"Let our ballots secure what our bullets have won": Union Veterans and the Making of 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. Though the electoral benefits of African American suffrage were clear, Republicans had to overcome party divisions and racist voters. This paper argues that the war imbued Northern veterans with the belief that true victory required renewing the Union by abolishing slavery and establishing (imperfect) legal equality. This made veterans more receptive to Radical Reconstruction and ignited activism for it from below. Using difference-in-differences, I show that greater enlistment increased Republican vote-share, particularly in pivotal post-war elections. Moreover, "as-if" random exposure to combat deaths increased Republican partisanship among soldiers after the war. Finally, I show that veterans became more likely to vote for African American suffrage. The paper concludes that Union veterans, through their votes and their activism, were a decisive part of the white coalition that backed America's "Second Revolution."

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

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Introduction

In the years following the Civil War, the United States was in the midst of a "Second Revolution" (Foner 1988). A Radical Congress, controlled by Republicans, Reconstructed the South by passing amendments ending slavery, creating rights and protections of national citizenship, and prohibiting the denial of suffrage on the basis of race. To achieve and secure these new rights for African Americans, Congressional Republicans maintained a legal state of war against much of the South and then passed sweeping enforcement legislation that greatly expanded the power of the federal government (Downs 2015; Wang 1997). Despite the eventual erosion and elimination of many Reconstruction policies, this "unfinished revolution'' meaningfully improved the lives of freed people (Stewart and Kitchens Forthcoming; Logan 2018; Chacon and Jensen 2020; 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 for the fact that, despite widespread racism in the antebellum North, it was achieved through broadly free and fair elections. How did this happen? The dominant explanation suggests that the Republican party, facing the risk that restored representation to the White South would empower the rival Democratic party, strategically extended suffrage rights and protections to African Americans in order to build a viable national coalition to retain control of the federal government (Valelly 2004). This mirrors the process by which suffrage and civil rights for African Americans were restricted in order to meet the demands of forging the Democratic party into a successful national coalition that included Southern slaveholders (Bateman 2018). These twin narratives are consistent with arguments from comparative politics suggesting that the logic of electoral competition between rival groups of elites or political parties drives the expansion and contraction of the electorate (Llavador and Oxoby 2005; Teele 2018; Capoccia and Ziblatt 2010). According to the logic of the competition narrative, Republicans pursued these drastic reforms not out of an ideological commitment to equal civil and political rights, but out of partisan teamsmanship (Lee 2009) aimed at forestalling the return of Democrats to national power.

Moreover, some have argued that this instrumental and "thin" support for Reconstruction helps explain its eventual erosion.

While the electoral interests of the Republican party were undoubtedly relevant, the competition narrative leaves much unexplained. To begin with, the need for Republicans to expand their coalition in the South was obvious before the Civil War, yet Republicans only came to embrace radical reforms after the war, and then only reluctantly and fitfully (Downs 2015; Wang 1997). The fact is that Republicans were deeply concerned that giving civil and political rights to African Americans would be "political suicide" due to the racism of the Northern electorate (Wang 1997, 6). Their fears were well founded: recent evidence shows that such racial conservatism is durable (Acharya, Blackwell and Sen 2016), and that racial conservatives often abandon parties when they begin to endorse racial liberalism (Kuziemko and Washington 2018). How, then, were white voters convinced to support a Republican party moving forward with Radical Reconstruction? Second, if Republican elites initiated Reconstruction to build a national coalition, we would expect the push for suffrage expansion to start with national Republican leaders. Yet, it was state-level Republican activists who pushed forward the issue of equal suffrage at the end of the war, to the consternation of party leaders worried about the party's viability nationwide. Why did these local Republican party activists come to embrace an aggressive Reconstruction agenda?

To answer these questions and explain how the Republican party came to pursue Radical Reconstruction, I show that the wartime experiences of the nearly two million Union Army veterans instilled in many a deep sense that the purpose of their sacrifice was to renew the Union by punishing traitors, eradicating slavery, and, to an extent, extending equal civil and political rights. Compared to civilians, Union soldiers' unique sacrifices during the war, including in combat, reduced willingness to reconcile with the enemy (e.g. Grossman, Manekin and Miodownik 2015) and intensified their commitment to "win the peace'' (Gannon 2011; Janney 2013). Soldiers' first-hand experiences with slavery convinced many of both the strategic and moral imperative of eliminating the institution, ensuring that

emancipation was enshrined, alongside preserving the Union, as one of the twin accomplishments of the war (Manning 2007). Finally, through extensive collaboration with enslaved and freed African Americans in their fight against the Confederacy, many white Union soldiers came to believe that African Americans had earned new rights and protections through their loyalty and sacrifice.

This remarkable "political learning" (Parker 2009) among veterans helps explain the electoral success of the Republican Party in the critical elections from 1864 to 1868 that decided the trajectory of Reconstruction. While both Democrats and Republicans sought to frame the meaning of the war and the aims of victory (Kalmoe 2020), Republican campaign messaging aligned closely with how veterans and their organizations understood the meaning of the war. Motivated to secure the fruits of their hard-won victory, veterans were particularly likely to find elite messaging from the Radical wing of the Republican party resonant and persuasive. Given that white Union veterans made up a large share of the Northern electorate after the war, they were a pivotal constituency for Republicans.

Moreover, Congressional Republicans were moved to embrace a more expansive Reconstruction agenda in response to mobilization by veterans from below. Veterans had new capacities for organization (Jha and Wilkinson 2012), and became a constituency active in promoting a more radical Reconstruction agenda. Mobilization by veterans organizations during electoral contests and the entrance of veterans into the rank and file positions within the Republican organization likely increased the sway of the radical faction within the party.

I substantiate this argument first by presenting historical evidence for ideological changes in Union veterans over the course of the war. I then empirically test whether soldiers came to back Republicans. 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 as well as townships in Iowa and Wisconsin. Using a continuous difference-in-differences design to identify the effect of enlistment rates on Republican vote-share, I find that higher enlistment 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.

To demonstrate that these ecological estimates reflect effects on soldiers, I examine the *intensive* effects of wartime sacrifice on individual combatants, focusing on exposure to combat deaths. I expand the work of DeCanio (2007) by digitizing the partisanship of nearly thirty thousand people residing in nine Indiana counties in 1874, and link partisanship data to the service records of more than twenty thousand Union soldiers from those counties. 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 variation in casualties across companies from the same regiment was plausibly as-if random. I exploit this natural experiment to show that, within the same regiments, soldiers in companies with higher casualties became more likely to identify as Republicans after the war, reflecting their greater motivation to make their sacrifice meaningful.

I then connect these findings to the broader political struggle over Reconstruction. It is true that some soldiers may have backed Republicans after the war merely because they could not stomach voting for Democrats tainted by their affiliation with secessionists. However, I show that veterans also came to *directly* support civil rights expansions. Using a difference-in-differences design, townships in Iowa and Wisconsin with greater enlistment saw significantly larger increases in support for black suffrage in the post-war state referenda. Ecological bounds show these were indeed effects on veterans. This account is bolstered by qualitative and quantitative evidence demonstrating how the discursive and electoral activities of veterans and their organizations put pressure on legislators from below to back Radical Reconstruction measures.

Taken together, the evidence presented below has important implications for how we understand Reconstruction and the political development of race in the United States. For

example, focusing on the simultaneous expansion of white suffrage and contraction of black suffrage in the early Republic, Bateman (2018) argues that transformations in rights and citizenship depend on building winning electoral coalitions that share a vision of what it means to be a nation. This paper extends that logic to Reconstruction. Civil rights expansions became politically possible, not merely due to the strategic opportunity they offered to the Republican party, but because a new white constituency committed to ending slavery and punishing traitors had been forged in the crucible of the war. Across the longer arc of American history, coalitions built around white supremacy have been easier to cultivate and sustain. It is therefore all the more important to explain how the white coalitions that supported making African Americans citizens in the 19th and 20th centuries (see, e.g. Schickler 2016) emerged. Understanding how soldiers acquired a new vision of the place of African American citizens in the Union and how they translated that vision into politics will help us better understand the nature and limits of political support for Reconstruction.

I proceed by first complicating the view that Republicans pursued Radical Reconstruction for electorally strategic reasons, documenting how national leaders feared civil rights and suffrage would divide the party and lose elections. I then argue that military service turned veterans into an important constituency for Republicans. I provide qualitative evidence for political learning among white soldiers and the resonance of Republican messaging with these views. I then describe and report results from statistical tests showing that military service increased support for the Republican party and for their Reconstruction policies. I conclude by discussing the implications of this paper for understanding the political development of race in the United States, suffrage extensions, and the political legacies of wars.

Background

Conventional accounts of Reconstruction in political science claim that Republicans in Congress, facing the reentry of the South with greater representation, strategically allied with African

Americans to preserve their hold on the government. There is undoubtedly some truth to this. Before the war, Republicans won virtually no votes in the states that seceded. The 13th Amendment, freeing millions of enslaved African Americans, had the consequence of *increasing* seats apportioned to the South. With the seating of Southern delegations, it was clear that Republicans' hold over the national government would become precarious. These fears were well founded: as Bateman, Katznelson and Lapinski (2018) document, once Southern Democrats reentered Congress, they decisively shaped federal legislation for decades to come. And while, as early as 1865, some Republicans acknowledged that this dilemma might necessitate the enfranchisement of freedmen in the South (Bonadio 1970; Wang 1997), there are four problems with the partisan competition explanation for the emergence of Radical Reconstruction.

First, despite the obvious incentives, Republicans did not rush to pursue an expansive Reconstruction agenda. Instead, they came to it in fits and starts, avoiding suffrage and focusing initially on securing African Americans civil rights and enforcing the end of slavery (Wang 1997; Downs 2015).

Second, this account ignores the very real possibility that Republicans could have embraced an all-white coalition, rather than back suffrage for African Americans. At its formation barely ten years earlier, the Republican party was an uneasy coalition of factions with conflicting reasons for opposing slavery. And while many in the radical wing sought to pair an end to slavery with civil rights, this was a minority position that often divided the party (Bateman 2020); moderate and conservative Republicans opposed slavery for its deleterious effects on whites and hoped the colonization of freed people elsewhere would solve "the race problem'' (Foner 1979). By 1865, with the abolition of slavery achieved, rumors of party realignment were rampant, and many conservative and moderate Republicans expressed the desire to cut the Radicals loose from the party (Cox and Cox 1963; Bonadio 1970; Cook 1994). This was motivated by disagreement with radicals' insistence on the full inclusion of African Americans as citizens, which conservatives derided as both a cause of the rebellion

and a fatal political liability (Cox and Cox 1963, Ch 2).

Conservative Republicans, including sitting US senators, governors, and powerful state party bosses, envisioned an alternative all-white coalition composed of conservative and moderate Republicans, War Democrats, and white Southerners loyal to the Union to the exclusion of Radicals who had "the negro on the brain'' (Bonadio 1970). These groups sought to forge this realignment with the help of President Andrew Johnson, a southerner and former Democrat who had opposed slavery and backed the Union during the war. Between 1865 and 1866 this effort was sustained in earnest: Johnson and his conservative allies held numerous meetings and engaged in extensive correspondence that resulted in coordination during state elections, with Democrats endorsing Johnson's Reconstruction policy, and Johnson using patronage jobs to empower conservatives (Cox and Cox 1963). During the 1866 Congressional election, these efforts culminated in a National Union party convention, which attracted Democrats and Republicans to coordinate a campaign against the radical efforts of Republicans in Congress (Riddleberger 1979).

Third, regardless of whether Republicans genuinely accepted the conservative position that restoration of the Union and the formal abolition of slavery had completed the war aims, moderate Republicans were deeply concerned about what policies a racist Northern public would support. While majorities of Republicans in two states voted in favor of black suffrage in pre-war referenda (Bateman 2020), the majority of *voters* rejected suffrage, and, in most states, Republicans did not dare to put the issue on the ballot (Dykstra 1993). After the war started, Illinoisans voted overwhelmingly to restrict blacks from entering the state and forbid them from voting (Allardice 2011, 101), and even in 1865, referenda in Connecticut and Wisconsin failed. All the while, Democrats made explicit racist appeals to voters, accusing Republicans of plotting to invert the "natural'' supremacy of whites by setting African Americans as civic and "social" equals (implying inter-racial relationships) (Dykstra 1993; Kalmoe 2020), and that, 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 the failure of suffrage referenda in particular. They waited until after the convincing victory in the 1866 elections to start a gradual push to extend suffrage to African Americans (Wang 1997).

Finally, when Republicans began publicly pushing for Radical Reconstruction measures, including suffrage, it came from activists in the state and local parties. Nationally-minded party leaders sought to steer a more moderate course. In the summer of 1865, state party leaders and Congressional representatives in Ohio, Wisconsin, and Iowa worked to keep African American suffrage off of the party platform in the fall elections, fearing that it would drive a wedge between the party and President Johnson and cost the party votes. But local party activists in Iowa voted the issue onto the platform, and despite maneuvers to prevent this in Wisconsin and Ohio, these states saw substantial mobilization by radicals to force the issue into the campaign (Cook 1994; Bonadio 1970; McManus 1998). When Congress began legislating Reconstruction policy in early 1866, many Congressional Republicans were upset by Johnson's veto of the Freedmen's Bureau act and the Civil Rights Act. However, they were also concerned about party infighting just before the upcoming Congressional elections, and so they strove to publicly downplay any break with the President and hoped to salvage a compromise with him (Bonadio 1970; Cox and Cox 1963). By contrast, immediately after Johnson vetoed the Freedmen's Bureau Act, Republican state legislatures passed resolutions that explicitly called on Congress to override the veto, openly criticized Johnson, and in the case of Wisconsin, censured one of the state's Republican senators for siding with Johnson (Cook 1994; McManus 1998). Furthermore, that summer, Republican House members faced challengers with more radical bona fides in nominating conventions, pushing them to more openly challenge Johnson and his policies (Bonadio 1970).

All of this points to the period of 1865-1866 as an important "counterfactual node'' (Bateman and Teele 2020)—a point at which the course of Reconstruction could have taken

a more conservative direction. Had moderate Republicans embraced a coalition with Johnson or had voters, persuaded by racist appeals, handed Republicans a minority or reduced majority of seats in the fall of 1866, Reconstruction may well have ended without even the ratification of the 14th Amendment. Yet this did not come to pass. Instead, between 1865 and 1866, moderate Republicans rejected a conservative coalition and embraced more radical positions when Johnson's conservative plan yielded Southern statehouses filled with former secessionists, the return to de facto slavery through "Black Codes",[1] and mass violence against freed people in Memphis and New Orleans (Foner 1988; Wang 1997). And despite this increasingly radical trajectory, Republicans won elections by large margins that fall.

Argument

Missing from these accounts is the key role played by Union veterans. I argue that experiences in the war imbued veterans with a commitment to a vision of the Union that extirpated slavery, and to an extent, incorporated African Americans as citizens. This made white Union veterans both more receptive to Republican arguments in favor of Congressional Reconstruction and more likely to mobilize for Reconstruction from below.

Wartime Experiences

There is broad historical consensus that enlistment in the Union Army was not driven by commitments to end slavery. Soldiers routinely 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 (white) democratic self-government (Manning 2007; Gallagher 2011).

Yet, military service was a life-altering event for Union veterans (Costa and Kahn 2008).

[1]These were laws passed by Southern state governments that restricted the labor and movement of freed men and women under penalty of imprisonment.

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 can 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 primed for political transformation.

Sacrifice Serving in the Union Army transformed soldiers in three ways that made them more likely to be a constituency for Reconstruction. First, compared with other Northern citizens, Union soldiers made much greater sacrifices in the war. While Faust (2008) argues that the scale of death in the Civil War was felt at home, Marshall (2014) shows 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 during and after combat. Experiments in social psychology show that making sacrifices for a cause can intensify commitments to that cause, while military service (Koenig 2020) and combat experience in particular (Grossman, Manekin and Miodownik 2015) have been shown to produce intense antipathy against former enemies. Thus, Union veterans had more at stake in ensuring that their sacrifices during the war were meaningful, and in particular, that their hard-won victory over the South was secure. And for decades after the war, veterans worked to remind an increasingly disinterested public of their sacrifice in the war (Janney 2013; Cook 2021).

Meaning of the War As one soldier asked: "If all this untold expense of blood and treasure, of toil and suffering, of want and sacrifice, of grief and mourning is … to result in no greater good than the restoration of the Union as it was, what will it amount to?'' (Manning 2007, 84). Through their service, many soldiers had come to understand that, alongside preserving the Union, the abolition of slavery was the central achievement of the war and gave their sacrifice meaning (Janney 2013; Gannon 2011). Prior to the war, few

Union soldiers had first-hand experiences with slavery. Evidence from diaries and letters suggests that when soldiers saw slaves in person, it elicited moral outrage—among other reasons, many were convinced that slavery eroded the virtues and civic institutions necessary for republican government—and roused the strategic recognition that slavery would have to be destroyed to win the war and prevent future conflict (Manning 2007, Ch 2–4). Thus we find soldiers putting to pen sentiments like this:

> the rebellion is abolitionizing the whole army. You have no idea of the changes that have taken place in the minds of the soldiers in the last two months; indeed, men of all parties seem unanimous in the belief that to permanently establish the Union, is to first wipe [out] the institution [of slavery]. (Manning 2007, 45)

As the human cost of the war mounted, soldiers found meaning for the bloodletting in the Christian salvation of the Union through what Lincoln called "a new birth of freedom" brought about emancipation (McConnell 1992; Hunt 2010; Gannon 2011; Janney 2013).

Some historians have questioned whether soldiers sincerely came to support emancipation during the war (Gallagher 2011; White 2014). Yet, there is considerable evidence that this support was present just after the war in the campaign slogans of veterans' groups during the elections of 1866 and 1868: "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—"the Won Cause" (Gannon 2011)—decades later: At "Blue and Gray" reunions, Union veterans never relinquished the moral supremacy of their cause, and they ardently disputed the Southern "Lost Cause" narrative of the war, which denied the moral achievement of emancipation and the Civil Rights Amendments (McConnell 1992; Gannon 2011; Janney 2013; Cook 2021). For white Union veterans, "winning the peace'' included more than restoring the Union: it also entailed bringing an actual end to slavery.

Collaboration Third, Union soldiers actively collaborated with enslaved and freed African Americans in their fight against the Confederacy, leading some veterans to acquire new attitudes about race and civil rights (see, e.g. White 2016). While there is limited evidence that "contact" *as such* reduces racial prejudice, its effects are stronger when contact is prolonged, socially condoned, and toward a shared purpose (see, e.g. Mo and Conn 2018), as in the case of collaboration between Union soldiers and African Americans against the Confederacy.

Most white Northerners had limited or no interaction with African Americans, whereas Union soldiers who spent time in the South frequently met enslaved and freed African Americans. They 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 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, therefore, either knew of or fought alongside African American regiments. Though some initially opposed the formation of the USCT, this resistance faded (Manning 2007), and some Union veterans came to believe that, through their loyalty on and off the battlefield, African Americans had earned status as citizens (Gannon 2011). One officer exclaimed that "seeing 115,000 colored...soldiers fighting equally...for our common country'' made him believe that "the colored man'' should be "ELEVATED'' (Manning 2007, 192), while another soldier wrote that "the slaves have been our only friends'' which "entitles them to their freedom, or whatever they desire'' (Hunt 2010, 95).

These beliefs in equality were persistent, if limited. For decades after the war, the Grand Army of the Republic (GAR)—the largest Union veterans' organization—in contrast to almost all other social organizations of the time, 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. 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. Still, it would be wrong to paint to rosy a picture of this post-war integration: African American members held only symbolic offices (McConnell 1992), and pleas from African American veterans in the South for the GAR to take a stand against the rise of lynching and Jim Crow laws went unanswered (Gannon 2011).

