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Abstract

This paper presents new evidence on the critical role of lower-level organizational leaders. Unlike top managers, frontline leaders are essential for implementing organizational strategies by maintaining team cohesion when shirking is profitable for workers. We study this in the context of the Union Army during the U.S. Civil War, using data on 2.2 million soldiers and tracking captains and their 100-soldier companies at weekly frequency throughout the conflict. We estimate leader fixed effects during non-combat weeks to measure leadership quality in a leader value-added framework. We validate this measure by showing that captains were not assigned based on prior unit performance or observable pre-war characteristics. High-quality leaders earned more after the war, but not before, and were more frequently recognized as good leaders in their postwar biographies. Daily event-study estimates around major battles show that better captains significantly reduced desertions in combat. Exploiting quasi-random leader turnover, we find evidence that this effect is causal. Using digitized battle maps, we rule out risk aversion as a mechanism and find instead that better leaders had higher mortality rates, consistent with a leading-by-example explanation. We also document modest learning-by-doing effects. These findings highlight the often-overlooked importance of frontline leadership, where direct supervision and interpersonal influence are strongest.

Keywords: Leadership, Leader Value-Added, Group Cohesion, Social Capital

JEL Codes: N11, J24, D9, M12

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1 Introduction

The importance of leaders in all aspects of life—social, political, economic, and others—is now well-established in the economics literature.[1] Most of the empirical work emphasizes individuals at the top who set strategic direction. Yet many of the core functions of leadership—motivating, coordinating, and guiding others—take place further down the hierarchy, where frontline leaders interact directly with the groups they aim to influence. This is precisely where leadership, understood as the "ability to induce others to follow absent the power to compel or to provide formal contractual incentives" (Hermalin, 2012, p. 2), becomes most visible. Despite their central role in translating strategy into action, our understanding of these frontline leaders remains limited—largely due to data constraints and the challenges of measuring leadership ability.[2]

To study frontline leaders' ability to maintain cohesion and motivate group members to work toward a common goal under pressure, we exploit data on 2.2 million Union Army soldiers during the U.S. Civil War (1861–65) to construct a weekly panel of all companies. Each company consisted of about 100 soldiers led by a captain, and we observe all battles these companies fought in. This dataset provides a unique opportunity to observe captains and their soldiers in situations where battlefield success depended on collective effort but where incentives to shirk (i.e., desert) were strong and enforcement tools were limited. These constraints reflected the Union Army's reliance on volunteer soldiers and its reluctance to punish desertion, in part to avoid deterring future enlistment (see e.g. Costa and Kahn, 2010).[3] Tracking leaders and their soldiers over the course of the war allows us to assess leader quality as a captain's ability to prevent desertions. Desertions are our preferred measure of group cohesion and motivation because they are consistently available in our high-frequency panel and had direct consequences for combat effectiveness. When too many soldiers deserted, companies lost the manpower needed to fight effectively, reducing their chances of success in battle and, ultimately, in the war. We validate the relevance of our desertion measure by showing that captains with lower desertion rates were more often described as leaders, heroes, or brave in 6,006 postwar biographies. Finally, our panel data allow us to examine changes in leaders' experience, battlefield performance, and economic outcomes before and after the war, shedding new light on the role and impact of frontline leaders.

We measure leader quality as captain fixed effects from a regression of desertions on company characteristics during weeks without battle or fighting. Outside of battle, soldiers deserted for various reasons—including dissatisfaction with pay, poor hygiene, food rations, homesickness, or simply the opportunity to leave. Because these desertions were not primarily driven by immediate threats to life, we hypothesize that a captain's ability to keep the unit together during these periods reveals an underlying dimension of leader quality: the ability to maintain cohesion and motivation where pressure is sustained but formal enforcement is weak. If this is correct, it should carry over to more consequential moments,

[1] Important contributions include work on company CEOs (Bertrand and Schoar, 2003; Benmelech and Frydman, 2015), cultural leaders (Calderon et al., 2023; Bazzi et al., 2023; Ang and Chinoy, 2025), political leaders (Jones and Olken, 2005; Besley et al., 2011), coaches of sports teams (Berry and Fowler, 2021), religious leaders (Mehmood and Seror, 2023), and local community leaders (Kosfeld and Rustagi, 2015).

[2] Leadership ability is multidimensional and context-dependent, encompassing social influence (Akerlof and Holden, 2016), leading by example (Hermalin, 1998), and providing direction, conviction, and vision (Dewan and Myatt, 2008; Bolton et al., 2012).

[3] Only around 40,000 soldiers were inducted via a draft in the later stages of the war (McPherson, 2003). Of the 2.2 million Union Army soldiers, 220,000 deserted but only 147 deserters were ultimately executed according to the U.S. Office of the Judge Advocate General, "Proceedings of U.S. Army courts-martial and military commissions of Union soldiers executed by U.S. military authorities, 1861-1866" (National Archives and Records Administration, 1988).

such as cohesion in high-stakes combat. We confirm this and show that the ability to reduce out-of-battle desertions is a strong proxy for maintaining group cohesion in high-stakes combat.[4] Our approach follows earlier work on the effects of CEOs and managers on firm productivity (e.g., Bertrand and Schoar, 2003; Lazear et al., 2015), as well as the teacher value-added literature (see Chetty et al., 2014), where individual fixed effects are used to capture persistent differences in effectiveness across leaders.

One concern with the leader value-added approach is that the resulting fixed effects are a black box. We propose several strategies to unpack these effects and make them interpretable. First, we examine potential selection of leaders into units and validate that our estimated fixed effects capture individual leadership ability as defined in our framework. We show that (i) high-quality leaders were not systematically assigned to units with higher or lower desertion rates, and that the fixed effect of the current captain is uncorrelated with both the desertion rate and fixed effect of the previous captain, ruling out assignment based on prior outcomes or leadership quality; (ii) leader fixed effects cannot be predicted by a broad set of pre-war captain characteristics drawn from linked 1860 census data, including wealth, previous occupation, age, or literacy, among others; (iii) leader fixed effects reflect traits valued in labor markets, as better leaders experienced earnings gains after the war, but not before.[5]

A standard deviation increase in the leader fixed effect is associated with a 7 percent increase in a captain's 1870 occupational income score, consistent with previous work on leadership wage premia (Kuhn and Weinberger, 2005; Hoffman and Tadelis, 2021). This income effect is robust to post-double selection estimation (Belloni et al., 2014), alternative census linking methods, and adjustments for differential linking probabilities; finally, as noted above, (iv) better captains were more likely to be described as *leader*, *hero*, or *brave* in their postwar biographies, with consistent results across both bag-of-words and ChatGPT-based classification.[6] Interestingly, the Union Army itself was not effective at recognizing good leaders in their promotion decisions. We find no significant effect on promotions, which is consistent with the historical narrative (Glatthaar, 1995).[7] This is helpful in our setting, as it suggests that leader rotation was not driven by the promotion of better captains—otherwise, effect sizes would be attenuated, since weaker leaders would have remained in place longer.

Next, we provide evidence that our fixed effects approach to measuring leadership quality has meaningful implications for desertions in battle. This reinforces findings from the existing literature on the importance of leaders, with both the theoretical and empirical management literature emphasizing the importance of leadership at 'critical junctures' (Horowitz and Kenerly, 2014). To this end, we consider the 19 largest and most decisive battles of the conflict, as listed in Selcer (2006),[8] when team cohesion was severely tested and officers had to prove their leadership by preventing their units from breaking

[4] This relates to work by Metcalfe et al. (2023) who show that managers who perform well in high-growth periods also perform well during more difficult times regardless of worker composition and firm-level policies.

[5] As a placebo exercise, we also show that leader fixed effects are unrelated to regular soldiers' postwar economic outcomes to rule out the effect of social networks established during the war or better socio-economic conditions in counties from which better captains originated.

[6] Using ChatGPT-4o mini, we submitted all biographies separately via the OpenAI API and asked: "Does the following biography mention that this person is a good leader? Answer with yes or no only". While it is not exactly clear how the large language model makes this determination, it is likely unrelated to our choice of words to use as outcomes. Hence this second classification approach provides another layer of evidence that supports our main findings.

[7] Citing several examples of promotions, Glatthaar (1995) notes (p. 24): "There was no set formula or guideline that determined who were the good officers and who were the bad ones". Failure to promote competent soldiers into officer ranks was common and the lack of a formal process was oftentimes perceived as unfair by the troops (Bledsoe, 2015).

[8] Selcer (2006) originally lists twenty battles. However, the last was a naval engagement, which we do not observe in the data. Examples of such large battles are Shiloh, Antietam, Gettysburg, or the First and Second Bull Run.

under flying bullets (Linderman, 1987; McPherson, 1997). Similar to the management literature, historians of war also emphasize the critical importance of effective leadership in high-pressure situations (Grinker and Spiegel, 1945; Savage and Gabriel, 1976; Kellet, 1982).

For this analysis, we construct a daily company-level panel spanning the seven days before and after each battle. Event study regressions confirm that desertions spike on the day of the battle and then fade over the two subsequent days, with no significant anticipatory responses in the days leading up to the battle. This result is robust to using either never-treated or not-yet-treated units as control group, and we adjust the latter for potential weighting biases highlighted in the recent two-way fixed effects literature by using the estimators developed by Dube et al. (2023) and Sun and Abraham (2021). In a triple-differences specification, where we additionally differentiate units with above and below-median leadership quality, we find that units led by above-median quality captains experienced a cumulative desertion increase of 2.5 percent over the battle day and the following two days. In contrast, units with below-median quality leaders saw a 4.3 percent increase in desertions over the same period. That is, units with lower-quality leaders had a desertion rate nearly 1.72 times higher than those with high quality captains.

The effect of leadership quality on desertions is also evident at the individual level. Using data on soldiers' desertion decisions during the main day of each major battle, we find that a one standard deviation increase in leader quality reduces the probability of desertion by 1.6 percentage points, a sizable drop relative to the unconditional desertion probability of 3.9 percent and consistent with company-level results. This effect is robust to a wide set of controls, including fixed effects and individual, unit, and leader characteristics, as well as different standard error specifications. To assess causality, we exploit captain deaths in the previous battle as an instrument for changes in leader quality. Given that even major battles had limited spillovers on post-battle desertions, and battles were spaced an average of 63 days apart, this provides a plausible source of exogenous variation. The analysis suggests that positive changes in leader quality due to death-induced turnover reduce desertions by about 7 percent in the next battle. This result is robust to controls for unit composition, past deaths and desertions, and previous battle outcomes.

We consider two mechanisms that might explain why leadership quality affects desertions: risk aversion and learning-by-doing. First, if risk averse leaders stayed behind with their units, and therefore sustained lower battle casualties—resulting in lower desertion rates—then our interpretation of the estimated leader fixed effect would be flawed. To test this, we digitized 125 battle maps via a pattern recognition algorithm which enables us to measure the distance of each Union Army unit to the nearest enemy unit on the battlefield. Results show that units with better captains were neither closer nor further away from enemy units, with a precisely estimated null effect. This suggests that risk aversion did not vary with estimated leader quality and therefore does not appear to be an explanation for our findings. Interestingly, despite having lower desertion rates, units with better captains also had higher rates of battle deaths among their soldiers.[9] At the same time, high-ability captains also had a higher probability of dying in battle, which is in line with a leading-from-the-front and leading-by-example explanation of leadership emphasized by Hermalin (1998), and is also consistent with historians' assessment that officers motivated troops by displaying courage (Linderman, 1987; McPherson, 2009; Costa, 2014).

Second, to test the learning-by-doing explanation, we take our company-week panel and divide each

[9] In a placebo exercise, we find no effect on accidental deaths.

captain's tenure in a given company into six-weeks spells. We then re-estimate the leader fixed effects for each spell separately and observe the evolution of this leader-spell fixed effect over time. Regressing the leader-spell fixed effect on an index of the number of spells served and the square of the index, we find a significant relationship between the increase in spells served and a captain's leadership quality over time. This relationship is highly non-linear, with rapid gains in the first two to three spells, and a plateau thereafter, indicating that captains adapt quickly to their leadership roles. Serving one additional six-week spell increases the estimated captain fixed effect by 2.5 percent of a standard deviation, controlling for regiment and week fixed effects as well as unit characteristics. While this learning effect is modest relative to the substantial cross-sectional differences driven by time-invariant leadership ability, it remains meaningful in capturing the pace of adjustment. This result is not mechanically driven by losing the highest propensity deserters in the first leader spell, by changing unit composition, or by exits of good leaders due to promotion, or exits of bad leaders due to death in battle. When plotting the evolution of leader spell fixed effects by the year of entry into the army, we find that earlier captains started worse and declined over time in quality — consistent with the historical narrative that early captains were elected with little to no information and oftentimes performed poorly (Bledsoe, 2015) — while captains in later cohorts exhibit better and more consistent growth in terms of their quality.

To further strengthen the robustness of our findings, we address two additional concerns related to inference and the estimation of the leader fixed effects. First, because our leader fixed effects are generated regressors, we consistently bootstrap standard errors when using them as explanatory variables. Second, we show that our results hold across three alternative approaches to estimating leader fixed effects, providing reassurance that our findings do not hinge on a single measure. Specifically: (i) instead of using the inverse hyperbolic sine transformation, we use desertions as count variable and estimate leader fixed effects via Poisson pseudo-maximum likelihood (PPML) regressions; (ii) we estimate fixed effects using only out-of-battle observations prior to the first battle a leader and his unit are involved in, which limits the impact of changing unit composition; (iii) we include company fixed effects following the mover design framework of Abowd et al. (1999) and Card et al. (2016), absorbing constant but unobserved unit characteristics; (iv) we link individual soldiers to the 1860 census, allowing us to estimate leader fixed effects using individual-level data as in the CEO and teacher value-added literature (e.g. Bertrand and Schoar, 2003; Chetty et al., 2014), while controlling for a wide range of soldiers' pre-war characteristics.[10] Together, these checks underscore the consistency and credibility of our core results, reinforcing the conclusion that leadership quality as measured by the fixed effects approach is a meaningful predictor of group cohesion under pressure.

What generalizable insights can be gleaned from our setting? The military shares many similarities with modern firms, a view that is articulated in a large management literature connecting military and corporate leadership (Duffy, 2006; Jha and Wilkinson, 2012; Benmelech and Frydman, 2015; Law and Mills, 2017; Horowitz, 2019). Similarities include hierarchical structure, division of labor into smaller teams, a team production function that depends on cooperation and strategic complementarities, and the challenge that workers have incentives to shirk while leaders have limited options to induce effort. Union Army captains could bring deserters before a military court, and managers can fire workers, but both are corner solutions that risk damaging morale among those who remain or merely induce effort just above the threshold of sanction, similar to modern-day quiet quitting (see Lee et al., 2023). We therefore

[10]The construction, rationale, and up- and downsides of all estimation approaches are described in detail in Appendix B.

believe that our results have implications for modern-day firms, where recent work has emphasized the importance of management styles and managerial effectiveness (Bertrand and Schoar, 2003; Bloom and Van Reenen, 2007; Bloom et al., 2019; Hoffman and Tadelis, 2021).[11]

More broadly, our study contributes to a growing literature in organizational and managerial economics that examines how managers influence firm outcomes, the training and development of managerial skills (Bianchi and Giorcelli, 2022; Giorcelli, 2023), and the mechanisms through which managers enhance worker productivity (Lazear et al., 2015; Fenizia, 2022). Our high-frequency panel of companies as military firm-equivalents allows us to quantify the importance of captains—low-level military managers—in maintaining team cohesion when stakes are high. While much of the literature has focused on higher-level managers, such as CEOs, we shed new light on lower-level leaders, their importance, and how they influence their followers to comply with orders in the absence of strong mechanisms to compel as theorized by Hermalin (2012). This complements recent work by Metcalfe et al. (2023), who use data from two large retail firms to show that individual store managers, rather than firm-wide management practices, account for a substantial share of store-level productivity variation. They find that manager quality is hard to observe but highly consequential, and that it is hard to explain with observable characteristics, as in our case. This again highlights the similarities between modern private sector firms and the military setting, as in both cases lower-level leaders are crucially important.[12]

We also contribute to the broader empirical literature on leadership, which has examined various socioeconomic and political dimensions (Jones and Olken, 2005; Besley et al., 2011; Kosfeld and Rustagi, 2015; Dube and Harish, 2020; Dippel and Heblich, 2021; Mehmood and Seror, 2023; Ottinger and Voigtländer, 2025; Assouad, 2025). Studying leaders in a military setting offers valuable insights into how effective leaders emerge and what makes them effective, particularly by highlighting the importance of maintaining teamwork and team cohesion under pressure. Our analysis also adds to our understanding of the role of leadership in the historical context of the Union Army, a setting that has been used to study social networks (Costa and Kahn, 2003; de Paula, 2009), the intergenerational effects of military service (Costa et al., 2020; Dupraz and Ferrara, 2025), and links to social movements and socio-political norms (Dippel and Heblich, 2021). Finally, our empirical findings on the role of captains' leadership in the Union Army complement and support the work of Civil War and military historians such as Bledsoe (2015), McPherson (1997), and Linderman (1987). They further relate to findings from other conflicts, such as the role of leadership in maintaining soldier accountability in Italy during World War II (Bertazzini and Giorcelli, 2025), or the subsequent diffusion of extreme political behavior via leaders in France (Cagé et al., 2023), for instance.

[11]Even though we do not take a strong policy stance, a potential policy recommendation would be to prioritize managerial training that focuses on people skills that improve team cohesion and teamwork. Theoretical support for this suggestion is provided by Weidmann and Deming (2021) and Deming (2017).

[12]This is consistent with our data. When we regress the inverse hyperbolic sine of desertions on fixed effects for different types of leaders (captains, colonels, generals), the lowest-level officers, i.e., captains, who are in touch with soldiers the most, have the highest explanatory power. This is shown in Figure A.1.

2 Data and Historical Background

2.1 Citizen Officers in Frontline Leadership Roles

On April 15, 1861, President Lincoln called for 75,000 volunteers after the attack on Fort Sumter. Three months later, he had to request another 500,000 volunteers (Selcer, 2006). The Union Army began to grow to over 2.2 million soldiers, requiring more than 24,000 officers to lead these men. It soon became clear that the regular Army could not staff all companies and regiments with their own officers, who totaled one thousand at the time (Bledsoe, 2015). As regiments were organized by the individual states, units were formed locally with people from the same communities. A key question then was who should lead these units. The volunteer soldiers, who comprised 94 percent of the Union Army, were morally rooted in the citizen-soldier ethos that had existed since the time of the Revolutionary War; a key aspect of this ethos was not only to make a sacrifice for the Republic, but also to maintain democratic rights, the most important to soldiers back then being the right to elect their own officers (Ural, 2019).[13]

The early officer elections oftentimes evolved into popularity contests with wealthy or politically influential individuals seeking an edge to obtain a company's captaincy.[14] Before the start of most major battles in 1862, soldiers could not make an informed decision as to who would be a good leader. Once an officer was elected, their soldiers were stuck with them until the officer died or got severely injured, promoted, or when his enlistment contract expired. There were no formal systems in place to check the officers' quality or qualifications, or to provide them with training. Such requirements were introduced only later and after the first major battles were fought, showing results in 1863 when due to "practical experience in the field, junior-officer quality and training showed a gradual improvement" (Bledsoe, 2015, p. 46). In the beginning of the war, captains were older, more urban, more likely to be managers or to work in other high-skilled professions, less likely to be laborers, and more likely to be wealthy than regular soldiers. These differences became significantly smaller over the course of the war as soldiers became better at electing captains based on military leadership and quality than other non-military credentials (see Table A.1). Even then, electing good leaders remained largely a lottery, as even good soldiers did not always make good leaders.

Citizen officers faced the task of leading men whom they had lived with in their local communities. In the early stages of the war, citizen officers had to strike a balance between maintaining good relationships with their soldiers and commanding them with authoritative respect, yet without coming across as tyrannical. In addition, they also had to learn how to be soldiers themselves. Early citizen officers therefore oftentimes sought to motivate their men by own displays of courage and bravery to establish their leadership (Linderman, 1987). As the war progressed, however, company leaders "had learned that good commanders had to do far more than look impressive and be recklessly brave under fire; they had to constantly attune themselves to the condition, emotions, and well-being of their companies and labor incessantly to maintain the fighting effectiveness of their volunteers at all times. Even the slightest slip could compromise their ability to maintain control on the battlefield" (Bledsoe, 2015,

[13]The initial laws set out that officers be appointed by governors or the president, but given the number of required appointees, this was soon abandoned and elected officers were typically rubber stamped by officials. Later in the war, regimental commanders still had the ability to override or surpass elections, but this was the exception and commanders had to have good reasons and a firm grip over their unit to unilaterally make this decision (Bledsoe, 2015).

[14]In a letter to his sister, Henry P. Goddard of the 14th Connecticut Infantry's company G strongly expressed his dissatisfaction with the qualifications of the candidates who ran for captain in his unit, stating that "[i]f we were a civilian and they should come into my office, we would not make one of them a printer's devil" (Zon, 2008, p. 44).

p. 153). This also pertained to situations outside of battle. Long periods of idleness were not uncommon for many units during the war and maintaining discipline in camp was another important skill to deter desertion, as boredom, disease, alcohol, and other vices chipped away at units' morale. The complex task of *leadership* therefore meant that company officers had to understand their soldiers, their needs, and what motivated them the most, while being an example, brave, competent, the social anchor of the group, as well as a figure of authority.

2.2 Individual-Level Union Army Soldiers Data

Our main data source is the Union Army's regimental rosters from each state's *Adjutant General's Reports*, which contain information on individual soldiers by regiment and company.[15] These data were collected after the war from the roster, roll call, and other military records to determine soldiers' eligibility for pensions. Figure A.2 shows example pages of the Adjutant General's reports. The reports exist for every state that provided troops for the Union Army with information on 2.2 million soldiers. This is close to the universe of Union Army soldiers and, even though not every soldier is recorded, the quality and completeness of the data are generally high.[16] In total, our data contain 2.9 million soldier records for 2.2 million Union Army soldiers. The number of records is higher than the number of soldiers due to re-enlistments. The data include the soldiers' full name, company and regiment, state and county of residence, age, place and date of enlistment, place and date when a soldier left service, the reason for the exit, such as a regular discharge, but also wounds, disabilities, being taken prisoner, death, or desertion. The data further include each soldier's rank and information on promotions. Using their personal information, we can further link 920,244 soldiers to the 1860 census, of whom we can link 247,044 to the 1870 census as well.[17]

Enlistment rates for the Civil War, especially among prime-aged men in the early 1860s, were high and were only exceeded during World War II. 80 percent of men born between 1837 and 1845 eventually served in the Civil War (Costa and Kahn, 2010). The vast majority of soldiers were volunteers who comprised 94 percent of all soldiers and the most common enlistment contract was for a duration of three years. Most soldiers saw out the end of their enlistment term, at which point they were mustered out or re-enlisted. After regular exits, the second-most common reason to end their term were injuries, disabilities or illness. The third-most common reason was death; over 16 percent of men died and almost half of those deaths were battle deaths. The fourth-most common reason was desertion, with 220,000 out of 2.2 million soldiers deserting — for comparison, out of 16 million U.S. soldiers serving during World War II, only 50,000 deserted (Glass, 2014). Civil War soldiers deserted for many reasons, including dissatisfaction with pay or rations, low morale after large-scale battles, diseases in camps, missing their home and families, adverse weather or climatic conditions, or simply because there was an opportunity to fall behind their own lines (Costa and Kahn, 2010). Deaths in battle and desertions often coincided (see Figure A.3) but since the Union Army was a volunteer force, desertion was rarely punished and fewer than 200 Union Army soldiers were ever executed for desertion (McPherson, 2003).

[15] For references to each report see Table A.2.

[16] William Schouler, Adjutant General of Massachusetts, noted that "most of the regiments and batteries are perfect, every man accounted for; of the whole number there are but 1,205 who are not accounted for" (1868, p. 121). These 1,205 unaccounted soldiers were 1.1 percent of all soldiers who hailed from Massachusetts.

[17] This means we can link 41.8 percent of soldiers to the 1860 census, and of those we can observe 26.8 percent in 1870. Cross-census links were made using Census Tree Project crosswalks (Price et al., 2021).

This explains why good leadership played such an important role in maintaining group cohesion and combat effectiveness.

