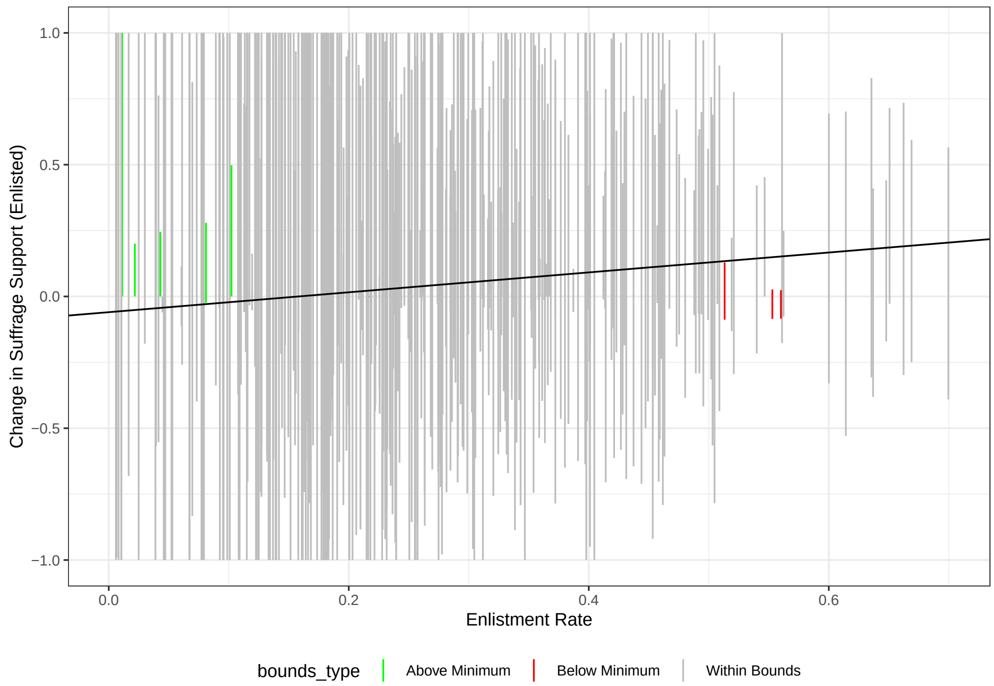
Second, I look at bounds for townships where pro-suffrage votes in the pre-war period were near 0. These are natural to consider, because they are likely to have the tightest bounds. This is because, when pre-war suffrage support is near 0, the upper and lower bounds of pre-war support for suffrage among enlistees and civilians must be narrowly bounded near 0. When calculating the bounds on the ATT, then, we mostly rely on the bounds from $t = 1$. Figure C4 reports the lower and upper bounds of the ATT for all towns below different thresholds of pre-war suffrage support. There is a region where pre-war suffrage support was

Figure C1: ATT of enlistment on suffrage support across possible values sizes of linear contextual effects (Wisconsin Referenda)