Electoral Consequences

More than 2 million men served in the Union Army during the Civil War, proportionally, the largest mobilization in US history other than the Second World War. Because women were denied suffrage and most Southern states were denied representation in the first peace-time elections, Union veterans constituted an outsized share of the post-war electorate. Under conservative assumptions (see SA E.1)), I estimate that, by 1870, nearly 24 percent of eligible voters in the North were veterans. Because veterans' understanding of the war resonated with Republican framing of Reconstruction, they proved to be a pivotal constituency for Republicans in post-war elections.

Both Republicans and Democrats sought to win soldiers to their side. At the height of the war, Democratic newspapers framed the conflict as a partisan effort by Republicans that wasted the lives of thousands of soldiers. They made extensive appeals to white supremacy, accusing Republicans of making emancipation the prime goal of the war and arguing that preserving slavery could obtain peace and restore the Union with no further loss of life. By contrast, Republican newspapers railed against rebels and the treason of their alleged Northern allies, and portrayed the war as a sacrifice in the service of a noble cause. Nearly all Republican papers endorsed emancipation—though few actually suggested it was the central objective of the war—and most came to embrace military service by African Americans (Kalmoe 2020, Ch 5). In short, Republican framing closely paralleled the understanding of the war that soldiers had acquired, while Democratic frames alienated even Democrats

in the ranks (Manning 2007, 150–3). Whether Republican frames informed soldiers' self understanding or the other way around, Republican campaign messages were far more likely to resonate with soldiers.

In the aftermath of the war, Democrats adopted an electoral strategy of courting veterans by splitting support for the Union from civil rights. They put forward as candidates "War Democrats'' who had joined Republicans in 1864 and backed slates of ex-officers candidates in so-called "soldiers' parties,'' with names like the "Union Anti-Negro Suffrage Party" and platforms that called for payments to veterans and praised white soldiers for saving the Union while denouncing civil rights reforms (Dearing 1952, 66) (Dykstra 1993; Field 1982).

By contrast, Republicans in this period earned a reputation for "waving the bloody shirt.'' Dearing (1952) recounts numerous cases in which Republican candidates and former military commanders alerted veterans in campaign speeches 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. Republican newspapers and campaign speeches drew attention to 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 as evidence of the resurgent political power of the former Confederacy (Riddleberger 1979). While Republicans, who shared the Democrats' belief that soldiers harbored racial animus, initially sought to downplay issues of African American rights, this did not last. Civil rights, including the 14th Amendment, were justified to the public as morally right, necessary to ensure a de facto end to slavery, and as a way to prevent over-representation of the South (Wang 1997).

A sample of 20 Republican newspapers in Wisconsin (SA Section D.5) shows that, even when national and state leaders kept support for black suffrage off the party platform in 1865, 90% published overwhelmingly pro-suffrage content. 85% expressed that suffrage had been earned by African Americans through their loyalty, an idea found in 35% of pro-suffrage coverage. 50% of papers justified suffrage as a means to secure war aims like ending slavery

and punishing Southern treason. By contrast, only 35% of Republican papers, and 5% of pro-suffrage articles framed suffrage as a strategic necessity to prevent rebels from returning to power.

Moreover, electoral evidence supports the idea that Republican campaign messaging resonated with Union vets. Union Army soldiers voting in 1864 supported Lincoln over his Democratic rival by a margin of 78 to 22, breaking toward Republicans by 25 points more than voters at home (White 2014). Meanwhile, Democatic "Soldiers' Parties" lost by wide margins in 1865 (Dykstra 1993). Thus, even though Republicans pursued policies that risked alienating many Northern voters, they likely picked up pivotal votes from returning veterans.

Party Activists

Veterans not only came to support the Republican party, but also played important roles shaping the party from within. Over the course of the war, many Democratic office-holders who served in the Army became Republicans; and after the war, Union Army veterans filled party caucuses. Out of the 97 legislators elected to the Iowa General Assembly in the fall of 1865, 37 were veterans; 33 of them had never held office before and 34 were Republicans. But more importantly, veterans' organizations became a vital part of Republican grassroots mobilization.

Given the repugnance veterans had for Democrats, voting for the Republican party might have been a choice for the "lesser of two evils." Yet, veterans' decisions to join social organizations, where they had more choice, revealed strong commitments to Radical policies. While many soldiers joined apolitical veterans' groups, the largest and most important—the Boys in Blue, the Soldiers' and Sailors' National Union League, GAR, and the White Boys in Blue—took explicitly political positions on the issues of the war and Reconstruction (Dearing 1952, 80–123). All but one of these major veterans' organizations backed Radical Republicans.

Of the Union veterans organizations, the GAR was the largest, longest lasting, and

most important. Every Republican President from Grant to McKinley was a member, and, at its height in the 1890s, its membership reached 500,000 (McConnell 1992). Founders and leaders of the GAR understood that political mobilization to protect war-time gains was the heart of the organization (McConnell 1992), leading them to rally behind Radical Republicans. This appears to have animated grassroots membership as well: the GAR grew rapidly despite its sharply political reputation; its leaders and historians attributed the rapid atrophy of the organization after 1868 to flagging motivation after the achievement of Radical Reconstruction (Dearing 1952; McConnell 1992).

During the elections of 1866 and 1868, the GAR and other veterans' organizations were an integral part of Republican voter mobilization: they canvassed through parades and mass meetings, attended conventions, and maintained campaign clubs (McConnell 1992; Dearing 1952). In September, veterans' groups from across the North sent 15-25,000 delegates to a convention in Pittsburgh that attacked President Johnson as a traitor, endorsed the 14th Amendment and Congressional power over the South, and called for the protection of freed men and women (Dearing 1952; Cashdollar 1965). Newspapers and returning delegates carried this message to voters. James Blaine—a leading House Republican—credited the convention with "consolidat[ing] almost en masse the soldier vote of the country in support of the Republican party" and the ratification of the 14th Amendment (Cashdollar 1965). When the GAR professed partisan neutrality in 1867, its commander—House Republican John Logan—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). And in the 1868 campaign, there was active coordination at the national level between the Republican party and veterans' organizations, including the GAR (Dearing 1952).

Veterans' organizations generated and disseminated discursive frames that memorialized the sacrifices made by soldiers, gave meaning to that sacrifice, and warned of the threat posed by their former enemies. It was the GAR that first popularized the observance and rituals of Memorial Day. During elections, veterans' organizations warned their members

that former Rebels and their Copperhead allies who "yesterday were using the bullet to overthrow the government, [and] to-day ... are using the ballot to control it" (Dearing 1952, 150). They called upon their members to win "another victory at the ballot box" to avert this "calamity" (Dearing 1952, 117).

In sum, though a radical faction that embraced abolition and civil rights for African Americans existed in the Republican party before the war (Bateman 2020), it was not obvious that they could win the support of moderate Republicans or voters. But large numbers of veterans exited the war with a new vision of the Union: where slavery was abolished, traitors punished, and those loyal to the Union, including African Americans, endowed with equal rights of citizenship. Following a logic similar to the process of African American disenfranchisement outlined by Bateman (2018), these veterans and their organizations comprised a new constituency within the Republican party that made a political coalition for Radical Reconstruction electorally and politically viable.

Testing the Argument

Despite compelling qualitative evidence, there are reasons to take pause. Historical explanations for the electoral success of Republicans in the key elections of 1866 and 1868 make almost no mention of veterans (Foner 1988; Riddleberger 1979). Some historians dispute the claim that soldiers' views on slavery and race changed during the war (Gallagher 2011), and others argue that the 1864 soldiers' vote is evidence of partisan selection into service rather than of growing support for Republicans and emancipation (White 2014). Moreover, statistical analyses show that enlistment was higher in states and counties with greater pre-war Republican vote-share (Costa and Kahn 2008, 52–3) (Kalmoe 2020). At the same time, data on the individual attitudes and votes of soldiers and citizens are not available.

To address these concerns, I triangulate evidence from statistical tests of different implications of my argument. First, I examine whether veterans became *more likely* to vote

for the Republican Party after the war at both an ecological and individual level. Then, I explore whether veteran support for Republicans actually reflected agreement on the issues of Reconstruction. Finally, I examine whether constituency pressure by veterans drove Republican legislators to embrace more radical legislation.

Design

Enlistment Rates and Republican Voteshare I first show that, at an ecological level, higher enlistment rates in the Union Army caused an increase in voting for Republicans. I identify this effect 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, addressing concerns of partisan selection into service. 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 (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). α_i is a county-level fixed effect, and α_e is a state-election fixed effect, which imposes the assumption of parallel trends within states. Enlistment Rate$_i$ is constant within counties and Civil War$_e$, is constant within state-elections, so only the interaction remains in the

equation and is captured by the parameter β (Angrist and Pischke 2008, 233–4). 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 "end" of Reconstruction.

In SA Section 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.

Combat Experience and Individual Partisanship These ecological effects could be consistent with increased Republican voting among *non-veterans* in high-enlistment areas, rather than among veterans.[2] To demonstrate that the war caused soldiers to vote Republican, I also examine the effects of service on individual partisanship.

It would be natural to compare the partisanship of those who served versus those who did not, but this is fraught with problems. (i) The effect of enlistment on partisanship is likely to be confounded, while data on conditioning variables for this time-period are limited and noisy. (ii) While there were "random" draft lotteries during the war, few men were actually drafted, there was extensive two-sided non-compliance, and uncovering the pool randomized into service would be extremely difficult. (iii) War-time service drastically increased post-war mobility, producing differential attrition. Solutions for this attrition would involve conditioning, negating the advantages of the lottery.[3]

[2]This, however, would be consistent with my argument that veterans mobilized votes for Republicans.

[3]Similar problems arise for using regression discontinuity around enlistment age-cutoffs.

Instead, I examine the *intensive* effects of wartime sacrifice on soldiers. Not only does this permit estimating effects on all, rather than a small subset, of soldiers, it tests a key mechanism in my argument: Soldiers who made greater sacrifices should have been more motivated to secure the victory won in the war, making them more receptive to Republican framing of Reconstruction, and thus more likely to vote Republican.

I operationalize wartime sacrifice as exposure to combat casualties, and identify its causal effect on the post-war partisanship of individual soldiers using a natural experiment. Exposure to combat was driven by selection at several levels. First, men chose when to enlist, their term of enlistment, the kind of unit (infantry, cavalry, or artillery), and their 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 and to more or less risky duties depending on their experience, morale, and reliability. At each stage, soldiers' partisanship may have driven selection (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 decision on when and

how they would serve, traveled through and fought in the same places. 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.[4]

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 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 other 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 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

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

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 baseline partisanship of soldiers based on demographic characteristics of name, birth year, and birthplace (See Section B.4). I show that companies with greater casualties are balanced in mean 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 and official records, 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.[5] I then match these residences to counties in 1860. I compute the county-level enlistment rate by dividing the number of 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, 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.

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

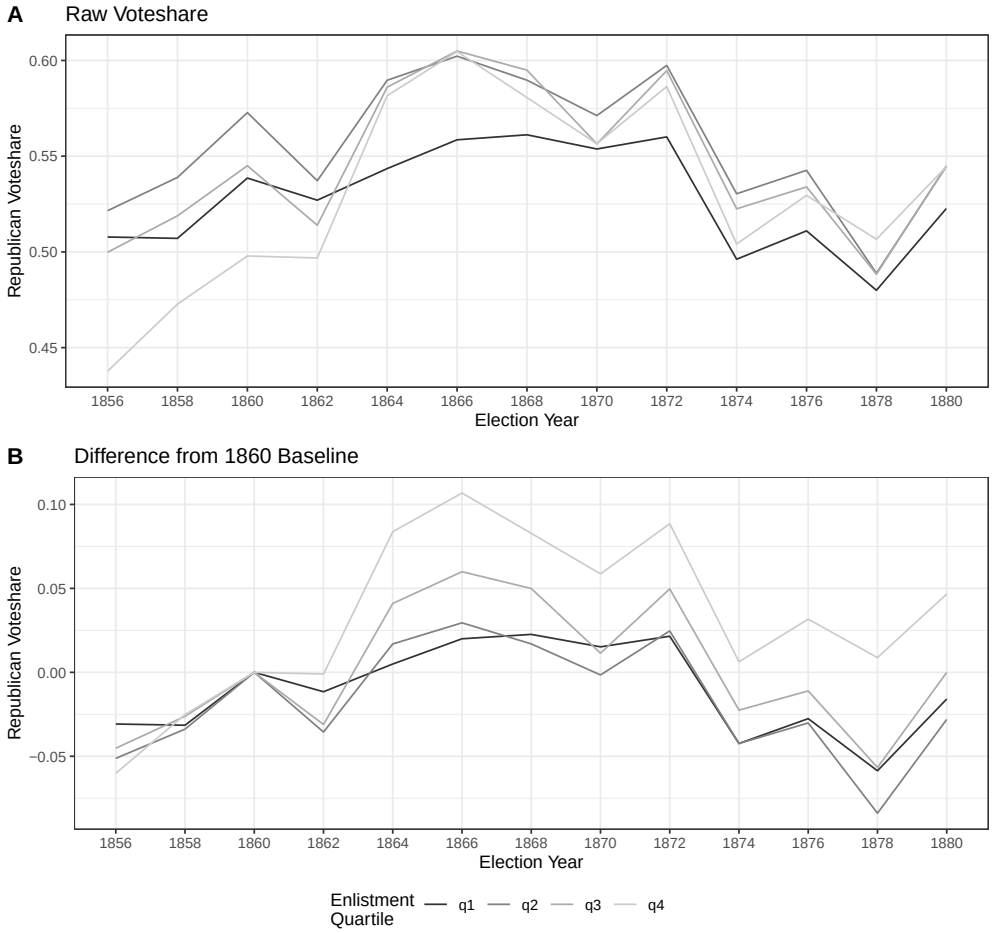
Elections: Black Suffrage I measure direct support for Reconstruction policies using votes in state referenda to remove the word "white" from the qualifications for suffrage in the state constitutions in Iowa (1857 and 1868) and Wisconsin (1857 and 1865). Other states held similar referenda, but either 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.

Service and Individual Partisanship First, I measure individual 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.

Veterans and Voting Republican

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 quartiles. 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.

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 from 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.[6] 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). Furthermore, the effects of enlistment were higher in counties where the average soldier experienced higher regimental casualties, which is consistent with my argument that sacrifice made soldiers more receptive to Republican messaging (A6).

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 vote-share 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 in GOP vote-share between 1860 and every other election from 1854 to 1920. In pre-war elections, these differences are not significantly different from 0.

Second, it could be that baseline differences between high and low enlistment counties affected how they responded to the war, producing time-varying confounding. Figure A2 shows the within-state relationship between enlistment and 32 pre-war demographic, economic, and political covariates. Demographically, enlistment was higher in counties with smaller populations, fewer white people, more southern-born, and more men. Economically, enlistment was higher where agricultural output was less and more people worked in manufacturing. Politically, enlistment was higher where the Republican party and its antecedents had performed worse. Yet, the results are substantively the same when including interactions between each covariate and the post-war indicator (see Figure A3 and Table A1).

Third, and alternatively, if the effects of enlistment were heterogeneous across counties with different levels of enlistment, estimates might be biased. I address this concern by flexibly modelling heterogeneity in the DD effect of enlistment for each county, weighting

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

cases based on similarity in pre-war covariates. The average partial effect of enlistment across all counties is substantively the same (See Section A.4).

Table 1: Difference-in-Differences Estimate of Effect of County Enlistment Rate on Republican Voteshare

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Enlist % · Postbellum	0.415^{***}	0.353^{***}	0.336^{***}
	(0.095)	(0.065)	(0.055)
No Rep. Candidate			-0.537^{***}
			(0.035)
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

Data from Congressional and Presidential elections across 384 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.

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[7] increases support for Republicans by 0.9 percentage points ($p = 0.025$), decreased support for Democrats by 1 percentage point ($p = 0.005$), and swung the margin toward Republicans by 1.9 percentage

[7]The SD of company combat deaths within regiments is 2.05.

points ($p = 0.008$). These results remain virtually unchanged when including covariates drawn from enlistment records (columns 4–6) and when further adding Census covariates (columns 7–9).[8] Consistent with the idea that support for Republicans was tied to viewing emancipation as an achievement of the war, the effects of casualties were larger for soldiers who enlisted after the Emancipation Proclamation (See Figure B14).

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 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).

These effects are 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) these effects persist in 1874, which was nearly 10 years after war and a year of tremendous electoral losses for Republicans.

Interpretation Taken together, these analyses, alongside the historical evidence, demonstrate that wartime service turned Union soldiers from Democrats into Republicans. These wartime changes resulted in large and sustained increases in support for Republicans at the polls, particularly in vital elections that determined the fate of Reconstruction.

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

Table 2: Effect of Company Casualties on Post-war Partisanship (Census-Linked, Best Matches)

	Dependent variable:								
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	Baseline			Army Controls			All Controls		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Company KIA	0.009*	−0.010**	0.009**	0.010*	−0.008*	0.009*	0.009*	−0.007*	0.008*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(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	3,559	3,559	3,559	3,559	3,559	3,559	3,559	3,559	3,559

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 3105 individual soldiers, serving in 538 companies, across 211 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Observations are weighted by 1 over the number of post-war matches for each unique soldier.Standard errors are clustered by company. Full results reported in the Supplementary Tables.

It is difficult to dismiss these results as the result of selection by Republicans or racial liberals into service. There is no reason to believe there were large numbers of racial liberals outside of the Republican party who could have flocked to the party afterwards. Nor can it be that racial liberals enlisted at higher rates, and remained loyal to the party while moderates and conservatives defected over Radical policy changes. Republican vote-shares in 1864 through 1868 *increased* over 1860, but increased *more* in places with higher enlistment.

Even the individual effects are 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).

Between the changes in veterans themselves and their campaign mobilization, they provided Republicans the votes they needed to pursue and secure their Reconstruction agenda.

A Constituency for Reconstruction

Even if veterans voted Republican, does that mean they were a constituency for Reconstruction? Or did they vote Republican for other reasons?

Patronage? 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.

Lesser Evil? 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. Democrats may have been tainted by association with Southern Democrats who led secession and Northern "Copperheads" who had opposed the war and pushed for a negotiated peace with "traitors" (White 2014). Yet, as the failure of "soldier's parties" to attract support and the overwhelmingly Radical hue of veterans' organizations suggest, veterans were interested in securing many of the same goals as Republicans.

Voting for Suffrage

The extension of suffrage to African Americans was one of the most radical reforms adopted during Reconstruction. This makes suffrage a strong litmus test of increased veteran support for the policy agenda of Radical Reconstruction. I estimate the effect of military service on voting for suffrage in pre- and post-war referenda in Iowa and Wisconsin. Table 3 reports the result of difference-in-differences and lagged-dependent variables ecological regressions. Both designs yield virtually identical results: veteran support for suffrage increased by 31 and 32 percentage points, compared to those who remained home ($p < 0.001$). These results persist when 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 fraction of Republicans who did not support suffrage 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 (SA Section C). The central problem with ecological regression is that there may be "contextual" effects: in our case, the change in support for suffrage among soldiers and civilians may differ across areas with different enlistment rates. Aggregation makes it impossible to identify this pattern, potentially biasing ecological estimates. I follow Jiang et al. (2020) and partially identify contextual effects and the individual effects of

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

	Dependent variable:					
	Yes (Post)	Yes (Diff.)		Yes (Post)	Yes (Diff.)	
	Full Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Enlistment Rate	0.322***	0.307***	0.310***	0.223***	0.212***	0.220***
	(0.059)	(0.060)	(0.060)	(0.046)	(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/counties in Iowa and Wisconsin. All models include state fixed effects. Lagged dependent variables are allowed different slopes by state. Control variables include: (i) the fraction eligible to vote in 1857 over those eligible to vote in the post-war referenda. 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. Full results reported in the Supplementary Tables.

service. For the effect *on veterans* to be non-positive, the contextual effects must have been very large, and focusing on townships in which pre-war support for suffrage was close to zero, the effect of service on veterans is mathematically bounded above zero (SA C.3).

