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The U.S. Civil War's Impact on Women's Work and Political Participation*

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Abstract

This paper studies the role of economic participation via the labor market in enabling the political mobilization of an underrepresented group. Specifically, we study the wives and daughters of disabled Union Army soldiers after the U.S. Civil War. Linking Union Army enlistment records to the 1860 and 1870 U.S. censuses, we find that the wives and daughters of disabled veterans were significantly more likely to participate in the labor force than those of non-disabled veterans. Historical evidence suggests that disabled veterans were also more exposed to postwar alcohol and substance abuse, increasing the household burdens faced by women. Town-level data show that increases in women's labor force participation combined with higher shares of disabled veterans predict more Temperance Crusade activity in 1873–74. Information provision via newspapers and proximity to other protest towns amplify these effects. Using unit-level disability rates as an instrument for veterans' disability status supports a causal interpretation of the labor market effects. Our results suggest that labor force participation can be an important enabling factor for the political mobilization of underrepresented groups.

Keywords: U.S. Civil War, Female Labor Force Participation, Temperance, Political Activism
JEL Codes: N31, J15, J18, D72

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1 Introduction

Participation in the political process is a crucial mechanism through which citizens can influence policy and the distribution of a society's resources. In the absence of formal or informal enfranchisement, collective action via grassroots political mobilization has been key to gaining this ability for many disenfranchised groups. Examples include the women's suffrage movement in the U.S. (Doepke and Tertilt, 2009; Moehling and Thomasson, 2020), the Civil Rights Movement (Calderon et al., 2023), or the Arab Spring (Campante and Chor, 2012), among others. Such movements can be effective policy shifters even in fully enfranchised and functional democracies (Gethin and Pons, 2024). An important question, therefore, is what role economic factors and institutions play in enabling successful political mobilization.

In this paper, we focus on the labor market as an institution and a potential enabling factor of political mobilization of a politically excluded group. Whether economic participation leads to increased political participation is an interesting empirical question, as the reverse may also hold.[1] To answer this question in a causal way, we study the relationship between the U.S. Civil War and women's postbellum participation in both the labor force and the temperance movement. After the war, around half a million men returned home with wounds and disabilities that impacted their ability to work (Skocpol, 1993). Our first hypothesis is that the wives and daughters of disabled veterans were more likely to work outside their own household in 1870 relative to comparable wives and daughters of non-disabled veterans. Aside from disabled soldiers' reduced capacity to earn an income, they also struggled with pain management. This led to a steep rise in alcoholism in the years after the war (Achenbaum et al., 1993; Lewy, 2014), with a corresponding increase in cases of domestic violence (Carroll, 2016). The spread of alcoholism ultimately led to the Temperance Crusades in 1873–74, which started in Ohio and quickly spread across the Midwest and Northeast. These protests gave rise to the Woman's Christian Temperance Union, the first large-scale, women-led political organization in the U.S. (Bordin, 1981). The second point of our hypothesis is that Temperance Crusade protests were significantly more likely in towns where a larger share of women had entered the labor market by 1870 due to higher disability rates among returning Union Army veterans, thus establishing the link between economic and political participation.

To test these hypotheses empirically, we link the universe of the 2.2 million Union Army soldiers to the 1860 census and subsequently track them to the 1870 census using the crosswalks by the Census Tree Project (Price et al., 2021). We then observe around 300,000 veterans in 1870 who were of working age and married, a fifth of whom also had working-aged daughters. The military records provide us with information on whether a veteran was discharged with a disability. The census allows us to observe a broad array of pre- and postwar veteran characteristics and labor market outcomes, which we also observe for their wives and daughters in 1870. The historical nature of the data is not without its drawbacks. One issue is that women's work at the time was not well-enumerated. We therefore take a conservative approach and treat women as working if they reported an occupation in the 1870 census, meaning that they had to work for a wage outside of their own household.[2] Other papers have assigned

[1] For instance, Slotwinski and Stutzer (2023) show that the enfranchisement of women in Switzerland resulted in more women entering the labor market and increased human capital accumulation. Conversely, Doepke and Tertilt (2009) provide a model in which technological change increases the importance of human capital, leading men to relinquish voting power to women.

[2] Only the occupations of adult men were enumerated. Women and minor male family members were assumed to not work,

women's employment status in the same way (e.g. Rashid, 2025).