2.3 Constructing a High-Frequency Panel of Union Army Companies

Since the individual data provide us with the regiment and company, entry and exit dates, rank, and promotions of each soldier, this allows us to construct a company-level panel of all Union Army companies for the duration of the war. The lowest organizational unit in the Union Army was the company, comprised of a hundred soldiers each, and ten companies would form a regiment.[18] A regiment would normally be led by a colonel and companies were led by captains. When constructing the company panel, we focus on the weekly time frequency because a daily panel would be far too computationally intensive. We will later on use daily information when studying leadership and desertions during event windows around the main battles of the war in an event study design. To construct the panel, we summed up all soldiers who belonged to a specific company and regiment in a given week which we know from their entry and exit dates. There are 8,537 unique companies, organized into 1,061 regiments, with 1,279,271 company-week observations in our sample.[19]

Summary statistics for the companies' characteristics are reported in Table A.3. The average company existed for 150 weeks which is almost three years, coinciding with the duration of the most common enlistment contract available to soldiers. The average company had 68 soldiers, of whom 10 would die — half of those died in battle — and almost 6 soldiers would desert at some point during their service. Even though a typical infantry company would consist of 100 men, cavalry and artillery units tended to be smaller since they were more capital- rather than labor-intensive. More importantly, however, units were not replenished during the war, meaning that unit strength tended to gradually decline and many units never reached the theoretical full regimental strength mandated by military regulations (Bledsoe, 2015). This means that, aside from attrition due to deaths and desertions, the composition of units was mainly fixed which is going to simplify the interpretation of our results later. The average company fought in five battles and in one of the major battles listed by Selcer (2006), had three and a half captains, and every tenth company lost at least one captain due to death.

Using the military and linked census data, we generate additional unit characteristics. To account for pre-existing social networks, considering that most soldiers came from the same geographic area, for each company-week observation we compute the share of soldiers of the same nationality - American, German, Italian, Irish, other - and the share of soldiers who resided in the same county. We also consider such relationships between soldiers and their leaders by computing both the share of soldiers who resided in the same county and who were of the same nationality as their captain. We also compute the log number of soldiers in each unit, and create indicators for the unit type for infantry, cavalry, artillery, and support units.

Lastly, we collect information on units' involvements in battles throughout the war. We collected this information and matched it to each regiment and their companies from Fox (1889) and Dyer (1908).

[18] Cavalry and artillery regiments tended to have fewer soldiers and companies, since they were more capital- than labor-intensive. Regiments were organized in Brigades, led by a Brigadier General. The next higher units would be the Division, Brigade, Corps, and Army. The latter three were commanded by Major Generals. Figure 1 shows an example of the Union Army organizational structure, which we collected from Dyer (1908).

[19] Throughout the war, there were almost 3,000 regiments but many of these were support or guard units that did not see actual combat. Out of the 2.2 million soldier records, the 1,061 regiments cover 1.5 million of them since support and guard units were typically not at full regimental strength.

An example of a page in Fox (1889) is provided in Figure A.4.[20] The books list regiments and battles they were involved in, including the name and dates of the battles, as well as losses, wounded, and missing soldiers. These sources not only mention major battles but also smaller battles and skirmishes, i.e., minor engagements with enemy units where fighting occurred. We further consider any week in our company-week panel as *battle week* if one or more soldiers in the unit were recorded as killed in battle, even if neither Fox (1889) nor Dyer (1908) had recorded a battle for the unit in that week. These are typically for minor engagements that wouldn't be listed in other sources but we add them for completeness. Another focus later in the paper will be on the largest and most decisive battles of the war, where good leadership was crucial for success. Selcer (2006) lists 19 battles that stood out as the bloodiest. The full list and information on these battles is reported in Table A.4.[21] The majority of battles were one-day engagements, but some, like Gettysburg, lasted two days and a handful stretched for several days; the longest battle in Selcer's list was the 'Seven Days Battle' in June 1862. Panel D of Table A.3 shows that the average company in our data participated in one of the 19 major battles, but some companies participated in up to five of those most bloody Civil War battles.

3 Estimating and Unbundling Leadership Ability

3.1 Estimating the Leader Fixed Effects

A key challenge to study why and how leaders matter is to find a good proxy for their quality or ability. We follow the value-added approach that has been applied to managers (Bertrand and Schoar, 2003; Lazear et al., 2015) and teachers (Chetty et al., 2014), among others. This approach typically regresses the outcome of interest on indicators for each manager - or in our case for each officer. A higher fixed effect coefficient then implies that a given manager contributed more to the outcome, which is interpreted as their quality. In our context, we focus on a particular dimension of officer quality which is their ability to reduce desertions. While there are many other dimensions of leadership, such as planning or administrating, we focus on the ability to maintain group cohesion because of the Union Army's limited power to compel volunteers to risk their lives and the importance of team cohesion in battle, which was discussed in the previous section.

To estimate fixed effects for the Union Army captains in our sample, we use the company-week panel introduced in section 2.3. Compared to the managerial economics literature, which mostly relies on quarterly or annual profit reports as outcomes to quantify managers' quality, our high-frequency data combined with battles as well-defined events when leadership is particularly tested provide us with a unique setting: when estimating the fixed effects, we exclude all weeks in which a company was engaged in battles, skirmishes, or if they recorded any battle deaths. These are about 8 percent of all company-week observations.[22] This allows us to estimate captain fixed effects outside of battle and to

[20] A map of the battles and major battles in our sample is provided in Figure A.5.

[21] The twentieth, Fort Fisher, was primarily a naval engagement, had far fewer regiments engaged in it, and far fewer lives lost, and appears to have been added to Selcer's list because of its strategic importance as it effectively ended the Confederate's ability to run the North's naval blockade. We therefore focus on the 19 land battles in Selcer's list.

[22] If we were to estimate the fixed effects over the full sample, this would generate a mechanical correlation when focusing on in-battle performance. Our daily event studies later will show that there was no anticipatory behavior and that desertion effects after major battles lasted for no more than three days. Hence excluding weeks in which battles occurred is sufficient for our purposes but we also describe alternative fixed effect estimation approaches below that will be used for robustness checks.

then use these fixed effects later on to assess the relationship of captain quality and desertions during major battles. The idea is that captains who were successful at maintaining group cohesion outside of battle — when mundane tasks, boredom, disease, and other factors gnawed at soldiers' discipline and willingness to stay — must have had some ability to keep their group together in the absence of formal mechanisms to compel their soldiers, which then might be a suitable proxy for their effectiveness as leaders during battles when team cohesion was even more important.[23]

To estimate the captain fixed effects, we estimate the following panel model using the out-of-battle company-week panel,

$$\text{asinh(desertions)}_{ct} = \kappa_c + \kappa_r + \beta' \mathbf{X}_{ct} + \gamma_t + \varepsilon_{ct}, \tag{1}$$

where $\text{asinh(desertions)}_{ct}$ is the inverse hyperbolic sine transformed number of deserted soldiers in company c in week t, while controlling for the log total number of soldiers on the right-hand side to account for unit size.[24] The main parameters of interest are the captain fixed effects κ_c, which we multiply by negative one to ease interpretation: a higher fixed effect implies *lower* desertions in that captain's unit. We further standardize the fixed effects such that their coefficients in later exercises can be interpreted as the effect of a one standard deviation increase in a captain's leadership quality on the outcome.

The leader fixed effect is essentially a black box that may capture other company-related characteristics that do not reflect the leader's ability. We therefore seek to separate this fixed effect from other factors that potentially correlate with both leadership skill and desertions. Previous work has specifically highlighted the importance of social networks in Union Army units for group cohesion that may be picked up by the captain fixed effect (Costa and Kahn, 2007; de Paula, 2009). We control for unit characteristics $\mathbf{X}_{ct}$ such as the log number of soldiers, and social network measures like the share of soldiers of the same county, the same nationality (Americans, Germans, Irish, and Italians), and both the share of soldiers of the same county and nationality as their captain to control for potential prior connections to the leader. Both McPherson (1997) and Costa and Kahn (2010) argue that captains often came from the same social backgrounds and communities as the men they commanded. We also control for the share of soldiers from counties that had a majority vote share for Abraham Lincoln in the 1860 presidential election, unit type - infantry, cavalry, artillery, staff company, or other specialized units, such as sharpshooters - aggregate time trends in desertion via calendar week fixed effects γ_t, and potential effects from upper-level regiment leaders by including colonel fixed effects κ_r. The importance of company leaders over other leaders or even unit fixed effects is visualized in Figure A.1 where we estimate equation (1) with a given set of leader or unit fixed effects and plot the resulting adjusted R^2 measure. Company leader fixed effects have more explanatory power than company fixed effects, and company leader fixed effects have more explanatory power than those of any higher-up leaders or unit fixed effects.

Figure 2 shows a descriptive binned scatter plot of the *asinh* of in-battle desertions over our estimated

[23] Bledsoe (2015, p. 63) summarizes the importance of leadership during these times as follows: "Camp life, drill, marching, and other duties occupied the majority of the daily routine of the volunteers' experiences. The leadership exhibited by officers in tiresome or mundane periods was critically important, for it shaped experiences, formed expectations, and dictated the actions, instincts, and training that the men all had to bring to bear in times of crisis".

[24] The inverse hyperbolic sine transformation is computed as $\ln(x + \sqrt{x^2 + 1})$. It preserves the properties of the natural logarithm and its interpretation for data with many zeroes without having to add an arbitrary constant to x that can potentially affect the results.

captain fixed effect in panel a. Leaders who were better at maintaining group cohesion outside of battle, i.e., those with a higher fixed effect, indeed also saw lower desertions on days when a battle occurred. Bledsoe (2012) describes how trusted and competent captains managed to rally their soldiers behind them, while those who performed poorly or lost said trust experienced the opposite effect, especially when the stakes were high. Our data and leadership quality measure reflect this narrative. Panel b of the same figure plots the estimated leader fixed effects by week for the duration of the war. To avoid double counting, we only consider the first week of a leader's captaincy in their unit, meaning that the graph reflects the average leader quality of new captains in a given week. The time series shows no discernible patterns over time in terms of the variation in the fixed effects. A simple Dickey-Fuller test cannot reject the null of the series being a random walk ($p = 0.481$).

Alternative measures: To increase confidence in our fixed effects approach, we provide several alternative ways to estimate the captain fixed effects. Each of these has its own strengths and weaknesses, the weaknesses oftentimes relating to a less variation or available data, but we will show robustness of later results to different ways of estimating these leader fixed effects. The first alternative is to use desertions as a count variable in equation (1) instead of the inverse hyperbolic sine transformation, and to estimate the fixed effects via Poisson pseudo-maximum likelihood. The second approach is to only use desertion information under a given captain until the first battle occurred in which the captain leads the unit. This avoids potential selection issues with regards to unit composition.[25] This is one of the more costly options since it throws out 65 percent of the original data. The third alternative is to further reduce the influence of unobserved unit characteristics by including company fixed effects. The fixed effects are then identified by captains who move across different units as in the seminal mover-design paper by Abowd et al. (1999) and Card et al. (2016). The downside is that only about two percent of captains in the sample move to other companies. Fourth and lastly, we use the individual-level soldier data for privates and regress their probability of deserting on individual and unit-level characteristics, as well as the captain fixed effect. This is equivalent to the typical value-added approach with individual data (Bertrand and Schoar, 2003; Chetty et al., 2014). We also show robustness of all results to changes in the functional form of the leader fixed effect by applying the percentile transformation to reduce the influence of outliers (e.g. Derenoncourt, 2022). We provide more details and discussion of the different alternative fixed effects approaches in Appendix B with an illustration of the sampling schemes in Figure B.1.

3.2 Understanding and Unpacking the Leader Fixed Effects

After having estimated the leader fixed effect as a proxy for a captain's quality, we next want to unpack this fixed effect to show that it is a reasonable measure of leadership quality and to understand what made a good leader. In particular, we will empirically show that i) good leaders were not selected into units with systematically higher/lower desertions, and that there is no correlation between captains' quality and that of their predecessors; ii) who would be a good leader could not be predicted based on pre-war observable characteristics; iii) pre-war occupational income scores did not correlate with the leader fixed effect but there was a significant postwar premium for better leaders, implying that our leadership quality

[25] For instance, if only the most trusting soldiers remain in the unit even after a poor performance of the captain in battle, then the later observations would make this captain look like a better performer in terms of reducing desertions.

measure captures something that was valued in the labor market; and iv) leaders with better fixed effects were recognized as better leaders in their postwar biographies.

Testing for selection of leaders into units: Next, we test whether good leaders were strategically assigned to high (or low) desertion units by the Union Army. We collapse the 1,279,271 company-week observations into company-captain observations, meaning that we average company characteristics over the duration of a given captain's tenure in the company, such as the average desertion rate under a given captain in the unit. This produces a sample of 18,074 observations. In Table 1, column 1 reports results from regressing the average inverse hyperbolic sine of desertions under a leader on their estimated fixed effect, controlling for fixed effects for the calendar week when a captain is first observed, regiment fixed effects, and average company characteristics.[26] A standard deviation increase in a leader's fixed effect is associated with an approximate 7.6 percent reduction in desertions in the company during their tenure. Since we include regiment fixed effects, this is relative to other company leaders in the same regiment. This effect is remarkably stable and does not change when we include the previous captain's fixed effect as control (column 2) or when we control for the desertion rate under the previous leader (column 3). This suggests that captains were not systematically assigned to low or high desertion units based on the performance of their predecessor. As a sanity check, when we change the outcome to mean log desertions under the previous leader, then only the previous leader's fixed effect has a negative and statistically significant coefficient with a similar magnitude compared to the main result, while the current leader's fixed effect is entirely insignificant (column 4). Lastly, if we regress the current leader's fixed effect on that of his predecessor and on the mean log desertions under the previous captain, neither variable shows up as significant (column 5). This means that higher quality leaders were not assigned to units where the previous captain did particularly well (or poorly), or to units that had higher or lower desertion rates before the current captain arrived.

Predicting good leaders: A key question for the interpretation of our results is whether good leaders were selected, or whether it was possible to forecast who would become a good leader. For this purpose, we regress the estimated leader fixed effect $\widehat{\kappa}_c$ on observable pre-war characteristics from the 1860 census. All variables, except for indicators, are standardized to have mean zero and unit variance. Figure 3 plots the coefficients from this exercise for the full sample of 10,742 captains who we could link to the 1860 census (panel a) and for the sample of those 6,900 captains whom we could additionally link to the 1870 census (panel b).[27] In either plot, we do not find that pre-war characteristics, such as age, occupational income scores, wealth, literacy, urban residence, or managerial experience can be used to predict good leaders.[28] We also used the sample of regular soldiers in each leader's unit and regressed their desertion indicator during the major battles of the war on their own pre-war characteristics to

[26]Unit characteristics are averaged to the company-leader spell and include the share of soldiers of the same nationality, share of soldiers from the same county, share of soldiers who were of the same nationality as the captain, share of soldiers from the same county as the captain, the average Republican vote share in soldiers' home counties during the 1860 presidential election as measure of pro-Lincoln sentiment, and the log number of soldiers in the first week when a captain took control of the unit.

[27]We consider the linked 1860-70 sample of captains to validate that these individuals are not significantly different from other captains in the sample, as we will examine these leaders' postwar outcomes to test for leadership premia.

[28]The only marginally statistically significant coefficient is surname length, a proxy for having wealthier or foreign-sounding names, but it is not economically significant with a standard deviation increase in this measure being associated with a less than two percent of a standard deviation increase in leadership quality. Given a total of 30 coefficients, one would expect at least one coefficient to be significant by chance on average given a 95 percent testing threshold for significance.

generate a predicted battle desertion variable, which also shows up as insignificant meaning that good leaders did not come from communities with inherently braver soldiers. While leaders were selected based on wealth or socio-political standing in their communities, at least in the early stages of the war (see Table A.1), *good* leaders could not be predicted based on their pre-war observables. This is consistent with the historical narrative that soldiers had a rather limited ability to foresee the quality of the officers they elected (Glatthaar, 2009; Bledsoe, 2012).

Estimating postwar leadership income premia: Another test for whether our fixed effects approach measures leadership skill is to consider the impact of the leader fixed effect on the captains' postwar economic outcomes. If maintaining group cohesion was a skill with value beyond the military, then we should expect a positive impact on captains' postwar incomes, proxied by their occupational income scores in 1870. For this purpose, we use the sample of 6,900 captains whose military records we could link to the 1860 census, and whom we could additionally link to the 1870 census which corresponds to the sample used in constructing the coefficient plot in panel b of Figure 3 in the preceding exercise. Table 2 regresses the inverse hyperbolic sine of each captain's 1870 occupational income score on their leader fixed effect. Column 1 shows that a standard deviation increase in leadership quality was associated with an approximately 6.9 percent higher occupational income score in 1870. Alternatively, a leader in the 90th percentile compared to one in the 10th percentile of the leader quality distribution, who would be 1.6 standard deviations apart in terms of their estimated leader fixed effect, could expect to earn a 11 percent higher income after the war. This magnitude is consistent with work using modern data, such as Kuhn and Weinberger (2005), who estimate a leadership wage premium of between 4 and 33 percent within a sample of managers while controlling for cognitive skill. Similarly, Hoffman and Tadelis (2021) find that a manager's salary increases by between 17.5 and 35 percent for a standard deviation increase in their manager quality measure, which they derived from survey data.

Two additional observations are important to highlight. First, recall that the captain fixed effect is uncorrelated with leaders' pre-war occupational income score in 1860 (see Figure 3). Finding an effect on their 1870 occupational income scores implies that this fixed effect does capture some type of ability or skill that i) reduces desertions and increases group cohesion during the war, and ii) is valued in the labor market. This is consistent with work that has documented that military leadership experience is valued in the private sector (Duffy, 2006; Jha and Wilkinson, 2012; Benmelech and Frydman, 2015). Second, the effect of the leader fixed effect on the 1870 income scores is extremely stable. The coefficient barely moves as we include pre-war individual, economic, and family controls (columns 2–4), such as age, age squared, birth place and county of residence fixed effects, the inverse hyperbolic sine of a captain's 1860 income score, personal and real estate wealth, as well as indicators for industry and occupational groups, urban residence, literacy, family size, and their number of children. Also military controls for whether the soldier had originally enlisted as an officer, promotion, or whether they were awarded a Medal of Honor do not significantly change the coefficient (column 5), implying that the captain fixed effect does not simply capture a popularity or fame effect from exceptional service or social standing. The result is also unchanged by using the doubly-robust selection algorithm by Belloni et al. (2014),[29] implying that functional form concerns do not drive the result.

[29] The method regresses both the outcome and the treatment on a saturated set of all controls, their squares, and cross-term interactions, and selects significant coefficients via the LASSO. Then the outcome is regressed again on the treatment, including any of the controls selected in the previous step.

Robustness checks: Table A.5 shows results when using alternative methods to estimate the captain fixed effect, which yields slightly smaller but overall similar results - we discuss the trade-offs for each method in Appendix B. The estimated leadership quality income premium is robust to weighting captains by the number of census linking methods that had successfully linked them from 1860 to 1870 using the links provided by the Census Tree Project (Price et al., 2021) (Table A.6),[30] and to using linking methods that, instead of considering exact name matches, allow for slightly different spellings by matching names based on an index for similarly sounding names (Table A.7).[31] To ensure that captains whom we could link across census years are not systematically different from the general population of captains, we use the inverse propensity score reweighting method proposed by Bailey et al. (2020), which yields similar results (Table A.8).[32] Lastly, we perform a placebo check by regressing the postwar economic outcomes of soldiers who served under the captains in our sample on the captain fixed effect in Table A.9, where we find a precise null effect of leader quality on their soldiers' postwar income, personal or real estate wealth, employment in managerial jobs, and labor force participation. This rules out potential alternative network stories whereby good captains and soldiers bonded during the war, with the social ties then leading to better economic outcomes. It also reduces the likelihood that the captain fixed effects pick up something that is innate to the soldiers or the companies as a whole, and not to the captain himself.

Biographic postwar recognition of good leaders: As a last piece of evidence that our estimated leader fixed effect captures captains' leadership ability, we relate the leader fixed effect to measures of how these captains were talked about in their postwar biographies. We collected the biographic records for 10,150 captains from various sources including the eight-volume strong encyclopedia "The Union Army: A History of Military Affairs in the Loyal States 1861-65" published by the Federal Publishing Company (1908), and the American Civil War Research Database (ACWRD, 2022).[33] Almost all captains have a biographic entry in any of these sources, and of the 6,900 captains whom we could link from 1860 to 1870, we found biographies for 6,006 of them. Examples of typical biography entries are shown in Figure A.6. We extract the entire promotion history of each officer, whether they were wounded or injured in battle, which battles they fought, whether they received any medals or honors, as well as the words that were used to describe them and their actions. Using this information, we generate indicators for whether a captain was referred to as *hero*, *leader*, or *brave*. We consider the stemmed version of these words, meaning that the term *hero* includes other related words based on this stem, such as *heroic* or *heroism*. To show that the results do not depend on the particular words we picked, we also used ChatGPT to analyze each biography, asking whether a captain was described as a good leader, for which we received a binary answer.[34] We further include information from Beyer and Keydel (1994) and Lang et al. (1995) on whether any of these officers were awarded the Congressional Medal of Honor. Lastly,

[30]The Census Tree Project also contains links provided by the Census Linking Project (Abramitzky et al., 2020) and the IPUMS Multigenerational Longitudinal Panel (Helgertz et al., 2023).

[31]The phonetic encoding system used was the NYSIIS (New York State Identification and Intelligence System) index, which is a standard method for name transformations in historical census linking (Ruggles et al., 2018).

[32]Figure A.7 shows the overlap in the propensity scores of being linked based on observables for linked and not-linked individuals, showing a high degree of common support which is required for this adjustment to be accurate.

[33]To be able to test the leadership effect on biographical entries for higher-ranking officers, we also collected this information for colonels based on the book volumes by Hunt (2007).

[34]For full transparency and replicability, the precise question we submitted was: "Does the following biography mention that this person is a good leader? Answer with yes or no only". One caveat is that while ChatGPT will return a prediction for who was a good leader, it is not clear how exactly the large language model made this determination. However, this is also a strength given that it comes to similar conclusions as our simple binary word outcome approach.

we also construct a promotion indicator as additional outcome using the biographic and military data.

Table 3 reports results from regressing each outcome on the captain fixed effect, controlling for pre-war census controls used in the previous income regressions, and basic military controls such as indicators for the state of enlistment, age at enlistment, and company controls to capture social network dynamics. We find a positive and significant relation between the captain fixed effect and the indicators for whether they were mentioned as a hero, leader, or brave. The effects are pretty sizable given the relatively low frequencies of these words in the biographies. For instance, a standard deviation increase in leader quality is associated with a 1.2 percentage point increase in the probability of being referred to as hero. This is almost twice the baseline probability of 0.7 percent. Better captains were also more likely to be referred to as *leader* or *brave*. We also find a positive relationship with the good-leader indicator generated by ChatGPT, which is significant at the five percent level. There is also a positive effect on the probability that a captain received a Medal of Honor, however, this is only significant at the ten percent level. Interestingly, there is no significant relationship between the leader fixed effect and an indicator for promotion beyond captain. Historians have described the Union Army as fairly inept when it came to recognizing and promoting good leaders (Glatthaar, 1995), hence the result is not unexpected.[35]

Robustness checks: Table A.5 re-estimates the previous regressions using alternative methods to estimate the captain fixed effect, especially the results for the hero and leader outcomes, which are the most important for us. Table A.10 regresses the biographic outcomes of colonels, i.e., the regiment leaders, on the estimated colonel fixed effect. We do not find a similar impact for regiment leaders, which suggests again that the main relationship between leader quality and unit- or soldier-specific outcomes plays out at the company-level instead of higher up the chain of command. While each soldier would have had daily contact with their captain, the same cannot be said of their regiment leader who was responsible for one thousand soldiers. This level of detachment to the higher-ups can be explained by neurobiology, which has shown that human brains typically do not effectively handle network sizes of over 150 individuals (Dunbar, 1992).

In summary, the collection of evidence suggests that the quality of captains in terms of their ability to maintain group cohesion could not be predicted based on their observable characteristics, and that leaders were not selected based on a unit's previous desertion rate or the performance of their predecessors. This supports the historical narrative that soldiers could not perfectly elect leaders based on merit, either because of the institutional setup where political sway and social influence tended to obscure military skill, or because they simply lacked accurate indicators of potentially successful leaders. Especially given that there is no relationship between good leaders succeeding worse captains suggests that the type or quality of each captain was only revealed once they were elected. Some soldiers, such as James Henry Langhorne, wrote home after regretting their own vote: "Mr. John Wade is an excellent man, but he makes a much worse captain than we had any idea he would. The men do not fear to violate his orders, and the company has lost more than we had any idea it would in discipline and morale" (Bledsoe, 2012, p. 76). Our measure of leader ability, however, appears to pick up a certain skill that was valued

[35] Soldiers often complained about the lacking system of merit-based promotion. Bledsoe (2012, p. 104) cites from a soldier's letter, Henry Perkins Goddard, objecting to the appointment of a captain from higher up instead of his preferred candidate, of whom he said that "qualifications and just claims are ignored; merited promotion is refused him, and a pampered pet of wealth and fashion, in the shape of a pale-faced, beardless boy is raised from the lap of unmanly and enervating luxury, and presented to a company of camp-worn or battle-scarred soldiers as an officer to be respected and obeyed".

in the labor market, and that was acknowledged in the captains' postwar biographies.