Nevertheless, it could be that veterans were different in ways other than military service that made them more likely to become supportive of suffrage after the war. The most plausible source of this bias would be partisanship. Republicans may have been both more likely to enlist and, either because party elites endorsed suffrage or ongoing activism by pro-suffrage radicals within the party, more likely to become pro-suffrage. There are a few reasons to doubt this. First, effects of enlistment on suffrage are robust to conditioning on pre-war suffrage support and pro-Republican/anti-suffrage vote-share in 1857, as well as restricting the analysis to townships in which Republicans and suffrage received support from less than 1 percent of voters in 1857 (Figure C3). Second, the Wisconsin referendum took place in November 1865, well before national Republicans endorsed suffrage. In Wisconsin, the suffrage question divided Republicans; led by one of the state's Republican Senators, the party 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 statements to the contrary appeared in only 1% of Republican coverage of the issue. The Republican gubernatorial candidate repeatedly refused to take any public stance on the suffrage referendum (McManus 1998). Instead, as I report above, Republican newspapers, like many veterans, argued for suffrage based on African American loyalty during the war.

Mobilizing for Suffrage

Yet veterans did not just passively receive messaging from party activists. There is preliminary evidence that veterans *as constituents* pushed Republican legislators toward more expansive Reconstruction aims.

In March 1866, legislators in the Iowa General Assembly voted whether to remove racial qualifications on rights, including suffrage, from the state constitution. While Republicans

had won the preceding fall on a platform that included black suffrage, they had considerable trepidation about following through: party leaders did not want to appear out of step with President Johnson, who was against suffrage (Cook 1994). Iowa legislators had the choice to support qualified suffrage for African Americans, universal suffrage for men, and whether to eliminate *all* racial restrictions, including those on holding office. Even conditioning on pre-war support for Republicans and black suffrage in their constituencies, Republican legislators who had more veteran constituents were significantly more likely to endorse universal over limited suffrage for African American men, and though not significant, more likely to endorse the most radical position of eliminating restrictions on office holding (See Table C2).

Conclusion

Taken together, the evidence presented above tells a clear story. Reconstruction has often been portrayed as an archetypal case of suffrage extension driven by the strategic needs of political parties (Valelly 2004). Yet, despite clear incentives, opposition from white voters and party factionalism could have rendered Reconstruction stillborn. It was therefore decisive that Union veterans exited the war with a sense that the Union must be remade, not just restored. They placed the destruction of slavery at the center of their wartime victory, and became receptive to a vision of national membership that incorporated, in limited ways, African Americans. As a consequence, they emerged as an important new constituency that provided pivotal support as both voters and activists for Reconstruction.

This has clear and important implications for the history of the American Civil War and Reconstruction. In Congressional elections of 1866, Republicans won a supermajority that empowered them to pursue equal suffrage laws and enabled them to override Johnson's veto. Historians documenting this consequential election make little to no mention of veterans (Foner 1988; Riddleberger 1979). Yet, I show that Republicans benefited immensely from veterans' votes. The pivotal seats needed to secure their supermajority were won by margins

of 4 points, which could have been more than achieved by a one standard deviation increase in enlistment. And because enlistment was higher where Republicans had performed poorly before the war, these gains likely translated into more seats.

Historians have fiercely debated whether diaries show that Union soldiers came to support emancipation and the Republican party (Manning 2007) or that their politics were unchanged (White 2014; Gallagher 2011). This paper presents decisive systematic evidence of veterans' political transformation.

In political science, this finding contributes to a growing body of work that takes up Mayhew (2005) charge to delineate the political consequences of the American Civil War (e.g., Kalmoe 2020). War changed white Northern men who served, and in so doing, it shored up a coalition for Radical Reconstruction. This also speaks to comparative research on the effects of war on the politics of combatants (Jha and Wilkinson 2012; Koenig 2020; Grossman, Manekin and Miodownik 2015). In a departure from previous findings (though see White (2016)), I've shown that wartime sacrifice and antipathy toward a shared enemy can induce combatants to support policies to include allies belonging to a racial outgroup, even in the face of widespread racism.

More fundamentally, this paper adds to a recent reevaluation of the political development of race and civil rights in the United States. In conjunction with Bateman (2020), evidence of ideological transformation among white Union veterans and its political consequences provides an important corrective to elite-driven competition explanations of rights extensions during Reconstruction. The importance of bottom-up mobilization, alongside elite party interests, to the passage of Reconstruction parallels recent work showing that it was the local-level incorporation of African American voters and mobilization by labor unions that pushed Northern Democrats to embrace civil rights in the 20th century (Schickler 2016). In both Reconstruction and the Civil Rights Era a century later, African Americans' advocacy and activism were indispensable (Valelly 2004). Yet this activism depended on a political opportunity structure constrained by the nature of the white electoral coalitions

that supported their claims, in limited and flawed ways, to equal citizenship.

More work can be done to systematically probe the nature of veterans' support for Reconstruction and to understand how it shaped the legislation that was passed. Which Reconstruction issues most animated veterans and their organizations? Did legislators with veteran constituents endorse more expansive civil rights and enforcement legislation, or limited reforms? And in light of the reversal of Reconstruction in the subsequent decades (Foner 1988), how durable was veterans' support for civil rights? Despite the obstruction of Southern Congressional delegations, Republican efforts to secure civil rights did not end in 1877 (Wang 1997; Bateman, Katznelson and Lapinski 2018). While some of these efforts were sparked by the electoral needs of the party, Republicans in several Northern state legislatures passed equal accommodation laws after the Supreme Court struck down much of the 1875 Civil Rights Act (Johnson 1919). Were these efforts animated by veterans? And was Republican abandonment of African American voting rights in the 1890s enabled by growing disinterest among veterans? Or might it have been that aging veterans no longer provided pivotal votes, even as thousands of them joined an early civil rights organization and expressed their anger and dismay that Republican failures were "responsible for the condition that the colored citizen [is] in the South to-day" (Cook 2021, 18)? These questions deserve investigation from the starting point of the perspective, provided here, that the transformation of white constituencies, whether veteran or otherwise, cannot be explained with instrumental party-competition narratives alone. An earnest understanding of the racial history of the United States requires that we take seriously the ideological changes of tide that have fed in and out of the pivotal moments, such as the Civil War and Radical Reconstruction, and that still define our politics today.

The author affirms this research did not involve human subjects. The author declares no ethical issues or conflicts of interest in this research. Research documentation and

the data that support the findings of this study are available in the APSR Dataverse at https://doi.org/10.7910/DVN/RZVMJW.

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Supplementary Appendix

"Let our ballots secure what our bullets have won": Union Veterans and the Making of Radical Reconstruction

Find the full Supplementary Appendix in the replication file.

Full table results for all regression analyses reported in tables and figures in the Paper and Supplementary Appendix may be found in the Supplementary Tables document in the replication file.

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

A.1 Design

In this paper, I apply a 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.1.1 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.

---

[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.

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.

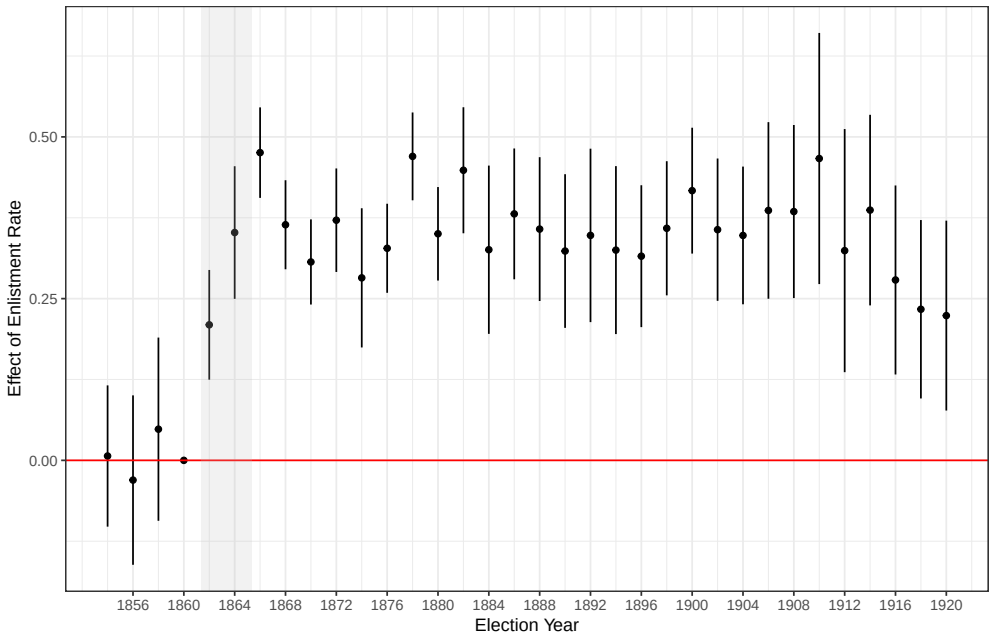
A.2 Checking Parallel Trends

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.

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. Data from Congressional and Presidential elections across 384 counties between 1854 and 1880. Counties with election cycles in which the GOP does not contest an election cycle are dropped. Standard errors clustered by county and election year. Bars show 95 percent confidence intervals.

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

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).

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

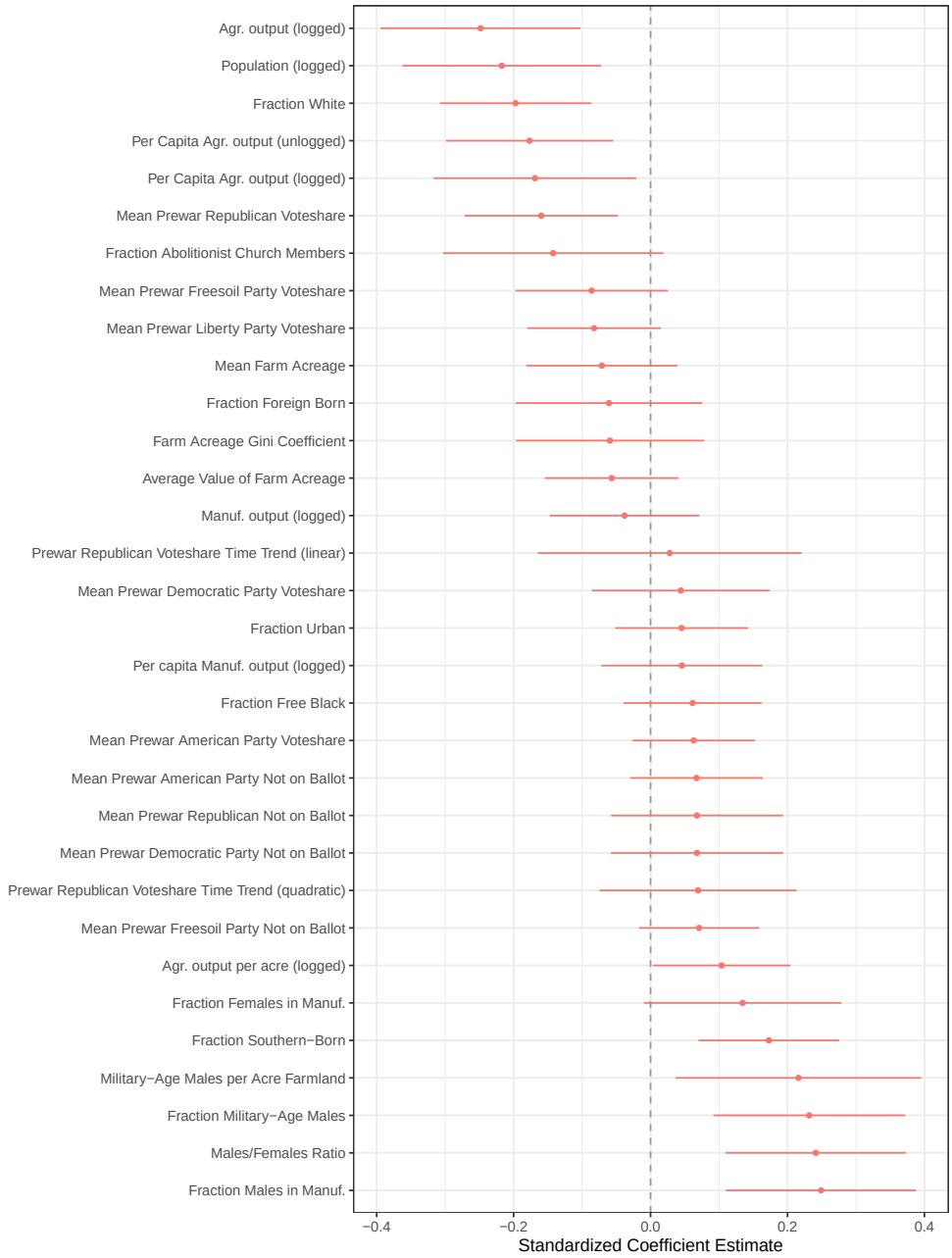
The results are substantively unchanged, even though we allow for possible time-varying confounding.

A.4 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 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.

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

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



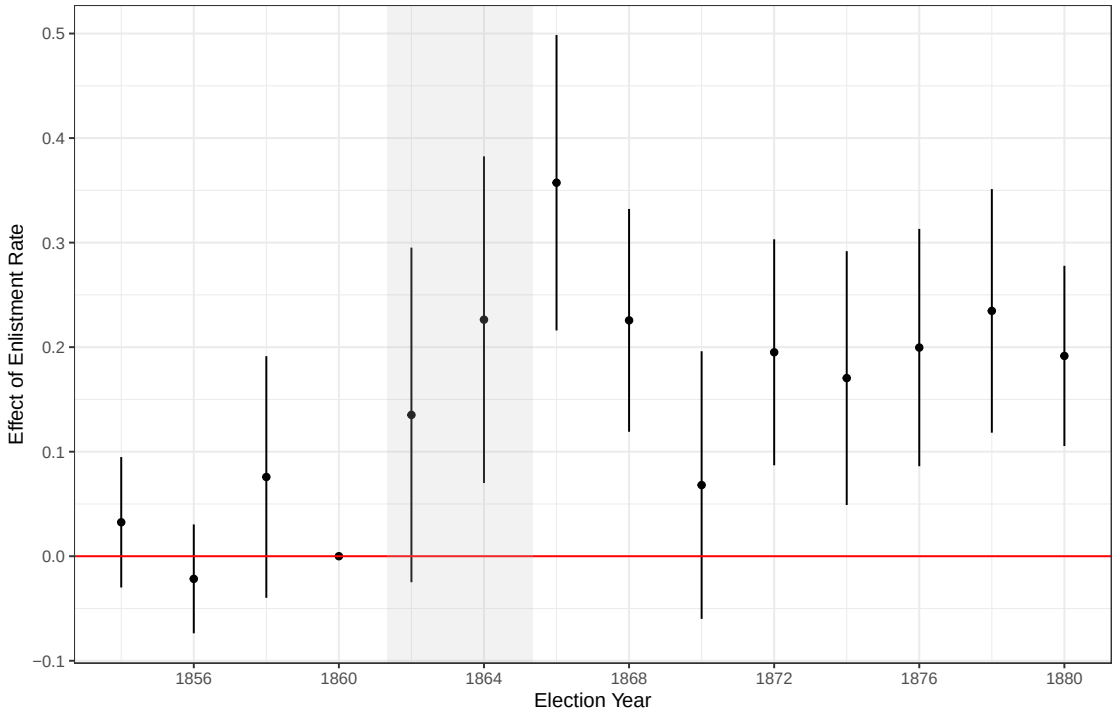
This figure plots the standardized coefficients from bivariates regressions of county enlistment rates on 32 different covariates, with state fixed effects. Data from 384 counties, dropping counties with values in top and bottom 0.5 percentiles of the respective covariate. HC1 Standard Errors. Bars show 95 percent confidence intervals.

Table A1: Difference-in-Differences Estimate (With Time-Interacted Controls) of Effect of County Enlistment Rate on Republican Voteshare

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Enlist % · Postbellum	0.175***	0.192***	0.177***
	(0.031)	(0.043)	(0.036)
GOP no contest	included	dropped	dummy
County FE	X	X	X
State-Election FE	X	X	X
Covars · Postbellum	X	X	X
N Counties	330	257	330
Observations	6,930	5,397	6,930
R^2	0.785	0.770	0.840
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.

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



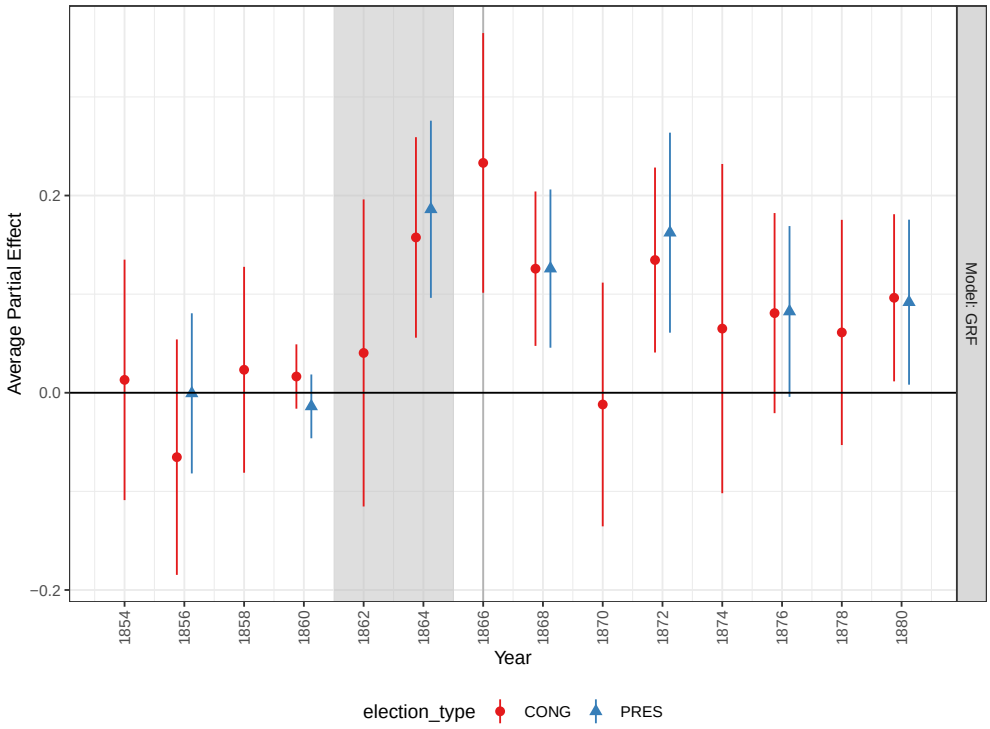
This figure plots the year-specific effect of enlistment rates on Republican vote-share for federal elections with 1860 as the reference year. The model includes county and state-election fixed effects. The year indicators, with 1860 as the reference, are interacted with 32 demographic, economic, and political covariates. Standard errors are clustered by county and year. Bars show 95 percent confidence intervals. Data from Congressional and Presidential elections across 257 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 are dropped. Counties with missing data or extreme outliers in the covariates are dropped.