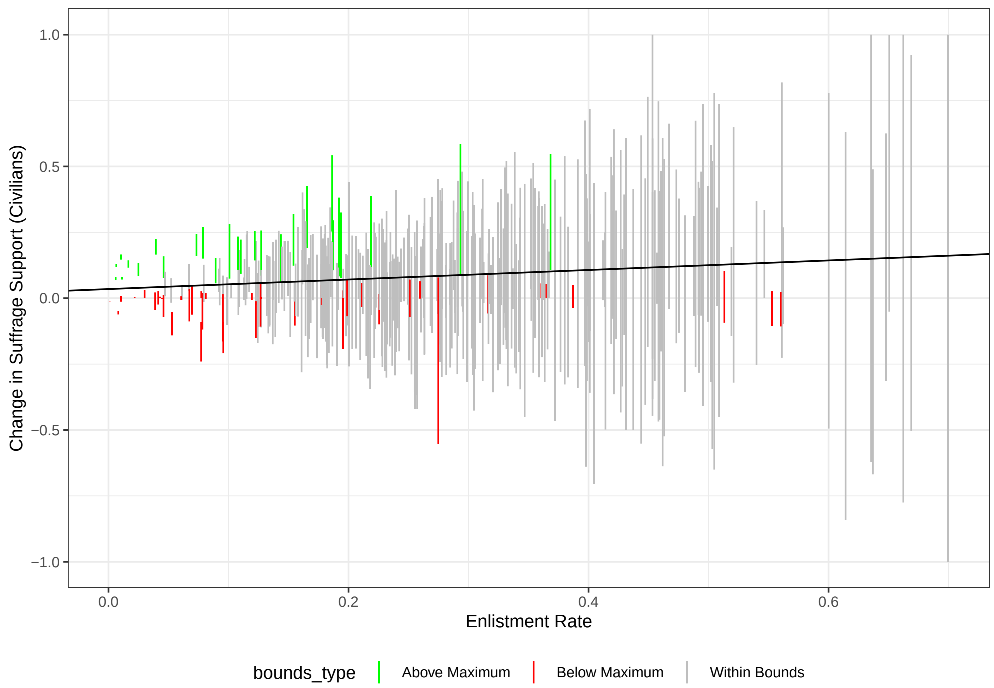
This figure shows the size of the contextual effects needed for the ATT on veterans to be less than 0. The x axis shows the $t_1 - t_0$ changes in support for suffrage in low enlistment townships and the y axis shows the $t_1 - t_0$ changes in support for suffrage in low enlistment townships that are possible given the bounds on linear contextual effects. The color of the dots shows the ATT associated with the contextual effect associated with the values of x and y. The black lines show the contextual effects implied for an effect size 0; the red lines show the contextual effects implied by the estimate obtained using the difference ecological regression.

Figure C2: Minimum contextual effects for veterans for $ATT \geq 0$ and Duncan Davis bounds for townships



This figure show the Duncan Davis bounds for veterans' change in support for suffrage over time in townships and indicates whether these bounds are *above*, *below*, or *include* the minimum linear contextual effects for which the $ATT \geq 0$.

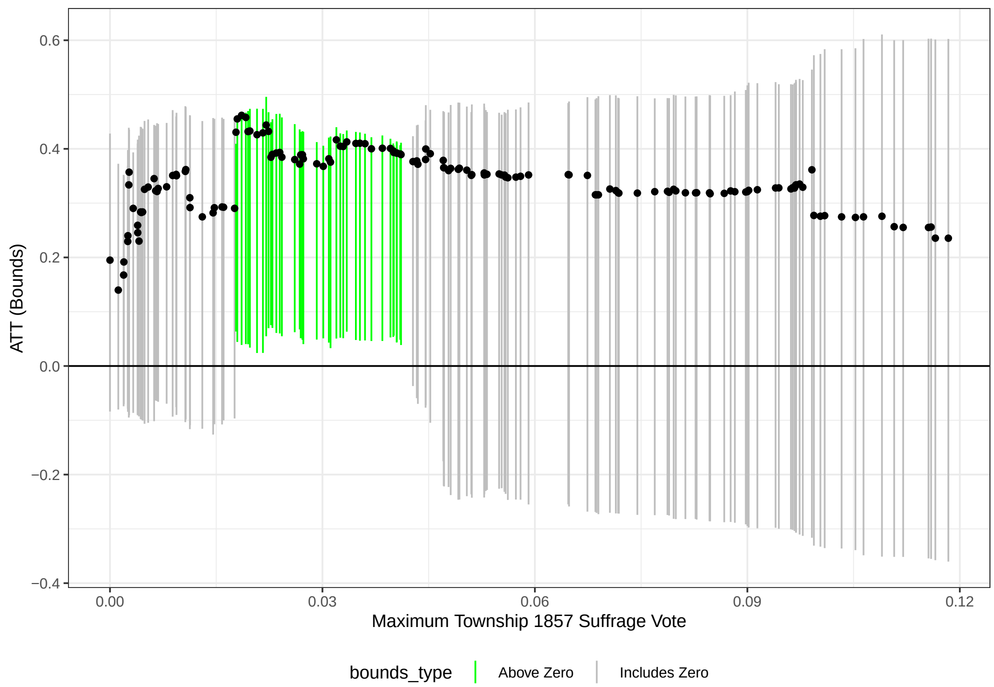
Figure C3: Maximum contextual effects for non-veterans for $ATT \geq 0$ and Duncan Davis bounds for townships



This figure show the Duncan Davis bounds for non-veterans' change in support for suffrage over time in townships and indicates whether these bounds are *above*, *below*, or *include* the maximum linear contextual effects for which the $ATT \geq 0$.

lower than approximately 4 percent[6] for which the the ATT of military service on support for suffrage is bounded above 0. Since these are deterministic bounds, this approach does not amount to "p-hacking."