We first show with a simple regression analysis that disabled veterans had significantly lower occupational income scores and were more likely to not work compared to non-disabled veterans. This holds while accounting for a wide range of prewar observable veteran characteristics,[3] county, and military unit fixed effects. The results reveal considerable heterogeneity in treatment effects, with semi-skilled and younger veterans suffering the largest income score declines of around 7 percent. While we mostly observe postwar disability status as a binary variable, we have information on amputations for a small subset of around 2,000 soldiers. Matching them to non-disabled control veterans on a broad range of prewar characteristics, we find much larger negative effects of amputations on affected veterans' occupational income scores. On average, amputees had 14 percent lower income scores compared to observationally equivalent control veterans. Amputations of the left and right arm were associated with a 15.3 and 27 percent lower income score, respectively. This is consistent with a higher share of right-handed individuals in the population, and comparable to effect sizes found in studies on modern-day disability wage penalties (Reville and Schoeni, 2001; Dworski and Powell, 2022).

After establishing the significant earnings losses suffered by disabled veterans, we study the implications for the labor force participation of their wives and daughters. We only compare women in households with veteran household heads to abstract from selection into service. Regression results show that the wives and daughters of disabled veterans were 0.55 percentage points more likely to work. While this seems small, the outcome mean is also low at only 3.9 percent considering that most women in the sample are wives.[4] To put this into perspective, even after accounting for the underenumeration of women's work, women's labor force participation rose by 0.45 percentage points between 1870 and 1900 (Boustan and Collins, 2014). Our results are mainly driven by younger wives and daughters, and by households in which the veteran left the labor force entirely. Family structure, including other adult male family members or those who could provide help with childcare, such as grandparents, have the expected sign but are not statistically significant. We also find no heterogeneity between rural and urban households, or foreign-born individuals.

Our regression coefficient is remarkably robust and persists after including county fixed effects, prewar observables, post-double selection (Belloni et al., 2014), tests for selection on unobservables (Oster, 2019), and other robustness checks that include different geographic fixed effects, weighting schemes, different clustering of standard errors or adjustments for spatial autocorrelation (Bester et al., 2011), excluding states one-by-one, or spillover effects by controlling for the county-level share of disabled soldiers. We also rule out that the control group is partially treated, not by physical disabilities but mental ones, such as PTSD.[5] When only looking at soldiers without physical disabilities, and if participation in the largest battles of the war is a proxy for trauma, we find no impact of the number of large battles fought by the veteran on their own labor market outcomes or on those of their female

even if they were actually working in the family business or family farm. Only wage work in a different household or business was recorded otherwise.

[3]These include age, age-squared, number of own children, the inverse hyperbolic sine (asinh) of the veteran's occupational income score, the asinh of their personal and real estate wealth, as well as indicators for birthplace, skill and industry groups based on the 1950 Census Bureau definitions, school attendance in 1860, literacy, urban residence, farm status, group quarter residence, and labor force participation status.

[4]The social convention at the time was that women did not work outside the household upon marriage or childbirth at the latest.

[5]We also study the effect of deceased veterans on the female household members' labor market participation with significantly larger effect sizes, consistent with work in other settings such as Boehnke and Gay (2022).

household members.[6] We also account for selection into the sample. For a veteran to be observed — and therefore their wives and daughters — they had to i) survive until the census was enumerated in 1870 and ii) be able to marry. Both of these conditions are potentially affected by a soldier's disability status. If the veterans with the worst disabilities did not live until 1870 or could not marry, then our results will likely underestimate the true effect. We provide several exercises to argue that such concerns are warranted but empirically small in magnitude, including balancing tests of the disability variable and instrument by census linking status, and propensity score reweighting.

We further support a causal interpretation of these results by employing an instrumental variables approach. To generate an instrument for a given soldier, we compute the share of other soldiers who served at the same time and who left service with a disability. We exclude the veteran himself and his company from this computation, only exploiting variation in disability exits in the rest of his regiment.[7] Such leave-one-out style instruments leveraging unit-level risk have been used in past studies on the U.S. Civil War (Lee, 2005; Costa and Kahn, 2007b; Dupraz and Ferrara, 2025) and in modern settings (Lyle, 2006; Bruhn et al., 2024). The idea is that, conditional on regiment-level observables that capture socioeconomic composition, unit-level disability risk is determined by military strategy and chance, and therefore should not have a direct effect on the future labor market outcomes of veterans' wives and daughters other than through veterans' disability status.