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 in Figure A2. (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: 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, conditioning on political, demographic, and economic covariates. Standard errors are robust. Bars show 95 percent confidence intervals.

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.5 Robustness

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

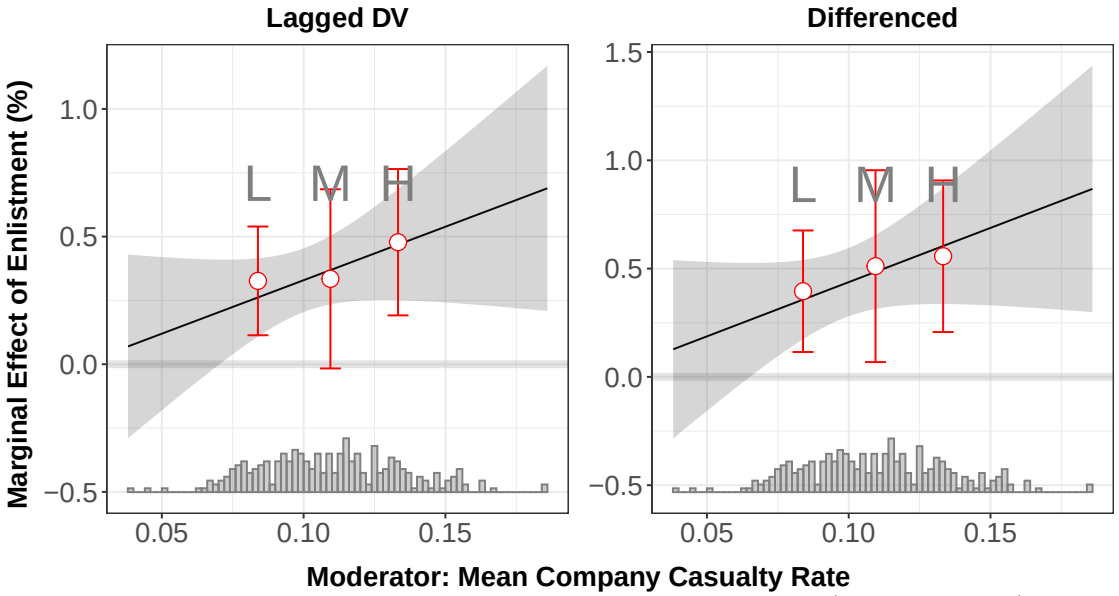
Table A2: Difference-in-Differences Estimate of Effect of County Enlistment Rate (Survivors Only) on Republican Voteshare

	Dependent variable:		
	Republican Voteshare		
	(1)	(2)	(3)
Survived % · Postbellum	0.414^{**}	0.323^{***}	0.322^{***}
	(0.104)	(0.076)	(0.062)
No. Rep. Candidate			-0.585^{***}
			(0.029)
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

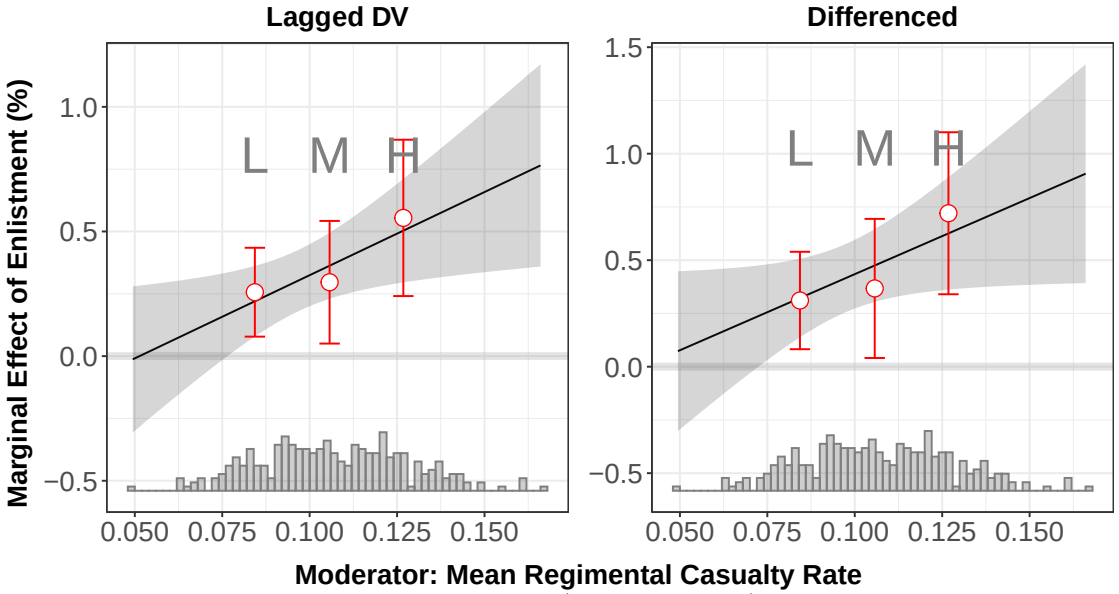
Data from Congressional and Presidential elections across 293 counties between 1854 and 1880. Standard errors clustered by county and election year. Counties with election cycles in which the GOP does not contest an election cycle either treated as a 0, the election is marked with a dummy, or the all 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 County Enlistment Rate (survivors only) on Republican Voteshare in 1864-1868 elections vs 1854–1860, across Mean Company Casualty Rate, conditional on state fixed effects. Sample includes 293 counties, and excludes Indiana for which we cannot calculate mean casualty rates. Standard errors are robust (HC1).

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



Marginal effect of County Enlistment Rate (survivors only) on Republican Voteshare in 1864-1868 elections vs 1854–1860, across Mean Regimental Casualty Rate, conditional on state fixed effects. Sample includes 293 counties, and excludes Indiana for which we cannot calculate mean casualty rates. Standard errors are robust (HC1).

B Company Casualties in Infantry Regiments: A Natural Experiment

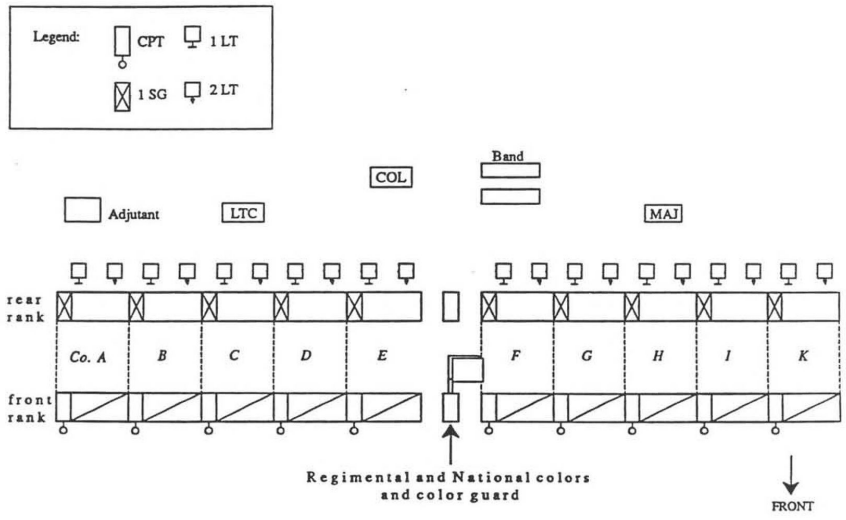
B.1 Company Exposure to Combat Fatalities

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



Regiment Formed Into Line of Battle

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 below comes almost exclusively from (Hess 2015), which is the only contemporary academic historical work devoted to Civil War infantry tactics.

B.2.1 Soldiers

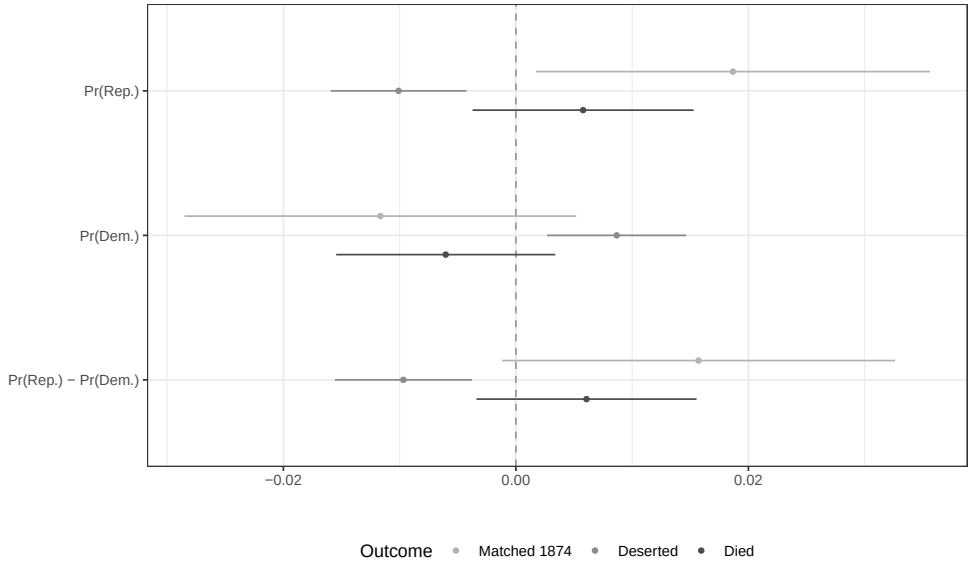
Information While soldiers could choose which company they joined when regiments were formed (Costa and Kahn 2008, 52–57), they could not have known which companies were more likely to receive casualties. (i) All companies stood together in a line across. (ii) While drill manuals specified which position 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. Sample includes men serving in Indiana Regiments who were matched to the 1860 Census. Matches only include 'best' post-war links for each individual. 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.

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 performance.

(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.

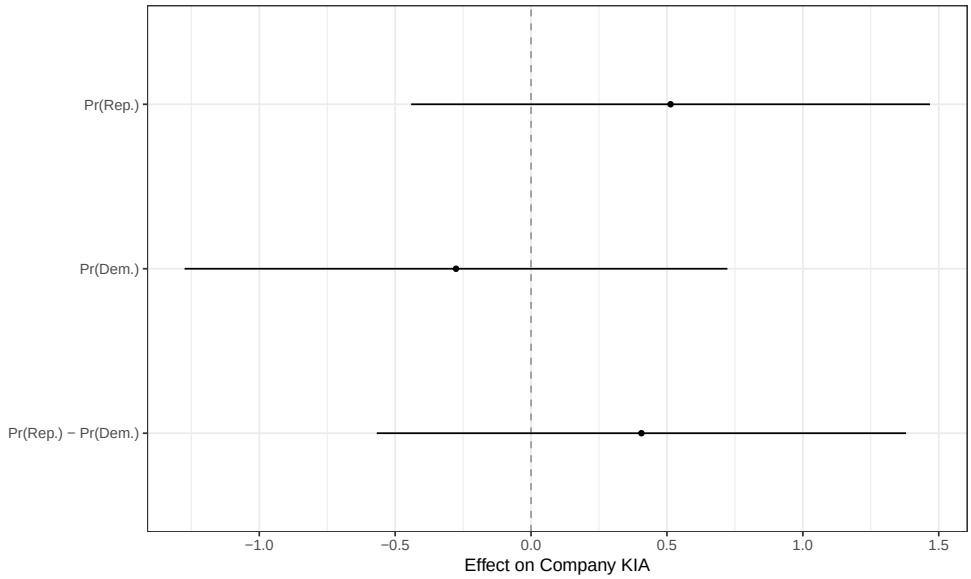
Another consideration was **terrain**. As a unit marched into combat, companies could become detached to the side or the rear of the regiment if they came upon obstacles. The affected companies moved to the rear of the rest of the regiment and retook their place in line when the obstacle was passed.

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.

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.

Figure B3: Within-Regiment Relationship between mean Company Partisanship and mean Company KIAs



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. Results from least squares regression of mean company casualties, by company, on mean company partisanship, conditional regiment-enlistment year fixed effects. Sample includes serving in Indiana Regiments who were matched to the 1860 Census:10329 unique soldiers, in 896 companies, across 288 regiments. Companies are weighted by the number of soldiers. Standard errors HC robust.

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 enthusiasm 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 adult men 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 2007; 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.

[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.

This figure shows an example page listing biographical entries in one of the 1874 People's Guides.

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 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

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

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 only first initials are available.

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 4895 Union soldiers. On average, each soldier is matched to 1.51 people post-war, with a max of 14. When restricting to best matches, the average number of matches is 1.18, 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 12103 soldiers to the 1860 Census. The mean number of matches per soldier is 1.14, and the maximum number is 14. 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 name 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 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.

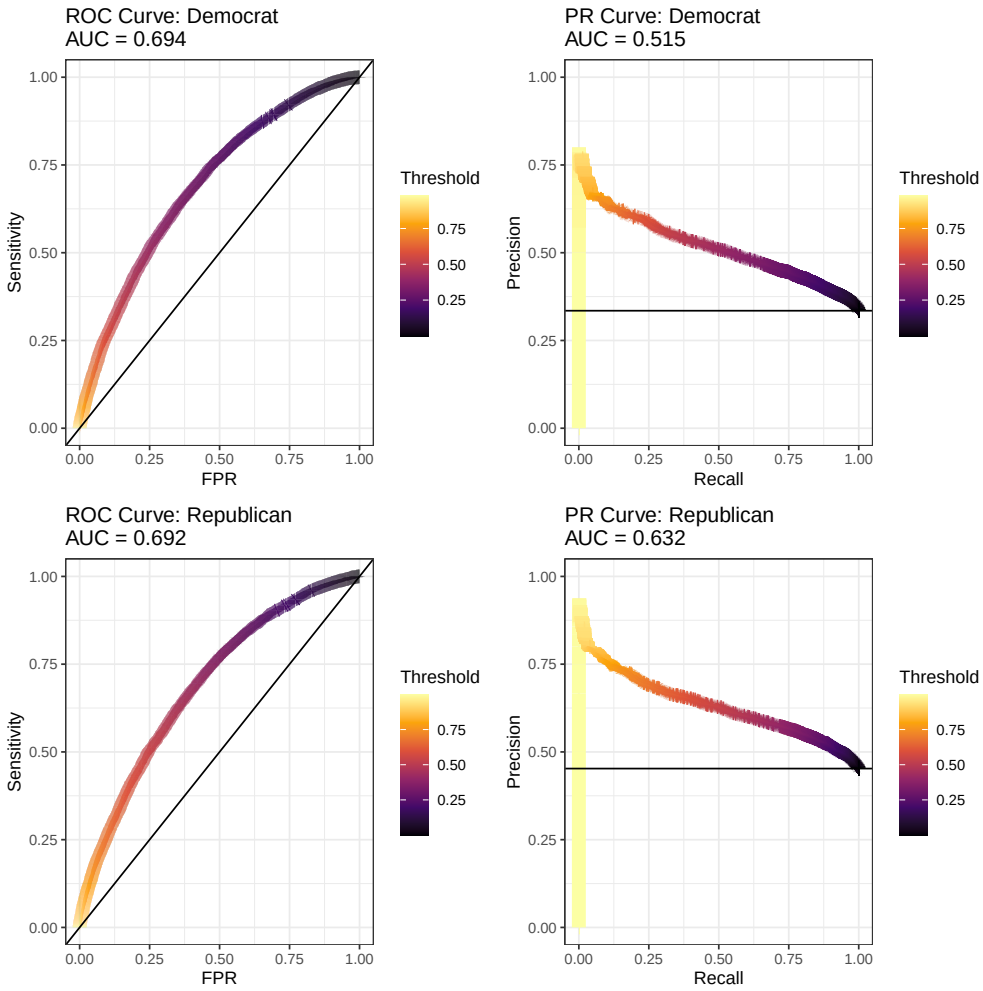
Performance in the "test'' set In figure B5, I show that the ROC and PR curves for the classifier in the "test'' set, while imperfect, substantially outperforms the naive classifier for both Republicans and Democrats. And in figure B6, I show the predicted probability of being a Republican and Democrat 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?

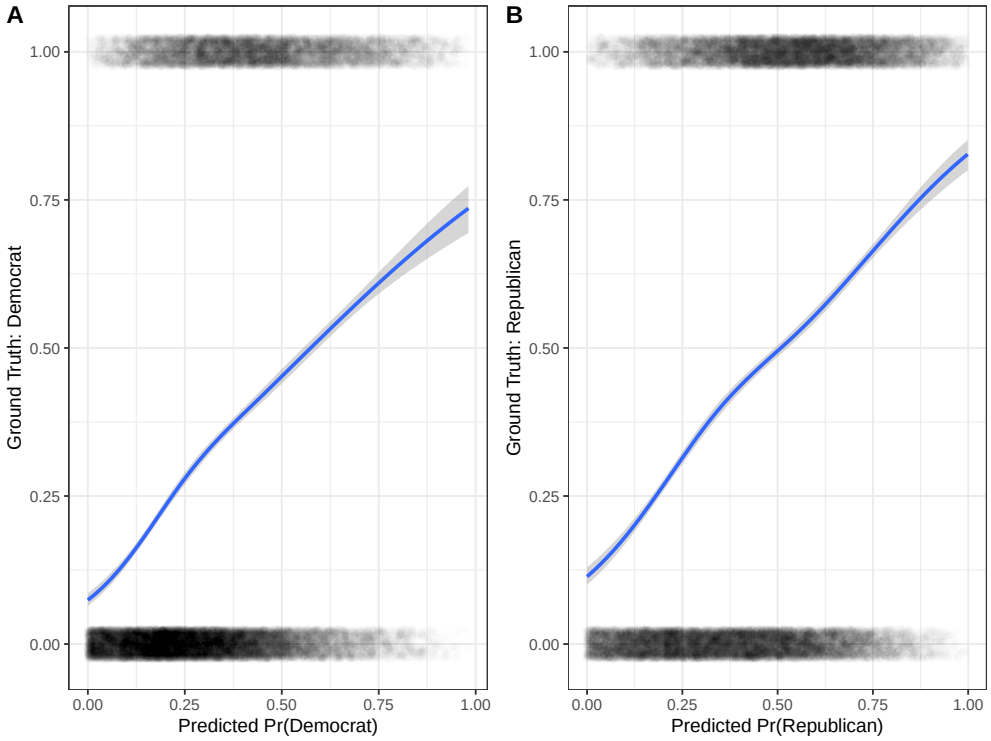
To assess this, I aggregate the predicted probabilities of Democratic and Republican partisanship for men eligible to vote in the 1860 elections and living the 9 Indiana counties,

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



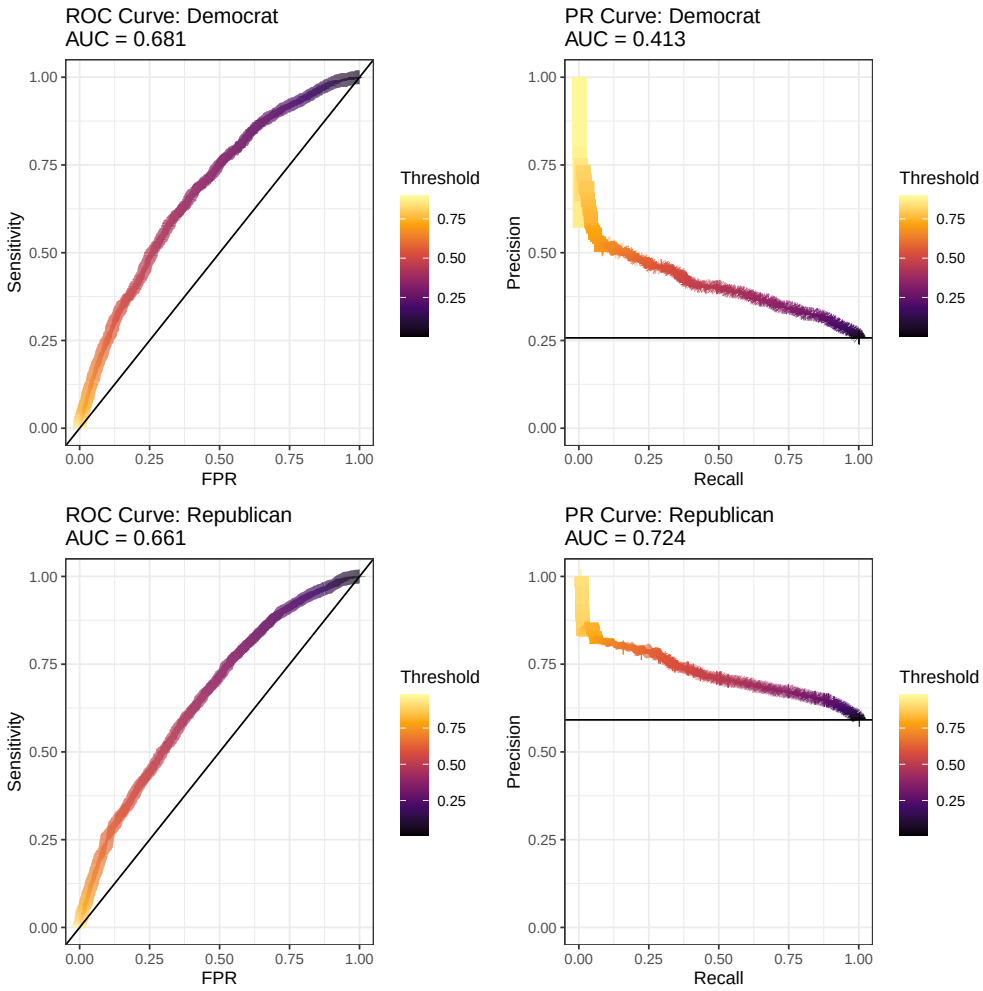
This figure shows the ROC and PR curves for the fastText classifier of Republican and Democratic partisanship in 1874, for the "test" set or cases held out in each fold when training the classifier.