4 Leadership at Critical Junctures During the Civil War's Major Battles

Western military strategy has historically been dominated by seeking out decisive battles designed to cause the extreme stress under which units "crack" (Hanson, 2001; Keegan, 1976). In this section, we focus on the impact of leadership during the bloodiest and largest battles of the war, when leaders and their soldiers were particularly tested. We first zoom in on the major battles of the war in an event study analysis using a daily panel of companies seven days before and after each battle. We then show that units with better captains, as per our fixed effects measure, saw lower rates of desertion in these important junctures of the war. Lastly, using the quasi-random deaths of leaders, we provide evidence that the relationship between leader quality and desertions at the company-level is causal, reaffirming previous work which shows that leaders do matter.

4.1 Major Battles Event Study Setup

We draw from the list of the 19 deadliest battles of the war published in Selcer (2006), leaving out the 20th battle due to it being a naval engagement,[36] and construct a daily company-level panel for the seven days before and after each battle following a similar approach to constructing the company-week panel described in section 2.3. This yields 1,829,548 observations for the total of 20,629 companies in 2,377 regiments who were active during the 19 battles. Note, however, that not all of these units participated. 960 regiments and 8,537 companies participated in at least one of the major battles. The main difference between the company-day panel and the company-week panel is that here we do not exclude guard and support units, or companies that never saw any combat, as these may be used as potential control units. Panel a of Figure 4 provides a visualization of the raw data by plotting the inverse hyperbolic sine of desertions over event time for i) companies that participated in the battle, ii) companies that participated in another of the largest battles but were not involved in the current battle, and iii) companies that never participated in any of the main battles. Only the treated group reacts to the battle exposure in terms of desertions, which spike on the day of the battle and then return to the pre-treatment level three days later.

Using the daily company panel, we estimate the following equation,

$$\text{asinh(desertions)}_{ct} = \mu_c + \theta_t + \underbrace{\sum_{l=\underline{l}}^{-1} \gamma_l \cdot \text{D}(t - E_c^b = l)_{ct}}_{\text{days before battle}} + \underbrace{\sum_{l=1}^{\bar{l}} \gamma_l \cdot \text{D}(t - E_c^b = l)_{ct}}_{\text{battle day and aftermath}} + \varepsilon_{ct}, \quad (2)$$

where the outcome is the asinh of desertions in company c on day t, E_c^b is the date of a given major battle b, $D(\cdot)$ represents the indicator function which equals one if the condition in parentheses is true and zero otherwise, μ_c are company fixed effects that absorb unobserved time invariant company characteristics, and θ_t are calendar day fixed effects that account for aggregate changes in desertions common to all units. Treatment effects are expressed over an event window $l \in [\underline{l}, \bar{l}]$ that we set to be $[-7, +7]$ relative to the omitted day, which is the day before the battle $t-1$. Based on the suggestion in Baker et al. (2022)

[36] See Table A.4 for summary statistics and additional information for the 19 largest battles of the war, including their dates, outcome, Union Army soldiers, killed, wounded, and missing or those taken prisoner of war (POW).

to avoid endpoint bias, we report coefficients for the fives days before and after each battle instead of all coefficients estimated in l. We further account for the number of soldiers prior to the start of the battle, such that any battle-induced increases in desertions are not merely driven by scale effects. To adjust our inference for heteroscedasticity and autocorrelation, we cluster standard errors at the company level.

The resulting coefficient plot from estimating equation (2) is shown in panel b of Figure 4. While the results are not particularly surprising, they provide a useful benchmark for later exercises and a sanity check that the data indeed do make sense. Relative to the pre-battle day, we mostly find no significant pre-trends. The coefficient four days prior to the battle is significant at the 95 percent level, but negative, i.e., it would go in the opposite direction of anticipatory behavior that would lead to increased desertions even before the battle occurred. On the day of the battle, treated units see a 1.75 percent increase in desertions.[37] The battle-day effect is followed by significant post-battle desertions over the next two days before dissipating on day three. Cumulatively, the day of the battle and the following two days raise desertion rates in treated companies by 2.92 percent. For a full-strength company, this implies the loss of 3 soldiers due to desertion as a result of a single battle. Given a ten percent overall desertion rate across Union Army units, a loss of three soldiers represents 30 percent of total expected desertions in a company for the duration of the war.

For robustness, we consider never treated units as control group, dropping companies that were already treated or not yet treated from the sample. The result is reported in panel c of Figure 4, which shows similar results to the baseline result in panel b. To address the weighting issue associated with staggered event study designs using more recent econometric advances, we employ the local projections difference-in-differences estimators by Dube et al. (2023), which reaffirms the baseline results as shown in panel d of the same figure. Additional robustness checks are provided in Figure A.8, which show that results do not change substantially when we i) re-estimate the event study from equation (2) excluding longer battles and only focus on battles that lasted for two days or less, ii) exclude the never treated from the estimation using the Dube et al. (2023) estimator, iii) use the estimator by Sun and Abraham (2021), and iv) exclude the never treated when using the Sun and Abraham (2021) estimator.

4.2 Leadership Quality During the Major Battles

The hidden role of good leaders: After establishing the event study design, we return to assessing the role and importance of high-quality leaders during these critical junctures. To relate the previous event study to our leadership quality measure, we obtain the residuals from estimating equation (2). We then plot the residuals over event time for captains with above and below-median quality as per our fixed effect $\widehat{\kappa}_c$ for those units that participated in the battle. Even though equation (2) had no control or measure of captain quality, the importance of leader quality is clearly evident in this exercise, which is plotted in panel a of Figure 5. Prior to the battle, the desertion residuals for above and below-median quality captains behave very similarly. From the battle day onward, we see a large divergence with a positive uptick in the residuals for below-median quality captains while the opposite is true for captains with above-median leadership quality.

Event studies by leader quality: To more directly quantify the importance of good captains in our

[37] Relative to the unconditional mean of battle day desertions of 3.4 percent, this treatment effect implies a 51.4 percent increase in desertions from baseline. This baseline is relatively low, as most soldiers did not desert during battles, hence the relative effect may seem large but it is plausible in magnitude.

event study design, we next re-estimate equation (2) as a triple-differences regression by including the interaction term between the indicators for treatment, event time, and above-median captain quality.[38] The difference-in-differences coefficient then captures the increase in desertions due to the battle treatment in units with below-median quality captains, while the sum of the difference-in-differences and the corresponding triple interaction coefficients captures the same effect in companies with above-median quality captains — these are the two coefficient series plotted in panel b of Figure 5. As before, the pre-battle trends are indistinguishable between the two types of companies. On the battle day and the following two days, companies with high quality captains saw a markedly lower uptick in desertions. Cumulatively, units with below-median quality captains saw a rise in desertions of 4.3 percent over these three days, whereas desertions in units with high-quality captains increased by 2.5 percent. This means that units with better captains had 1.8 percentage points, or almost 42 percent, lower desertions than units with below-median quality captains. Taken together, the triple interaction coefficients for the battle day and the two following days are statistically significant at the 5 percent level ($p = 0.027$). Relative to the average in-battle desertion rate in the Union Army of 3.9 percent, a 2.5 percent desertion rate under above-median quality captains implies a 36 percent lower desertion rate.

The causal effect of good leaders: An important question that the leadership literature grapples with is whether the impact of good leaders on the outcome of interest is causal. It is typically difficult enough to find a good measure of leadership quality, let alone exogenous variation in said measure. Given that desertions in a company and leader quality of consecutive leaders appear to be uncorrelated (see Table 1), and that post-battle desertions dissipate within a few days (Figure 4, panel b), we exploit battle deaths among leaders as exogenous variation in leader quality changes across major battles. On average, the 19 major battles of the war were 63 days apart from each other, meaning that post-battle outcomes from previous battles should have no bearing on the current battle. Previous work has used deaths of company leaders as an exogenous shock to firm outcomes (Nguyen and Nielsen, 2014; Becker and Hvide, 2022; Sauvagnat and Schivardi, 2023), and battles provide an arguably exogenous increase in the risk of dying: "On the battlefield enemy fire did not discriminate between the excellent and the mediocre, and seemingly random bullets or capricious fragments of hot metal could snuff out the lives of outstanding leaders as well as incompetents or cowards" (Bledsoe, 2015, p. 190). Of course, leaders' own actions could have affected their own chances of dying in battle, but this should only affect desertions of soldiers they were leading and not the behavior of soldiers under their successor in the following battle.

We consider a panel of companies that participated in at least two of the 19 major battles, and estimate the following regression,

$$y_{c,b} = \mu_r + \theta_b + \pi \Delta \widehat{\kappa}_{c,b} + X'_{c,b}\lambda + X'_{c,b-1}\phi + \epsilon_{c,b} \tag{3}$$

where the main treatment is the change in leader quality between the current battle b and the previous battle $b-1$. To ease interpretation, we only compare units with a positive leader quality change to those who maintained the same leader and therefore had no change in leader quality. We drop observations where a negative change occurred, such that $\widehat{\kappa}_{c,b} \geq \widehat{\kappa}_{c,b-1}$. We instrument the change in captain quality

[38] We also include the additional interactions between the above-median captain quality and event time indicators, and with the treatment status variable, as well as the main effects.

$\Delta\widehat{\kappa}_{c,b}$ with an indicator for whether the previous leader died in battle. The exclusion restriction requires that the death of the previous captain does not directly affect desertions under his successor in the next battle through channels other than the leader change, as argued before. In addition to the regiment μ_r and battle θ_b fixed effects,[39] we control for company characteristics including both the share of soldiers from the same nationality and the same county, as well as the share of those of the same nationality and county as the captain, the share of soldiers from pro-Lincoln counties, and the log number of soldiers in the unit prior to the start of the battle, as before. Depending on the specification, we also include factors from the previous battle that may carry over to the current battle, such as lagged battle deaths, the share of soldiers of the same nationality and from the same county as the previous leader, and whether the prior battle was won.

Table 4 reports the results from this exercise. In column 1, we regress an indicator for a positive leader quality change from battle $b-1$ to b on an indicator for whether the previous captain died in battle, for which the resulting coefficient predicts that the next captain will have a higher fixed effect with a 37.4 percentage point higher probability. Whether the next leader will be better than the previous leader therefore has slightly worse odds than a coin toss. The result reaffirms that soldiers only had a limited ability, if any, to selectively pick better leaders, supporting arguments made by historians that were discussed in section 2, and the empirical finding that observables could not be used to predict good leaders (Figure 3). Column 2 reports the first stage relationship and the expected increase in leader quality if the previous captain was killed in battle. We then use this first stage relationship in columns 3 to 7 to instrument the observed change in leader quality. Column 3 reports the baseline IV results without company controls and with controls in column 4, which are almost identical. A battle death-induced positive change in leader quality is associated with an approximate 7.3 percent reduction in desertions on the day of the battle. Note though that the 7.3 percent reduction in battle day desertions is for a one unit increase in leader quality, i.e., a full one-standard deviation increase.[40] The first stage predicts an average leader quality change of 0.413 standard deviations. Scaling the positive leader quality change coefficient in column 3 by the expected amount of a battle-death induced change in leader quality from the first stage in column 2, this implies a 3 percent reduction in desertions on average. The results are almost unchanged if we include controls for the share of soldiers from the same county or of the same nationality as the previous leader to account for potential changes in unit composition (column 5), the number of soldiers who died in the previous battle (column 6), or whether the previous battle was won (column 7). Results are robust to using alternative ways to construct our leader quality measure (Table A.5). When we consider a binary change in leader quality, as opposed to a continuous change, we obtain similar results (Table A.11).

Desertion at the individual level: Lastly, we use a sample of regular soldiers linked to the 1860 census and for whom we observe their desertion decisions during and after the 19 largest Civil War battles. Using the linked census data allows us to control for additional individual characteristics that may be strong predictors of the desertion decision. In Table 5, we regress the desertion indicator on an indicator for whether a soldier's captain in a given battle had an above-median leader fixed effect. We find that

[39] Controlling for company fixed effects is possible, however, this implies that we lose all units where no change in leadership occurred which are our control group in this setting. Instead, the current specification compares companies with and without leader change within the same regiment, which is the unit at which battle assignments are made.

[40] Technically, this is a one captain fixed effect unit increase in leader quality, but since the captain fixed effects were standardized to mean zero and standard deviation one, a one unit change is equal to one standard deviation.

soldiers serving under such high-quality leaders were 1.6 percentage points less likely to desert on the day of the battle. This effect does not change when we include 1860 county of residence fixed effects, pre-war census controls including age, occupational income, wealth, labor force status, urban residence, skill and industry group indicators, literacy, and family characteristics, soldier- and unit-level controls such as experience, share of soldiers from the same county, unit size, as well as observable leader characteristics, such as the captain's experience, whether they were ever promoted, wounded, or killed. While the company-level results are the aggregated version of the individual-level data, the linked sample allows us to control for individual soldier pre-war characteristics. None of these appear to explain away the above-median leader effect, which is similar in magnitude to the triple-differences and instrumental variables results. The individual-level results hold when using different treatment definitions (Table A.5), the continuous leader quality measure (Table A.12), different fixed effects (Table A.13), or alternative standard error clustering units (Table A.14).

5 Mechanisms Through Which Good Leaders Matter

Why did captains with lower desertion rates outside of battle also have lower desertion rates during even the largest battles of the war? In our previous empirical exercises, we controlled for unit size, composition, as well as potential social networks between soldiers and their leaders to show that even after such factors were accounted for, good leaders were still crucial to group cohesion. We next turn to how and why better leaders were able to convince their soldiers to stay without formal means to compel them. We focus on two potential mechanisms: risk aversion and learning-by-doing. While there are potentially many other explanations, these are two compelling mechanisms for our leadership quality effect that we can test reasonably well with the data at hand.

5.1 Risk Aversion or Leading by Example

One possible explanation for our previous results could be that leaders with lower desertion rates in their units were simply more risk averse, i.e., they did not seek out engagement with the enemy, thus keeping their soldiers safe and hence fewer of them deserted. This style of leadership is likely to reduce desertions but is unlikely to contribute to the overall objective of winning the battle. We now use spatial data from battle maps to provide evidence that this is not the case. We find that units with more able leaders were no closer or farther away than units with less able captains. However, units with better captains not only had fewer desertions, they also sustained more casualties. This might seem counterintuitive, though we also find that better leaders tended to have a higher probability of dying in battle. We interpret this as evidence for leadership by example and own sacrifice as theorized by Hermalin (2012).

We digitized 125 battle maps from the American Battlefield Trust in order to relate leadership quality to distance from the enemy. These maps show the location of Confederate and Union regiments on the battlefield. Figure 6 provides an example of the Battle of Iuka. Panel a displays the original map where units' locations are shown at two points in time during the battle at 5 p.m. and 7 p.m. We applied a pattern recognition algorithm to identify units on the map, which are represented as rectangles for infantry and cavalry regiments, and as cannons for artillery regiments.[41] We then overlaid each map with a one-by-

[41] We relate units in the map to our Union Army data by digitizing the corresponding unit names via optical character recognition, which we then matched to the regiments in our data based on the regiment name and participation in a given battle.

one pixel grid, located all unit shapes and identified their centroids, which is shown in panel b. Lastly, for each Union Army unit, we computed the distance to the closest enemy unit as displayed in panel c of the same figure using the location of each unit's centroid. The battle maps tend to show multiple locations across time within the same battle for the different units,[42] however, since our desertion data are at the daily level, we take the average of the distance measure for each unit in the same battle. The maps also do not allow us to observe companies but only regiments, which were typically comprised of ten companies. We therefore also average the number of desertions and company leader fixed effects to the regiment-battle day level, which we relate to the inverse average distance of a regiment to the nearest enemy unit in the following regression,

$$\text{asinh(desertions)}_{rb} = \beta_1 \overline{\kappa}_{rb} + \beta_2 \ln\left(\text{distance}^{-1}\right)_{rb} + X'_{rb} + \delta_b + \epsilon_{rb} \quad (4)$$

where $\overline{\kappa}_{rb}$ is the average of the estimated company leader fixed effects $\widehat{\kappa}_{crb}$ in a given regiment and battle with the associated coefficient β_1 quantifying the impact of better average leadership in the regiment on desertions in the battle. The coefficient β_2 estimates the impact of being closer to the nearest enemy on desertions which is captured by the log inverse distance. We standardized the distance measure to have mean zero and variance one such that β_2 can be interpreted in terms of a standard deviation increase in proximity to the closest enemy. Since the battle maps are on different scales, which are unknown to us, we include battle fixed effects such that coefficients are identified within each battle meaning that all comparisons are made on the same scale. The fixed effects will also capture all other unobserved battle characteristics that are fixed during the battle day. The regiment characteristics include the average share of soldiers from the same county and from the same nationality as their company leaders, the mean share of soldiers from the same county and from the same nationality, and the mean share of the Republican vote share in soldiers' residence counties. Regiments are weighted by the number of soldiers in them.

The first column in Table 6 reports the coefficient from a regression of the standardized log inverse distance to the nearest enemy unit on the average company leader fixed effect in each regiment. The coefficient is close to zero and statistically insignificant, meaning that regiments with varying captain quality in their companies did not differ in terms of how far or close they were to the enemy. This does not support a risk-aversion story as described before. In columns 2 to 4, we regress the asinh of regimental desertions on the average captain fixed effect and find negative and significant results which is consistent with previous exercises. Column 2 shows the coefficient for a sample that includes all battles, whereas columns 3 and 4 split the sample between wins and losses. Regiments with higher leadership ability among their captains generally had lower desertions, especially when winning. In contrast, the relationship was slightly weaker in battles they lost. We also control for the log inverse distance to the nearest enemy, after having shown that it is unrelated to the leadership quality variable (column 1), and therefore is unlikely to be a bad control. Distance has no effect on desertions when winning but a positive effect in losing battles, however, the coefficient was not significant even though this made theoretical sense.

While regiments with better company leaders had lower desertions, they also had higher deaths in battle which is what we show in column 5 where we regress the asinh of battle deaths on the average leadership measure. A standard deviation increase in a regiment's captain quality raised battle deaths by

[42] 86 of the 125 maps have within-battle location variation as in the example of the Battle of Iuka. Most maps with such time variation show two phases of the same battle, but a few maps even provide up to three and four battle phases.

around 7.2 percent. Distance now also has a significant effect, as a percent increase in the proximity to the enemy was associated with a 31 percent increase in the unit's battle deaths. Column 6 repeats the same regression but using accidental deaths as a placebo test, where we find no effect of leader quality or distance as expected. Finally, Column 7 regresses an indicator for whether at least one captain in the regiment was killed in battle. As for regular soldiers, distance mattered in a similar fashion and regiments with higher quality captains had a 1.7 percentage point higher probability of at least one captain dying in battle. Compared to the baseline probability of 15.6 percent for this outcome, the effect size amounts to 15.7 percent relative to the mean. The importance of leading from the front and sharing the dangers of regular soldiers has been highlighted by historians (Holmes, 1989; McPherson, 2003),[43] but also by economists studying leadership by example and own sacrifice (Hermalin, 2012; Costa, 2014). Results are robust to using alternative ways to construct our leader quality measure (Table A.5) and to different levels of standard error clustering (Table A.15). We also find in additional results for data at the battle-level that a higher share of leaders in the top 5th percentile of the leader fixed effects distribution reduced desertions and increased the probability of winning, while a higher share of leaders in the bottom 5th percentile did the opposite (see Table A.16).

5.2 Learning-by-doing

A key concept in economics is learning-by-doing, i.e., the improvement of one's own skill or ability to perform a task by practice (Arrow, 1962). The vast majority of Civil War captains were civilians with no prior military experience. How important was learning-by-doing for these citizen officers and how quickly could they adapt, if at all, to their leadership roles?

To answer this question, we return to our company-week panel introduced in section 2.3 and the estimation of the captain fixed effects. Rather than estimating these effects over the full period when the unit was out of battle, we now split this sample into spells of 6 weeks for each leader and re-estimate their fixed effect using a similar specification as in equation (1), separately for each leader-spell. The average company leader serves for 8 spells (42 weeks), with a minimum of 2 and a maximum of 28. If out-of-battle desertions are a good proxy for a leader's ability to maintain group cohesion, and if leaders improve over time, we should observe an increase in the estimated fixed effects across spells, $\widehat{\kappa}_{c,s}$, with c indexing captains and s spells.

Figure 7 plots the average estimated captain-spell fixed effects by year of entry into service. Regardless of when captains entered service, they tended to start out with lower fixed effects, i.e., a lower estimated leadership quality. As expected, our measure of leadership quality rises over time. The strongest increase in leader quality occurs within the first couple of spells after which gains appear more modest.

An exception is the cohort that entered in 1861: while their performance initially improves, their estimated fixed effects decline over time. Notably, this group also begins with the lowest fixed effect in the first period. As discussed in Section 2, these captains were often elected before any major battles, typically based on popularity or political connections at home. Once battles began and these individuals revealed themselves as ineffective leaders, soldiers had little recourse to remove them. Instead, they appear to have voted with their feet by deserting outside of battle, a pattern reflected in the declining

[43] Holmes (1989, p. 341) notes that "[i]t is a fundamental truth that a military leader will not succeed in battle unless he is prepared to lead from the front and to risk the penalties of doing so. This need to lead from the front is as relevant to unpleasant tasks off the battlefield as to dangerous ones on it".

leader-spell fixed effect for the 1861 cohort of captains.

Although soldiers had limited ability to forecast effective leaders early in the war, we observe an improvement in the leader fixed effect of later cohorts, especially in the first spell. Combined with increased efforts by the Union Army in early 1863 to assess and improve captain training and performance (Bledsoe, 2015), the learning-by-doing patterns become more stable in the 1863 cohort. The highest starting fixed effects are observed among leaders entering in 1864, though their performance declines as the war nears its end, visible in the dip in estimated leader-spell fixed effects during the final periods, when desertion rates rose.

We provide a more rigorous analysis of the data in Table 7, where we regress the estimated captain-spell fixed effect $\widehat{\kappa}_{c,s}$ on tenure and its squared terms, i.e., the total number of spells served. We include regiment and time fixed effects, where time is measured as the start week of a given spell, as well as company-level controls.[44] The regression therefore compares captains in the same regiment who enter service at different times. To account for unit size in a way that it is not mechanically correlated with the leader-spell fixed effect, we weight by the lagged number of soldiers in each company.

Column 1 provides the baseline specification. Since the Union Army did not replenish companies, company strength declined over time, and changes in group composition were driven solely by deaths and desertions. One concern that cannot be resolved by the visual inspection of Figure 7 is that leaders may only appear to improve at maintaining group cohesion because soldiers with the highest propensity to desert left during the first period. To test this hypothesis, we control for the desertion rate in the first spell under a new captain in column 2, which leaves the results unchanged. Another question is whether battles weed out high-propensity deserters, i.e., the more battles a company leader fights, the more the unit consists of battle-hardened veterans who are less likely to desert. We therefore control for the asinh of battle deaths in the previous spell and whether any battle occurred in the previous spell—recall that these weeks were not used when estimating the fixed effects. After controlling for lagged battle exposure in column 3, we find that this does not impact the estimated learning-by-doing result. Column 4 excludes leaders who were promoted, while column 5 excludes leaders who died, to rule out issues of selectivity. For example, if the worst leaders die in battle or the best are promoted based on performance, then the learning-by-doing effect could be underestimated or overestimated, respectively. Column 6 excludes both groups, which again does not change the main result. Overall, the learning-by-doing effect is robust across specifications and holds under different tests for leader selectivity and changes in group composition.

The regression consistently estimates a concave relationship between tenure and the growth in leader quality, with a positive coefficient on the main effect and a negative coefficient on the squared term. This implies that leader quality increases over time as captains continue to serve in the same company, but at a declining rate. The main effect suggests that the leader-spell fixed effect increases by 1.3 percent of a standard deviation for each additional spell served, while the squared term indicates that this effect diminishes by 0.06 percent of a standard deviation with every further spell. To put this into perspective, the model predicts that serving for the average period of 8 spells increased leader quality by approximately 4.2 percent of a standard deviation.[45] This is a non-negligible but modest effect compared

[44]Company-level controls include the share of soldiers from the same county, the share of soldiers of the same nationality, the share of soldiers from the same county as the captain, and the share of soldiers of the same nationality as the captain.

[45]This total effect is computed as the difference in predicted fixed effects between spell 8 and spell 2 using the quadratic model: $\widehat{\kappa}_8 - \widehat{\kappa}_2 = \beta_1 \cdot (8-2) + \beta_2 \cdot (8^2 - 2^2) = 6\beta_1 + 60\beta_2 = 0.042$ (based on column 3).

to the death-induced change in leader quality of 41.3 percent estimated in column 2 in Table 4.

Table A.17 shows results when we vary the spell length used to estimate the captain fixed effect using 4, 5, 7, or 8 weeks, in order to demonstrate that our findings are not driven by the choice of 6-week spells. The only notable deviation occurs when spells are just 4 weeks long. In this case, each leader fixed effect is estimated from only four observations per captain. Shorter spells reduce the number of observations per estimate, increasing sampling variance. When this noisier measure is used as the outcome variable in the second stage, we observe attenuation of the tenure coefficients and a drop in explanatory power. For all other spell lengths, the results are closely aligned with those in our main specification in Table 7. We also find no significant interaction effects between tenure and other unit characteristics, such as the share of soldiers with the same ancestry as the captain, the share from the same county, general social network strength (e.g., ancestry or county similarity within the company), the log number of battle deaths in the preceding spell, or whether a battle occurred. These results are reported in Table A.18. The only significant interaction effect is with the log number of soldiers who deserted in the first spell, though this may reflect a mechanical relationship, as discussed earlier. Overall, the learning-by-doing channel appears to be primarily driven by captains themselves, rather than by the composition or characteristics of their companies.