The instrumental variables results estimate a positive and significant impact of veterans' disability status on the labor force participation of their wives and daughters in 1870, raising their likelihood of working by around 1.5 percentage points. This is about three times the magnitude of the corresponding OLS results. We unpack this difference in effect sizes by arguing that the instrument, a measure of unit-level disability risk, is capturing the impacts of larger battles. Using battle-level data, we show that larger battles are associated with higher rates of disability exits among Union Army regiments, especially if the Confederacy fielded more artillery units. The amputation data also show that larger battles lead to disabilities with larger earnings penalties for soldiers. In this case, the compliers, i.e., those soldiers who got disabled because they served in a high-risk unit and otherwise would not have become disabled, would have worse labor market outcomes leading to stronger effects on the labor market participation of their wives and daughters. This potentially explains the larger effect size of the IV versus the OLS estimates. When using assignment to large battles as instrument, the effect sizes become even larger, in line with the previous explanation. Just as the OLS results, the IV results are also robust to alternative standard error clustering, excluding states one-by-one, spillover effects from overall disability rates at the county level, inverse propensity score reweighting to account for potential sample selection, and when using a matched sample.

We then study the relationship between the veteran disability-induced increase in women's economic participation and women's political participation. Using town-level information on the occurrence of Temperance Crusade protests in 1873–74, we build a town-level data set using the 1870 census with the crosswalks provided by the Census Place Project (Berkes et al., 2023). We regress the protest indicator on the share of disabled veterans, controlling for town-level observables in 1870, such as the shares of

[6] We also use alternative measures, including the share of other soldiers in a veteran's unit who were killed, which yields the same results.

[7] A typical regiment was composed of a thousand soldiers, subdivided into ten companies of one hundred soldiers each. Since enlistment happened locally, many soldiers in the same company tended to also come from the same town or county (see Costa and Kahn, 2003).

Black and foreign-born populations, log population size and its square, employment share in agriculture, real estate and personal wealth per capita, the share of women, churches per capita, the share of working women in 1860, indicators for connection to the railroad and telegraph networks, and 1-by-1-degree grid cell fixed effects, which is also our clustering unit.[8] Results show a positive and significant effect of the share of disabled veterans on Temperance Crusade protests both in the OLS and IV regressions, where the instrument is the share of soldiers in high-risk units, i.e., the town-level equivalent to the individual-level instrument.

Relating this reduced form effect of disabled veterans on anti-alcohol protests back to women's labor force participation, we find that i) the share of disabled veterans predicts women's work in 1870 but not in 1860, and that ii) the share of working women in 1870 predicts Temperance Crusade activity while female labor force participation in 1860 has no effect. This indicates that economic participation can be a significant enabling factor of political participation for disenfranchised groups. However, we also show that incentives matter. The relationship between Temperance Crusade activity and women's labor force participation in 1870 only exists for towns with disabled soldiers while there is no effect in towns without disabled soldiers. Disabled soldiers were not the only ones prone to alcohol addiction. We show that soldiers exposed to large battles, as proxy for potential PTSD, also had a significant impact on crusade activity but without affecting women's work. We find no significant interaction between the disability and PTSD channel, meaning that these two war-related shocks appear to operate independently of each other. Only the share of disabled soldiers impacts both women's work and participation in the temperance crusade, and causal mediation analysis suggests that the effect of the share of disabled soldiers on temperance activity is entirely mediated through the female labor force participation channel.[9]

In terms of mechanisms, we test two types of explanations. The first relates to labor-market factors that could enable political participation. These include women's share in household income computed from the 1870 census, and the share of women working in the same occupations as proxy for potential mobilization via workplace contact. At the town-level, we find that a higher average ratio of women's occupational income relative to that of the veteran household head is a strong predictor of Temperance Crusade activity, and the share of disabled veterans predicts a higher female-to-male income ratio. We find no relationship between female workplace concentration and the share of disabled soldiers, and a small negative effect on Temperance Crusade activity.