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



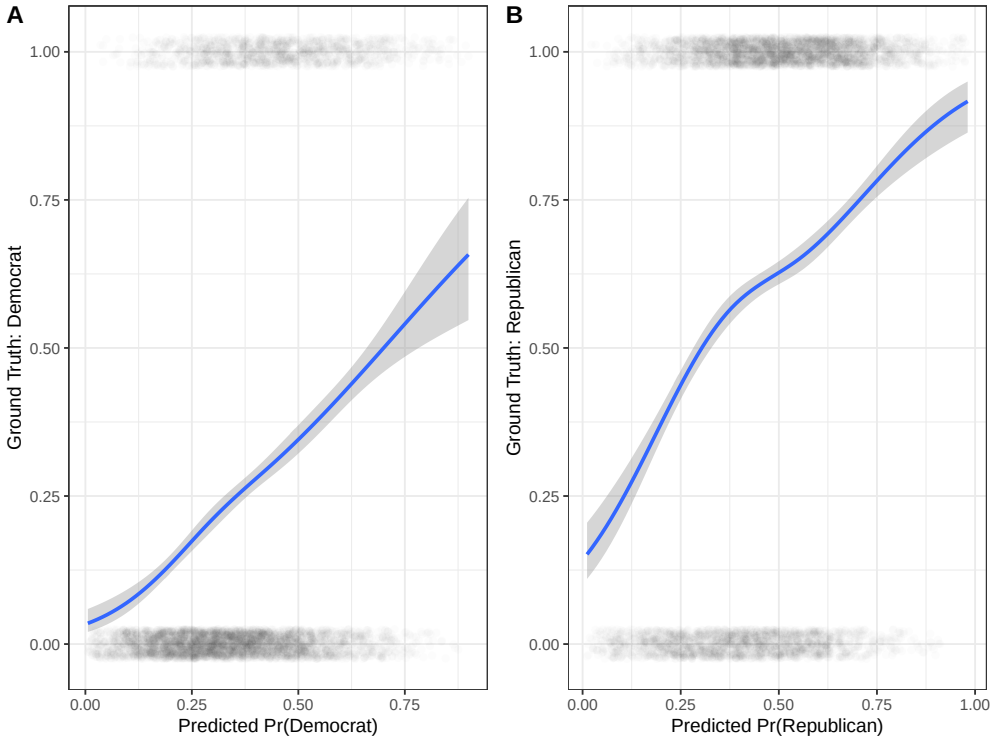
This figure shows the calibration curve of predicted partisanship vs. actual partisanship from the fastText classifier of Republican and Democratic partisanship in 1874, for the "test" set or cases held out in each fold when training the classifier.

Figure B7: AUC/PR Curves for "Validation" Set



This figure shows the ROC and PR curves for the fastText classifier of Republican and Democratic partisanship in 1874, for the "validation" set or cases linked to veterans and not used in training the classifier.

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

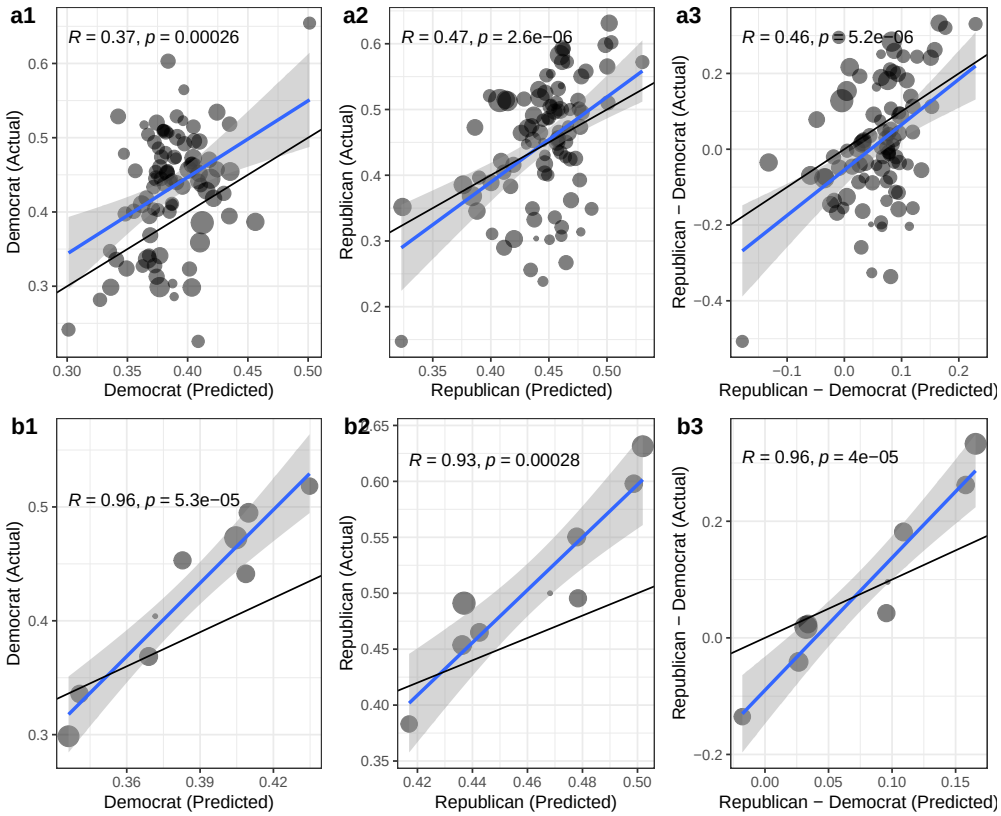


This figure shows the calibration curve of predicted partisanship vs. actual partisanship from the fastText classifier of Republican and Democratic partisanship in 1874, for the "validation" set or cases linked to veterans and not used in training the classifier.

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 townships in 4 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 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]

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

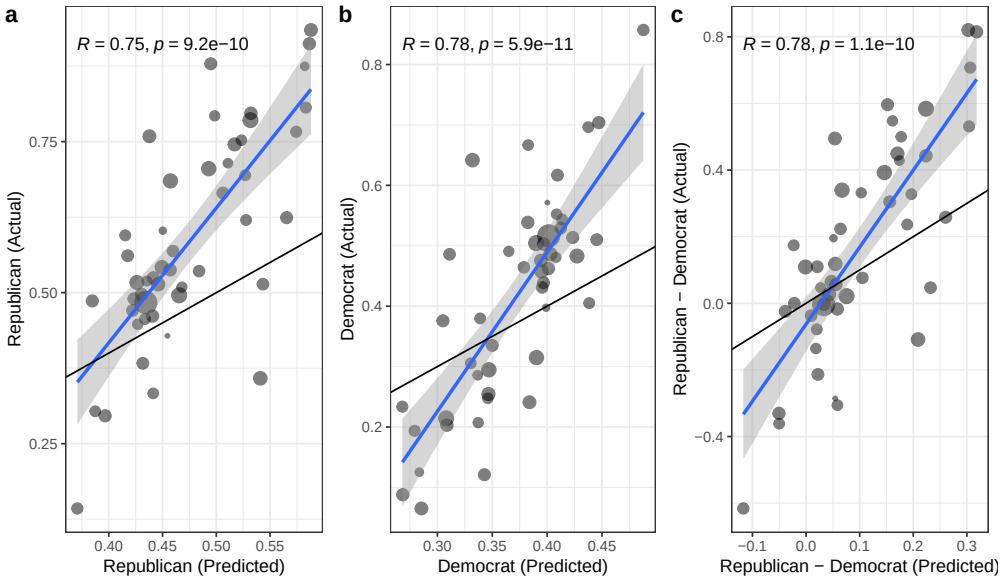


This figure shows the calibration curve of predicted partisanship vs. actual partisanship from the fastText classifier of Republican and Democratic partisanship in 1874, for the Indiana Counties in the 1860 Presidential election. Predicted partisanship was obtained by applying the fastText classifier to all voting-eligible males in each county and taking the mean probability of Democratic and Republican partisanship.

I repeat the same procedure using township level returns in Hendricks, Henry, Montgomery, 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.

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

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



This figure shows the calibration curve of predicted partisanship vs. actual partisanship from the fastText classifier of Republican and Democratic partisanship in 1874, for the townships in Hendricks, Henry, Montgomery, and Morgan counties in the 1860 Presidential election. Predicted partisanship was obtained by applying the fastText classifier to all voting-eligible males in each township and taking the mean probability of Democratic and Republican partisanship.

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 die (Figure B2).

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.	0.442***			0.460***		
	(0.041)			(0.043)		
Dem. Prob.		0.408***			0.417***	
		(0.039)			(0.041)	
Diff Prob.			0.228***			0.236***
			(0.019)			(0.019)
Observations	5,965	5,965	5,965	4,541	4,541	4,541

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.

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. The dashed vertical lines indicate 0.1 SD change in company casualties.

While there appear to be large differences by draft and substitute status, this is because there is little variance in draft/substitute status within regiments.

B.6 Robustness

Tables B3, B4, and 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.

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

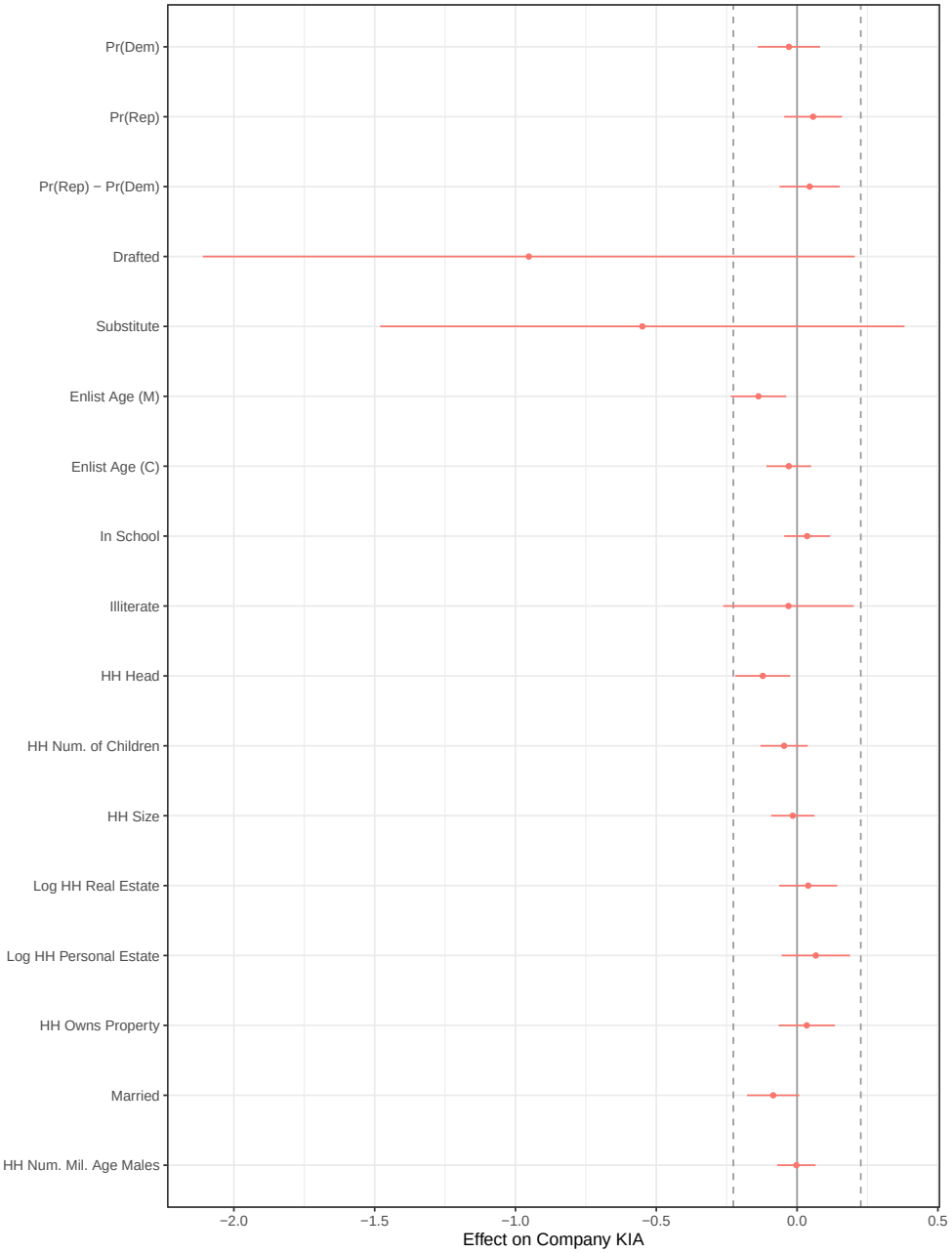
Only 33 percent of soldiers found in the 1860 Census were matched to the 1874 People's Guides and only 23 percent of soldiers over-all were matched. While this is not far off from other attempts to link historical records (see, e.g. Abramitzky et al. 2019), there are reasons to be concerned about attrition 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.

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.

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.Vertical dashed lines indicate 1/10th of within-regiment standard deviation of company casualties. Estimates derived from regression of Company KIA on pre-muster covariates, conditioning on regiment fixed effects. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 10288 unique soldiers, in 889 companies, across 288 regiments. Individuals are weighted by 1 over the number of census matches. Standard errors are clustered by company.

Table B3: 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	0.006	-0.008^{**}	0.007^{*}	0.007	-0.006^{*}	0.007^{*}
	(0.003)	(0.003)	(0.003)	(0.004)	(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	4,659	4,659	4,659	4,659	4,659	4,659

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 3885 individual soldiers, serving in 617 companies, across 234 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Observations are weighted by 1 over the number of post-war matches for each unique soldier.Standard errors are clustered by company.

Table B4: Effect of Company Casualties on Post-war Partisanship (Census-Linked, All Matches)

	Dependent variable:								
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	Baseline			Army Controls			All Controls		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Company KIA	0.007*	−0.008**	0.008*	0.008*	−0.007*	0.008*	0.008*	−0.006*	0.007*
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
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	4,669	4,669	4,669	4,669	4,669	4,669	4,669	4,669	4,669

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 3134 individual soldiers, serving in 540 companies, across 211 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Observations are weighted by 1 over the number of post-war matches for each unique soldier.Standard errors are clustered by company.

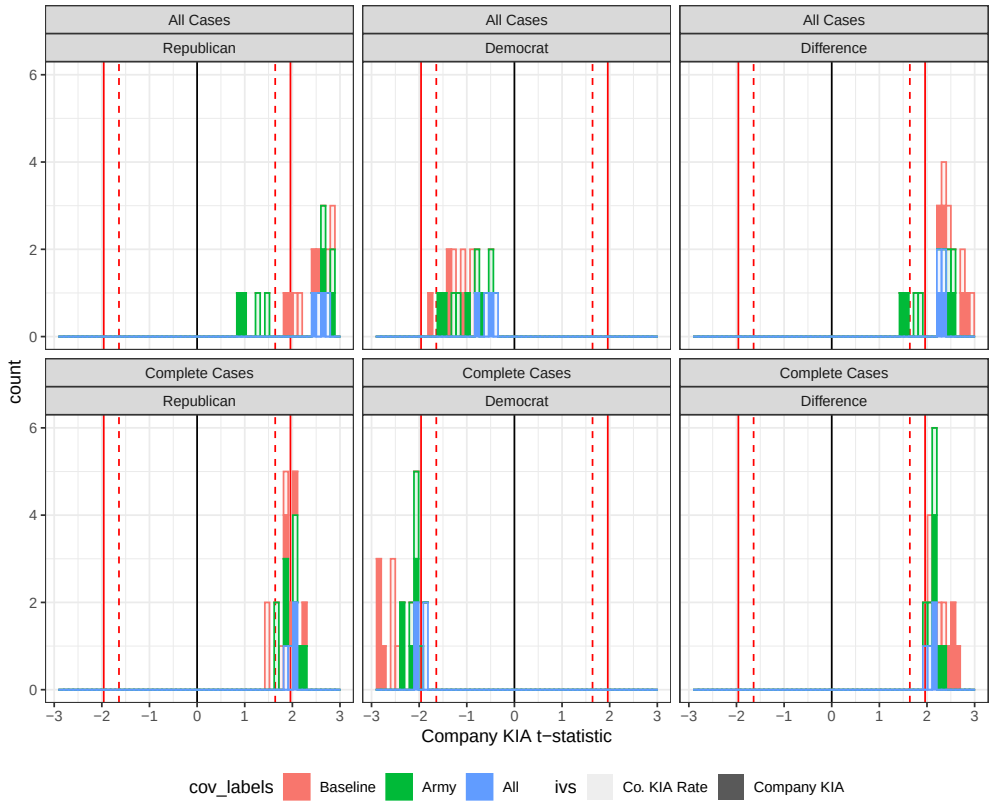
Table B5: Effect of Company Casualties on Post-war Partisanship (All Matches)

	Dependent variable:					
	Rep.	Dem.	Party Diff.	Rep.	Dem.	Party Diff.
	Baseline			Army Controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Company KIA	0.005	-0.007^{**}	0.006^{*}	0.006	-0.006^{*}	0.006^{*}
	(0.003)	(0.002)	(0.002)	(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	5,976	5,976	5,976	5,976	5,976	5,976

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

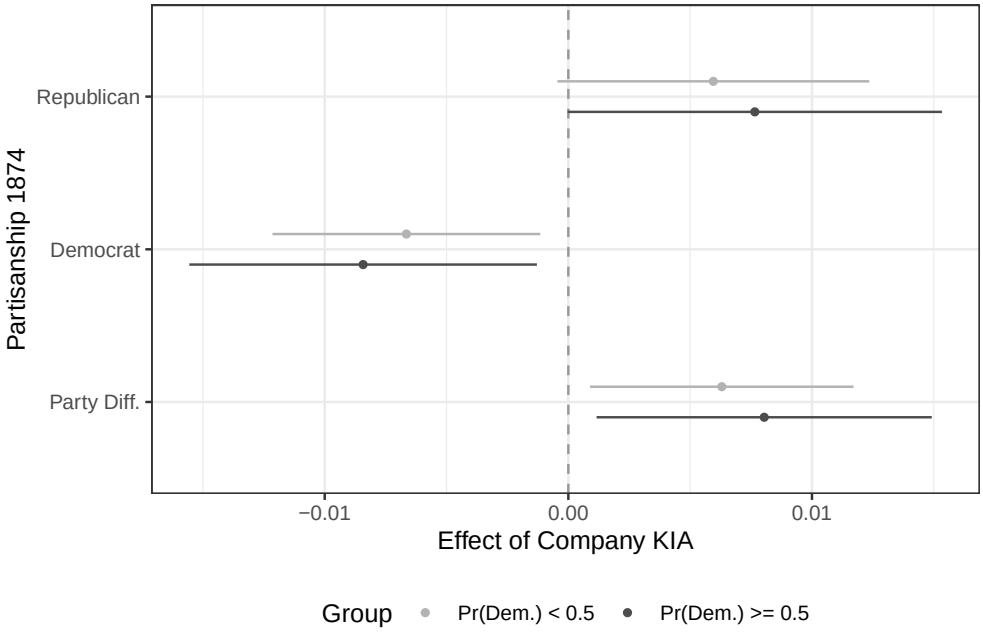
Sample includes men serving in Indiana Regiments who were matched to the 1860 Census and their best match, if any, in the 1874 People's Guides. Baseline and control models, respectively, include data on 3910 individual soldiers, serving in 618 companies, across 234 regiments. Regiment fixed effects includes a dummy for each group of soldiers who joined a regiment in the same year. Observations are weighted by 1 over the number of post-war matches for each unique soldier.Standard errors are clustered by company.