6 Conclusion

This paper sheds light on the critical role of frontline leaders—those at the bottom of the organizational hierarchy who operate closest to day-to-day team activity. While leadership is often associated with top-level management focused on ideation and strategy, it is at the frontline where leadership has the greatest impact on motivation, coordination and cohesion. Following Hermalin (2012), we define leadership as "the ability to induce others to follow absent the power to compel or to provide formal contractual incentives." We explicitly distinguish between high-level authority and hands-on leadership, and we quantify the importance of proverbial frontline leaders in a high-stakes setting.

Our setting is the U.S. Civil War (1861–65), where we study captains leading companies of roughly 100 men. Using weekly data on 2.2 million Union Army soldiers, we estimate captain fixed effects in a regression of desertions on unit characteristics during non-combat weeks to measure leadership quality. Outside of battle, soldiers deserted for various reasons—dissatisfaction with pay, poor hygiene, food rations, or homesickness—rather than immediate survival concerns. As such, captain fixed effects reflect the ability to maintain cohesion in lower-stakes conditions. Armed with this revealed measure of leadership quality, we then examine outcomes in combat and find that out-of-battle desertion rates strongly predict cohesion under fire. High-quality leaders significantly reduced desertions in combat.

To understand the mechanisms through which leadership matters, we show that units with better captains were neither systematically closer to nor further from enemy lines, suggesting risk exposure did not vary with leadership quality. However, high-quality captains were more likely to die in battle, consistent with leadership-by-example or leading-from-the-front. We also examine learning-by-doing by dividing captain tenure into spells. While we find clear evidence of early gains in leadership effectiveness, the magnitude of these improvements is modest relative to the substantial differences in performance across captains, suggesting that leadership ability—rather than experience alone—is the primary driver of cohesion. Finally, we validate our measure outside the military context by examining postwar outcomes:

better wartime leaders end up having higher occupation scores despite not being selected on observable pre-war characteristics. Taken together, these findings suggest that leadership effectiveness is driven more by inherent ability than by learning, and that such ability may remain latent until individuals are placed in situations that demand decisive leadership.

While the Civil War provides a unique setting to study leadership effects, we believe our findings have broader implications for team-based production in modern firms and organizations. Strategic complementarities within teams amplify the value of leadership, particularly when formal authority is limited. We find that leadership was most consequential at the "primary group" level—about 100 individuals—where direct supervision and interpersonal influence are strongest. Effective leaders were older, courageous, and socially proximate to their teams. Their presence held groups together under stress: desertions spiked when leaders died but declined when they were wounded. Acts of valor by leaders fostered cohesion like nothing else. These results highlight the importance of identifying and supporting frontline leaders who sustain performance when it matters most.

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Tables

Table 1: Testing for Selection Among Company Leaders

	Mean desertion$_{cl}$			Mean desertion$_{c,l-1}$	Leader FE $\widehat{\kappa}_{cl}$
	(1)	(2)	(3)	(4)	(5)
Leader FE $\widehat{\kappa}_{cl}$	-0.076***	-0.076***	-0.075***	-0.000	
	(0.001)	(0.001)	(0.001)	(0.001)	
Prev. leader FE $\widehat{\kappa}_{c,l-1}$		0.000		-0.072***	0.026
		(0.000)		(0.001)	(0.017)
Mean desertions$_{c,l-1}$			0.014***		-0.082
			(0.004)		(0.197)
Regiment FE	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes
Unit controls	yes	yes	yes	yes	yes
Observations	18,074	18,074	18,074	18,074	18,074
Adj. R^2	0.867	0.867	0.867	0.847	0.581

Note: Regressions of mean desertion rates on the current captain's predicted leader fixed effect $\widehat{\kappa}_{ct}$ (columns 1–3), mean desertions under the previous leader on the current and previous captains' fixed effects (column 4), and of the current captain's fixed effect on the fixed effect of the previous captain and desertion rates under the previous captain (column 5). Data have been collapsed by company and leadership spell, i.e., the total weeks for which company c was lead by captain l. The predecessor of captain l is denoted by $l-1$. All regressions include regiment fixed effects and time fixed effects for the start week of leader l's command, as well as company-leader spell averaged controls including the share of soldiers of the same ancestry (American, German, Italian, Irish, other), the share of soldiers of the same ancestry as the captain, the share of soldiers from the same county, the share of soldiers from the same county as the captain, the Republican vote share in soldiers' home counties in the 1860 presidential election, and the log number of soldiers in the week when a captain started leading the company. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Relating Leader Fixed Effects to Leaders' Income Score in 1870

	Outcome: Asinh of leaders' occupational income score in 1870					
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE κ_c	0.069***	0.069***	0.072***	0.073***	0.072***	0.068***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.024)
Individual controls		yes	yes	yes	yes	yes
Economic controls			yes	yes	yes	yes
Family controls				yes	yes	yes
Military controls					yes	yes
Post-Double selection						yes
Observations	6,900	6,900	6,900	6,900	6,900	6,900
Adj. R^2	0.135	0.139	0.152	0.154	0.155	0.136
Outcome mean	3.437	3.437	3.437	3.437	3.437	3.437

Note: Regressions of the inverse hyperbolic sine (asinh) of leaders' postwar occupational income scores in 1870 on their estimated leader fixed effects, $\widehat{\kappa}_c$. Leader fixed effects are standardized to have mean zero and variance one. The sample includes individual leaders whose military records matched with the 1860 census and who could be linked from the 1860 to the 1870 census using the crosswalks provided by the Census Tree Project (Price et al., 2021). Individual controls (column 1) include the leader's age and age squared, indicators for their place of birth, and 1860 county of residence fixed effects. Economic controls (column 2) contain pre-war economic variables from the 1860 census including the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, indicators for labor force participation, urban status, literacy, nine occupational skill groups (professional/technical, managers/officials/proprietors, clerical/kindred workers, sales workers, craftsmen, operatives, service workers, farm laborers, and laborers) and for 13 industry groups (farm, mining, construction, durable and non-durable manufacturing, transportation, telecommunication, utilities, wholesale, retail, finance, services, public administration). These definitions follow the 1950 occupation and industry classifications by the Census Bureau. Family controls (column 4) contain pre-war variables in 1860 including family size, number of children, and group quarter indicators. Military controls (column 5) include indicators for whether the leader had original enlisted as an officer, indicators for whether they were ever promoted above their current rank of captain or colonel, and an indicator for receipt of the Medal of Honor. Column 6 uses the post-double selection estimator by Belloni et al. (2014), which uses LASSO selection models to pick significant predictors of the outcome and the treatment from the set of controls, including the squares and cross-term interactions of all controls, and includes the set of selected variables in the main regression to show coefficient stability with regards to functional form. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The Effect of Leader Quality on Leaders' Biographies and Military Career Outcomes

	Biography mentions leader as				Military outcomes	
	hero	*leader*	*brave*	ChatGPT leader = 1	Medal of Honor	Promoted
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE $\widehat{\kappa}_c$	0.012***	0.005**	0.008*	0.010**	0.006*	0.004
	(0.005)	(0.002)	(0.004)	(0.004)	(0.004)	(0.007)
1860 census controls	yes	yes	yes	yes	yes	yes
1860 county FE	yes	yes	yes	yes	yes	yes
Military controls	yes	yes	yes	yes	yes	yes
Observations	6,006	6,006	6,006	6,000	6,006	6,006
Adj. R^2	0.386	0.311	0.431	0.396	0.381	0.225
Outcome mean	0.007	0.001	0.004	0.007	0.007	0.740

Note: Regressions of indicators for whether a leader's biography refers to them with the words *hero*, *leader* (columns 1–3), or *brave*, and an indicator for whether ChatGPT classified a leader as a "good leader" based on their biography (column 4), and military outcomes including indicators for whether a captain received the Medal of Honor, and later promotions on the captain fixed effect $\widehat{\kappa}_c$. Leader fixed effects $\widehat{\kappa}_c$ are standardized to have mean zero and variance one. The prompt for ChatGPT-4o mini asked: "Does the following biography mention that this person is a good leader? Answer with yes or no only". The sample includes individual leaders whose military records matched with the 1860 census. Pre-war controls from the 1860 census include a leader's age, indicators for literacy, urban status, labor force participation, place of birth, fixed effects for county of residence, number of children, as well as the inverse hyperbolic sine of their personal and real estate wealth. We also include indicators for nine skill groups and 13 industry groups (for the specific groups see the table note of Table 2). Military controls include indicators for the state of enlistment, age at enlistment, and the week-month-year in which they enlisted, and unit characteristics like the share of soldiers of the same county and ancestry in their unit, and the share of soldiers of the same ancestry and from the same county as the captain, and the mean Republican vote share in soldiers' counties during the 1860 presidential election. Observations are weighted by the total number of words in the biography. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: IV Regressions of Desertions on Leader Quality Changes

	$\Pr(\Delta\widehat{\kappa}_{c,b} > 0)$	$\Delta\widehat{\kappa}_{c,b}$	Inverse hyperbolic sine of desertions				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D(Prev. capt. died in battle)	0.374***	0.413***					
	(0.043)	(0.072)					
Pos. leader change			-0.073***	-0.071***	-0.071***	-0.073***	-0.072***
			(0.021)	(0.020)	(0.021)	(0.024)	(0.023)
% same ethnicity as prev. leader					-0.016	-0.016	-0.014
					(0.018)	(0.017)	(0.017)
% same county as prev. leader					0.076***	0.078***	0.078***
					(0.027)	(0.028)	(0.028)
Lagged battle deaths						0.001	0.005
						(0.005)	(0.004)
Prev. battle = win							-0.060***
							(0.017)
Regiment FE	yes	yes	yes	yes	yes	yes	yes
Battle FE	yes	yes	yes	yes	yes	yes	yes
Unit size	yes	yes	yes	yes	yes	yes	yes
Unit controls	yes	yes		yes	yes	yes	yes
Observations	8,386	8,386	8,386	8,386	8,386	8,386	8,386
First stage F-statistic			32.301	32.597	27.722	25.121	25.120
Outcome mean	0.142	0.180	0.030	0.030	0.030	0.030	0.030

Note: First stage regressions of an indicator for a positive leader quality change (column 1) or the continuous change in leader quality between major battles, defined as $\Delta\widehat{\kappa}_{c,b} = \widehat{\kappa}_{c,b} - \widehat{\kappa}_{c,b-1}$ (column 2), on an indicator for whether the preceding captain died in the previous battle ($b - 1$). Columns 3–7 report instrumental variables regressions of the inverse hyperbolic sine of desertions in company c on the difference in leader quality since the previous battle, using the specification from column 2 as first stage. The sample is a panel of companies observed on the days of the 19 largest battles, restricted to instances where either no change or a positive change in leader quality occurred between battles (i.e., no negative changes). The list of major battles was taken from Selcer (2006). On average, major battles were 63 days apart, hence any post-battle desertions from the prior battle will have occurred long before the current battle (see Figure 4, panel b). All regression include regiment and battle date fixed effects, the log number of soldiers a week before the battle (unit size), the share of soldiers of the same nationality and county, the share from the same nationality and county as their captain, the share from Republican majority counties, and additional controls listed in the table (columns 5–7). Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Leadership and Desertions in High-Stakes Battles at the Individual Level

	Outcome: Pr(deserted)$_{it}$=1				
	(1)	(2)	(3)	(4)	(5)
Above median leader FE	-0.016***	-0.016***	-0.016***	-0.019***	-0.019***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Regiment fixed effects	yes	yes	yes	yes	yes
Geographic fixed effects		yes	yes	yes	yes
1860 census controls			yes	yes	yes
Soldier controls				yes	yes
Unit controls				yes	yes
Leader controls					yes
Observations	204,653	204,653	204,653	204,653	204,653
Adj. R^2	0.001	0.001	0.002	0.006	0.006
Outcome mean	0.039	0.039	0.039	0.039	0.039

Note: Regressions of individual soldiers' binary desertion decisions during the 19th largest battles of the war on an indicator for whether their company leader's predicted leader fixed effect $\widehat{\kappa}_c$ was above the median. The list of major battles was taken from Selcer (2006) and originally contained 20 battles, however, the 20th battle was a naval battle which is not covered in our data. The estimation sample consists of rank and file soldiers of the Union Army for whom we were able to link their military records to the 1860 census. All regressions include regiment fixed effects. Geographic fixed effects (column 2) control for soldiers' 1860 county of residence. Other pre-war controls from the 1860 census (column 3) include the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, age, indicators for labor force participation, urban status, literacy, nine skill groups and 13 industry groups (for the specific groups see the table note of Table 3), family size, the number of children, group quarter indicators, and indicators for their place of birth. The soldier controls (column 4) include a soldier's tenure in the military in months, and unit controls (column 4) include the log number of soldiers, the number of soldiers who had received a Medal of Honor, the share of soldiers from the same county and ancestry, and the share of soldiers from the same county or ancestry as the company leader. Leader controls (column 5) include the captain's experience in months, whether the captain was ever promoted, wounded, or whether he was killed. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Distance, Leadership, and Desertions in 125 Digitized Battle Maps

	Inv. log distance	Inv. hyperbolic sine of desertions All battles	Wins	Losses	Battle Deaths	Acc. Deaths	Pr(Leader Death)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leader FE $\widehat{\kappa}_c$	0.001	-0.105***	-0.148***	-0.082*	0.072***	-0.001	0.017**
	(0.017)	(0.035)	(0.035)	(0.045)	(0.024)	(0.002)	(0.007)
Log inv. dist.		0.013	-0.003	0.045	0.313***	-0.003	0.054***
		(0.023)	(0.033)	(0.039)	(0.038)	(0.003)	(0.013)
Observations	2,823	2,823	1,461	1,362	2,823	2,823	2,823
Outcome mean	0.000	0.607	0.602	0.612	2.381	0.009	0.156
Adj. R^2	0.598	0.266	0.255	0.279	0.336	0.006	0.052

Note: Regiment-level regressions of the standardized log inverse distance to the nearest enemy (column 1), the inverse hyperbolic sine (asinh) of desertions, subdivided by battle result (columns 2–4), the asinh of battle deaths (column 5) and accidental deaths (column 6), as well as the probability that any captains in the regiment were killed in battle (column 7) on the average of company leaders' predicted leader fixed effects in the regiment $\widehat{\kappa}_r$ on the day of the battle. The unit of observation is the regiment-battle, which includes 125 unique battle maps and 720 unique regiments. Regiment-level controls include the standardized log inverse distance to the nearest enemy unit (columns 2–7 only), the share of soldiers in the regiment who resided in the same county, the share with the same ancestry, the share who originated from the same county as their captain, and the share of the same ancestry as their captain, as well as battle fixed effects to account for different and unknown scaling of the maps which would affect the interpretation of distance. The mean in column 1 is zero because the inverse log distance measure was standardized. Observations are weighted by the total number of soldiers in the regiment. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results with alternative estimation of the standard errors are reported in Table A.15 using cluster bootstrapped standard errors at the battle, regiment, or battle-regiment level, respectively.

Table 7: Tenure Effect on Leader Quality

	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.0120***	0.0130***	0.0130***	0.0124***	0.0127***	0.0121***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0019)	(0.0019)
Tenure squared	-0.0006***	-0.0006***	-0.0006***	-0.0005***	-0.0005***	-0.0005***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Regiment FE	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes
Company controls	yes	yes	yes	yes	yes	yes
Baseline desertions		yes	yes	yes	yes	yes
Battle controls			yes	yes	yes	yes
Excl. promoted leaders				yes		yes
Excl. killed leaders					yes	yes
Observations	167,172	167,172	167,172	163,316	158,185	154,360
Adj. R-squared	0.551	0.553	0.553	0.555	0.555	0.558

Note: Regressions of the leader fixed effect $\widehat{\kappa}_{c,s}$ for leader c in each 6-week spell s on captains' tenure and tenure squared in their current unit. The leader-spell fixed effect $\widehat{\kappa}_{c,s}$ is estimated by regressing the inverse hyperbolic sine (asinh) of desertions of leader indicators—as in equation (1)—using a company-week panel excluding weeks with battles, skirmishes, or any recorded battle deaths, and dividing this out-of-battle sample into 6-weeks intervals (spells s) in which the fixed effect is estimated. Company controls include the share of soldiers from the same county, the share of the same ancestry, the share from the same county as the captain, and the share of the same ancestry as the captain. Column 2 includes the baseline desertion rate in the current leader's first spell to rule out the possibility that leaders with a large initial share of "cowards" appear to have better fixed effects but later on, simply because the soldiers most prone to desert have already exited. Column 3 includes battle controls which are the lags of the asinh of battle deaths, indicators for any battle, a top-19 battle, or a skirmish. Column 4 excludes leaders whose spells end due to promotion to a higher level to rule out tenure effects being attenuated by exits of the best leaders. Column 5 excludes leaders who were killed to rule out upward biases from exits of the worst leaders. The final column 6 excludes both groups that were excluded in columns 4 and 5. Observations are weighted by the lagged number of soldiers in the company. All regressions include regiment fixed effects and fixed effects for the starting week of each spell. Standard errors are clustered at the level of the captain and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

Figure 1: Data and Organization of the Union Army

Army of the Potomac

As organized into Divisions Oct., 1861, to March, 1862.

BANKS' DIVISION.—Organized Aug. 17, 1861, by transfer from Dept. of the Shenandoah to Dept. of the Potomac.

COMMANDER.

N. P. Banks..........	Major General....................	Aug. 17, 1861, to March 13, 1862.

1st BRIGADE.— COMMANDERS.

Geo. H. Thomas.......	Col. 2d U. S. Cav................	Aug. 17, 1861, to Aug. 28, 1861.
Geo. H. Gordon......	Col. 2d Mass. Infy................	Aug. 28, 1861, to Oct. 18, 1861.
A. S. Williams.........	Brigadier General.................	Oct. 18, 1861, to March 13, 1862.
D. Donnelly	Col. 28th New York Infy..........	March 13, 1862, to March 20, 1862.

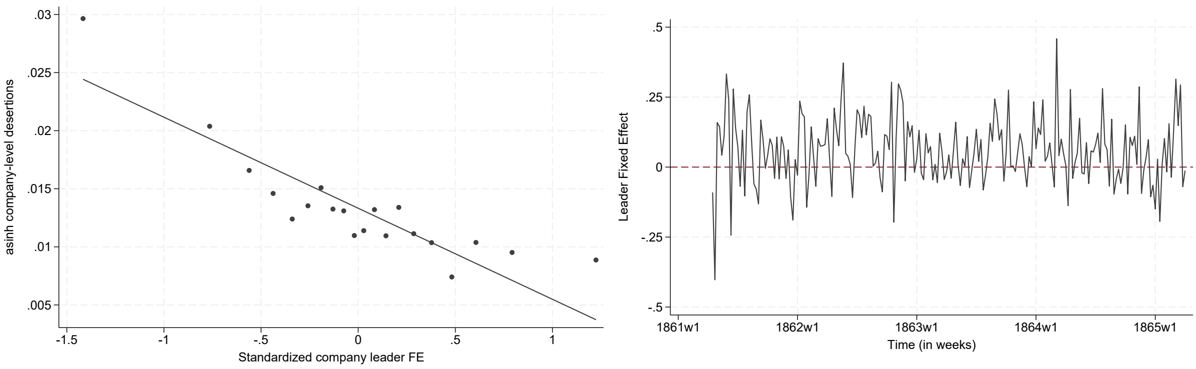
19th New York Infy...	Aug., 1861	From 1-B. Banks' D. Shenandoah..	To reorganization as 3d N. Y. Arty.	Dec., 1861
28th New York Infy......	Aug., 1861	From 1-B. Banks' D. Shenandoah..	To 1-Brig. 1-Div. Banks' 5-C. Pot..	Mch., 1862
5th Connecticut Infy....	Aug., 1861	From 1-B. Banks' D. Shenandoah..	To 1-Brig. 1-Div. Banks' 5-C. Pot..	Mch., 1862
28th Pennsylvania Infy	Aug., 1861	From 1-B. Banks' D. Shenandoah..	To Geary's Indpt. Brig. 5-Corps Pot.	Mch., 1862
2d Massachusetts Infy..	Aug., 1861	From 2d B. Banks' Div. Shenandoah	To 3-Brig. Banks' Div. Pot.........	Mch., 1862
1st Maryland Infy.....	Aug., 1861	From Dix's Div. Baltimore, Md.....	To 1-Brig. 1-Div. Banks' 5-C. Pot..	Mch., 1862
46th Penna. Infy........	Nov., 1861	From New Organization..........	To 1-Brig. 1-Div. Banks' 5-C. Pot..	Mch., 1862

Note: Example of the Union Army organization and command at military hierarchies above the regiment from Dyer (1908) (p. 274). In this case, Banks' Division, organized under Major General Banks, was directly assigned to the Army of the Potomac without a corps intermediate between the two organizational units. This is for the period from October 1861 until March 1862, when the next larger restructuring of the unit occurred. The snippet shows the 1st Brigade, its commanders by date of command, as well as the regiments that belonged to the brigade by date of assignment. The colored boxes mark the different hierarchies in descending order: red = Army, green = division, orange = brigade, blue = regiment. Sub-regimental units are the companies, which are led by captains. A typical army would be comprised of 2-3 corps (ca. 80,000 soldiers), a corps usually consisted of 2-3 divisions (ca. 26,000 soldiers), and a division would include around 2-4 brigades (ca. 8,000 soldiers). Each of these would be led by a Major General. Brigades consisted of 2-5 regiments (ca. 2,600 soldiers) and were led by brigadier generals, a regiment consisted of 6-10 companies (ca. 1,000 soldiers) and was led by colonels, and a company would have 100 soldiers with a captain as their leader. Artillery and cavalry units would be smaller in size due to their higher reliance on capital than labor.

Figure 2: Raw Data Comparison of In-Battle Desertions and the Captain Fixed Effect

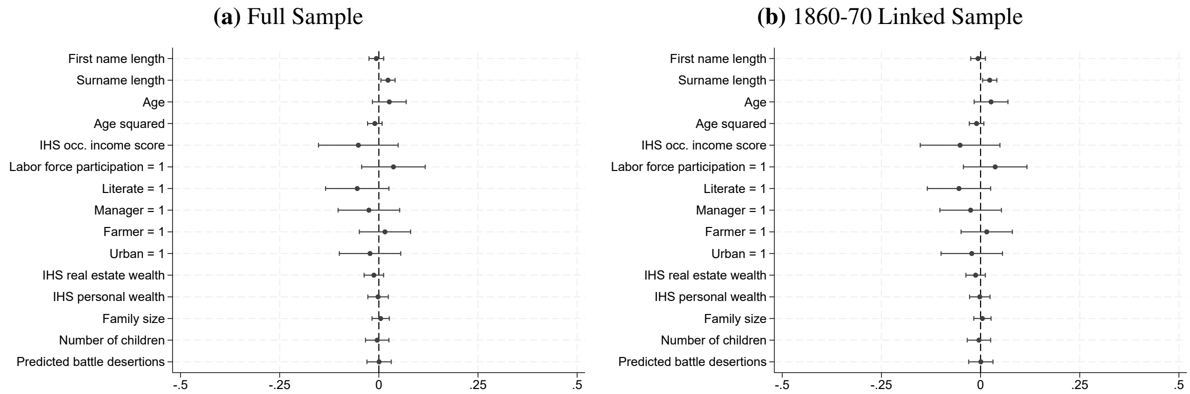
(a) In-Battle Desertions and Leader Quality

(b) Leader Quality over Time



Note: Panel a displays a binned scatter plot of company-level desertion rates in battle over the estimated company-leader fixed effects. The leader fixed effect was estimated on a company-week panel using only non-battle weeks (i.e., weeks without known battles, skirmishes, or recorded battle deaths), controlling for compositional characteristics of the companies using the regression specification in equation (1). Panel b shows the time series of the average leader fixed effect by week over the duration of the war. To avoid double counting, we only consider leaders in their first week of service as captains of their company.