The second set of mechanisms relates to incentives and communication mechanisms that can promote coordination (García-Jimeno et al., 2022). We find significant effects on Temperance Crusade protests from interaction effects between the share of disabled soldiers and the share of bartenders working in a town, highlighting the relationship to liquor as motivating factor. Network effects and information transmission also played an important role. Interactions with an indicator for being within 20 miles of other protest towns and the number of newspapers per capita also had significant impacts. While railroad and telegraph connections significantly predicted Temperance Crusades, they had no relationship with the share of disabled soldiers. Overall, these results support previous findings on the importance of information transmission and coordination channels for political mobilization (Chwe, 1999; Manacorda and Tesei, 2020).

Our paper contributes to the political economy literature that studies the determinants and conse-

[8] We show robustness to alternative grid cell sizes and clustering at the county-level.

[9] See Figure A.1 for a graphical representation of the conceptual framework of how different war-related shocks impact either or both the probability of women to participate in the labor market and in the Temperance Crusades.

quences of political mobilization Chwe (1999); Gethin and Pons (2024), and provides an empirical test for theories of political power sharing between groups (Acemoglu and Robinson, 2000; Lizzeri and Persico, 2004; Doepke and Tertilt, 2009). In particular, we provide a new perspective on the role of the labor market as an institution in enabling mobilization and political participation of a previously underrepresented group. In her Nobel lecture, Goldin (2024) noted that the "movement of women from the home to the market provides what is probably the most important change in the history of the labor force in many nations". Our findings support this view by highlighting the additional causal link between market participation and political participation.

We complement prior work studying the changes in American women's socioeconomic and political status during the 19th and early 20th centuries which has focused on legislation to protect women's earnings (Geddes and Lueck, 2002; Geddes et al., 2012), the expansion of women's education (Goldin et al., 2006; Goldin and Katz, 2011), gender roles at the frontier (Bazzi et al., 2024), the suffrage movement (Moehling and Thomasson, 2020; Calder, 2025), changes in sex ratios (Braun and Kvasnicka, 2013), and the role of information networks in mobilizing women (García-Jimeno et al., 2022). From a bigger picture perspective, the increase in women's economic and political participation has been shown to be important for intergenerational transmission of economic mobility (Olivetti and Paserman, 2015) and men's attitudes towards women's work (Fernández et al., 2004).

We also contribute to a growing literature that studies the unforeseen consequences of the U.S. Civil War. The war has been related to politically motivated migration (Eli et al., 2018; Bazzi et al., 2023), intergenerational labor market and health outcomes (Costa et al., 2018, 2020; Ager et al., 2021; Dupraz and Ferrara, 2025), racial animus (Masera et al., 2024), marriage markets (Salisbury, 2017), or social networks (Costa and Kahn, 2003, 2007b; Ferrara et al., 2025).[10] We provide new insights into its consequences for veterans and their wives and daughters after the war. Building on the historical literature on postwar alcohol and substance abuse (Carroll, 2016; Jones, 2020) and a modern literature that highlights the associated issues of domestic violence in households with affected veterans (Brady et al., 2009; Jacks, 2020), we provide empirical evidence on the link between disabled veterans, the need for their wives and daughters to enter the labor market, and how economic participation together with the incentive to mobilize against alcohol helped spark the Temperance Crusades of 1873–74. To the best of our knowledge, ours is the first study to empirically link the Civil War directly to changes in women's work and participation in the temperance movement as the first large-scale, women-led political movement in the U.S.

2 Historical Background and Data

2.1 The Civil War, Postwar Addiction among Veterans, and Temperance Activism

Although temperance organizations existed during the antebellum period, women typically participated in men-led organizations, which dominated the movement at the time (Tyrrell, 1982). Mixed-gender organizations did not allow women to hold office, vote, or speak at meetings (Dannenbaum, 1981).[11] The antebellum temperance movement was ultimately overshadowed by the outbreak of the Civil War,

[10] For a more general overview of how wars impact the labor market outcomes for women in the U.S. in historical perspective see Ferrara (2025).

[11] For example, when Susan B. Anthony tried to speak at a Sons of Temperance meeting in 1852, she was told that "the sisters were not invited there to speak, but to listen and learn" (Dannenbaum, 1981, p. 240).

and most early temperance organizations were not revived after the conflict.[12] The war itself, however, intensified alcoholism as a societal problem on a new scale. Over the course of four years, the U.S. Civil War (1861–65) pitted 2.2 million Union Army soldiers against 750,000 to 1.2 million Confederate soldiers in more than 300 battles and 8,000 skirmishes (Dyer, 1908). America's bloodiest conflict killed an estimated 750,000 soldiers (Hacker, 2011), and the Union Army discharged around half a million young men with wounds or disabilities suffered during the conflict (Skocpol, 1993). With a total population of only 31 million in 1860, this meant that a significant share of prime-aged men in the U.S. had died or returned with lasting impairments.