Figure C4: Deterministic bounds on the ATT of military service on suffrage for townships with low pre-war support for suffrage

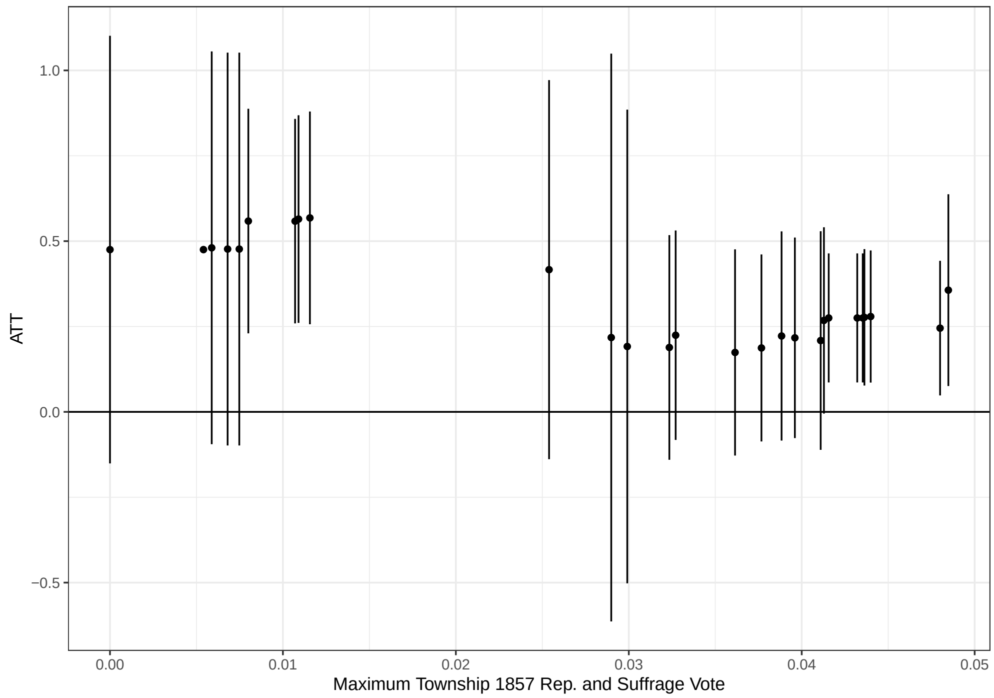


This figure show the Jiang et al. (2020) bounds for the ATT of military service on support for suffrage for towns below different thresholds of pre-war support for suffrage. Black dots indicate the estimated ATT from an ecological difference-in-difference.