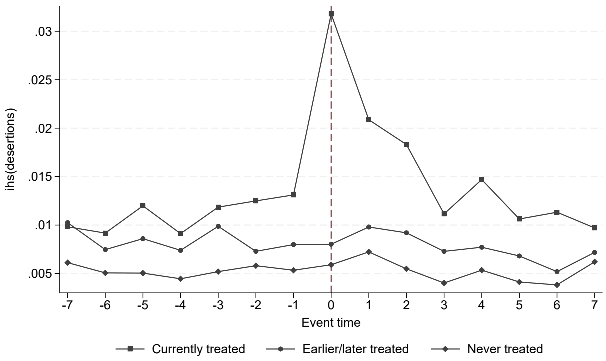
Figure 3: Leader Fixed Effects Regressions on Leaders' 1860 Observable Characteristics



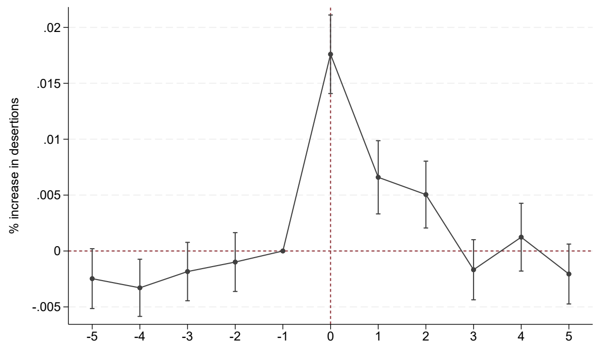
Note: Coefficient plot from a regression of the captain fixed effect $\widehat{\kappa}_c$ on observable pre-war characteristics using the linked sample of 10,742 captains whom we linked to the 1860 census (panel a), and 6,900 captains who were linked to the 1860 and 1870 censuses (panel b) based on their first and last name, age, and state and county of residence. Since it is a horizontal match as opposed to one across long periods of time, as in typical census linking, we used the residence information to increase the quality of the matches. First and surname length are the number of letters in each. IHS occ. income score is the inverse hyperbolic sine of a captain's occupational income score, which is the median earnings in each occupation in 1950 in hundreds of dollars. The same transformation was applied to real estate and personal wealth. The predicted battle desertion rate in a leader's unit was estimated from regressing the in-battle desertion indicator on soldiers' pre-war characteristics using a sample of soldiers that we linked to the 1860 census and who were observed during the 19 major battles of the war. More information on this individual-level soldier sample is provided in Table 5. The regressions also include county of residence and birth place fixed effects. Standard errors are clustered at the county level. Error bars show 95 percent confidence intervals.

Figure 4: Event Studies of Desertions During the 19 Major Battles of the Civil War

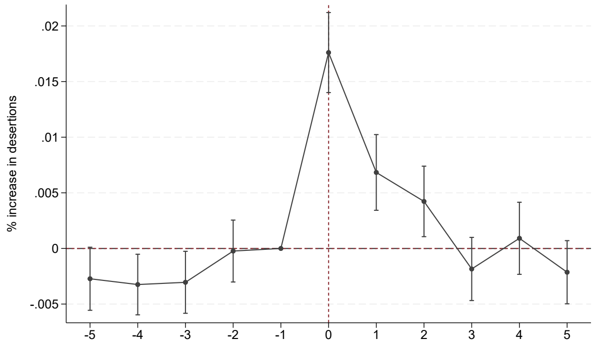
(a) Raw Desertions Data by Treatment Status



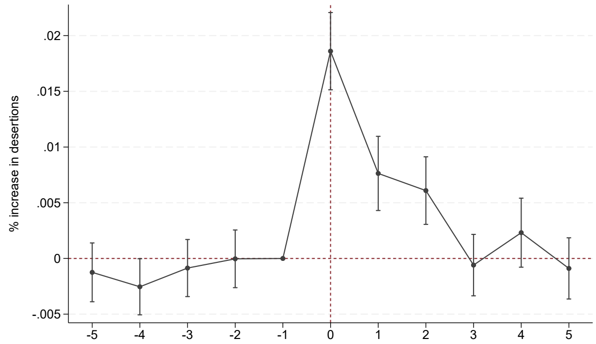
(b) Battles Event Study



(c) Event Study using Never-Treated Control



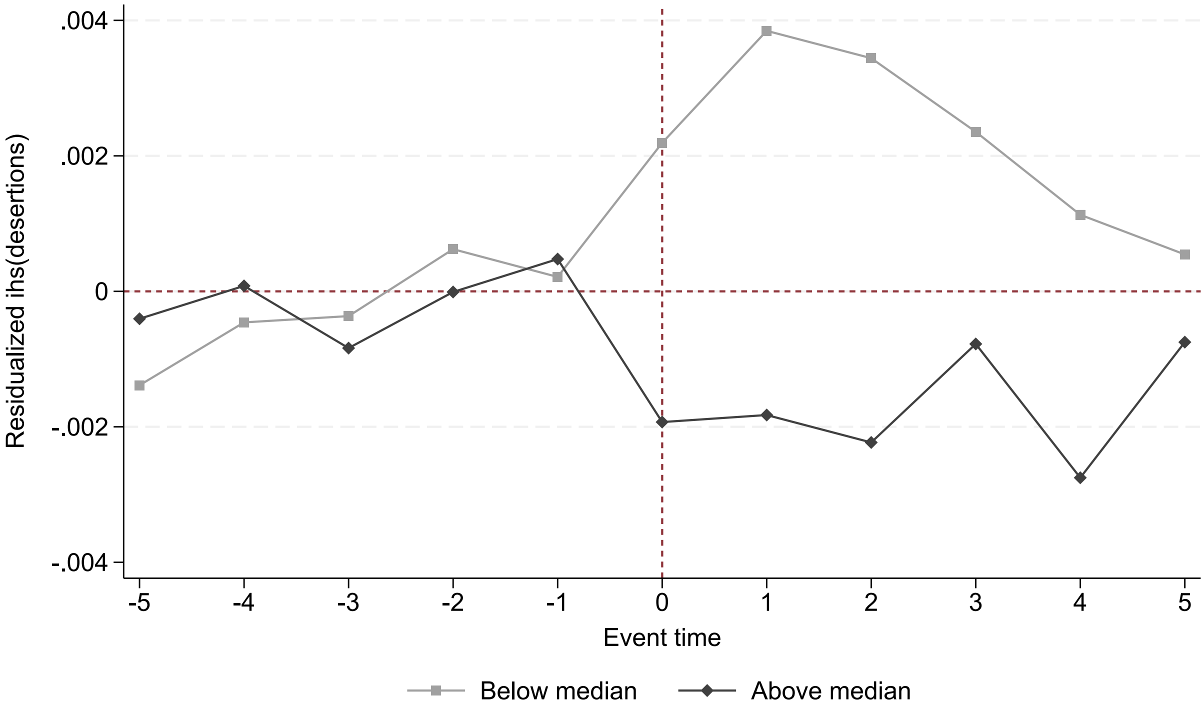
(d) Dube et al. (2023) Event Study Estimator



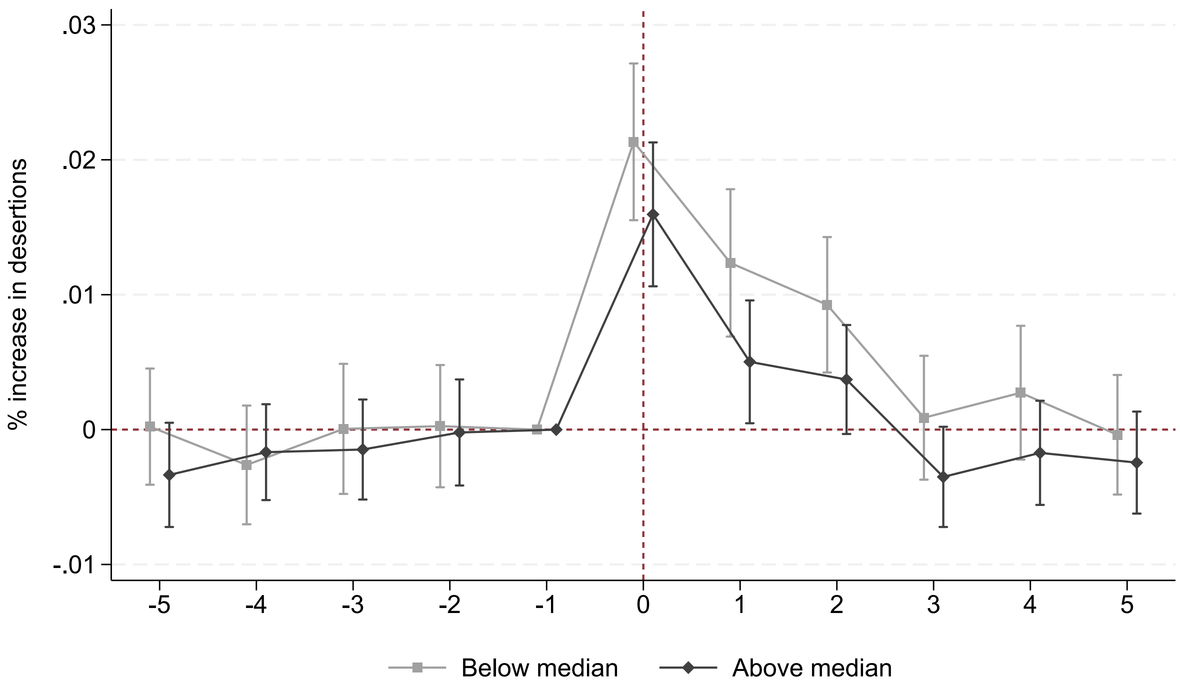
Note: Panel a plots the raw data averages of the inverse hyperbolic sine of desertions by treatment status over battle event time for the seven days before and after each major battle. These include the 19 largest battles of the war as per Selcer (2006). Treatment status is either currently treated, i.e., units participating in the current battle, units that were not in the current battle but were treated earlier or later in the war, and the never treated units that did not participate in any battle. Panel b plots the event study coefficients from regressing the asinh of desertions on a treatment indicator (battle participation) interacted with event time fixed effects, leaving out the pre-battle day ($t = -1$) as the baseline, as well as company and time fixed effects and controlling for pre-battle unit size. The coefficients times one hundred estimate the approximate increase in company-level desertion rates on any given day in the event window. The control units are the not yet and later treated units, as well as the never-treated units. Standard errors are clustered at the company level. Error bars display 95 percent confidence intervals. The event study is estimated over the whole sample from $t = -7$ to $t = 7$, but only coefficients for $t = -5$ to $t = 5$ are reported to avoid potential end-point bias as suggested by Baker et al. (2022). Panel c repeats the event study regression but only uses the never-treated companies as control units. Panel d uses the same control units as in panel a but adjusts the estimates for potential weight-bias in twoway fixed effects models by applying the local projections difference-in-differences method proposed by Dube et al. (2023).

Figure 5: Leader Quality and Desertions in the Major 19 Battles of the Civil War

(a) Event Study Residuals by Above/Below-Median Leader Quality

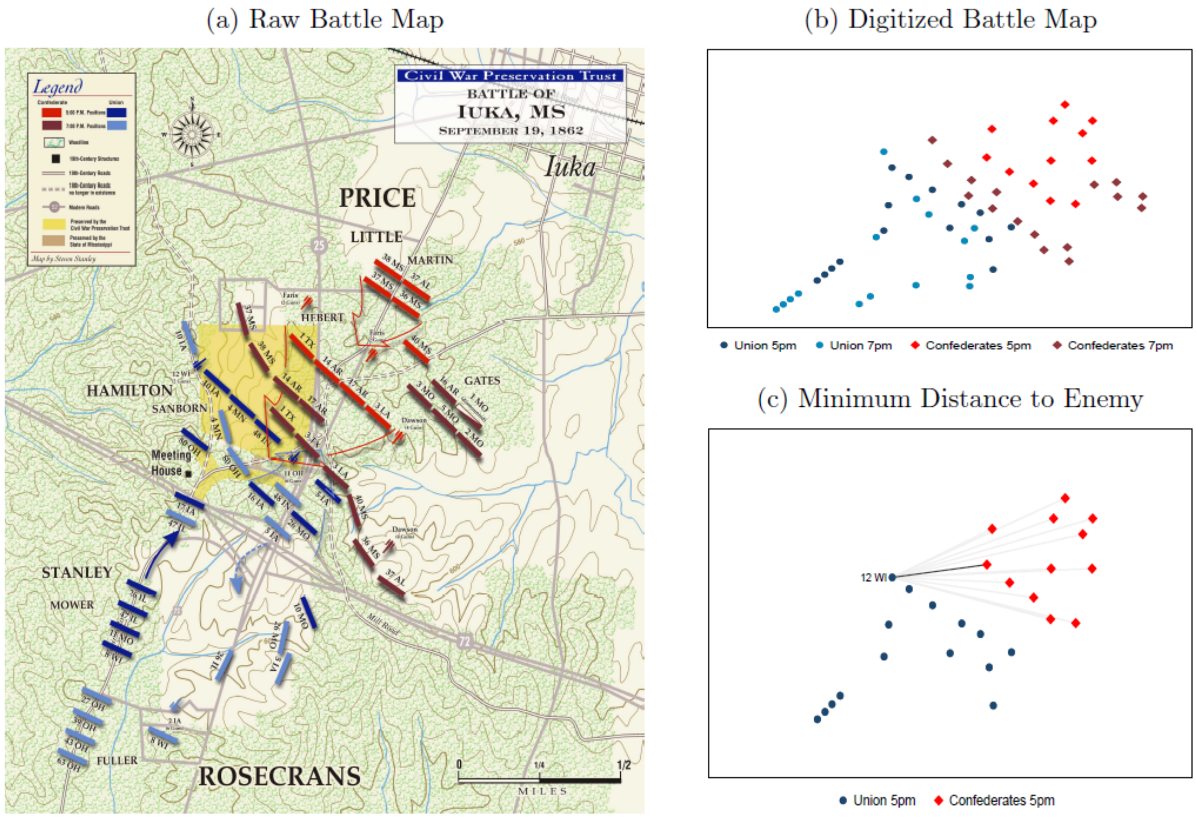


(b) Event Study by Above/Below-Median Leader Quality



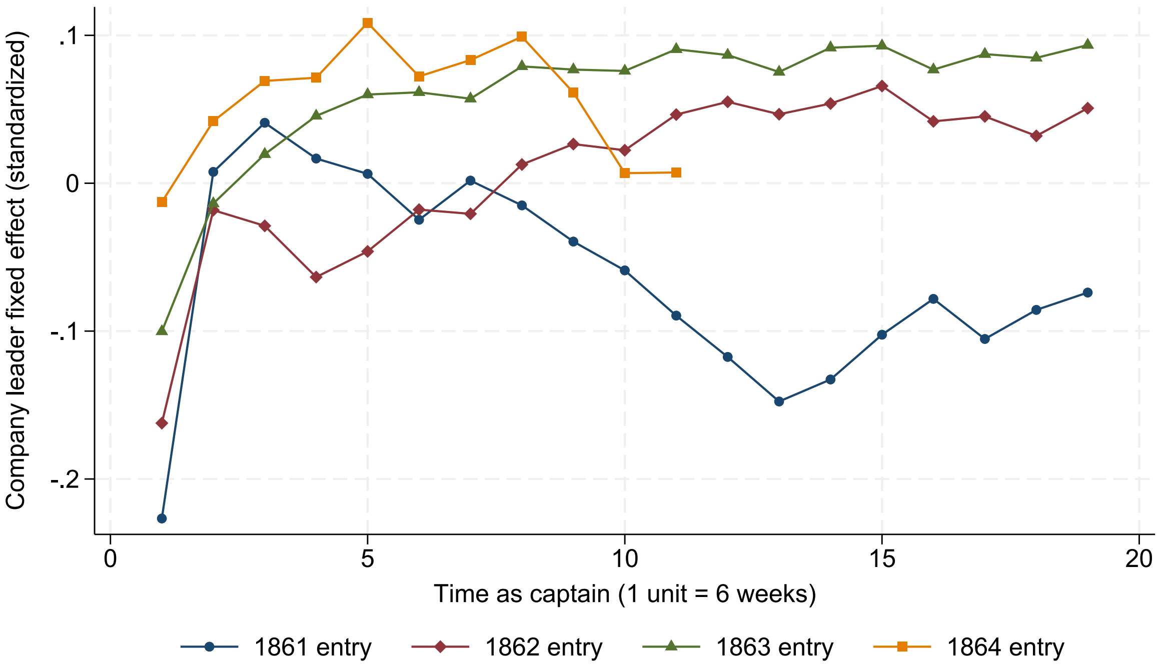
Note: Panel a plots the residuals from the event study regression in panel b of Figure 4, splitting them by above- or below-median leader quality, as measured by the leader fixed effect $\widehat{\kappa}_c$ of the captain who led the company in the given battle event. Panel b re-estimates the event study in panel b of Figure 4 as a triple-differences regression using the above-median leadership quality indicator as additional differencing variable. The regression includes the triple interaction between treatment, event time, and above-median leadership quality indicators, the pairwise interactions, as well as the main effects. The plotted coefficients are from the interactions of treatment and event time indicators (gray squares), and the linear combination of the coefficients from the interaction of treatment and event time indicators plus the coefficients from the triple interactions (black diamonds). These two sets of coefficients estimate the effect of battle treatment on desertion on units with below or above-median quality captains. Standard errors are clustered at the company level, and error bars show 95 percent confidence intervals.

Figure 6: Battle Map Example



Note: Panel a shows the raw battle map for the Battle of Iuka, Mississippi, on September 19, 1862. Union and Confederate regiment positions are shown for two phases of the battle. These are at 5 p.m. (dark blue Union, light red Confederacy) and at 7 p.m. (light blue Union, dark red Confederacy). Panel b plots the digitized version of the map. Panel c shows Union and Confederate regiments in their 5 p.m. location, computes the distances to the closest enemy units from the 12th Wisconsin, and marks the minimum distance with a black line, with all other distances displayed as gray lines to visualize how the distance to the nearest enemy unit is computed. Battle maps were obtained from the Civil War Preservation Trust and digitized by the authors by using a pattern recognition algorithm in Python. We thank the American Battlefield Trust (www.battlefields.org) for granting written permission to use their maps and to reprint the original battle map in panel a.

Figure 7: Learning-by-Doing Effects by Entry Year



Note: Estimated company leader quality over 6-week tenure spells by year of entry into service for the Union Army. Company leader quality was estimated from a company-week panel in which we observe leaders over time for different spell lengths. We regressed the asinh of desertions in a company on company characteristics, calendar week, and captain fixed effects similar to the specification in equation (1). The predicted captain fixed effect, multiplied by -1 (so that a higher fixed effect means lower desertions), in each 6-week spell is then the estimated leader quality as it captures a captain's ability to maintain group cohesion in their unit. Weeks in which the company was involved in a battle are excluded from the sample.

Appendix

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A Additional Empirical Results

Tables

Table A.1: Observational Differences between Captains and Soldiers over Time

	1861		1862		1863		1864		1865	
	Diff.	t-stat	Diff.	t-stat	Diff.	t-stat	Diff.	t-stat	Diff.	t-stat
Age	7.45	48.61	6.31	37.19	4.85	13.66	5.06	16.02	2.73	4.57
Foreign-born	-0.07	-10.51	-0.06	-9.00	-0.05	-3.34	-0.08	-7.45	-0.10	-4.91
Urban	0.10	11.32	0.08	8.51	0.08	4.14	0.04	2.41	0.00	0.07
On farm	-0.16	-23.35	-0.14	-16.23	-0.12	-6.61	-0.10	-6.27	-0.11	-4.01
Family size	-0.26	-5.59	-0.30	-5.98	-0.29	-2.67	-0.26	-2.91	-0.17	-0.99
No. of own children	0.97	26.85	0.76	19.73	0.66	8.18	0.61	9.23	0.46	3.61
Manager	0.09	16.65	0.09	15.03	0.07	6.42	0.06	6.52	0.05	3.09
High-skilled	0.08	15.24	0.08	14.55	0.05	5.64	0.05	6.10	0.03	2.61
Semi-skilled	0.09	10.19	0.06	7.53	0.08	4.43	0.07	4.42	0.09	3.16
Laborer	-0.16	-33.04	-0.14	-26.95	-0.13	-11.72	-0.07	-6.30	-0.06	-2.61
Asinh property wealth	0.95	11.34	1.26	13.69	0.86	4.49	0.85	5.11	0.18	0.61
Asinh personal wealth	1.45	17.67	1.53	17.29	1.30	7.03	0.93	5.80	0.50	1.69
Asinh occ. score	0.27	14.56	0.28	15.79	0.15	3.67	0.13	3.73	0.09	1.43
Died during the war	-0.06	-10.75	-0.06	-11.29	-0.06	-6.66	-0.05	-7.12	-0.01	-1.96
Died of disease	-0.04	-20.40	-0.06	-22.31	-0.04	-7.87	-0.03	-7.76	-0.01	-2.67
Died in battle	-0.01	-3.45	-0.01	-1.79	-0.01	-2.28	-0.01	-1.90	-0.01	-1.38
Deserted	-0.07	-89.15	-0.07	-89.43	-0.12	-101.61	-0.07	-49.05	-0.09	-68.00

Note: Differences in average pre-war characteristics and bivariate t-tests for captains (N = 7,673) and regular soldiers (N = 644,612) whom we could link to the 1860 census by year of enlistment. Occupation classifications follow the occ1950 IPUMS codes: manager (codes 200-290), other high-skilled (codes 1-100, 300-390), semi-skilled (codes 400-690), and laborer (codes 800-990). Asinh property wealth, personal wealth, and occupational income score represent the inverse hyperbolic sine of these variables. Indicator variables include foreign-born, urban, on farm, the occupational variables, and death and desertion indicators. Variance estimates adjusted for the unequal group sizes.

Table A.2: List of Sources for the Union Army Soldier Data

- **California**: Orton, R.H. (1890) "Records of California Men in the War of the Rebellion 1861 to 1867", State Office, J. D. Young, Supt. State Printing, Sacramento, CA
- **Connecticut**: Barbour, L.A., Camp, F.E., Smith, S.R., and White, G.M. (1889) "Record of Service of Connecticut Men in the Army and Navy of the United States During the War of the Rebellion", Case, Lockwood, & Brainard Company, Hartford, CT
- **Illinois**: Reece, J.N. (1900) "Report of the Adjutant General of the State of Illinois", Vols. 1-9, Philips Bros. State Printers, Springfield, IL
- **Indiana**: Terrell, W.H.H. (1866) "Report of the Adjutant General of the State of Indiana", Vols. 1-5, Samuel M. Douglass State Printers, Indianapolis, IN
- **Iowa**: Thrift, W.H. (1908) "Roster and Record of Iowa Soldiers in the War of Rebellion", Vol. 1-6, Emory H. English State Printers, Des Moines, IA
- **Kansas**: Fox, S.M. (1896) "Report of the Adjutant General of the State of Kansas", The Kansas State Printing Company, Topeka, KS
- **Maine**: Adjutant General (1861-66) "Supplement to the Annual Reports of the Adjutant General of the State of Maine", Stevens & Sayward State Printers, Augusta, ME
- **Massachusetts**: Schouler, W. (1866) "Report of the Adjutant General of the Commonwealth of Massachusetts", Wright & Potter State Printers, Boston, MA
- **Michigan**: Crapo, H.H. (1862-66) "Report of the Adjutant General of the State of Michigan", John A. Kerr & Co. State Printers, Lansing, MI
- **Minnesota**: Marshall, W.R. (1861-66) "Report of the Adjutant General of the State of Minnesota", Pioneer Printing Company, Saint Paul, MN
- **Nebraska**: Dudley, E.S. (1888) "Rosters of Nebraska Volunteers from 1861 to 1869", Wigton & Evans State Printers, Hastings, NB
- **New Hampshire**: Head, N. (1865) "Report of the Adjutant General of the State of New Hampshire", Vols. 1& 2, Amos Hadley State Printers, Concord, NH
- **New Jersey**: Stryker, W.S. (1874) "Report of the Adjutant General of the State of New Jersey", Wm. S. Sharp Steam Power Book and Job Printers, Trenton, NJ
- **New York**: Sprague, J.T. (1864-68) "A Record of the Commissioned Officers, Non-Commissioned Officers and Privates of the Regiments which were Organized in the State of New York into the Service of the United States to Assist in Suppressing the Rebellion", Vols. 1-8, Comstock & Cassidy Printers, Albany, NY
- **Ohio**: Howe, J.C., McKinley, W., and Taylor, S.M. (1893) "Official Rosters of the Soldiers of the State of Ohio in the War of the Rebellion 1861-65", Vols. 1-12, The Werner Company, Akron, OH
- **Pennsylvania**: Russell, A.L. (1866) "Report of the Adjutant General of Pennsylvania", Singerly &ers State Printers, Harrisburg, PA
- **Vermont**: Peck, T.S. (1892) "Revised Roster of Vermont Volunteers and Lists of Vermonters who Served in the Army and Navy of the United States during the War of the Rebellion 1861-66", Press of the Watchman Publishing Co., Montpelier, VT
- **Wisconsin**: Rusk, J.M. and Chapman, C.P. (1886) "Roster of Wisconsin Volunteers, War of the Rebellion 1861-65", Democrat Printing Company, Madison, WI
- **Multiple states**: Adjutant General's Office (1861-65) "Official Army Register of the Volunteer Force of the United States for the Years 1861, '62, '63, '64, '65", Part 1-8, Adjutant General's Office, Washington DC

Table A.3: Company-Level Descriptive Statistics

	N = 8,537 companies			
	Mean	St. Dev.	Min.	Max.
Panel A: company characteristics				
Company tenure (weeks)	149.850	45.697	1	208
Number of soldiers	67.698	14.504	28	132
Number of deserters	5.671	7.860	0	91
Number of deaths	10.507	5.619	0	34
Number of battle deaths	5.087	4.075	0	31
Infantry unit (1=yes)	0.917	0.276	0	1
Cavalry unit (1=yes)	0.069	0.254	0	1
Artillery unit (1=yes)	0.003	0.057	0	1
Panel B: leader statistics				
Number of company leaders	3.525	1.483	1	10
Number of regiment leaders	3.895	1.651	1	11
Number of captains who died	0.103	0.251	0	2
Number of colonels who died	0.007	0.058	0	1
Panel C: battle statistics				
Battles fought	5.154	3.129	0	21
Major battles fought	1.084	0.870	0	4
Panel D: organizational structure				
Number of Brigades assigned to	3.660	1.646	1	11
Number of Divisions assigned to	4.339	2.028	1	13
Number of Corps assigned to	3.900	1.746	1	11
Number of Armies assigned to	3.294	1.561	1	12

Note: The table reports observable characteristics of the 8,537 companies we observe in our data. All statistics are cumulative averages for each company over the sample period from the start of the war on April 12, 1861, to its official conclusion on April 9, 1865. Company tenure is the time a company existed throughout the war measured in weeks. The infantry, cavalry, and artillery unit indicators are mutually exclusive. They do not sum to one hundred because there is a small share of units that are engineers, sharpshooters, or other support units that do not fall within those three major service branches. There are 16 independent companies that were not assigned to any brigade, division, corps, or army but acted as stand-alone units. A small share of companies (1.31 percent) reported that they had more than 100 soldiers. The major battles participation indicator is one for units that fought in at least one of the top 19 deadliest battles (the original list in Selcer (2006) had 20 battles, but we excluded Fort Fisher because this was primarily a naval engagement). Battles do not nest major battles or vice versa.