Before the advent of modern pain medication, returning veterans most commonly managed pain by consuming alcohol and opioids.[13] Already during the war, both types of drugs were distributed to soldiers in significant quantities (Courtwright, 2001). Bever (2014) documents that soldiers were issued whiskey for picket or sentry duties, and outside of battle, soldiers spent downtime drinking, gambling, and using drugs. Even if soldiers tried to abstain, the Union Army relied heavily on opioids and alcohol for medicinal purposes, consuming over 10 million opium pills and 80 tons of powdered opium (Lewy, 2014). Alcohol and opioids were administered even for minor injuries, including stomach issues and diarrhea, to calm soldiers' nerves prior to battle, and as malaria prevention. The invention and spread of the hypodermic syringe in the 1850s made treatments with opioids cheap and easy, leading to an overuse by doctors (Courtwright, 1978). A Union Army surgeon bemoaned after the battle of Antietam "that the only medications in his possession were morphine and brandy" (Lewy, 2014, p. 103).

Alcohol and opioid addiction tended to reinforce each other after the war, a phenomenon that was extensively studied by physicians at the time. Hubbard (1881) describes the following patterns of abuse among his patients: "Liquor is taken under a delusive idea of self-defense. [...] In their desperate efforts to sustain the system with opium, on the one hand, and to allay dangerous symptoms of narcotism by liquor, on the other, victims of the habit greatly increase the amount taken of both stimulants" (p. 13). Veterans who had sustained wounds or disabilities were most vulnerable to addiction (Lewy, 2014),[14] an issue recognized by both the public and the press. In 1868, a reporter from the *Zion's Herald* wrote that men with "bodies maimed and mangled in battle, or abused in the hospital or prison" were particularly prone to addiction (Jones, 2020, p. 192). Although data on individual or veteran-specific alcoholism are not available for the period, Figure A.2 shows a basic correlation between the share of disabled veterans and increases in arrests for drunkenness across towns in Massachusetts.[15]

Veterans with war-related disabilities and addiction often faced economic hardship. Disabilities limited veterans' labor market participation, and addiction imposed additional financial burdens. Alcoholism also carried a strong social stigma as it was not seen as an illness in nineteenth-century America but as a flaw of character (Achenbaum et al., 1993). This is why the National Asylum for Disabled Volunteer Soldiers, which operated several homes for disabled veterans after the war, saw addiction

[12] Martin (2019) notes that "[m]ilitary enlistments, casualties, and members' preoccupation with the rebellion thinned temperance ranks, denying societies personnel and financial support" (p. 190), citing the example of the Maine Sons of Temperance who began to fade as early as 1862.

[13] Even today, alcoholism is among the leading problems of U.S. veterans exposed to war. 33 to 40 percent of veterans returning from Iraq and Afghanistan have been reported to consume alcohol in dangerous quantities, and 18 to 22 percent have shown signs of possible alcohol disorders (Frueh and Smith, 2012).

[14] Volumes I and II of Part 3 of the *Medical and Surgical History of the War of the Rebellion* alone reference opium, morphine, and alcohol over 740 times across 2,113 pages.

[15] The caveat of course is that inferring individual-level behavior from aggregate data would fall for the ecological fallacy, hence it is important to highlight that this is merely suggestive evidence and far from a causal claim.

to alcohol as a sufficient reason to expel former soldiers from veterans' homes, and veterans suffering from alcoholism could be denied pension payments (Jones, 2020).[16] Hence, the majority of veterans ultimately resided with their families, which is why the rise in alcoholism among wounded and disabled veterans also translated into increased domestic violence. Carroll (2016) tells the story of Adolph Ahlers, an infantryman from Ohio whose left arm was amputated after being wounded in battle. Unable to find work due to his disability, he turned to liquor and became more violent toward his wife. Adolph's son, Charles, later wrote: "[Mother] did not tell me of his treatment towards her until I had heard it from other parties who had known him in his palmy days, and it was only two years ago that she admitted it to me" (Carroll, 2016, p. 197). Such behavior was not uncommon, and modern-day public health research also confirms that domestic violence is substantially more severe when the perpetrator is under the influence of alcohol (Graham et al., 2011).[17]