Figure B12: Robustness of Company Casualties Results to alternate specifications



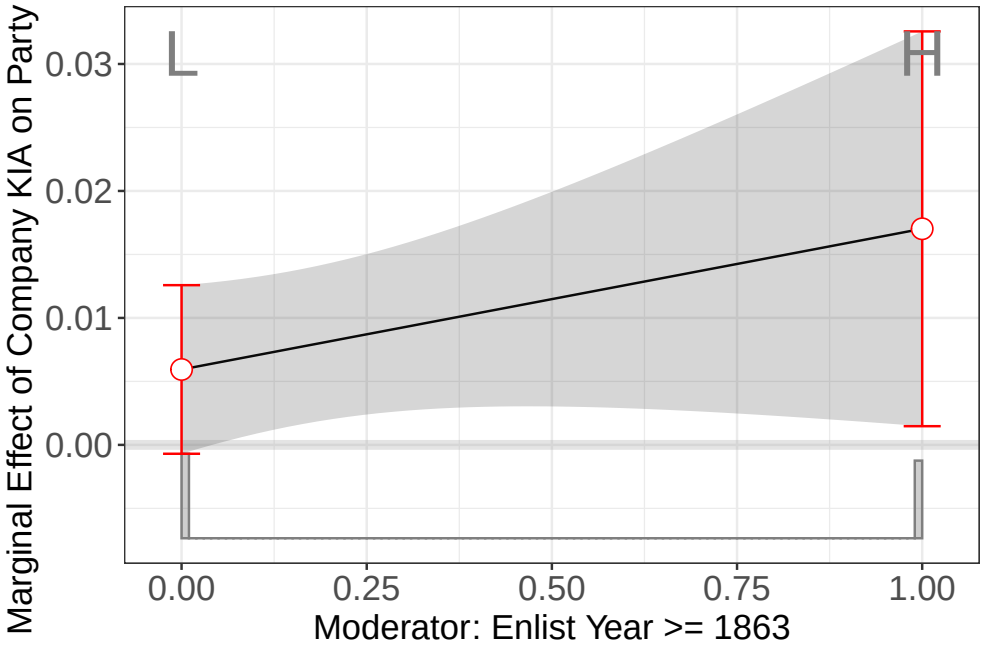
This reports t statistics for analyses of the company casualty natural experiment across different samples (all soldiers vs matched only; best matches vs all matches), covariates (none, army, army + census), and measures of the treatment (Company KIA and Company KIA rate). All models use standard errors clustering by company.

Figure B13: Effects of Company Casualties By Predicted Partisanship



This figure extends the results from Table 2, columns 1–3, by showing the marginal effect of Company KIAs on partisan swing, across predicted pre-war partisanship (probability of Democratic partisanship greater or less than 0.5), conditional on regiment fixed effects. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 3259 unique soldiers, in 564 companies, across 216 regiments. Individuals are weighted by 1 over the number of 1974 Peoples Guide matches. Standard errors are clustered by company.

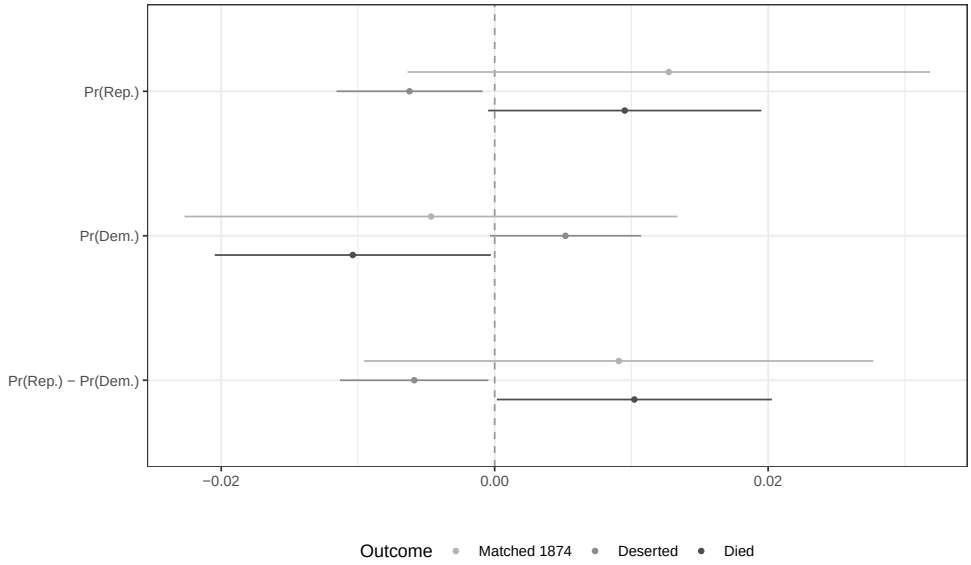
Figure B14: Effects of Company Casualties across early and late enlistments



This figure extends the results from Table 2, column 3, by showing the marginal effect of Company KIAs on partisan swing, for soldiers enlisting pre- and post- Emancipation Proclamation, conditional on regiment fixed effects. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 3237 unique soldiers, in 563 companies, across 216 regiments. Individuals are weighted by 1 over the number of 1974 Peoples Guide matches. Standard errors are clustered by company.

Within regiments, partisanship is not significantly related to overall attrition (Figure B15). 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.

Figure B15: 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.

B.7.2 Does treatment predict attrition?

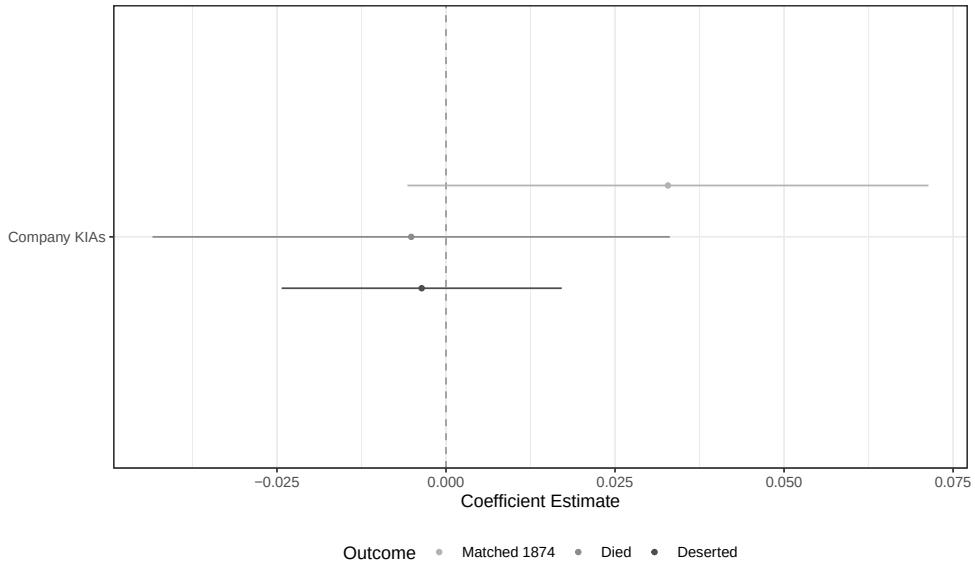
Attrition bias could arise if treatment is related to attrition and relationship between treatment and attrition is related to potential outcomes.

Using the design employed for analyzing the natural experiment (regiment fixed effects), do company-level casualties predict attrition? Figure B16 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 after the war (an increase of about 2.5 ppt). And Figure B11, above, shows that treatment is balanced across baseline partisanship.

Though not significant, I further test whether there is any relationship between the effect of treatment on attrition and latent partisanship. Figure B17 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.

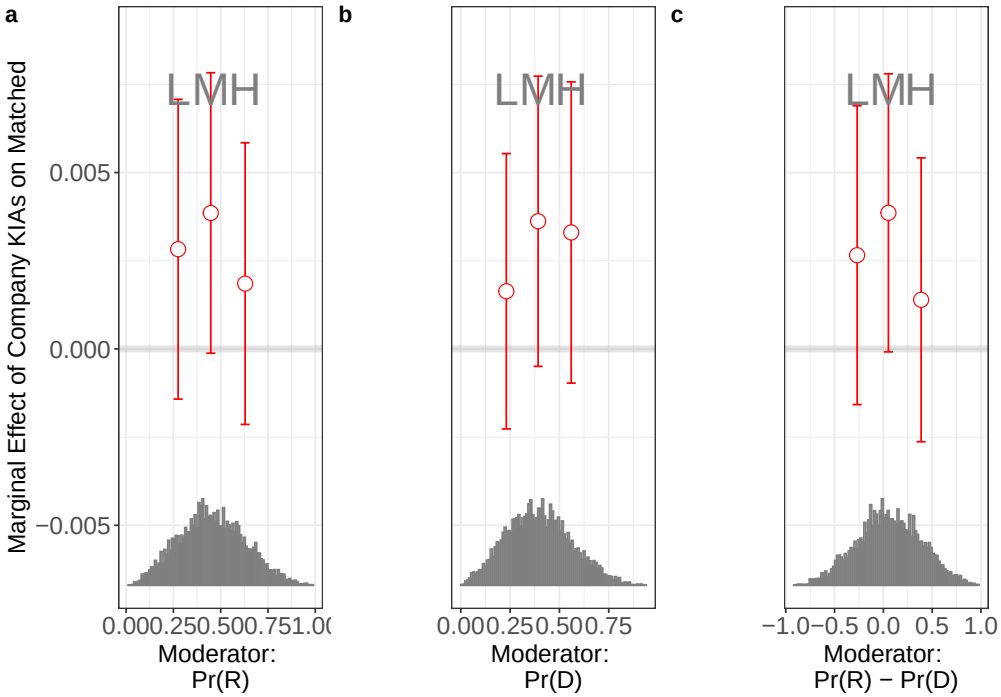
This is reassuring, as it does not provide any clear evidence of attrition by partisanship. If anything, the positive relationship between company combat deaths and attrition may reflect long-term trauma. On average, soldiers who were wounded or disabled were about

Figure B16: 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.

Figure B17: Differential Attrition by Treatment (Within Regiment) and Partisanship



This figure reports the marginal effect of Company KIAs on post-war matching, across bins of predicted partisanship probability, conditional on regiment fixed effects. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 10329 unique soldiers, in 896 companies, across 288 regiments. Individuals are weighted by 1 over the number of census matches. Standard errors are clustered by company.

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:

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

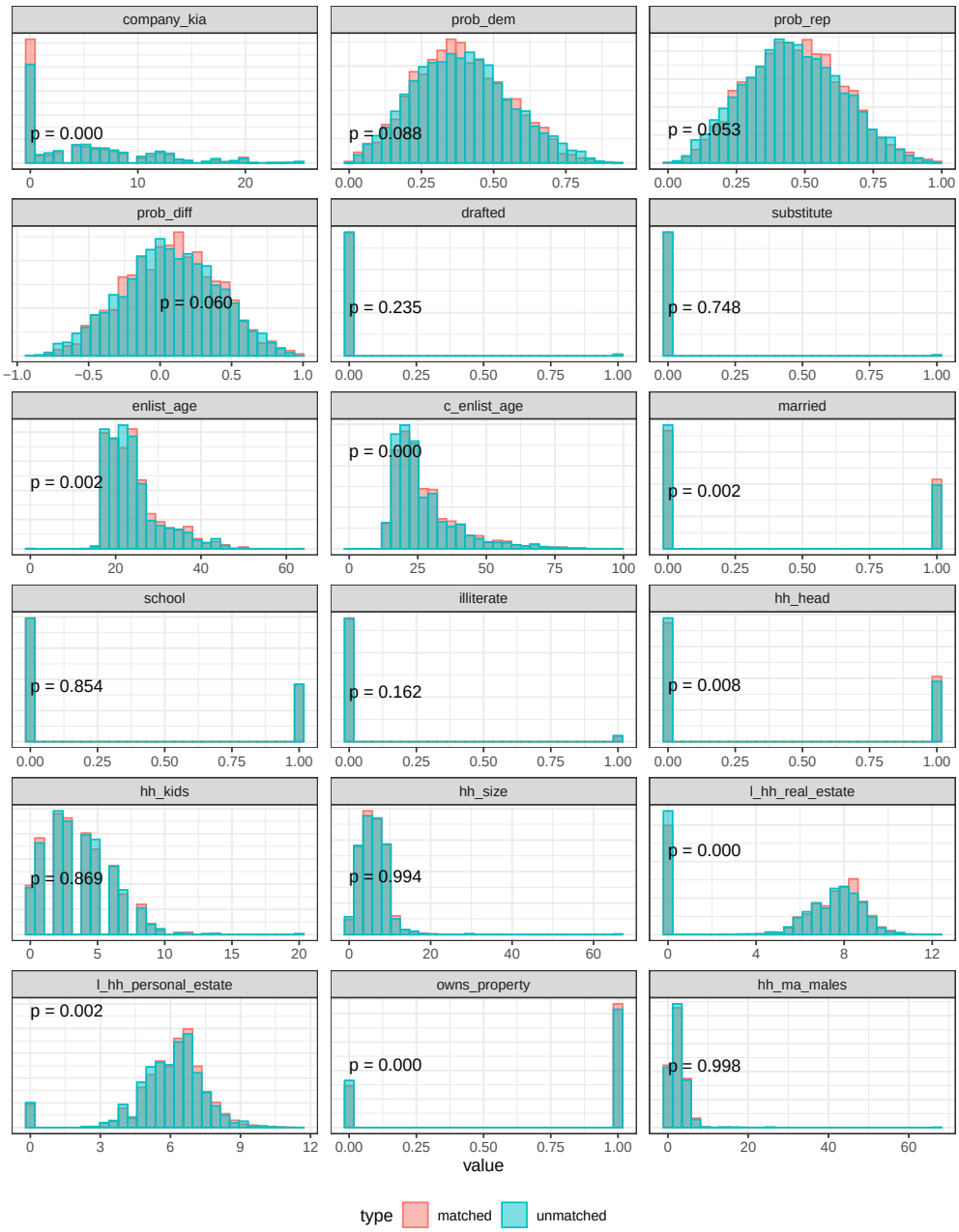
Where R_i is an indicator that case i is observed post-war; Z_i is level of treatment, and X_i is a vector of covariates (Gerber and Green 2012). This is an inverse probability weight estimator. A few issues arise when using inverse probability weights. First, these weights may perform poorly if there 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 unmatched sample of soldiers. While there is imbalance in the distribution of covariates (both raw (Figure B18), and centered within regiments (Figure B19)), 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,

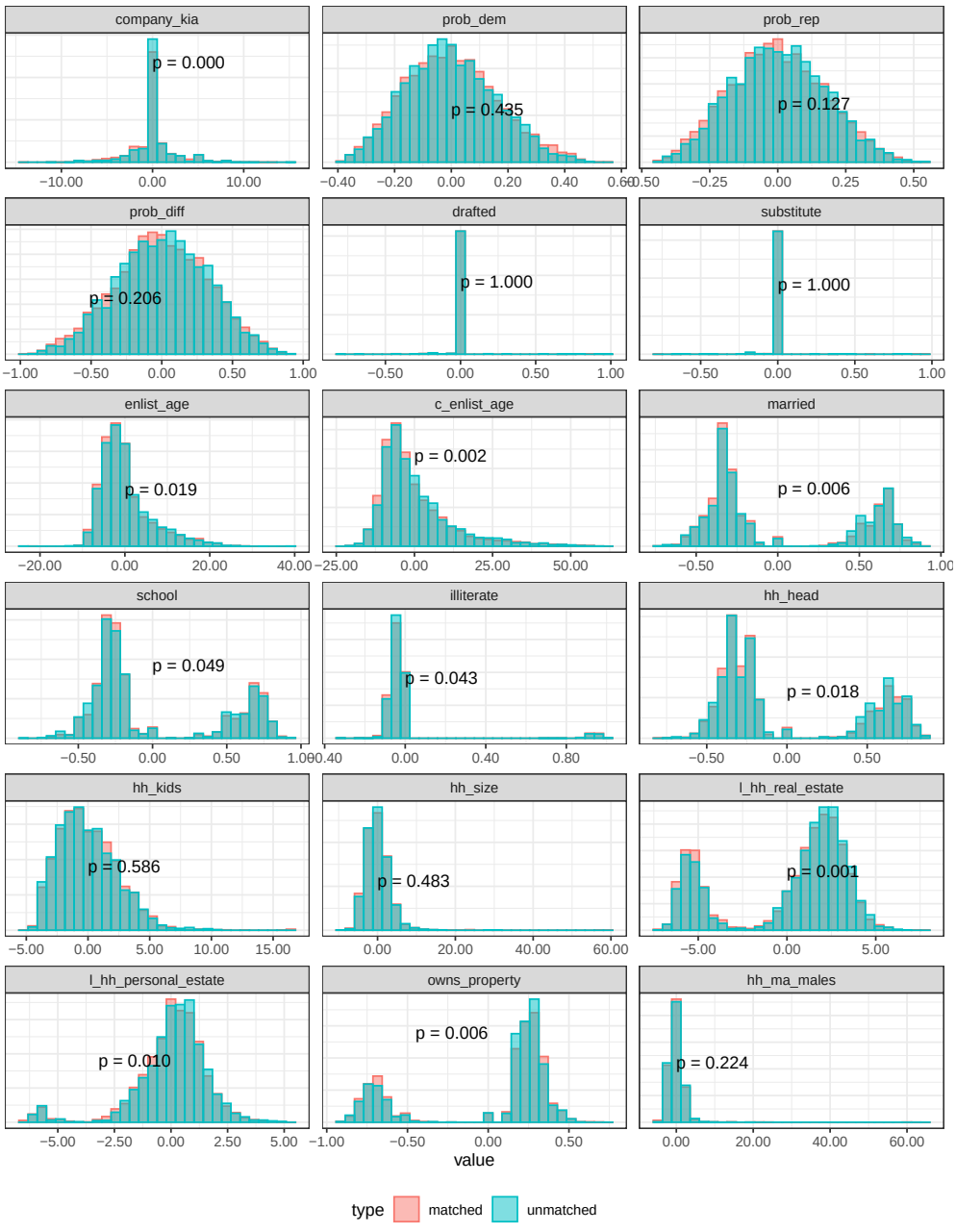
[4]Combat casualties, other deaths, company casualty rate, other death rate, number of men in company, date of muster into company, date regiment was organized, rank at enlistment, pr(Democrat), pr(Republican), pr(No party), drafted, substitute, Age (census), attended school in 1860, illiterate in 1860, 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 B18: Overlap on (raw) covariates for the soldiers matched and unmatched to 1874 People's guides.