Table A.4: Information on the Top 19 Major Civil War Land Battles

Battle	Dates	Union Win/Loss	Union Strength	Killed	Wounded	Missing / POW
1st Bull Run	July 21, 1861	Loss	35,732	481	1,011	1,216
Shiloh	April 6-7, 1862	Win	63,000	1,754	8,408	2,885
Seven Pines/Fair Oaks	May 31 - June 1, 1862	Inconcl.	34,000	790	3,594	647
Seven Days	June 25 - July 1, 1862	Inconcl.	114,691	1,734	8,062	6,053
2nd Bull Run	Aug. 28-30, 1862	Loss	77,000	1,747	8,452	4,263
Antietam	Sept. 16-17, 1862	Win	87,164	2,108	9,549	753
Perryville	Oct. 8, 1862	Win	55,000	845	2,851	515
Fredericksburg	Dec. 11-15, 1862	Loss	122,009	1,284	9,600	1,769
Stones River	Dec. 31 1862 - Jan. 2 63	Win	43,400	1,677	7,543	3,686
Chancellorsville	April 30 - May 6, 1863	Loss	133,868	1,606	9,762	5,919
Vicksburg	May 18 - July 4, 1863	Win	77,000	766	3,793	276
Gettysburg	July 1-3, 1863	Win	104,256	3,155	14,529	5,365
Chickamauga	Sept. 18-20, 1863	Loss	60,000	1,657	9,756	4,757
Chattanooga	Nov. 23-25, 1863	Win	72,533	753	4,722	349
Wilderness	May 5-7, 1864	Inconcl.	124,232	3,469	16,000	3,383
Spotsylvania	May 8-21, 1864	Inconcl.	110,000	2,725	13,416	2,258
Cold Harbor	May 31 - June 12, 1864	Loss	117,000	1,845	9,077	1,816
Petersburg (Crater)	July 30th 1864	Loss	125,000	1,688	8,513	1,185
Atlanta	July 22 , 1864	Win	34,863	3,641*		

Note: The 19 most important land battles of the Civil War from Selcer (2006). Casualty information is from various sources and authors' own computations. * marks joint casualty numbers for deaths, wounded, and missing combined. Note that the Seven Days battle actually was a collection of seven battles that happened over seven days between June 25 and July 1, 1862, but are counted together due to their geographic proximity and them being part of the same campaign. The Battle for Atlanta was part of the longer-lasting siege of Atlanta. The Battle of the Crater was the deadliest battle of the Siege of Petersburg, which lasted from June 1864 to April 1865. The 20th battle (not in this list) listed by Selcer was Fort Fisher, a naval engagement for which we do not have data and which would be very different in nature from land battles.

Table A.5: Robustness to Alternative Leadership Fixed Effect Measures

					Percentile transformed treatments				
	Poisson FE	FE using pre-battle data	FE incl. comp. FE	FE using individual-level data	baseline FE	Poisson FE	FE using pre-battle data	FE incl. comp. FE	FE using individual-level data
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Robustness checks for Table 2					
ln(occ score)$_{1870}$ (no ctrls)	0.047**	0.013	0.041	0.054**	0.066***	0.047**	0.021	0.034	0.042**
	(0.023)	(0.026)	(0.027)	(0.022)	(0.024)	(0.023)	(0.024)	(0.026)	(0.020)
ln(occ score)$_{1870}$	0.051**	0.016	0.038*	0.054**	0.072***	0.051**	0.023	0.030	0.043*
	(0.025)	(0.026)	(0.021)	(0.023)	(0.023)	(0.024)	(0.027)	(0.021)	(0.022)
				Robustness checks for Table 3					
Hero = 1	0.005**	0.004*	0.002	0.000	0.016***	0.007***	0.004	0.005**	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Leader = 1	0.004***	0.001	0.003***	0.005***	0.004***	0.004***	0.000	0.005***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Brave = 1	0.001	0.001	0.000	0.000	0.009***	0.003	0.000	0.003	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Gptyes = 1	0.002	0.000	0.005**	-0.000	0.012***	0.004	0.001	0.010***	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
MoH = 1	0.003	0.004*	-0.000	-0.004**	0.010***	0.004*	0.003	0.001	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ln bio length	0.010*	-0.001	-0.003	0.054***	-0.016***	0.003	-0.015**	-0.008	0.056***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
				Robustness checks for Table 4					
Previous leader killed	0.338***	0.297***	0.197***	0.313***	0.264***	0.316***	0.311***	0.182***	0.308***
	(0.082)	(0.068)	(0.031)	(0.062)	(0.035)	(0.077)	(0.073)	(0.033)	(0.058)
asinh(desertions)(col.2)	-0.083**	-0.158***	-0.202***	-0.022	-0.116***	-0.089***	-0.147***	-0.163***	-0.016
	(0.033)	(0.057)	(0.039)	(0.019)	(0.033)	(0.028)	(0.048)	(0.053)	(0.055)
asinh(desertions)(col.3)	-0.083***	-0.154***	-0.202***	-0.023	-0.112***	-0.089***	-0.142**	-0.158***	-0.014
	(0.028)	(0.049)	(0.037)	(0.019)	(0.031)	(0.028)	(0.055)	(0.053)	(0.056)

Note: Table continues on the next page.

Table A.5: Robustness to different leadership FE measures (continued)

					Percentile transformed treatments				
	Poisson FE	FE using pre-battle data	FE incl. comp. FE	FE using individual-level data	baseline FE	Poisson FE	FE using pre-battle data	FE incl. comp. FE	FE using individual-level data
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Robustness checks for Table 5								
Pr(deserted)$_i$ (no ctrls)	-0.008***	-0.002*	-0.003**	-0.015***	-0.015***	-0.011***	-0.002	-0.004***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Pr(deserted)$_i$ (ctrls)	-0.008***	-0.002*	-0.003**	-0.014***	-0.020***	-0.011***	-0.003**	-0.005***	-0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
	Robustness checks for Table 6								
Log distance^{-1}	0.015	-0.008	-0.010	-0.027*	0.015	0.020	-0.003	-0.002	-0.021
	(0.014)	(0.014)	(0.013)	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)
Log desertions	-0.244***	-0.126***	-0.144***	-0.047**	-0.153***	-0.264***	-0.158***	-0.176***	-0.053***
	(0.015)	(0.021)	(0.023)	(0.019)	(0.020)	(0.016)	(0.019)	(0.020)	(0.018)
Log desertions win	-0.254***	-0.140***	-0.147***	-0.024	-0.175***	-0.281***	-0.165***	-0.214***	-0.031
	(0.019)	(0.033)	(0.036)	(0.028)	(0.030)	(0.023)	(0.029)	(0.026)	(0.027)
Log desertions loss	-0.235***	-0.115***	-0.143***	-0.070**	-0.133***	-0.250***	-0.152***	-0.144***	-0.075***
	(0.023)	(0.026)	(0.021)	(0.028)	(0.026)	(0.024)	(0.026)	(0.022)	(0.025)
Log battle deaths	0.001	0.021	0.010	0.011	0.054**	0.033	0.021	-0.001	-0.005
	(0.027)	(0.027)	(0.026)	(0.026)	(0.026)	(0.027)	(0.025)	(0.028)	(0.026)
Log accidental deaths	-0.004	0.002	0.002	0.000	-0.000	-0.004*	0.002	0.002	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Leader deaths	0.019*	-0.001	0.007	0.007	0.040***	0.024**	-0.001	-0.011	-0.003
	(0.011)	(0.009)	(0.011)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.012)

Note: Replication of all main results using the alternative leader quality measures. Column 1 estimates the leader fixed effect using only pre-battle information, column 2 includes company fixed effects in addition to leader fixed effects, and column 3 estimates leader fixed effects from an individual-level soldier panel similar to the approach taken by the teacher value-added literature. Details on the construction of the alternative leader fixed effects measures are provided in Appendix B. Columns 4–6 report results using the baseline measure and the three alternative measures and apply a percentile transformation to limit the potential impact of extreme values. More information on controls and the estimation of standard errors is provided in the respective notes of each replicated table. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Leader Fixed Effect and Leaders' Income Score in 1870 - Link Weights

	Outcome: Asinh of leaders' occupational income score in 1870					
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE $\widehat{\kappa}_c$	0.094***	0.093***	0.093***	0.094***	0.094***	0.088**
	(0.035)	(0.035)	(0.036)	(0.036)	(0.035)	(0.035)
Individual controls		yes	yes	yes	yes	yes
Economic controls			yes	yes	yes	yes
Family controls				yes	yes	yes
Military controls					yes	yes
Post-Double selection						yes
Observations	3,125	3,125	3,125	3,125	3,125	3,126
Adj. R^2	0.209	0.212	0.226	0.229	0.229	0.196
Outcome mean	3.454	3.454	3.454	3.454	3.454	3.453

Note: Regressions of the inverse hyperbolic sine (asinh) of leaders' postwar occupational income scores in 1870 on their estimated leader fixed effects, $\widehat{\kappa}_c$, weighting by the total number of links made for a specific person from the 1860 to 1870 census using information from the Census Tree Project (Price et al., 2021). If a person was matched across census years by multiple linking algorithms, this increases the certainty that the link is a correct match. Leader fixed effects are standardized to have mean zero and variance one. *Individual controls* include the leader's age, indicators for their place of birth, and 1860 county-of-residence fixed effects. *Economic controls* include pre-war economic variables from the 1860 census, such as the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, as well as indicators for labor force participation, urban status, literacy, nine skill groups, and 13 industry groups (for the specific groups see the table note of Table 3). *Family controls* contain pre-war variables in 1860, including family size, number of children, and group quarter indicators. *Military controls* include indicators for whether the leader had originally enlisted as an officer, whether they were ever promoted above their current rank of captain or colonel, and an indicator for receipt of the Medal of Honor. Column 6 includes the residuals from the regressions in Table 3 to account for unobserved popularity effects from more famous leaders who may have seen a return to their fame as opposed to their leadership skill. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Leader Fixed Effect and Leaders' Income Score in 1870 - NYSIIS Links

	Outcome: Asinh Occupation Score in 1870 Full Count Census					
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE $\widehat{\kappa}_c$	0.100***	0.099***	0.101***	0.104***	0.104***	0.095***
	(0.037)	(0.037)	(0.035)	(0.036)	(0.036)	(0.035)
Individual controls		yes	yes	yes	yes	yes
Economic controls			yes	yes	yes	yes
Family controls				yes	yes	yes
Military controls					yes	yes
Post-Double selection						yes
Observations	2,763	2,763	2,763	2,763	2,763	2,766
Adj. R^2	0.197	0.200	0.213	0.217	0.217	0.184
Outcome mean	3.454	3.454	3.454	3.454	3.454	3.453

Note: Regressions of the inverse hyperbolic sine (asinh) of leaders' postwar occupational income scores in 1870 on their estimated leader fixed effects, $\widehat{\kappa}_c$ using the sample of observations that were linked from the 1860 to 1870 census using the NYSIIS links provided by the Census Linking Project (Abramitzky et al., 2020) (the change in linking methods is because the Census Tree Project does not include NYSIIS links). These are based on a phonetic name transformation that seeks to limit the impact of misspelled or inconsistently spelled names. Leader fixed effects are standardized to have mean zero and variance one. *Individual controls* include the leader's age, indicators for their place of birth, and 1860 county-of-residence fixed effects. *Economic controls* include pre-war economic variables from the 1860 census, such as the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, as well as indicators for labor force participation, urban status, literacy, nine skill groups, and 13 industry groups (for the specific groups see the table note of Table 3). *Family controls* contain pre-war variables in 1860, including family size, number of children, and group quarter indicators. *Military controls* include indicators for whether the leader had originally enlisted as an officer, whether they were ever promoted above their current rank of captain or colonel, and an indicator for receipt of the Medal of Honor. Column 6 includes the residuals from the regressions in Table 3 to account for unobserved popularity effects from more famous leaders who may have seen a return to their fame as opposed to their leadership skill. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Leader Fixed Effect and Leaders' Income Score in 1870 - Representativeness

	Outcome: Asinh Occupation Score in 1870 Full Count Census					
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE κ_c	0.068**	0.067**	0.069**	0.070**	0.070**	0.063**
	(0.031)	(0.031)	(0.031)	(0.030)	(0.030)	(0.031)
Individual controls		yes	yes	yes	yes	yes
Economic controls			yes	yes	yes	yes
Family controls				yes	yes	yes
Military controls					yes	yes
Post-Double selection						yes
Observations	4,911	4,911	4,911	4,911	4,911	4,912
Adj. R^2	0.168	0.174	0.189	0.191	0.192	0.165
Outcome mean	3.450	3.450	3.450	3.450	3.450	3.450

Note: Regressions of the inverse hyperbolic sine (asinh) of leaders' postwar occupational income scores in 1870 on their estimated leader fixed effects, $\widehat{\kappa}_c$, weighting observations by the inverse propensity score (IPS). The IPS was estimated via a probit regression of an indicator for whether a person was successfully linked from the 1860 to 1870 census on 1860 characteristics (asinh occupational income score, asinh of personal and real estate wealth, labor force participation, skill and industry indicators, age, urban and farm status indicators, literacy, and state indicators). The IPS weight was computed following Bailey et al. (2020) using the predicted probit probability of being in the linked sample, $\widehat{p}_i$, and the average probability of being in the linked sample, q, as $\frac{1-\widehat{p}_i}{\widehat{p}_i} \times \frac{q}{1-q}$. Reweighting the regressions using the IPS weight ensures that the estimation sample is representative of the underlying population of captains who were supposed to be linked across census years. Figure A.7 plots the common support of the predicted linking probabilities and shows satisfactory overlap. Leader fixed effects are standardized to have mean zero and variance one. The sample includes individual leaders whose military records were matched with the 1860 census and who could be linked from the 1860 to the 1870 census using the crosswalks provided by the Census Tree Project (Price et al., 2021). *Individual controls* include the leader's age, indicators for their place of birth, and 1860 county-of-residence fixed effects. *Economic controls* include pre-war economic variables from the 1860 census, such as the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, as well as indicators for labor force participation, urban status, literacy, nine skill groups, and 13 industry groups (for the specific groups see the table note of Table 3). *Family controls* contain pre-war variables in 1860, including family size, number of children, and group quarter indicators. *Military controls* include indicators for whether the leader had originally enlisted as an officer, whether they were ever promoted above their current rank of captain or colonel, and an indicator for receipt of the Medal of Honor. Column 6 includes the residuals from the regressions in Table 3 to account for unobserved popularity effects from more famous leaders who may have seen a return to their fame as opposed to their leadership skill. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Leader Quality and Postwar Outcomes of Regular Soldiers

	asinh occ. score	zero occ. score	works	asinh real estate wealth	asinh property wealth	managerial job
	(1)	(2)	(3)	(4)	(5)	(6)
Leader FE $\widehat{\kappa}_c$	0.00042	-0.00050	0.00040	-0.00289	-0.00515	-0.00095
	(0.00289)	(0.00078)	(0.00072)	(0.00929)	(0.00930)	(0.00059)
Observations	262,049	262,049	262,049	262,049	262,049	262,049
Adj. R^2	-0.030	-0.033	-0.033	0.084	0.050	-0.026
Outcome mean	3.302	0.089	0.912	3.211	3.928	0.055

Note: Regressions of individual soldiers' postwar economic outcomes in 1870 (inverse hyperbolic sine (asinh) of occupational income score, indicator for whether they work, asinh of real estate and property wealth, and an indicator for whether they work in a managerial occupation identified as occupational codes between 200 and 290 in the occ1950 variable) on their company leader's predicted leader fixed effect $\widehat{\kappa}_c$. Leader fixed effects are standardized to have mean zero and variance one. The sample consists of rank and file soldiers of the Union Army for whom we were able to link military records to both the 1860 census as well as to the 1870 census using the crosswalks provided by the Census Linking Project (Abramitzky et al., 2020). All regressions include county-of-residence and birthplace fixed effects, pre-war controls from the 1860 census including the inverse hyperbolic sine of the soldier's occupational income score, personal and real estate wealth, occupational skill group, and age. We report additional digits up to the fifth significant digit to highlight that we estimate tight null effects. Due to the large sample size, it became impractical to bootstrap, however, this will result in standard errors that are too small given that the captain fixed effect is a generated regressor. Significance levels based on standard errors clustered at the county-level and are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Leader Quality, Biographies and Military Career Outcomes for Colonels

	Biography mentions leader as			Military outcomes	
	hero	*leader*	*brave*	Medal of Honor	Promoted
	(1)	(2)	(3)	(4)	(5)
Leader FE κ_r	-0.020	-0.019	0.025	0.002	-0.005
	(0.018)	(0.016)	(0.017)	(0.011)	(0.043)
1860 census controls	yes	yes	yes	yes	yes
Military controls	yes	yes	yes	yes	yes
Observations	420	420	420	420	420
Adj. R^2	0.465	0.372	0.441	0.526	0.428
Outcome mean	0.017	0.007	0.012	0.010	0.564

Note: Regressions of indicators for whether a leader's biography refers to them with the words *hero*, *leader* (columns 1–3), or *brave*, as well as military outcomes including indicators for whether a colonel received the Medal of Honor and later promotions (columns 4–5) on the colonel fixed effect $\widehat{\kappa}_r$. Leader fixed effects $\widehat{\kappa}_r$ are standardized to have mean zero and variance one. The sample includes individual leaders whose military records were matched with the 1860 census. Pre-war controls from the 1860 census include a leader's age; indicators for literacy, urban status, labor force participation, and place of birth; fixed effects for county-of-residence; group quarters; number of children; as well as the inverse hyperbolic sine of their personal and real estate wealth. We also include indicators for nine occupational skill groups (professional/technical, managers/officials/proprietors, clerical/kindred workers, sales workers, craftsmen, operatives, service workers, farm laborers, and laborers) and for 13 industry groups (farm, mining, construction, durable and non-durable manufacturing, transportation, telecommunication, utilities, wholesale, retail, finance, services, public administration). These definitions follow the 1950 occupation and industry classifications provided by the Census Bureau. Military controls include indicators for the state of enlistment, age at enlistment, and the week, month, and year in which they enlisted. Observations are weighted by the total number of words in the biography. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Instrumental Variables Regressions of Desertions on A Binary Leader Quality Change

	D(Pos. leader change)	Inverse hyperbolic sine of desertions				
	(1)	(2)	(3)	(4)	(5)	(6)
D(Prev. leader died in battle)	0.404^{***}					
	(0.095)					
D(Pos. leader change)		-0.145^{**}	-0.135^{*}	-0.135^{*}	-0.131^{*}	-0.128^{*}
		(0.072)	(0.070)	(0.073)	(0.074)	(0.074)
% same nationality as prev. leader				-0.030	-0.031	-0.030
				(0.039)	(0.039)	(0.039)
% same county as prev. leader				0.037	0.035	0.033
				(0.060)	(0.060)	(0.060)
Lagged battle deaths					-0.001	0.002
					(0.008)	(0.007)
Prev. battle = win						-0.022^{*}
						(0.013)
Controls	yes		yes	yes	yes	yes
Company FE	yes	yes	yes	yes	yes	yes
Battle FE	yes	yes	yes	yes	yes	yes
Observations	6,627	6,627	6,627	6,627	6,627	6,627
K-P F-statistic		17.933	18.262	15.965	16.756	16.739
Outcome mean	0.145	0.034	0.034	0.034	0.034	0.034

Note: Instrumental variables regressions of the asinh of desertions in company c on an indicator for a positive leader change since the previous battle b which equals one if $\widehat{\kappa}_{c,b} > \widehat{\kappa}_{c,b-1}$, zero otherwise. D(·) is an indicator function. The unit of observation is the company on the battle day for the 19 major Civil War battles. The list of major battles was taken from Selcer (2006). The 20th battle was a sea battle which is not covered by our data set. The instrument for the positive leader change indicator is an indicator for whether the preceding leader was killed in the previous battle. On average, major battles are 63 days apart, meaning that any post-battle desertions from the previous battle will have occurred long before the current battle (see the event study in Figure 4). Column 1 reports the first-stage results, columns 2–5 report the second-stage results. All regressions include company and battle date fixed effects, the share soldiers from the same nationality and county, the share of the same nationality and county as their captain, the share from Republican majority counties, and the controls reported in the table. Standard errors are clustered at the company-level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Individual-Level Desertions and the Continuous Leader Fixed Effect

	Outcome: Pr(deserted)$_{it}$=1				
	(1)	(2)	(3)	(4)	(5)
Leader FE κ_c	-0.015***	-0.014***	-0.014***	-0.015***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Regiment fixed effects	yes	yes	yes	yes	yes
Geographic fixed effects		yes	yes	yes	yes
1860 census controls			yes	yes	yes
Soldier controls				yes	yes
Unit controls				yes	yes
Leader controls					yes
Observations	204,653	204,653	204,653	204,653	204,653

Note: Regressions of individual soldiers' binary desertion decisions during the 19 largest battles of the war on their company leader's predicted leader fixed effect $\widehat{\kappa}_c$. Leader fixed effects are standardized to have mean zero and variance one. The list of major battles was taken from Selcer (2006) and originally contained 20 battles; however, the 20th battle was a naval battle that is not covered in our data. The estimation sample consists of rank and file soldiers of the Union Army whose military records we were able to link to the 1860 census. All regressions include regiment fixed effects. Geographic fixed effects control for soldiers' 1860 county of residence. Other pre-war controls from the 1860 census include the inverse hyperbolic sine of the leader's occupational income score and that of their personal and real estate wealth, age, indicators for labor force participation, urban status, literacy, nine skill groups and 13 industry groups (for the specific groups see the table note of Table 3), family size, the number of children, group quarter indicators, and indicators for their place of birth. Soldier controls include a soldier's tenure in the military in months, and unit controls include the log number of soldiers, the number of soldiers who had received a Medal of Honor, the share of soldiers from the same county and ancestry, and the share from the same county or ancestry as the company leader. Leader controls include the captain's experience in months and indicators for whether the captain was ever promoted, wounded, or killed. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Leadership and Desertions at the Individual Level Robustness to Different Fixed Effects

	Outcome: Pr(deserted)$_{ib}$=1				
	(1)	(2)	(3)	(4)	(5)
Above median leader FE	-0.016***	-0.017***	-0.017***	-0.017***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Regiment FE	yes	yes	yes	yes	
1860 county FE	yes	yes			
Birth place FE	yes		yes		
Battle FE	yes			yes	
Regiment-Company FE	yes				yes
Observations	206,270	206,270	206,270	206,270	206,130
Adj. R^2	0.059	0.062	0.057	0.056	0.117
Outcome mean	0.039	0.039	0.039	0.039	0.038