C.4 Interpretation

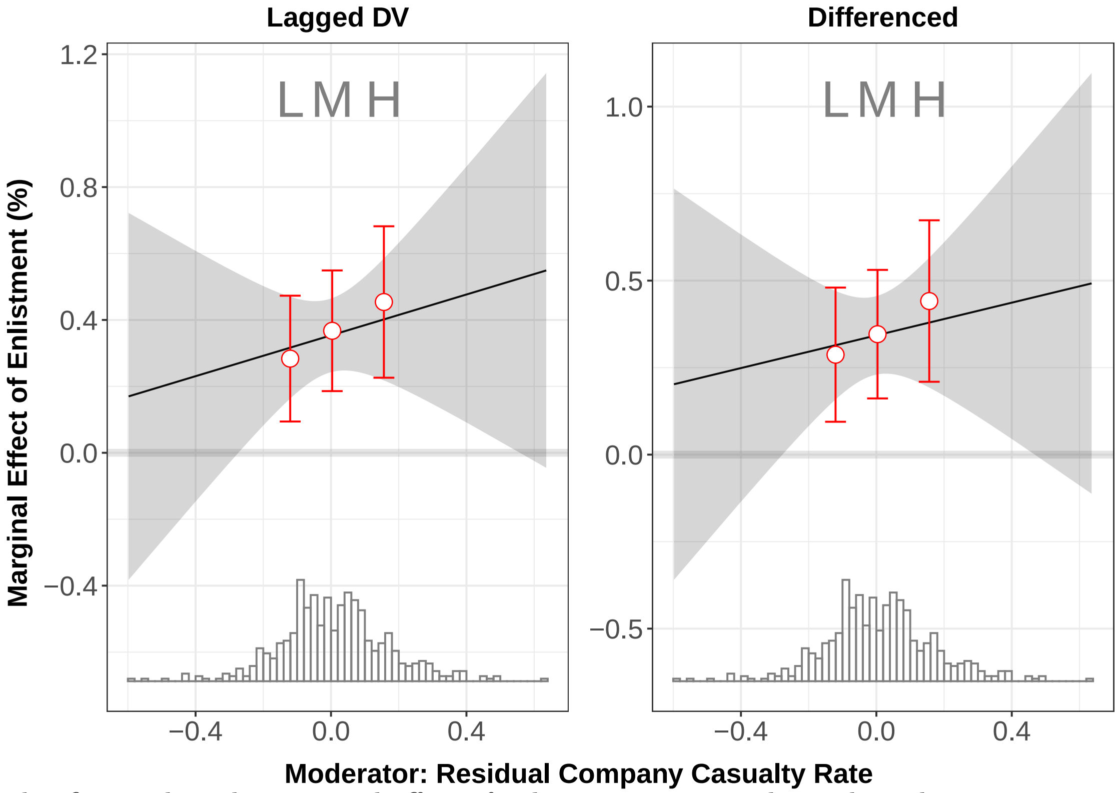
In the paper, I note that it could be that something other than military service is driving the different changes in support for veterans and non-veterans. Perhaps selection based on Republican Partisanship into military service makes veterans more susceptible to changing views on suffrage as the party changes its position. To address this, I estimate the difference-in-difference effect of enlistment on suffrage, conditioning on 1857 Republican support for townships with very low support for Republicans and Suffrage in 1857. Figure ? shows the results of these tests across a range of thresholds. Even where there is no possibility of selection based on partisanship or support for suffrage, we find a strong positive effect of enlistment on suffrage support after the war.

Figure C5: Difference-in-Difference effects of Enlistment on Support for Suffrage in Townships with Low Republic/Low Suffrage Support (Wisconsin)



This figure shows the diff-in-diff effect of enlistment on suffrage for townships with pre-war support for Republicans and Suffrage less a given threshold. These models also include a control for pre-war support for Republicans (effects stronger in absence of this control). Standard errors are the maximum of homoskedastic or heteoskedastic consistent 3 variances estimators.

Figure C6: Marginal Effect of Enlistment Rates on Votes for Black Suffrage Conditional on Company Casualty Rates



This figure plots the marginal effect of enlistment rates conditional on the average company casualty rate, in the Iowa and Wisconsin state constitutional referenda in 1857, 1865, and 1868. Average company casualty rate is calculated by taking the number of in-unit company deaths experienced by a soldier, minus the mean company casualties experienced in his regiment, averaged by townships cluster.