This figure shows overlap in the distribution of covariate values for the unmatched 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. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 10288 unique soldiers, in 889 companies, across 288 regiments.

Figure B19: Overlap on (regiment-mean-centered) covariates for the soldiers matched and unmatched to 1874 People's guides.



This figure shows overlap in the distribution of covariate values, centered on the regiment mean, for the unmatched 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. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census: 10288 unique soldiers, in 889 companies, across 288 regiments.

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 B20 and B21 show the improvement in balance on post-war missingness achieved.

Figure B22 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.

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-differences 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:

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

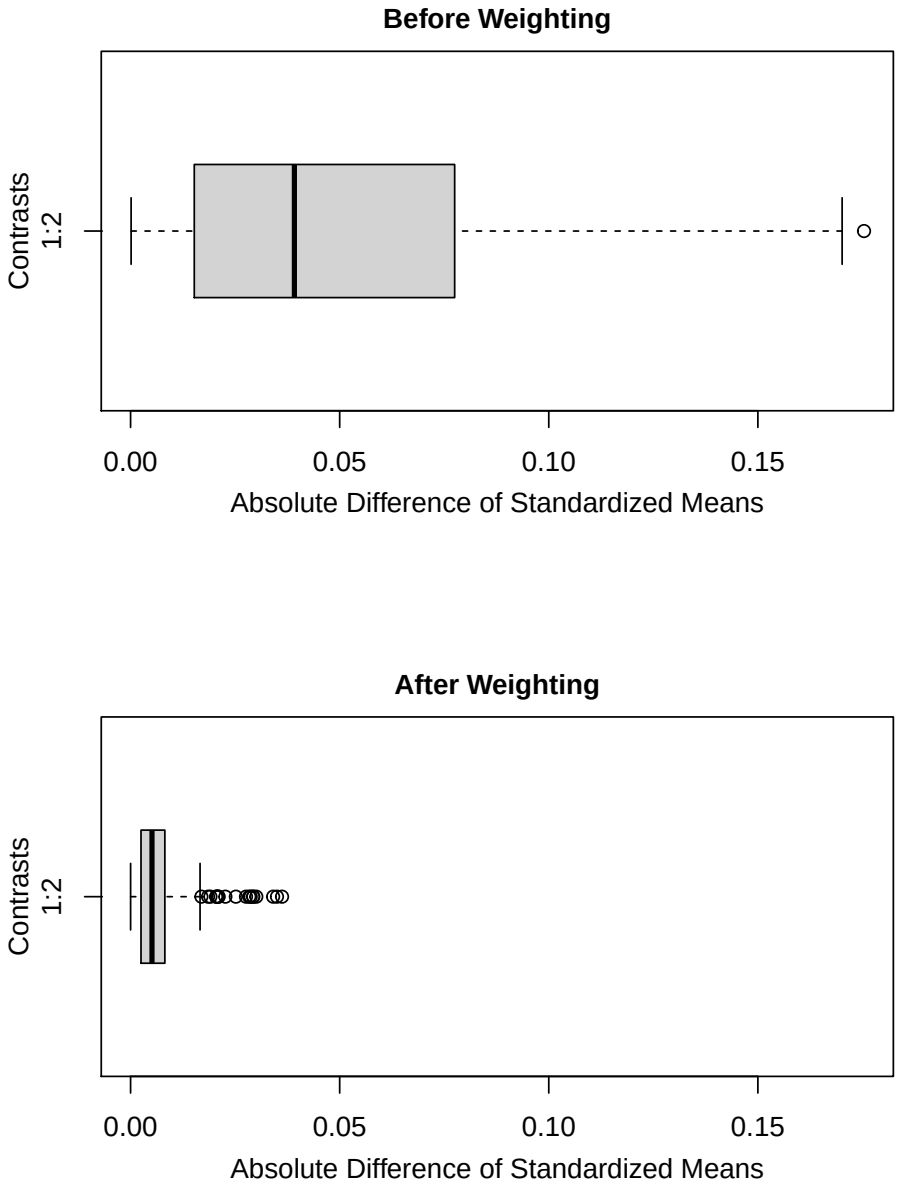
And the difference Δ in support for suffrage can be expressed as a function of the change in support among enlistees, and change in support among non-enlistees.

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

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.

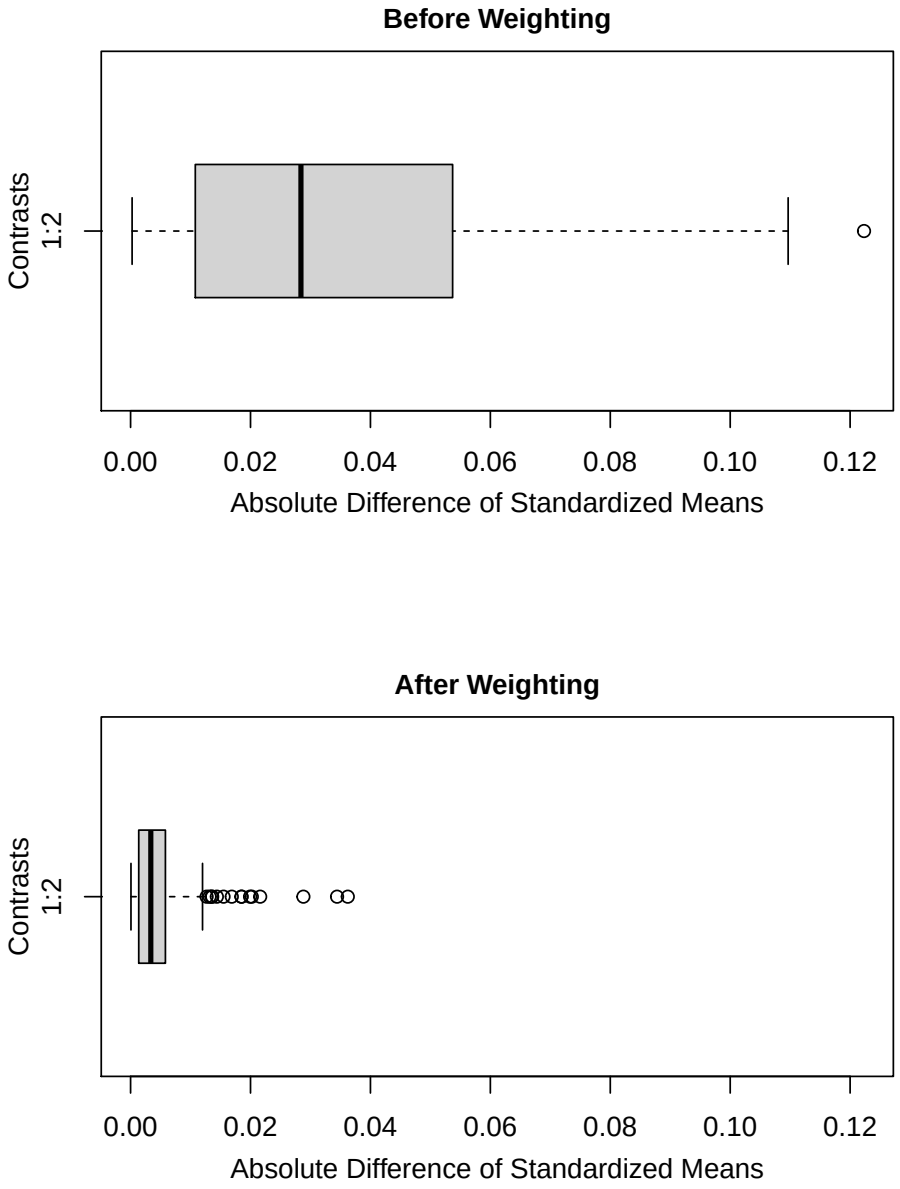
For further proof of this, the replication file includes code to simulate this process, allowing there to be contextual differences in the cross-sectional support for suffrage among enlistees and non-enlistees. The simulations show that the ecological difference-in-differences regression is an unbiased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

Figure B20: Balance between matched and un-matched soldiers before and after weighting using CBPS (option 1)



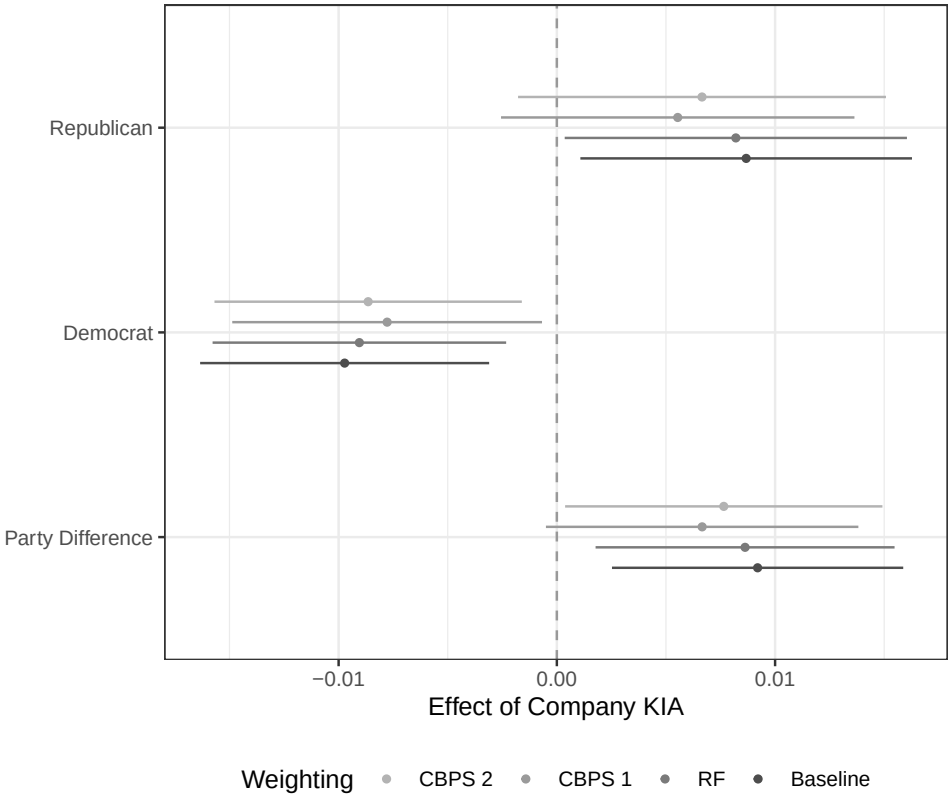
This figure shows balance between matched and unmatched soldiers before and after applying inverse probability weights generated using CBPS balancing on within- and between- regiment differences in treatment, covariates, interactions between treatment and covariates, and squared terms

Figure B21: 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 generated using CBPS balancing on differences from within-regiment means of treatment, covariates, interactions between treatment and covariates, and squared terms

Figure B22: 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. Estimates come from the least squares regression of post-war partisanship on Company KIA, conditioning on regiment fixed effects, using different weights for MIPO. Sample includes men serving in Indiana Infantry Regiments who were matched to the 1860 Census and the 1874 People's Guides: 3087 unique soldiers, in 521 companies, across 195 regiments. Individuals are weighted by 1 over the number of matches multiplied by their inverse probability weight of being matched post-war. Standard errors are clustered by company.

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 8.4 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.)		(Yes/Elig.)			(Yes/Elig.)		(Yes/Elig.)		
	Full Sample					Restricted Sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enlistment Rate	0.210***	0.184***	0.191***	0.149***	0.113***	0.179***	0.147***	0.160***	0.109***	0.084**
	(0.031)	(0.030)	(0.036)	(0.032)	(0.030)	(0.032)	(0.027)	(0.037)	(0.029)	(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 $-0.396, 0.686$, which includes the estimated effect, 0.265. 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 7.5 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.

Another approach to addressing the issue of bounds is to look at the bounds for subsets of the data. 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 C2 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 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."

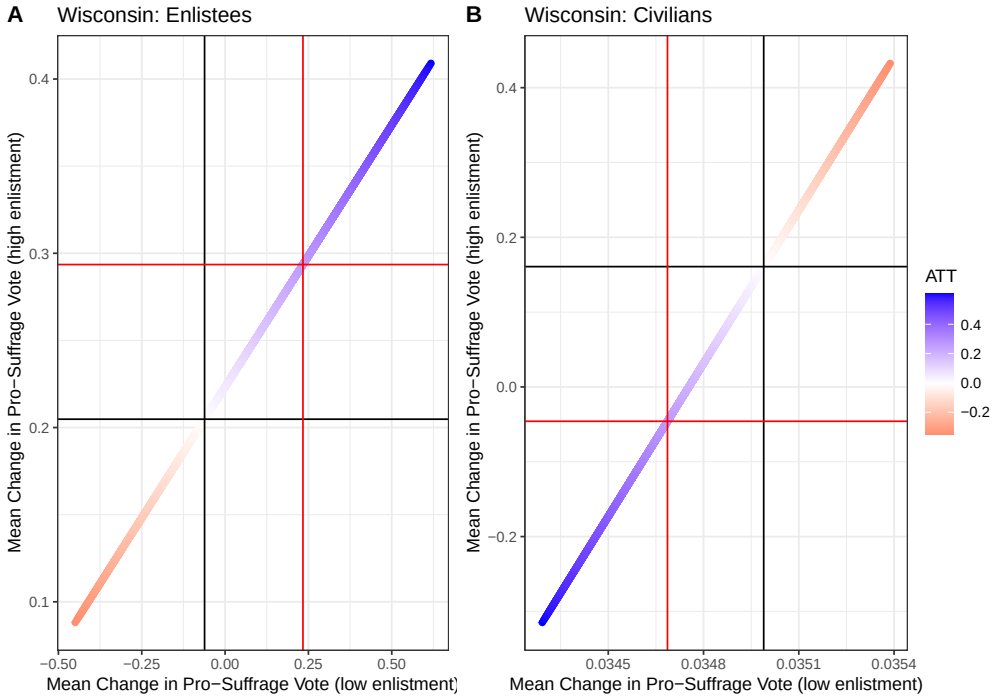
C.4 Bounds: Unobserved Confounding

Despite showing that partisan selection into service cannot explain why veterans were more likely to trend toward suffrage, it could be that there was some other form of unobserved confounding that led people who were going to trend toward suffrage anyway over the course of the war to enlist at higher rates. This would bias the ATT of enlistment on support for suffrage. The central concern is that people who were prone to become supporters of suffrage joined the army at higher rates.

To engage with this potential bias, I partially identify the effects of military service on support for suffrage in the presence of unobserved confounding, using data from the low-suffrage townships examined above. I first obtain the ecological bounds on how people who served and did not serve voted for suffrage, before and after the war. Then, I apply the bounds for difference-in-difference estimates of the ATT under unmeasured confound-

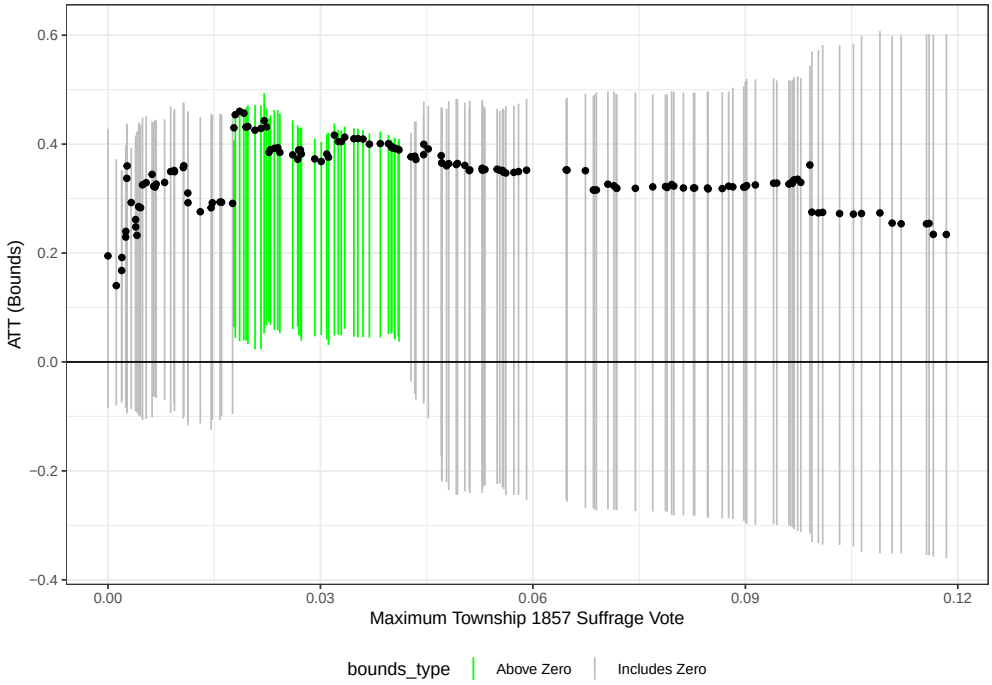
[6]This includes between 54 and 93 townships.

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: 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.

ing developed by the (Athey and Imbens 2006, 151–3), with one minor change. I add the assumption of monotone treatment response (that is, enlistment can not hurt support for suffrage). This seems plausible given that virtually no one supported suffrage (so cannot reverse positions) and it reflects the concern that soldier's support for suffrage reflected a predisposition to become supportive of suffrage.

$$ATT = [Y(1)_1|D = 1] - [Y(0)_1|D = 1]$$

To put bounds on the ATT given unmeasured confounding, we put maximum and minimum values on what soldiers would have done after the war, even if they had not enlisted. Modifying the bounds from (Athey and Imbens 2006, 151–3) using the monotone treatment response assumption, we find that:

$$max([Y(0)_1|D = 1]) <= [Y(1)_1|D = 1]$$

and

$$min([Y(0)_1|D = 1]) >= max\{[Y(1)_0|D = 1], [Y(0)_1|D = 0]\}$$

That is to say, in the absence of enlistment, soldiers would have supported suffrage after the war at least as much as the people who stayed at home and at least as much as they did before they went to war. And they would have supported suffrage after the war no more than they actually did after serving.

The most important thing to note about these bounds is that they set the lower extreme bound of the ATT to be 0. Conversely, the upper bounds (determined by the ecological bounds discussed above) imply that the ATT was an increase in support for suffrage of between 13.4 and 36.1 percentage points. Only in the most extreme case where all changes over time among soldiers were due to selection effects does the effect of enlistment disappear.

These bounds also help us interrogate how plausible this extreme scenario would be. Overall, it seems very unlikely.