Note: Regressions of individual soldiers' binary desertion decisions during the 19 largest battles (b) of the war on an indicator for whether their company leader's predicted leader fixed effect $\widehat{\kappa}_c$ was above the median. The sample consists of regular soldiers (not officers or sergeants) who have been linked to the 1860 U.S. census. The 1860 census controls include continuous measures such as the asinh occupational income score, age, family size, number of children, inverse hyperbolic sine of personal and real estate wealth, as well as indicators for a zero income score, labor force participation, literacy, group quarters, farm status, urban status, and for industry and skill groups as defined by the 1950 census definition of industries and occupations. Soldier controls include the number of battles the soldier has fought in and their experience, measured as time since enlistment. Unit controls include the total number of soldiers who deserted from the unit (excluding the current soldier), the number of deaths sustained by the unit in battle, unit size, the share of soldiers from pro-Lincoln counties in 1860, the number of other soldiers in the unit who previously received a Medal of Honor, and Herfindahl-type concentration indexes for the unit's composition with regards to German, Scandinavian, Irish, Italian, and other ancestries. The company leader controls include their experience, and indicators for whether they were previously promoted and for whether they were wounded or died in the battle. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Leadership and Desertions at the Individual Level - Alternative VCE Clustering

	Outcome: Pr(deserted)$_{it}$=1			
	(1)	(2)	(3)	(4)
Above median leader FE	-0.016***	-0.016***	-0.016***	-0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
Cluster unit:				
Regiment-Company	yes			
Regiment		yes		
1860 County			yes	
Battle				yes
Observations	206,270	206,270	206,270	206,270
Adj. R^2	0.059	0.059	0.059	0.059
Outcome mean	0.039	0.039	0.039	0.039

Note: Regressions of individual soldiers' binary desertion decisions during the 19 largest battles (b) of the war on an indicator for whether their company leader's predicted leader fixed effect $\widehat{\kappa}_c$ was above the median. The sample consists of regular soldiers (not officers or sergeants) who have been linked to the 1860 U.S. census. The 1860 census controls include continuous measures such as the inverse hyperbolic sine of occupational income score, age, family size, number of children, inverse hyperbolic sine of personal and real estate wealth, as well as indicators for a zero income score, labor force participation, literacy, group quarters, farm status, urban status, and for industry and skill groups as defined by the 1950 census definition of industries and occupations. Soldier controls include the number of battles the soldier has fought in and their experience, measured as time since enlistment. Unit controls include the total number of soldiers who deserted from the unit (excluding the current soldier), the number of deaths sustained by the unit in battle, unit size, the share of soldiers from pro-Lincoln counties in 1860, the number of other soldiers in the unit who previously received a Medal of Honor, and Herfindahl-type concentration indexes for the unit's composition with regards to German, Scandinavian, Irish, Italian, and other ancestries. The company leader controls include their experience, and indicators for whether they were previously promoted and for whether they were wounded or died in the battle. Significance levels based on bootstrapped standard errors with 200 replications are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Battle Map Analysis with Alternative Standard Error Clustering

	Inv. log distance	Inv. hyperbolic sine of desertions All battles	Wins	Losses	Battle Deaths	Acc. Deaths	Pr(Leader Death)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leader FE $\widehat{\kappa}_R$	0.001	-0.105***	-0.148***	-0.082*	0.072***	-0.001	0.017**
	(0.017)	(0.035)	(0.035)	(0.045)	(0.024)	(0.002)	(0.007)
Cluster unit:							
Battle	(0.017)	(0.033)	(0.027)	(0.050)	(0.027)	(0.001)	(0.016)
Regiment	(0.018)	(0.042)	(0.056)	(0.050)	(0.038)	(0.002)	(0.026)
Battle-Regiment	(0.017)	(0.035)	(0.035)	(0.045)	(0.024)	(0.002)	(0.013)
Observations	2,823	2,823	1,461	1,362	2,823	2,823	2,823
Outcome mean	0.000	0.607	0.602	0.612	2.381	0.009	0.156
Adj. R^2	0.598	0.266	0.255	0.279	0.336	0.006	0.052

Note: Regiment-level regressions of the log inverse distance to the nearest enemy (column 1), the inverse hyperbolic sine (asinh) of desertions by battle result (columns 2–4), the asinh of battle deaths (column 5) and accidental deaths (column 6), as well as the probability that any captains in the regiment were killed in battle (column 7), on the average of company leaders' predicted leader fixed effects in the regiment $\widehat{\kappa}_r$ on the day of the battle. The unit of observation is the regiment-battle, which includes 125 unique battle maps and 720 unique regiments. Regiment-level controls include the log inverse distance to the nearest enemy unit (columns 2–7 only), the share of soldiers in the regiment who resided in the same county, the share of soldiers of the same ancestry, the share of soldiers who were from the same county as their captain, and the share of soldiers of the same ancestry as their captain, as well as battle fixed effects to account for different and unknown scaling of the maps, which would affect the interpretation of distance. Observations are weighted by the total number of soldiers in the regiment. The bottom panel reports block-bootstrapped standard errors clustered at the level of the battle, the regiment, and bootstrapped two-way clustered standard errors by battle and regiment, respectively.

Table A.16: Leader Quality and Outcomes at the Battle-Level

	Inv. hyperbolic sine of desertions		Union Army Victory			
	(1)	(2)	(3)	(4)	(5)	(6)
Share top leaders	-0.123***		0.102**		0.078*	
	(0.037)		(0.048)		(0.044)	
Share bottom leaders		0.080*		-0.091**	-0.075*	
		(0.041)		(0.041)	(0.041)	
asinh(desertions)						-0.526*
						(0.302)
Observations	119	119	119	119	119	119
Outcome mean	0.588	0.588	0.487	0.487	0.487	0.487
Adj. R^2	0.140	0.112	-0.208	-0.192	-0.174	
K-P F-statistic						3.818

Note: Regressions of the inverse hyperbolic sine of desertions at the battle-level on the standardized share of captains with a leader fixed effect in the top 5th percentile of the leader quality distribution (column 1) and on the share of leaders in the bottom 5th percentile who fought in a given battle. Columns 3 to 5 regress an indicator for whether the Union Army won a given battle on the same variables as in columns 1 and 2. In column 6, we regress the victory indicator on the inverse hyperbolic sine of desertions which we instrument with the share of bottom-5th percentile quality leaders. The latter is to be interpreted cautiously because of the small-sample bias of IV estimators and the low first stage F-statistic, but we report it to provide cautious evidence for the causal chain from captain quality to desertions to battle outcomes. All regressions controls for the same variables as in Table 6 averaged to the battle-level, log number of soldiers, and then umber of regiments in the battle, as well as year-month fixed effects. The original sample included 125 battles of which 6 were dropped due to the fixed effects. Robust standard errors in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Tenure Effects and Leader Quality by Different Spell Lengths

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$ ($s = 4$ weeks)					
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.0013	0.0003	0.0003	0.0011	0.0005	0.0015
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
Tenure squared	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	228,297	228,297	228,297	222,828	215,602	210,182
Adj. R-squared	0.591	0.594	0.594	0.596	0.596	0.598
	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$ ($s = 5$ weeks)					
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.0146***	0.0136***	0.0136***	0.0141***	0.0139***	0.0145***
	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0016)	(0.0016)
Tenure squared	-0.0005***	-0.0005***	-0.0005***	-0.0006***	-0.0005***	-0.0006***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	196,669	196,669	196,669	192,052	185,908	181,330
Adj. R-squared	0.667	0.669	0.669	0.670	0.671	0.673
	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$ ($s = 7$ weeks)					
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.0042**	0.0036**	0.0035**	0.0039**	0.0036**	0.0040**
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Tenure squared	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	147,330	147,330	147,330	143,940	139,502	136,139
Adj. R-squared	0.735	0.735	0.735	0.736	0.737	0.739
	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$ ($s = 8$ weeks)					
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.0076***	0.0069***	0.0068***	0.0070***	0.0072***	0.0075***
	(0.0020)	(0.0020)	(0.0020)	(0.0020)	(0.0020)	(0.0020)
Tenure squared	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Regiment FE	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes
Company controls	yes	yes	yes	yes	yes	yes
Baseline desertions		yes	yes	yes	yes	yes
Battle controls			yes	yes	yes	yes
Excl. promoted leaders				yes		yes
Excl. killed leaders					yes	yes
Observations	128,193	128,193	128,193	125,253	121,398	118,482
Adj. R-squared	0.734	0.734	0.735	0.736	0.736	0.738

Note: Regressions of the leader fixed effect $\widehat{\kappa}_{c,s}$ for leader c in each spell s (spell length of 4, 5, 7, or 8 weeks) on captains' tenure and tenure squared in their current unit. The leader-spell fixed effect $\widehat{\kappa}_{c,s}$ is estimated by regressing the inverse hyperbolic sine (asinh) of desertions on leader indicators—as in equation (1)—using a company-week panel excluding weeks with battles, skirmishes, or any recorded battle deaths, and dividing this out-of-battle sample into intervals (spells s) during which the fixed effect is estimated. For a description of the controls, see the main Table 7. All regressions include regiment fixed effects and fixed effects for the starting week of each spell. Standard errors are clustered at the level of the captain, and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

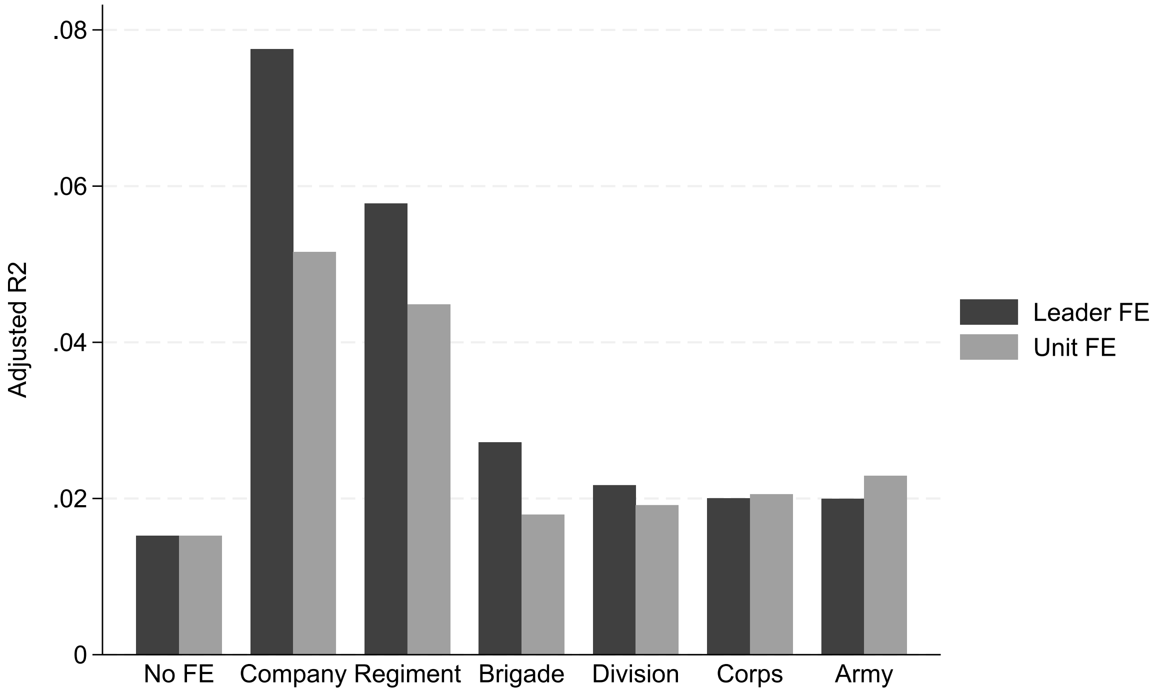
Table A.18: Tenure Effects on Leader Quality - Interactions

	Outcome: Leader Spell Fixed Effect $\widehat{\kappa}_{c,s}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tenure (T)	0.0130***	0.0130***	0.0130***	0.0131***	0.0137***	0.0131***	0.0132***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0019)	(0.0018)	(0.0019)
Tenure squared	-0.0006***	-0.0006***	-0.0006***	-0.0006***	-0.0006***	-0.0006***	-0.0006***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
T × % same ancestry as leader	-0.0001						
	(0.0005)						
T × % same county as leader		0.0000					
		(0.0005)					
T × % same ancestry soldiers			-0.0007				
			(0.0005)				
T × % same county soldiers				-0.0007			
				(0.0005)			
T × ln baseline desertions					0.0059***		
					(0.0007)		
T × ln battle deaths						-0.0007	
						(0.0005)	
T × Battle							-0.0007
							(0.0008)
Regiment FE	yes	yes	yes	yes	yes	yes	yes
Time FE	yes	yes	yes	yes	yes	yes	yes
Company controls	yes	yes	yes	yes	yes	yes	yes
Baseline desertions	yes	yes	yes	yes	yes	yes	yes
Battle controls	yes	yes	yes	yes	yes	yes	yes
Observations	167,172	167,172	167,172	167,172	167,172	167,172	167,172
Adj. R-squared	0.553	0.553	0.553	0.553	0.553	0.553	0.553

Note: Regressions of the leader fixed effect $\widehat{\kappa}_{c,s}$ for leader c in each 6-week spell s on captains' tenure and tenure squared in their current unit, as well as on interactions of tenure (T) and observable company characteristics. All regressions also include the main effects. The leader-spell fixed effect $\widehat{\kappa}_{c,s}$ is estimated by regressing the inverse hyperbolic sine (asinh) of desertions on leader indicators—as in equation (1)—using a company-week panel excluding weeks with battles, skirmishes, or any recorded battle deaths, and dividing this out-of-battle sample into 6-weeks intervals (spells s) during which the fixed effect is estimated. Company controls include the share of soldiers from the same county, the share of soldiers of the same ancestry, the share of soldiers from the same county as the captain, and the share of soldiers of the same ancestry as the captain; the baseline desertion rate in the current leader's first spell; and battle controls including the lags of the asinh of battle deaths, indicators for any battle, a top-19 battle, or a skirmish. Observations are weighted by the lagged number of soldiers in the company. All regressions include regiment fixed effects and fixed effects for the starting week of each leader spell. Standard errors are clustered at the level of the captain, and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

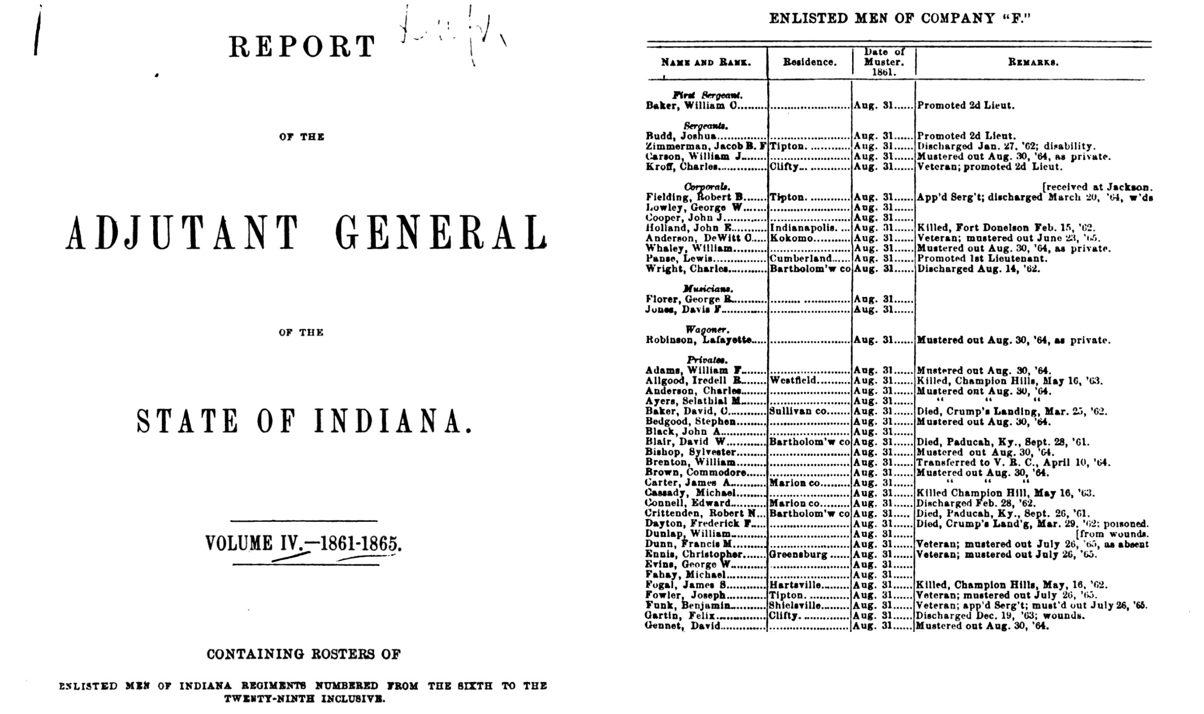
Figure A.1: Explanatory Power of Leader versus Unit Fixed Effects



Note: This figure plots the *adjusted R-squared* from regressions of the inverse hyperbolic sine of desertions on either the fixed effects for (i) the leader at a given unit level or (ii) unit fixed effects, using a company-week panel for 8,388 companies for the duration of the war (1,171,704 observations). We usually cannot include both unit and leader fixed effects because leaders rarely changed units within the same level of hierarchy (e.g., fewer than two percent of captains served as leaders of more than one company). The adjusted R-squared using leader or unit fixed effects at each level of hierarchy are displayed next to each other. The first two bars do not include any fixed effects and are intercept-only models, serving as benchmark. For a description of the organizational hierarchy of the Union Army, see Figure 1. All regressions include week fixed effects to absorb aggregate time trends in desertions.

Figure A.2: Example Pages from the Adjutant General's Report

(a) Adjutant General Report Cover, Indiana **(b)** Roster of Company F, 11th Indiana Infantry Regiment



REPORT

OF THE

ADJUTANT GENERAL

OF THE

STATE OF INDIANA.

VOLUME IV.—1861-1865.

CONTAINING ROSTERS OF

ENLISTED MEN OF INDIANA REGIMENTS NUMBERED FROM THE SIXTH TO THE TWENTY-NINTH INCLUSIVE.

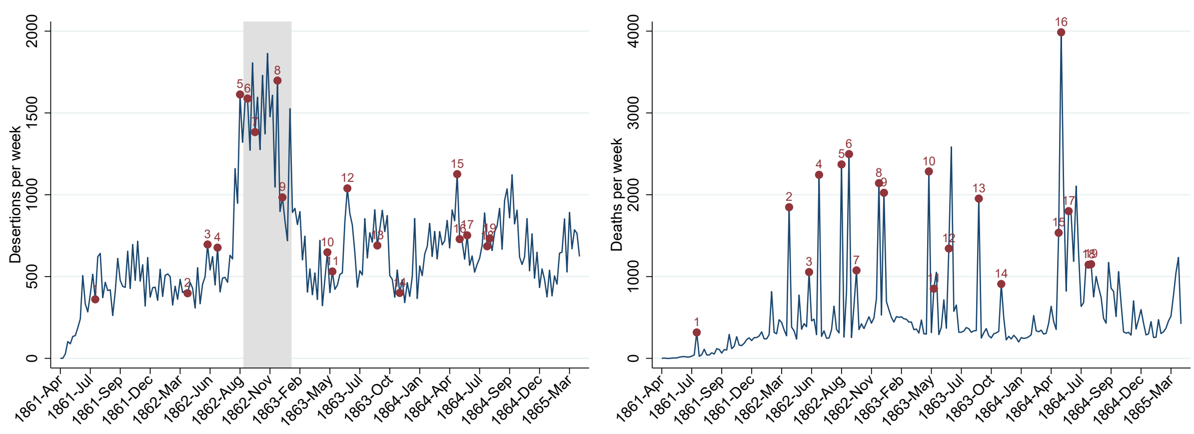
ENLISTED MEN OF COMPANY "F."

NAME AND RANK.	Residence.	Date of Muster. 1861.	REMARKS.
First Sergeant.			
Baker, William C		Aug. 31	Promoted 2d Lieut.
Sergeants.			
Budd, Joshua		Aug. 31	Promoted 2d Lieut.
Zimmerman, Jacob B. F	Tipton.	Aug. 31	Discharged Jan. 27, '62; disability.
Carson, William J		Aug. 31	Mustered out Aug. 30, '64, as private.
Kroff, Charles	Clifty	Aug. 31	Veteran; promoted 2d Lieut.
Corporals.			
Fielding, Robert B	Tipton.	Aug. 31	App'd Serg't; discharged March 20, '64, w'ds [received at Jackson.
Lowley, George W		Aug. 31	
Cooper, John J		Aug. 31	
Holland, John E	Indianapolis.	Aug. 31	Killed, Fort Donelson Feb. 15, '62.
Anderson, DeWitt C	Kokomo	Aug. 31	Veteran; mustered out June 23, '65.
Whaley, William		Aug. 31	Mustered out Aug. 30, '64, as private.
Panse, Lewis	Cumberland	Aug. 31	Promoted 1st Lieutenant.
Wright, Charles	Bartholom'w co	Aug. 31	Discharged Aug. 14, '62.
Musicians.			
Florer, George R		Aug. 31	
Jones, Davis F		Aug. 31	
Wagoner.			
Robinson, Lafayette		Aug. 31	Mustered out Aug. 30, '64, as private.
Privates.			
Adams, William F		Aug. 31	Mustered out Aug. 30, '64.
Allgood, Iredell B	Westfield	Aug. 31	Killed, Champion Hills, May 16, '63.
Anderson, Charles		Aug. 31	Mustered out Aug. 30, '64.
Ayers, Selathial M		Aug. 31	" " "
Baker, David, C	Sullivan co	Aug. 31	Died, Crump's Landing, Mar. 25, '62.
Bedgood, Stephen		Aug. 31	Mustered out Aug. 30, '64.
Black, John A		Aug. 31	
Blair, David W	Bartholom'w co	Aug. 31	Died, Paducah, Ky., Sept. 28, '61.
Bishop, Sylvester		Aug. 31	Mustered out Aug. 30, '64.
Brenton, William		Aug. 31	Transferred to V. R. C., April 10, '64.
Brown, Commodore		Aug. 31	Mustered out Aug. 30, '64.
Carter, James A	Marion co	Aug. 31	" " "
Cassady, Michael		Aug. 31	Killed Champion Hill, May 16, '63.
Connell, Edward	Marion co	Aug. 31	Discharged Feb. 28, '62.
Crittenden, Robert N	Bartholom'w co	Aug. 31	Died, Paducah, Ky., Sept. 26, '61.
Dayton, Frederick F		Aug. 31	Died, Crump's Land'g, Mar. 29, '62; poisoned.
Dunlap, William		Aug. 31	[from wounds.
Dunn, Francis M		Aug. 31	Veteran; mustered out July 26, '65, as absent
Ennis, Christopher	Greensburg	Aug. 31	Veteran; mustered out July 26, '65.
Evins, George W		Aug. 31	
Fahay, Michael		Aug. 31	
Fogal, James S	Hartsville	Aug. 31	Killed, Champion Hills, May, 16, '62.
Fowler, Joseph	Tipton.	Aug. 31	Veteran; mustered out July 26, '65.
Funk, Benjamin	Shielsville	Aug. 31	Veteran; app'd Serg't; must'd out July 26, '65.
Gartin, Felix	Clifty.	Aug. 31	Discharged Dec. 19, '63; wounds.
Gennet, David		Aug. 31	Mustered out Aug. 30, '64.

Note: Example pages of the Indiana Adjutant General's Report published after the Civil War in 1866 by Samuel M. Douglass, State Printer, Indianapolis, Indiana. Panel a shows the cover page of the report. Panel b provides an example of the individual-level soldier rosters for each regiment and company. Within units, soldiers were sorted by rank and then by surname, first name, and middle initial. This particular example shows the name of each soldier, their county of residence, date of muster, and additional information under "Remarks," including deaths, promotions, injuries, desertions, and the muster-out date and age, among others. Source: https://archive.org/ and https://www.hathitrust.org/.

Figure A.3: Desertions and Battle Deaths by Week

(a) Desertions **(b)** Battle Deaths



Note: Number of deserted soldiers (panel a) and soldiers killed in battle (panel b) by week from April 1861 to April 1865. Red markers label weeks with major battles from a list of the 19 most important battles, taken from Selcer (2006). The battles are: 1. First Bull Run, 2. Shiloh, 3. Seven Pines, 4. Seven Days Battle, 5. Second Bull Run, 6. Antietam, 7. Perryville, 8. Fredericksburg, 9. Stone's River, 10. Chancellorsville, 11. Vicksburg, 12. Gettysburg, 13. Chickamauga, 14. Chattanooga, 15. Wilderness, 16. Spotsylvania Courthouse, 17. Cold Harbor, 18. Atlanta, 19. Petersburg. The gray-shaded area in the top figure marks the low-morale period from September 1862 to January 1863, when discussions about the Emancipation Proclamation deteriorated morale.

Figure A.4: Battle Participation of the 55th Pennsylvania Infantry (Fox, 1889)

FIFTY-FIFTH PENNSYLVANIA INFANTRY.

WHITE'S BRIGADE — AMES'S DIVISION — TENTH CORPS.

COL. RICHARD WHITE.