C.5 Heterogeneous Effects

D Data

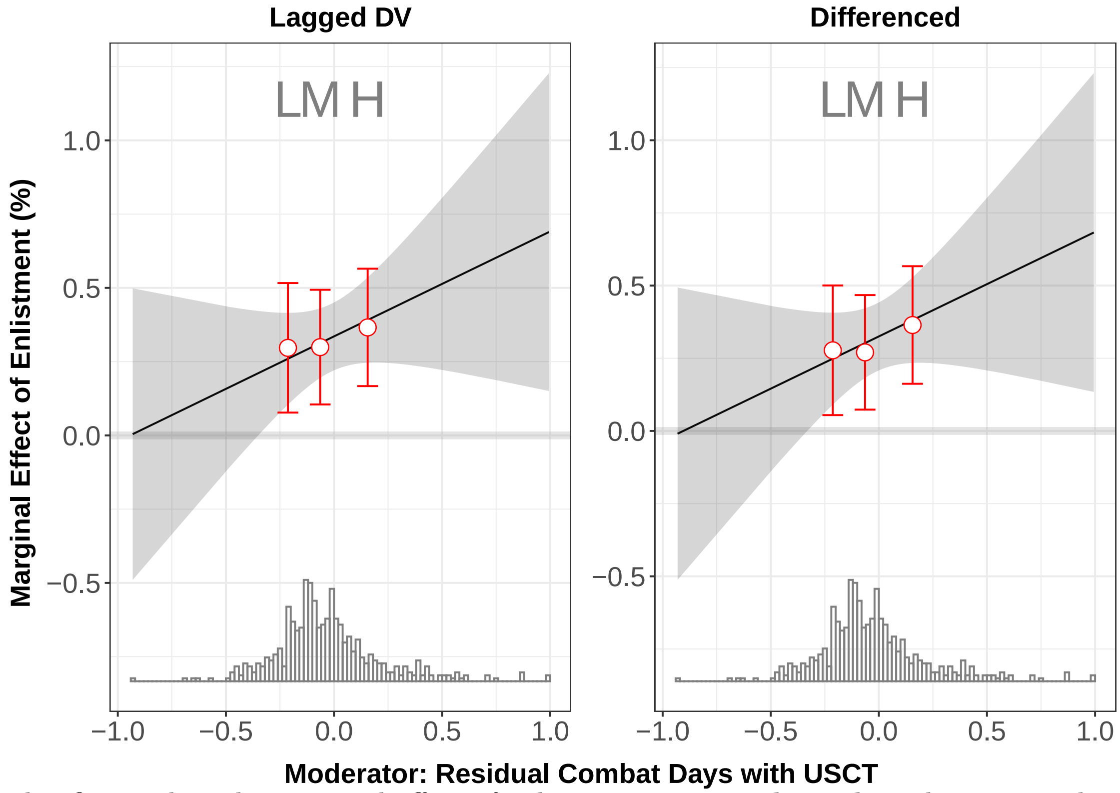
D.1 Demographic and Economic Data

Data on economic and demographic attributes come from the United States census county-level reports available on the ICPSR. Using data from 1860, I calculate the following characteristics of counties:

Demographic: Logged total population, fraction of the population that is military-aged males, the ratio of military age males to females, fraction of the population belonging to religious denominations associated with abolitionism, fraction of females in the manufacturing jobs, fraction of men in manufacturing jobs, fraction of the population that was foreign born, fraction of the population that is black, fraction of the population that lives in an urban

[6] This includes between 54 and 93 townships.

Figure C7: Marginal Effect of Enlistment Rates on Votes for Black Suffrage Conditional on Days of Combat alongside USCT Regiments



This figure plots the marginal effect of enlistment rates conditional on the average days spent in combat alongside USCT regiments on support for Black Suffrage, in the Iowa and Wisconsin state constitutional referenda in 1857, 1865, and 1868. Average days in combat is calculated by taking the number of days a soldiers experienced minus the average days experienced by soldiers enlisting in the same state, same type of unit, for same term, in the same year.

area, fraction of the population that is white, fraction of the population that was born in the South.

Economic: logged manufacturing output, logged manufacturing output per capita, logged agricultural output, log agricultural output per capita, logged agricultural output per acre of a improved land, mean farm acreage, farm value per acre, agricultural property gini coefficient, military-aged males per acre of improved land, per capita agricultural output.

D.2 Election Data

Township election returns for Iowa came from Dykstra (1993) and are publicly available at (Dykstra 2005). Township election returns for Wisconsin come from (?), but the data are not public and were provided by the author.

Because townships changed boundaries over time and election returns were not always available for every township in both elections, I created clusters townships that were stable in both geographic composition and reporting of election returns over time.

To construct these, I used the following procedure:

1. Identify a list of all townships in existence between 1857 and 1868 in Iowa and Wisconsin, using the 1870 Census reports.
2. Identify township boundary and name changes from the same.
3. Create clusters of townships which shared names over time.
4. Link election returns from Dykstra and McManus to townships.
5. For township clusters that have complete information on election returns in 1857 and 1865, calculate vote totals by cluster.
6. Subtract these the sum of vote totals across all complete township clusters from county returns.
7. Create new clusters out of the "remainder" of each county.

D.3 Enlistment Data

I obtain enlistment data by county using the ACWRD except for the case of Indiana. The data from the ACWRD was matched to counties using the following procedures.