- First, we can establish the proportion of the population that would have to be "trending" toward suffrage in order for the extreme case of $ATT = 0$ to be true. In this set of cases, it would imply that whereas only 1.7 percent of people were pre-war supporters of suffrage, 10 percent were going to support suffrage anyway. This seems implausible. Support for suffrage across all of Wisconsin increased by only 7.5 points. And in this sample, this is nearly equal to the proportion of the population most like to trend toward suffrage (Republican voters who did not vote for suffrage and Democratic voters who did not vote against suffrage made up 10.8 percent of people). Moreover, ecological bounds show that, at most, 65 percent of that group could have switched to vote for suffrage.

- Second, we can also assess the plausibility of the implied magnitude of the selection bias. At the *lowest* ecological bound on the veteran voting for suffrage, for the ATT to be 0, we'd have to believe that people trending toward suffrage enlisted with a probability of 0.42, while those who were not trending toward suffrage enlisted with probability of 0.23 (1.8 times more likely). At the *highest* ecological bound on veteran

voting, we'd have to believe that those trending toward suffrage enlisted with probability of 0.85 while those who were not enlisted with probability of 0.18 (4.7 times more likely). For comparison, the *maximum possible* ratio of Republican enlistment rates to Democratic enlistment rates implied by ecological bounds was 3.1 (the lowest possible ratio was 0.38).

Thus, in order to claim that confounding entirely explains the effect of service on soldier's support for suffrage, the most plausible scenario still requires that we assume that:

- veteran support for suffrage after the war was actually at the lowest possible value implied by ecological bounds
- that this was driven by some unobserved and unknown confounding that was larger than the most plausible source of confounding (Republicans not supporting suffrage trending toward support over time)
- that differences in selection into service by party were as large as possible
- that this unknown confounder induced differential selection into service almost as large as partisanship.

Overall, without a plausible account of what this confounder might be, these worries seem unreasonable.

C.5 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-differences effect of enlistment on suffrage, conditioning on 1857 Republican support for townships with very low support for Republicans and Suffrage in 1857. Figure C3 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.

C.6 Heterogeneous Effects

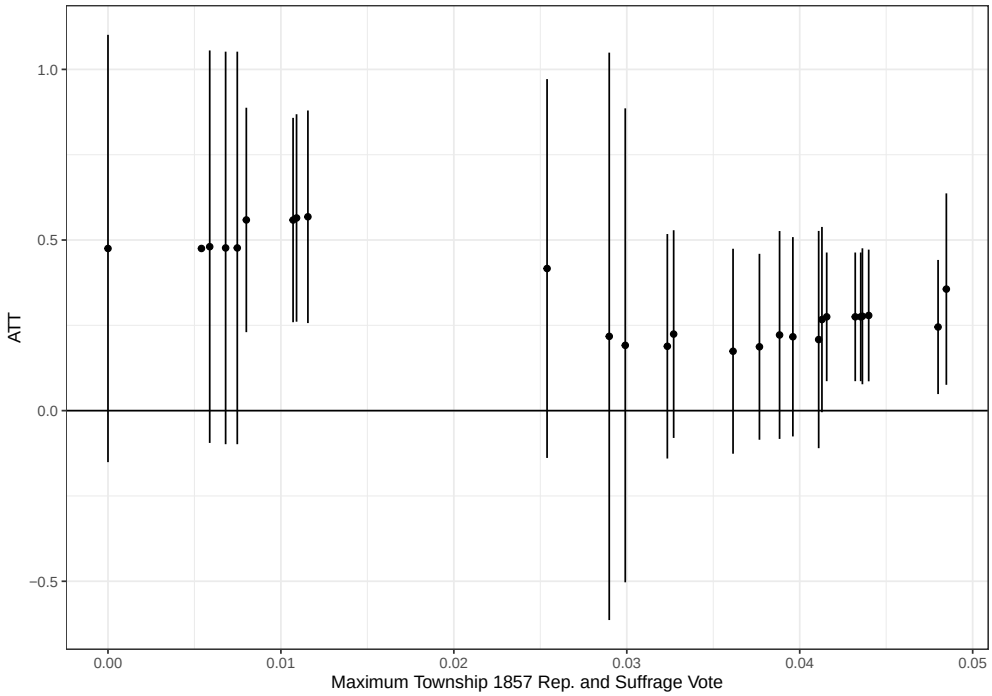
C.7 Effects of Legislators

D Data

D.1 Demographic and Economic Data

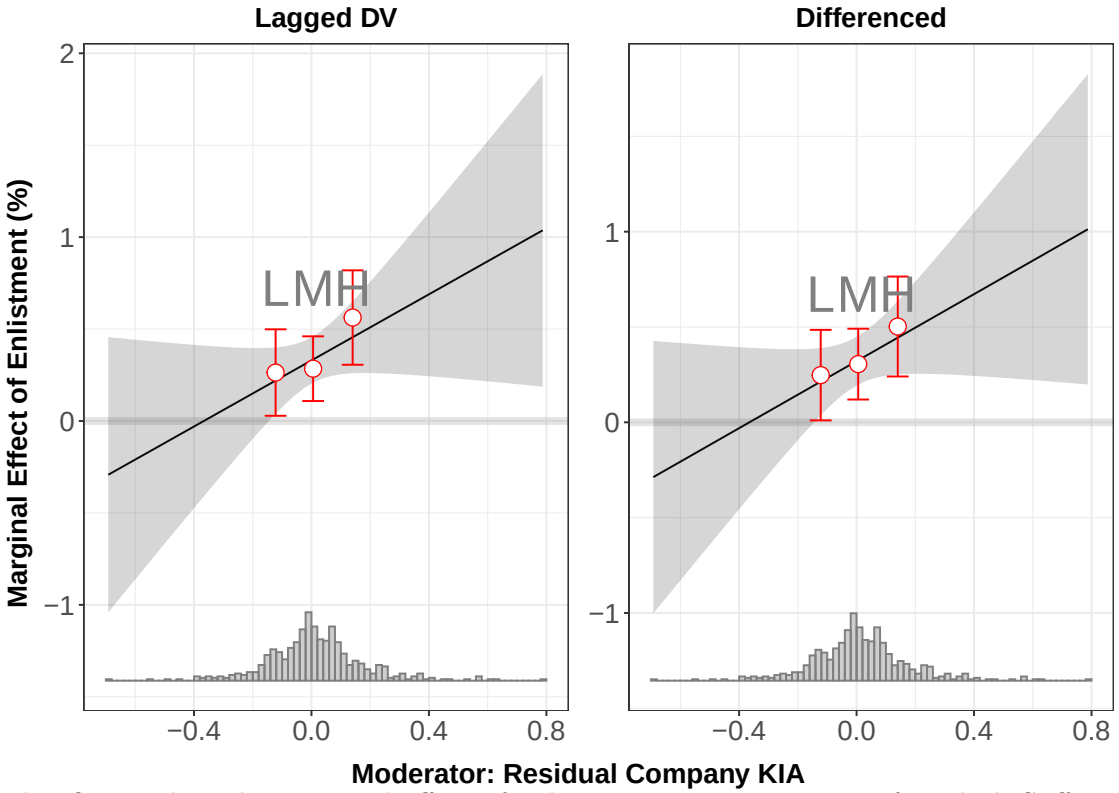
Data on economic and demographic attributes come from the United States census county-level reports available on the ICPSR. Using data from 1860, I calculate the following characteristics of counties:

Figure C3: Difference-in-Difference effects of Enlistment on Support for Suffrage in Townships with Low Republic/Low Suffrage Support (Wisconsin)



This figure shows the difference-in-differences 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. Standard errors are the maximum of homoskedastic or HC3 variance estimates.

Figure C4: Marginal Effect of Enlistment Rates on Votes for Black Suffrage Conditional on Company Casualty Rates



This figure plots the marginal effect of enlistment rates on support for Black Suffrage across mean experience of combat casualties in the Iowa and Wisconsin state constitutional referenda in 1857, 1865, and 1868. All models include state dummies. Enlistment rate is number of surviving men who enlisted over those eligible to vote in 1865. Residual Company KIA is the standardized difference between combat deaths in a soldier's company and the rest of his regiment, averaged by town. Suffrage vote totals come from state constitutional referenda returns in 545 stable clusters of townships/counties in Wisconsin and Iowa. Townships are weighted by number of white men. Standard errors are robust.

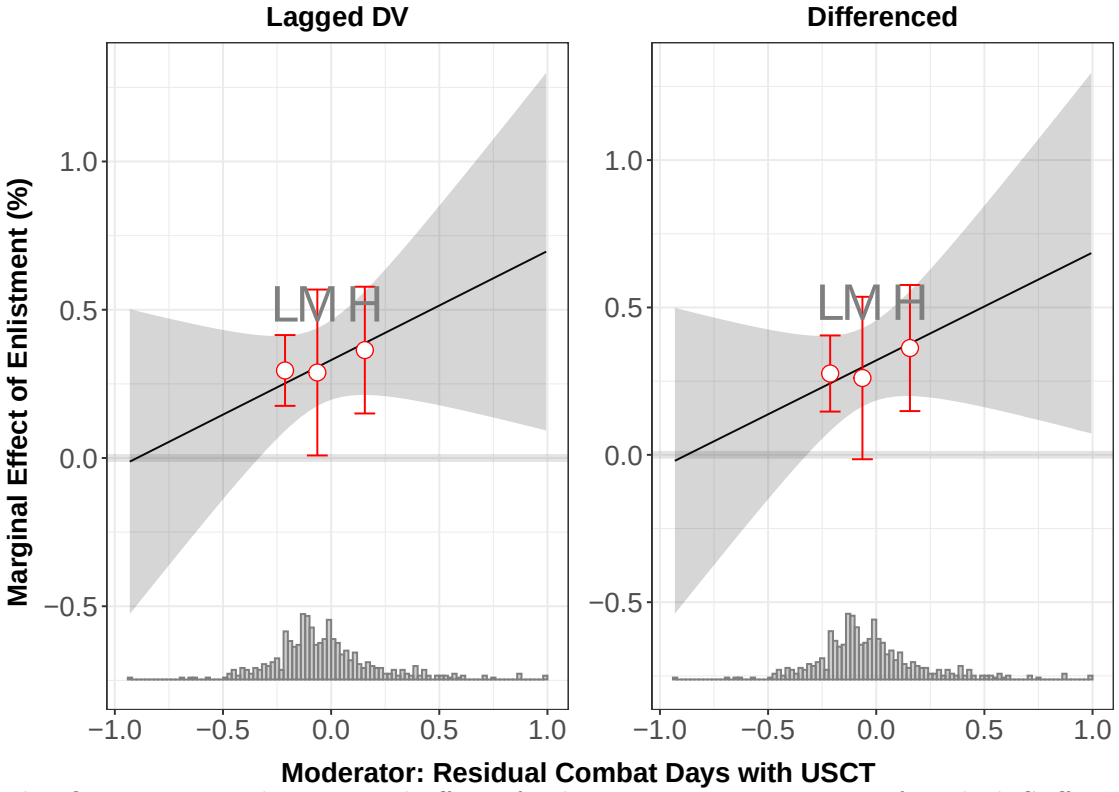
Table C2: Predictors of Iowa Republican Support for Removing Racial Qualifications

	Dependent variable:			
	Universal Suffrage		Full Equality	
	(1)	(2)	(3)	(4)
Enlistment Rate	6.292*	6.686*	3.183	5.132
	(3.446)	(3.760)	(3.297)	(3.772)
Pro-Suffrage VS (1857)		4.121		7.491**
		(3.655)		(3.733)
Republican VS (1857)		−0.985		0.034
		(3.576)		(4.167)
Observations	66	66	67	67

Note: *p<0.1; **p<0.05; ***p<0.01

Estimates from logit regression. Observations are Iowa General Assembly constituencies held by Republicans in 1866. "Universal Suffrage'' indicates a vote against amending the proposed constitutional amendment to grant limited suffrage to African Americans and then voting for suffrage extension. "Full Equality'' further indicates voting to eliminate all racial qualifications in the Constitution. Enlistment rate is number of men serving over those of military age in 1860. Suffrage voteshares come from state constitutional referenda returns in 1857. Republican voteshares come from the gubernatorial election of 1857.

Figure C5: Marginal Effect of Enlistment Rates on Votes for Black Suffrage Conditional on Days of Combat alongside USCT Regiments

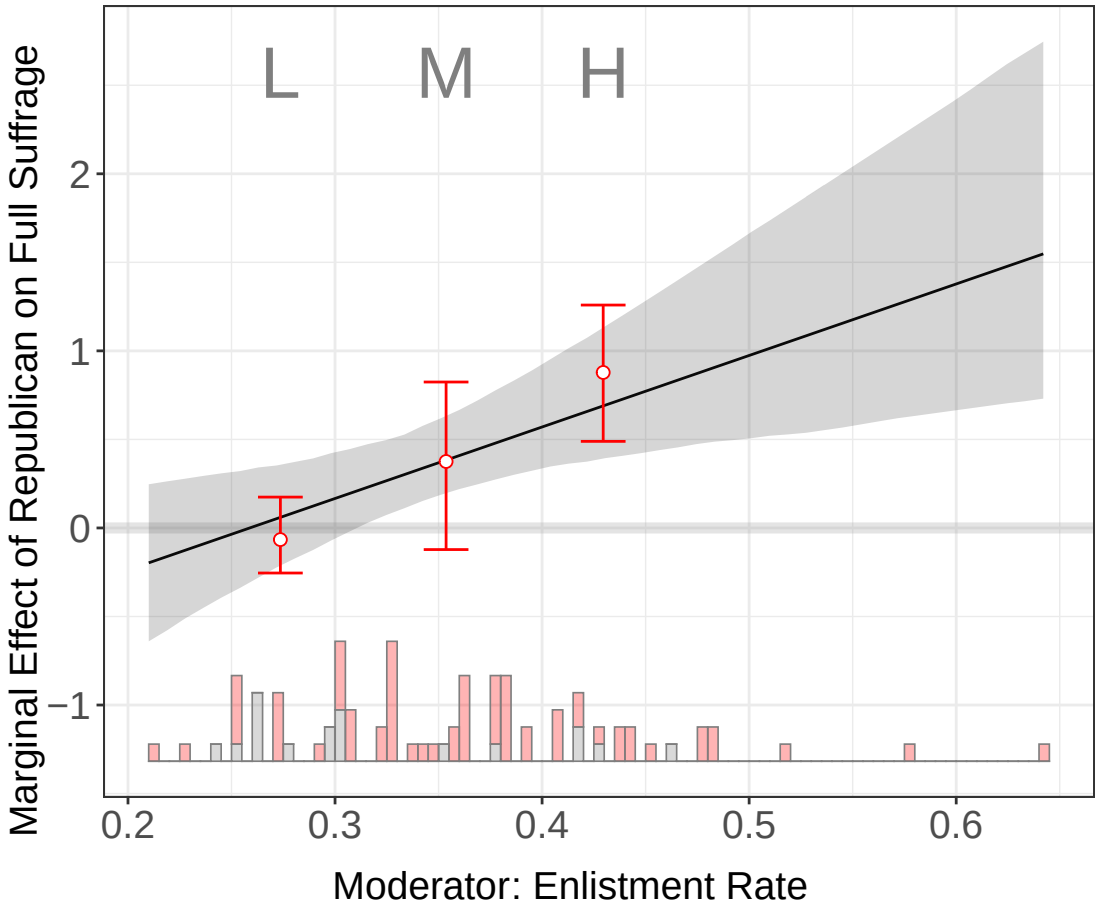


This figure reports the marginal effect of enlistment rates on support for Black Suffrage across mean experience of combat alongside USCT regiments in the Iowa and Wisconsin state constitutional referenda in 1857, 1865, and 1868.. All models include state dummies. Enlistment rate is number of surviving men who enlisted over those eligible to vote in 1865. Residual USCT Combat is the standardized difference between days a soldier spent fighting alongside USCT units compared to other soldiers who enlisted into the same branch of service and term of enlistment in the same year, averaged by town. Suffrage vote totals come from state constitutional referenda returns in 545 stable clusters of townships/counties in Wisconsin and Iowa. Townships are weighted by number of white men. Standard errors are robust.

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 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.

Figure C6: Marginal Effect of Republican Partisanship on Legislator Support for Full Suffrage Conditional on Enlistment Rates



This figure plots the marginal effect of Republican partisanship of Iowa state legislators on their support for full African American suffrage, conditional on the enlistment rate in their constituency. Republicans in low-enlistment constituencies were more similar to Democrats, while Republicans in high-enlistment constituencies were significantly more likely to support equal suffrage for African Americans.

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 (McManus 1998), 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.

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).

D.5 Newspaper Coverage

To characterize the nature of Republican messaging on Reconstruction-related issues, I coded the coverage of the November 1865 Wisconsin suffrage referendum in 28 newspapers. I tracked down all Wisconsin newspapers that have digitized issues for the month between October 3 and November 7 1865. This amounts to 168 newspaper issues. I then had an RA read through each issue and identify articles that discussed African American suffrage generally and/or the upcoming referendum in particular. For each article, the RA classified whether the content was pro- or anti-suffrage in nature. Then, they recorded which arguments were used to support or oppose suffrage, as well as some other attributes. While this is not a random sample, it nevertheless covers 20 different Republican newspapers across 20 different counties (there were 60 counties in Wisconsin at this time). The list of Republican newspapers can be found below.

The presence of various pro-suffrage arguments in newspapers were coded using binary indicators, as follows:

Table D1:

publication	county
The Wisconsin State Register	Columbia
Janesville Weekly Gazette	Rock
The Appleton Motor	Outagamie
The Racine Advocate	Racine
Vernon County Censor	Vernon
Grant County Witness	Grant
Waukesha Daily Freeman	Waukesha
The Wisconsin Lumberman	Portage
Racine Journal	Racine
Mineral Point Weekly Tribune	Iowa
The Saturday Evening Press	Winnebago
Dodgeville Chronicle	Iowa
Waupun Times	Dodge;Fond Du Lac
Wisconsin State Journal	Dane
Lafayette County Union	Lafayette
Whitewater Register	Walworth;Jefferson
Door County Advocate	Door
Oconto Pioneer	Oconto
Shawano County Journal	Shawano
Eau Claire Daily Free Press	Eau Claire

- African American people loyal in the war and earned citizenship/voting rights
- African Americans soldiers loyal in the war and earned citizenship/voting rights
- African American voters needed to ensure end to slavery
- African American voters needed to ensure victories of the war remain won
- Former soldiers/officers endorse suffrage
- Need black voters to prevent return of rebels to power
- Need loyal voters in the South
- Opposition to suffrage by ”Copperheads” or ”traitors”
- White southerners/confederates have unfair advantage in representation
- Punish south for Rebellion

The presence of various anti-suffrage arguments in newspapers were coded using binary indicators, as follows:

- Suffrage leads to ”social equality” or mixing of races
- Suffrage will establish ”black supremacy”
- Suffrage tramples on the rights of white men
- Suffrage is insincere partisan ploy
- Suffrage/civil rights not the purpose of the war (only Union)
- Suffrage the work of ”radicals” or ”abolitionists” in pejorative sense
- Suffrage will alienate white voters from Republican party
- Suffrage will antagonize white Southerners
- Former soldiers/officers oppose suffrage

Other features of suffrage content was coded using binary indicators, as follows:

- Claim that loyal Republicans/Union party members must support suffrage
- Republicans/Union party members attacking members of their own party for NOT supporting suffrage
- Link voting for suffrage to fighting the rebels
- Republicans/Union party members opposing suffrage
- Republicans/Union party members personally support suffrage
- Demand for Republican/Union party members to state a position on suffrage

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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