Companies.	Killed and Died of Wounds.			Died of Disease, Accidents, In Prison, &c.			Total Enrollment.
	Officers.	Men.	Total.	Officers.	Men.	Total.	
Field and Staff	.	..	..	1	1	2	19
Company A	.	22	22	.	37	37	195
B	.	22	22	1	30	31	179
C	.	15	15	.	31	31	158
D	1	15	16	.	28	28	166
E	1	18	19	.	22	22	168
F	2	18	20	.	25	25	170
G	.	22	22	1	21	22	175
H	.	15	15	.	30	30	163
I	2	28	30	.	20	20	167
K	1	26	27	.	23	23	198
Totals	7	201	208	3	268	271	1,758

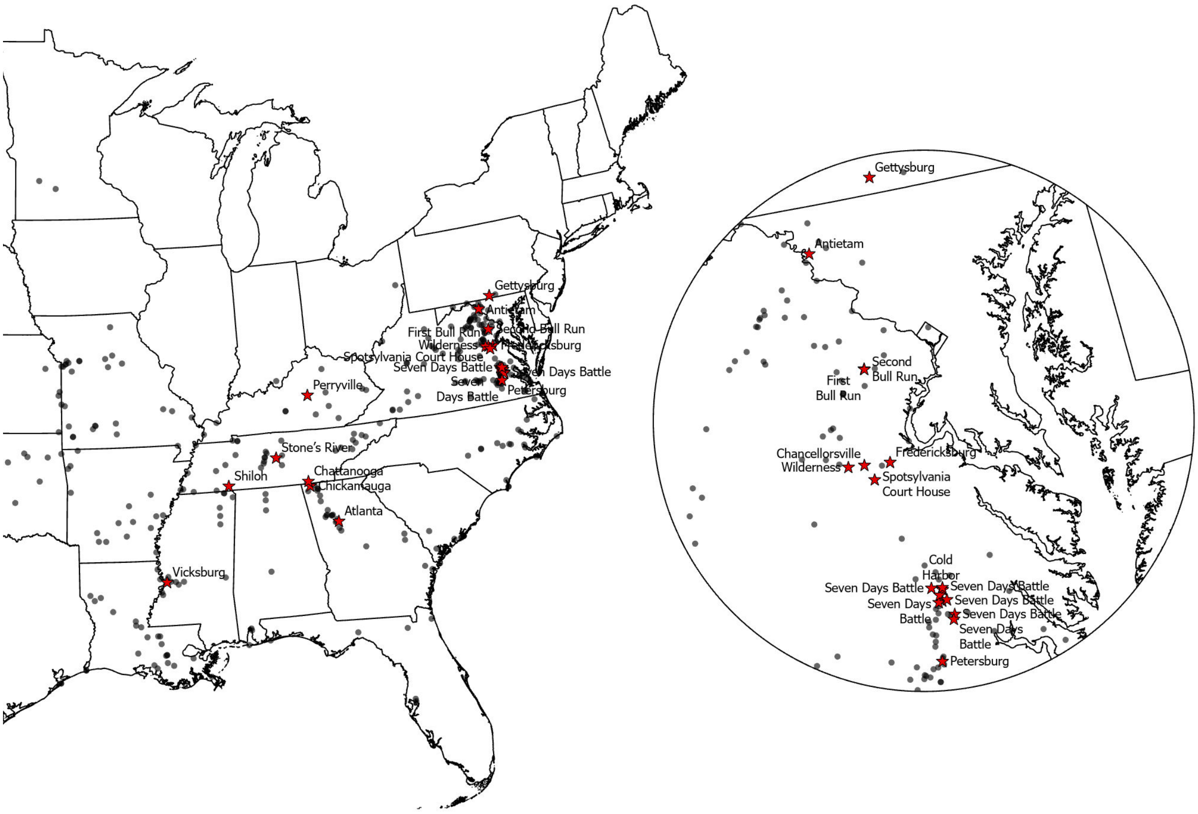
208 killed = 11.8 per cent.

Total killed and wounded, 782; died in Confederate prisons (previously included), 96.

Battles.	K. & M.W.
Edisto Island, S. C.	2
Pocotaligo, S. C.	6
Swift Creek, Va.	6
Drewry's Bluff, Va.	58
Bermuda Hundred, Va.	10
Cold Harbor, Va.	41
Picket, S. C., March 29, 1862	1
Petersburg, Va. (assault, June 15, 1864)	46
Petersburg Trenches, Va.	12
Chaffin's Farm, Va.	15
Hatcher's Run, Va., March 30, 1865	6
Fall of Petersburg, Va.	2
Rice's Station, Va.	1
Place unknown	2

Note: Example page from Fox (1889), showing the cumulative losses for each company in the 55th Pennsylvania Infantry regiment during the war, all battles fought by this regiment, and the associated losses sustained in each battle and engagement. Battle statistics aggregate killed, missing, and wounded ("K.&M.W.") soldiers. The example also shows the regimental leader and the associated higher-level units to which the regiment belonged, such as the brigade, division, and corps (see Figure 1 for a visualization of the organization chart of the Union Army provided by Dyer (1908)).

Figure A.5: Location of Major and Minor Civil War Battles



Note: This map depicts the location of all battles, with the 19 major ones listed in Table A.4 highlighted as red stars and other battles shown with gray dots. The list of the 19 major battles comes from Selcer (2006) and originally included 20 battles, though Fort Fisher was primarily a naval engagement and therefore was not included. Data used for this map come from Dyer (1908). The circle zooms in on the dense set of battles near the Chesapeake Bay area. The Seven Days Battle occurred at multiple locations, whereas the First and Second Bull Run occurred in the same location.

Figure A.6: Biography Examples

(a) Example Biography 1: Charles McCarthy

McCarthy, Charles, a prominent real estate and insurance man of Troy, N. Y., where he is known as one of the most prosperous and substantial business men of the city, was born in Essex county, N. Y., in 1832, but has lived in Troy ever since he was two years of age. Capt. McCarthy has a long and honorable record as a volunteer in the Civil war, and was known as a brave and highly efficient officer. On Sept. 28, 1862, he was commissioned captain of Company A, 175th New York volunteer infantry (Fifth regiment, Corcoran brigade), and served in that capacity until mustered out at Greenboro, Ga., Nov. 27, 1865. Capt. McCarthy was commissioned major, but was never mustered on his commission as such. He left the state with his command Nov. 21, 1862, and after a brief service at Suffolk, Va., embarked for Louisiana. Attached to Gooding's (3d) brigade, Emory's (3d) division, 19th corps, he participated in his first engagement at Fort Bisland, La., April 12-13, 1863; skirmished at Franklin May 25; and from May 30 to July 8 shared in all the work and dangers of the long siege of Port Hudson, including the assault of June 14. During the remainder of the year 1863, he was engaged with his command in the performance of post and garrison duty, the monotony of which was relieved by occasional skirmishes with the enemy. In the spring of 1864 attached to the 3d brigade of Grover's (2nd) division, 19th corps, he took part in Banks' Red River expedition, and was in the engagements at

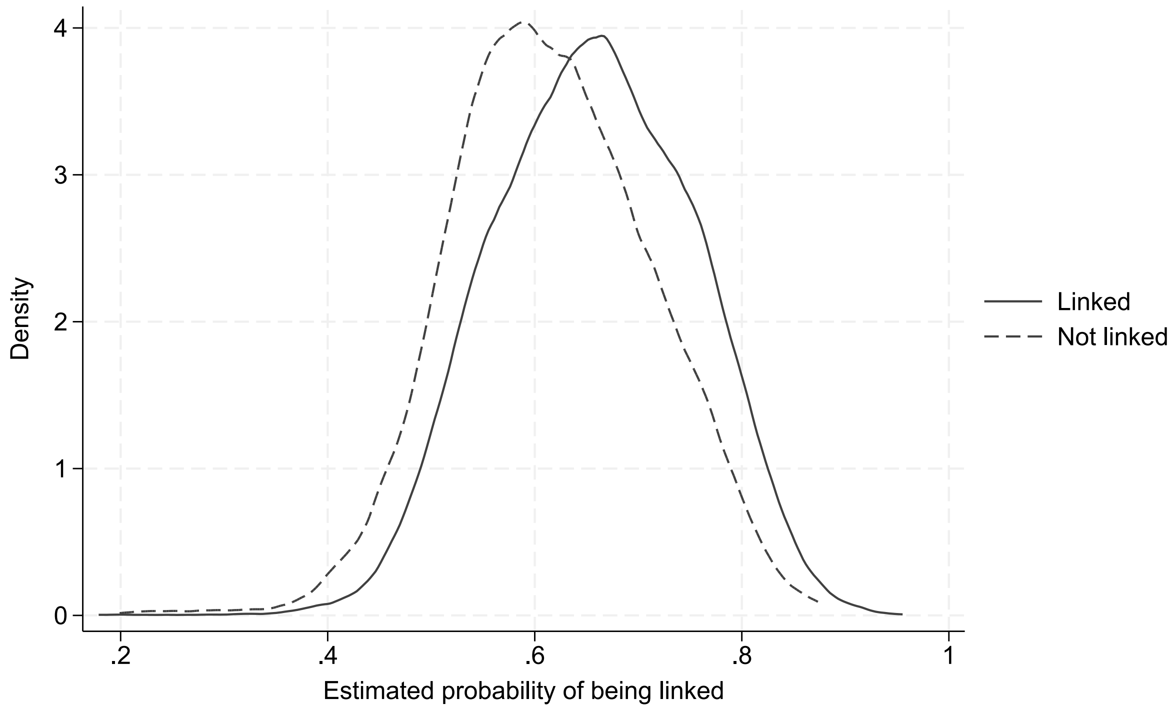
(b) Example Biography 2: James W. Johnston

Johnston, James W., a prominent contractor and business man of Utica, N. Y., has a long and distinguished career as a volunteer soldier in the War of the Rebellion. He was a brave and highly

efficient officer and on numerous occasions won the warm commendation of his superiors for gallantry in action. Going out as a private in 1861, he won his promotions through merit, and left the service in 1865 with the rank of captain and brevet major of volunteers. He was several times severely wounded in action, and it fell to his lot to participate in a large number of the great battles and campaigns of the war, as a member of the gallant and hard-fighting 6th army corps. Maj. Johnston first enlisted on Oct. 12, 1861, as a private in Company D, 53d New York volunteer infantry, Col. Lionel J. D'Epineuil commanding, and was honorably discharged with the regiment at Washington, D. C., March 21, 1862, as a private. As a member of

Note: Snippets from two example biographies, highlighting relevant search terms for the statistical analysis in Table 3. Source: Federal Publishing Company (1908) and ACWRD (2022).

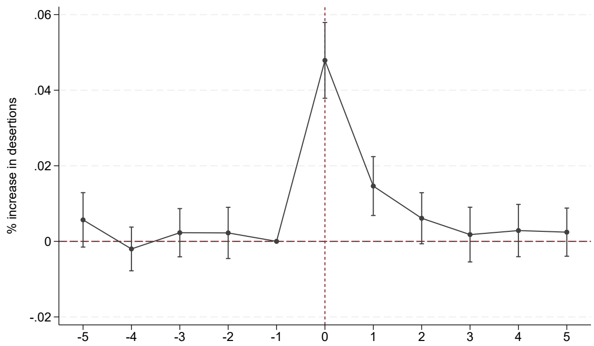
Figure A.7: Common Support for Linked and Non-Linked Captains



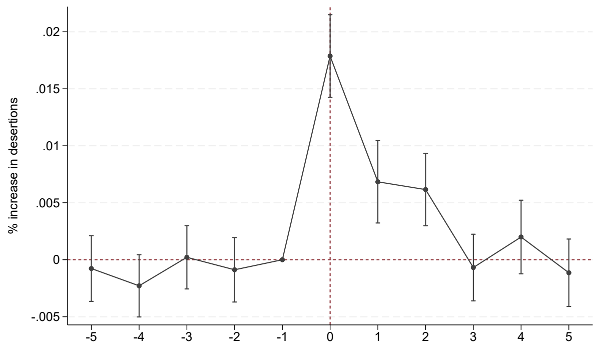
Note: This figure plots the density of predicted probabilities of being linked from the 1860 to the 1870 census using links provided by the Census Linking Project (Abramitzky et al., 2020), where we regressed an indicator for being linked on 1860 individual observable characteristics (inverse hyperbolic sine of occupational income score, inverse hyperbolic sine of personal and real estate wealth, labor force participation, skill and industry indicators, age, urban and farm status indicators, literacy, and state indicators). The sample included 8,039 captains with complete information who were linked from the military records to the 1860 census. Results from using the transformed probabilities in an inverse propensity score reweighted regression, as suggested by Bailey et al. (2020), to generate a representative sample of the underlying population of captains intended to be linked, are reported in Table A.8.

Figure A.8: Event Studies Robustness Checks

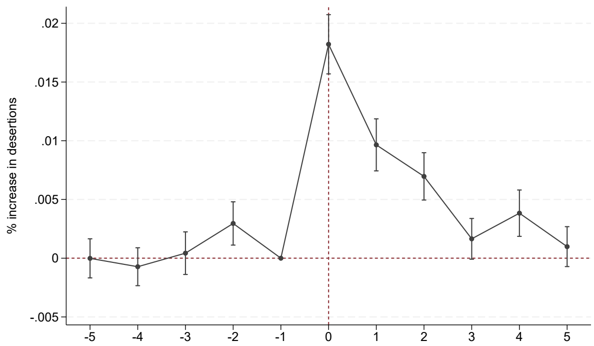
(a) TWFE Event Study Using Short Battles



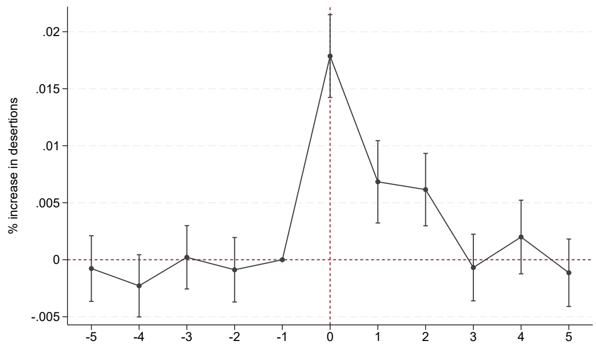
(b) Dube et al. (2023) Excl. Never Treated



(c) Sun and Abraham (2021) Event Study Estimator



(d) Sun and Abraham (2021) Excl. Never Treated



Note: Robustness checks for the results in Figure 3. Panel a estimates the baseline daily event study ($t = 0$ marking the day of the battle) using the standard two-way fixed effects estimator. The sample is restricted to short battles that lasted for two days or less. Panel b re-estimates the Dube et al. (2023) results from panel d in Figure 3, but excludes the never treated observations. Panel c estimates the event study on the full sample using the estimator by Sun and Abraham (2021), and repeats this exercise excluding the never treated observations in panel d. Standard errors are clustered at the company level. Error bars display 95 percent confidence intervals. The event study is estimated over the whole sample from $t = -7$ to $t = 7$, but only coefficients for $t = -5$ to $t = 5$ are reported to avoid potential end-point bias, as suggested by Baker et al. (2022).

B Constructing Alternative Leader Quality Measures

In our base specification in equation (1), we use a company-week sample in which we exclude all weeks with known battles, skirmishes, or reported battle deaths and regress the inverse hyperbolic sine of desertions in company c in week t on captain and colonel fixed effects, regiment and week fixed effects, and unit characteristics to estimate the leader fixed effects $\widehat{\kappa}_c$. To probe the robustness of our measure, we construct additional leader fixed effects using alternative approaches, which are described in more detail in this section. While these additional fixed effects seek to provide alternative quality measures, they also are inherently more noisy since they either use less data or data with a higher variance. Nonetheless, they provide us with an important way to show that our results do not hinge on a particular way of estimating the leader fixed effects which we show across a wide range of robustness checks in Table A.5. This table replicates all main results using the alternative fixed effects described below, as well as the percentile transformed versions of the main and alternative fixed effects. The percentile transformation helps with reducing the potential influence of outliers (e.g. Derenoncourt, 2022), and highlights that our results are robust to different ways of estimating the fixed effects but also to different data transformations.

Alternative Approach 1 - Estimating leader FE using PPML:
A main reason to estimate the leader fixed effects via OLS in equation (1) was the ease of interpretation. However, given the large number of zero desertions per company-week, this required us to transform the outcome using the inverse hyperbolic sine. To show that our results do not hinge on this transformation, we re-estimate the leader fixed effects without it and instead rely on Poisson pseudo-maximum likelihood regression using the same estimating equation as before but without transforming the outcome,

$$\text{desertions}_{ct} = \kappa_c + \kappa_r + \beta'\mathbf{X}_{ct} + \gamma_t + \varepsilon_{ct}, \tag{5}$$

Aside from the increased computational burden of estimating the fixed effects via PPML, the interpretation of the fixed effects also changes. While the inverse hyperbolic sine function approximates the log function for large values and behaves linearly near zero—thus accommodating zeros without dropping observations—the linear model still treats fixed effects as additive shifts in the transformed outcome. By contrast, the PPML model estimates fixed effects under a multiplicative functional form, modeling the outcome level directly and accounting more fully for heteroskedasticity. As a result, fixed effects from these two models may differ, particularly when zeros are prevalent or when unobserved heterogeneity interacts with the scale of the outcome. However, the qualitative rankings and relative comparisons across leaders remain largely consistent. Estimating the leader fixed effects using PPML therefore helps assess the robustness of results to functional form assumptions on the desertion variable.

Alternative Approach 2 - Using only pre-battle information:
This approach follows estimation of equation (1) using the company-week panel, however, we now exclude all weeks after the first battle has occurred. One concern with the baseline approach is that endogenous sorting potentially improves the leader fixed effect.

Consider two leaders, A and B, who serve as captains of their companies for ten weeks each, and that both leaders are of equal quality. Also assume that both companies have an equal share of soldiers with a high propensity to desert, and that these soldiers self-select into exiting the unit once the first battle occurs. Suppose that the first battle occurs in week three for leader A, and in week seven for leader B. Our current approach would attribute a better fixed effect to leader A because this leader will have more weeks with lower desertion rates after the high-propensity desertion soldiers exit the unit. The original company-week panel included over 1.2 million observations for 8,429 unique companies, while the panel that drops observations after the first battle under each leader only has around 422,000 observations which is only 35 percent of the original sample.

Using only weeks up until the first battle avoids such compositional effects and abstracts from potential learning effects on the leader's part. The big disadvantage is that estimation of the leader fixed effects on this restricted sample uses much less information which would increase the noise in the estimates when the leader fixed effects is a regressor.

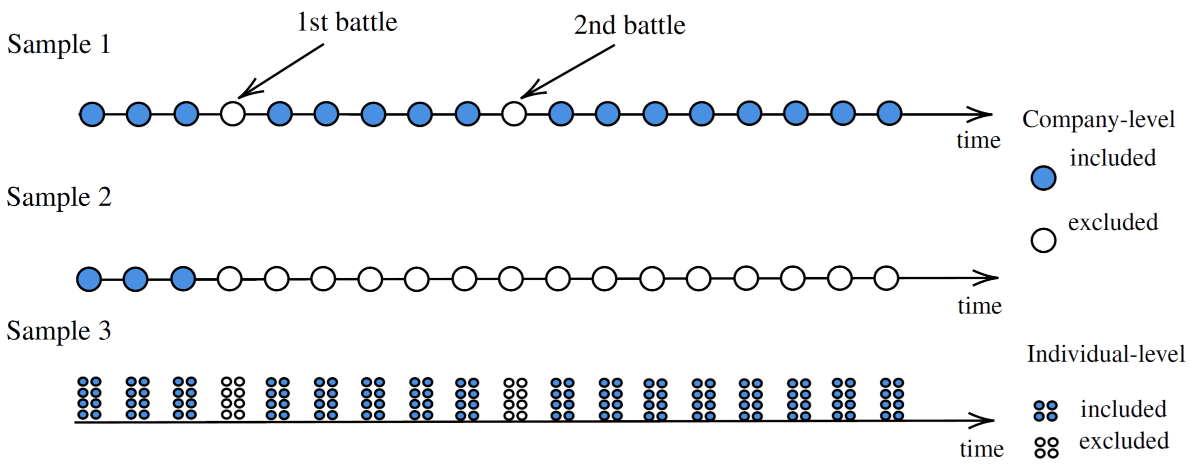
Alternative Approach 3 - Including company fixed effects:
When studying, for instance, the changes in the wages of workers, a common concern in the literature is how much of these changes can be attributed to individual unobserved characteristics and how much of it is driven by firm-specific unobservables (Abowd et al., 1999; Card et al., 2016). We follow this literature in trying to separating the quality of individual captains from unobserved unit characteristics by re-estimating equation (1) with additional inclusion of company fixed effects. As in the setting of Abowd et al. (1999), the identification of the fixed effect solely comes from workers who move firms, or here, captains who move across companies. While this type of horizontal movement is much more common in the labor literature when studying workers, it is much less common for captains in the Union Army. Since units were typically not replenished with new soldiers, movements of captains across companies are rare and only 2 percent of all captains move from one company to another. Vertical movements across the hierarchy are much more common, i.e., captains being promoted to majors, lieutenant colonels, or colonels - in which case they would not lead companies anymore but regiments. We nonetheless mirror the approach by Abowd et al. (1999) to show that the effect of captain fixed effects on their various outcomes is not driven by unobserved unit characteristics. Conceptually, this makes sense because unlike large companies with their own brand, reputation, and varying non-labor inputs for production, Union Army units only existed and were "productive" on the battle field due to the soldiers who served in these units as opposed to unobserved unit characteristics, such as possibly having better weapons or equipment.

Alternative Approach 4 - Estimating leader fixed effects using individual-level data:
The most stringent approach is to estimate the leader fixed effects based on individual-level desertion decisions similar to the teacher value-added literature (see Chetty et al., 2014). This literature regresses student outcomes on individual-level student characteristics and indicators for their teachers, where the coefficients on the teacher fixed effects are a proxy for the quality of a teacher. Likewise, we regress

$$\Pr(\text{deserted})_{icrt} = \kappa_c + \phi_r + \beta X'_{icrt} + \gamma_t + \varepsilon_{icrt} \tag{6}$$

where the outcome is an indicator for whether soldier i serving under captain c in regiment r deserted during captain c's leadership of the company which started in week t. As before, the object of interest to be estimated are the captain fixed effects $\widehat{\kappa}_c$. Similar to the previous approaches to estimating the leader fixed effects, we drop soldiers who deserted during weeks with recorded battles or battle deaths. Unlike the previous two approaches that use the company-week panel data, however, we cannot estimate the

Figure B.1: Sample Structure for Estimating Alternative Leader Fixed Effects



Note: This figure illustrates the sampling schemes we use to estimate alternative leader fixed effects. Samples 1 and 2 are at the company-week level, for which we see a given leader in a given company over time. The hollow circles are weeks that were excluded because they included any kind of battle, skirmishes, or recorded battle deaths. The blue-filled circles are in the estimation sample. Sample 1 excludes all weeks with battles or battle deaths, whereas sample 2 excludes all weeks after the first battle under a given leader has occurred. Sample 3 considers the same leader over the same time periods but now uses desertion information from the individual soldiers who served in the unit of the particular leader. All of the information is then collapsed to that leader's leadership spell which is the final sample on which the fixed effect is estimated and for which the regression equation is formulated in equation (6). As in sample 1, weeks with battles or recorded battle deaths are excluded. The advantage of sample 3 is that it allows us to make use of individual soldier and 1860 census characteristics to control for a richer set of soldier characteristics.

regression in an individual-week panel as this would create too large a data set to handle. It would also contain mostly zeroes in the outcome variable. We therefore constructed a soldier-spell panel where we observe each soldier under each captain the soldier served for during their time in the Union Army. So if a soldier served in three different captain spells, we will observe this soldier three times in the data set.

We use the set of regular soldiers who were successfully linked to the 1860, yielding a sample of 761,811 individuals, which allows us to control for important pre-war characteristics of the soldiers.[46] The vector X_{icrt} includes controls for soldiers' 1860 characteristics from the census such as their age, age squared, indicators for occupational skill and industry group,[47] the asinh of their personal and real estate wealth, asinh occupational income score, labor force participation, indicators for literacy, as well as for state of residence and place of birth. We also control for regiment fixed effect ϕ_r, and fixed effects for the week (t) when captain c started their command of the company.

Comparison of the different approaches:

The data structure for our main approach, and approaches 2 and 4 is illustrated in Figure B.1.[48] The main method used in the paper corresponds to sample 1 in the figure, followed by the two other approaches, respectively. For the individual-level data, the time dimension is a leader-spell but highlighting that individuals were dropped if they deserted during weeks of battle.

[46] Regular soldiers here refers to those who were not in an officer or sergeant rank. The majority of soldiers held the rank of private.

[47] These include nine occupational skill groups (professional/technical, managers/officials/proprietors, clerical/kindred workers, sales workers, craftsmen, operatives, service workers, farm laborers, and laborers) and 13 industry groups (farm, mining, construction, durable and non-durable manufacturing, transportation, telecommunication, utilities, wholesale, retail, finance, services, public administration)

[48] Approach 3 is the same as our main method to estimate the leader fixed effects in terms of the data structure. The only difference is the inclusion of company fixed effects.