- First, I examined the ACWRD to find which states contained residences (not muster locations) for more than 90% of soldiers serving in their regiments. I elected to ignore muster locations as a source of data on residence they are not guaranteed to take place in every county and may have been in more urban areas as well as in areas where there was greater Republican loyalty.
- I then geocoded each residence using online geolocation tools. I first passed each residence to Geolocate, which includes a large number of US place names, and then to Google Maps.
- I then assign men to counties of residence by the 1860 county boundaries in which their georeferenced point is located.
- Finally, I take the enlistment rate to be the number of men counted as serving as a fraction of white men aged 10 to 39 in 1860. These are men that could plausibly have served during the war. Men below 10 in 1860 or over 39 were very unlikely to serve due to age restrictions.

The biggest potential source of bias comes from geocoding. There are likely errors induced by Geolocate or Google Maps excluding or misplacing historical names. The scope of this bias is likely limited. First, for towns and villages that no longer exist or changed names, there are often other features, like roads, in their vicinity that still bear their name. Google Maps frequently picks up these road and other features names as the best match. Second, I am able to test this geocoding technique against "ground truth" data for Illinois. The Illinois

Adjutant General (responsible for oversight of Illinois regiments during the war) produced a report indicating the number of enlisted men "credited'' to each county. While credits sometimes were mis-allocated and some credits reflected local units enlisting men from other states while in the field, this administrative data provides a good check for the quality of the geocoding. The number of soldiers from each Illinois counties, based on the administrative and geocoded date have a correlation of 0.989. The fraction of eligible 1860 males who enlisted, based on these two sources of data, correlate at 0.83. This lends plausibility to using the geocoding procedure for other states.

Finally, I obtain data on county-level enlistment in Indiana through a different source. The Indiana Adjutant General reported enlistments and draftees by county for all periods of enlistment in the war exclude for a short interval between October 1862 and the Spring of 1863. This brief period excludes less than 10% of all enlistments in Indiana during the war, so it has about as good coverage as the geocoding approach.

D.3.1 Township Enlistment Rates

For Indiana and Wisconsin, I obtain enlistment rates for township clusters using the following procedure.

1. I created a list of all townships that existed in the states between their creation and 1870, using the 1870 Census.
2. I linked these townships into clusters connected by boundary changes between 1857 and 1868.
3. I combined these clusters of townships into larger units, if election results were not available for the township cluster in either 1857 or 1865.
4. I then created a list of unique enlistment places for soldiers in ACWRD.
5. For each enlistment place, research assistants matched that place to a county using lists of census townships/villages, records of old post office names in the GNIS database.
6. For each place, research assistants then matched it to township using 1875 maps of each county.
7. Because township names were often repeated in multiple counties, or people listed an entire county as their place of residence, I linked soldiers to each township election cluster they might possibly be in.

After linking soldiers to possible township election clusters, I then used the fastLink algorithm to link them to individuals.

1. Blocking on township-election clusters, I matched soldiers to people residing in those places in 1860 on first name, last name, cleaned first name, sound codes, and age.
2. Using matches with a probability threshold above 0.85, I assigned soldiers to township clusters using the following rules:

- If a soldier's only matches were within a single township cluster, assign him to that township cluster.
- If a soldier matched to multiple township clusters, assign his weight to each cluster in proportion to the probability score for his matches in those clusters.
- If a soldiers was not matched to the 1860 census, but his place of residence uniquely identifies a single township cluster, assign him to that cluster.
- If a soldier was not matched to the 1860 census, and his place of residence links him to more than one possible township cluster, assign his weight to each cluster in proportion to the military aged male population of each township cluster.

D.4 Wartime Experiences

I measure wartime experiences using a few different sources. First, I use the CWDB to identify experiences of combat and casualty rates. The database makes it possible to identify the number of combat injuries or fatalities within a unit on a given day, which makes it possible to count days of combat. It is also possible, because the CWDB includes data on who died (though this suffers from extensive undercounting), to calculate casualty rates for each unit during the war. Second, I also use CWDB to identify which regiments shared brigade duty at various times during the war. Third, the CWDB identifies officers within units and unit membership of individuals, making it possible to measure wartime organizational networks. I also draw on a digitization of Dyer (1908) which provides geographic location of Union Army units over time during the war (Nesbit 2012). This makes it possible to identify how much time veterans spent in counties with different attributes (based on the 1860 census).

using a digitization of Using this data, I created the following measures

- *Exposure to African American Soldiers* I measure exposure to African Americans in the military based on a soldier's proximity African American units — either *(a)* Number of days in which a soldier's regiment saw combat in the same place and time as USCT (African American) regiments. *(b)* Number of day a regiment belonged to the same brigade as African American regiments. Brigades were larger military formations that coordinated the logistics and combat of groups of 2 to 5 regiments and the smallest mixed race units in the Union Army.
- *Wartime Sacrifice*: This is measured as *(a)* regiment-level combat experience (in days of combat) and *(b)* regiment-level casualty rates (combat and non-combat deaths).

E Calculations

E.1 Veteran Share of Electorate

I start with the number of men who served in the Union Army. Army records suggest there were approximately 2.1 million men serving (this accounts for 2.6 million enlistments, subtracting approximately 500,000 reenlistments). I assume that all of surviving men were born citizens or had become citizens by 1870. Of these, 180,000 were African Americans and another 55,000 were from Confederate states. Subtracting these gives a total of 1.865 million men. Deaths due to combat and disease in the Union Army were approximately 360,000.[7] Assuming that the fatality rate is the same for Northern white troops as for Southern whites and the USCT, this suggests that 17.1 percent of Northern white soldiers died.[8] This gives approximately 1.545 million veterans after the war. Census records in 1870 show that there were 6.465 million naturalized white men in Northern states over the age of 21. This gives an estimate that 23.9 percent of the eligible voting population in states outside the Confederacy were Union veterans. In reducing soldier numbers due to re-enlistment, accounting for USCT and Southern Union soldiers, and calculating the distribution of deaths, I chose numbers that would deflate the overall count of white veterans in the North.

E.2 Indiana Effect Size

To give a more intuitive sense of the magnitude of the individual level effects of company casualties, I make a back-of-the-envelope calculation of how changes in exposure to company casualties would have affected the outcome of the 1874 Congressional Elections in Indiana. I do not claim that these calculations are valid counterfactuals. In actuality, Democrats won 8 out of 13 seats, while Republicans held 5.

I first approximate the fraction of the Indiana electorate that was a Union veteran. Using the 1870 Census, I find there are 380636 men in Indiana who were male citizens over the age of 21. Using the ACWRD, I find there were 149 thousand Indiana soldiers who are definitely recorded as having survived the war. This makes soldiers approximately 39 percent of the electorate.

For all Indiana soldiers, I then estimate the average partisan swing that would occur (using the estimates from Table 2, column (3) if I either (i) increased the combat casualties they experienced by one with-in regiment SD or to the maximum observed casualties in the regiment, choosing the smaller of the two; or (ii) decreased combat casualties they experienced by one with-in regiment SD or to the minimum observed casualties in the regiment, choosing the larger of the two. Increasing company casualties in this manner yields a 3.9 point swing

[7]Hacker (2011) estimates higher overall Civil War mortality, but his estimates for people born in Northern and Border states (and thus might be counted as in the "Union") is virtually identical.

[8]If anything, death-rates were much higher for African American regiments.

in the margin toward Republicans. Decreasing company casualties in this manner yields a 3.75 swing in the margin toward Democrats.

I then assume that this average affect can be extrapolated to all Indiana veterans in this manner, that all veterans remained living in the state, that they voted at the same rate as non-veterans, and comprised the same share of the electorate in each Congressional District. These are unreasonable assumptions about an actual counterfactual calculation, but the aim here is to give a sense of the effect size.

Using these assumptions, the increase in company casualties would have generated a 1.5 ppt increase in the overall margin for Republicans. Republicans lost Indiana's first Congressional district in 1874 by a margin of 1.3 ppt. This would have yielded 7 seats for Democrats and 6 seats for Republicans.

Using these assumptions, the decrease in company casualties would have generated a 1.47 ppt increase in the overall margin for Democrats. Republicans won Indiana's eight and thirteenth Congressional district in 1874 by smaller margins. This would have yielded 10 seats for Democrats and 3 seats for Republicans.

And in fact, several other elections had fairly close margins. Slightly larger changes in the company casualty rates would perhaps have given Republicans a majority or only one seat.

Appendix Sources

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