This burden on women was amplified by the need to offset veterans' reduced earnings and/or costs associated with alcohol use by seeking paid employment. Already during the war, many women had gained access to new occupations, especially in the needle trades and nursing (Attie, 1998). In the 1870s, the spread of the typewriter also opened opportunities in white-collar clerical work (Rashid, 2025). Earnings from these jobs helped sustain households affected by soldiers' deaths and disabilities, and in some families women became the primary breadwinners (Giesberg, 2012). These work experiences and increased visibility of women in the economic sphere fostered a stronger sense of competence and civic voice (Attie, 1998; Giesberg, 2012). A key manifestation of this civic voice was the Temperance Crusades that began in Hillsboro, Ohio, in late 1873. Historians widely regard these crusades as pivotal, since they directly led to the founding of the Woman's Christian Temperance Union (WCTU) the following year (Bordin, 1981). Unlike the more radical and controversial suffrage movement studied by Calder (2025), women-led temperance activism grounded its appeal in religion and the duty to protect families and children (Tilly and Gurin, 1990). By 1900, the WCTU was one of the largest membership-based reform organizations in the United States, and the largest women-led political organization. While historians have documented a link between the Civil War and veterans' alcohol use and women's temperance mobilization (Martin, 2019; Jones, 2020), as well as between the war and women's work (Giesberg, 2012), systematic causal evidence that links these three channels remains limited.

2.2 Data on Union Army Veterans and Women's Employment

Regiments in the Union Army were raised and furnished by individual states, and then mustered into Federal service. After the war, between the late 1860s and 1880s, states went to great lengths to publish the *Adjutant General's Reports*, which provided detailed rosters of the soldiers who had served in each regiment. While these reports served an administrative role in verifying pension eligibility, they were also intended to preserve historical memory. The data we use were jointly collected by Dippel and Heblich (2021) and Dupraz and Ferrara (2025), and cover almost all of the 2.2 million Union Army soldiers. They contain information on each soldier's name, regiment and company, unit type, age and county of residence at enlistment, promotions, and length of service (most commonly a three-year contract), as well as how service ended (through regular muster out, wounds, death, disability, or desertion).

[16] Relatively few pensions were granted initially. In 1875, only 6.5 percent of all soldiers received a pension, and rates were low even among wounded and disabled veterans, with the system expanding significantly only after the 1880s (Skocpol, 1993). The pensions were not particularly large at a quarter to two-thirds of a laborer's monthly wage.

[17] More expansive literature reviews on the topic are provided by Finley (2013) and Wilson et al. (2014).

Data on battles fought by each regiment were added from Fox (1889) and Dyer (1908). Regimental units were mobilized locally, typically by a prominent community member who, after recruiting about one thousand men, would be commissioned as colonel of the regiment. Each regiment was subdivided into ten companies of roughly one hundred soldiers, generally recruited together at the local level. The assignment to companies was typically determined either by the order in which soldiers enlisted or, especially later in the war, by their decision to follow a particular captain (McPherson, 1988).

We link the military roster data to the full-count 1860 decennial census using the crosswalks provided by the Census Tree Project (Price et al., 2021).[18] Based on name, county of residence, and age, where available, around 42 percent of records were linked to the 1860 census, totaling a little over 920,000 soldiers.[19] We then link these soldiers to the 1870 census, where we observe the veterans as well as their other family members in the household. This left us with 518,229 veterans. Lastly, we kept the sample of veterans who were of working age, i.e. 20 to 65,[20] and married in 1870, which resulted in a final sample of 294,587 veteran-headed households. Our main sample then consists of working-aged women in the 1870 census who lived in the same household as a Union Army veteran's wife or daughter. This sample includes 379,758 women of whom 80 percent are wives. For robustness checks, we also link the women back to the 1860 census to be able to control for their families' prewar characteristics if they had not yet been married to the husband they were observed with in 1870. Backward linking is only relevant for wives, because daughters were always in the same household as the veteran. Figure 1 provides a visual representation of the linking steps taken. Summary statistics for the veterans and for their wives and daughters are reported in Tables A.1 and A.2, respectively.

In our main sample of women living with Civil War veterans in 1870, we observe them together with the veteran household head and his 1860 prewar characteristics. We also observe the household head's labor force participation and that of the female household members, which is our main outcome of interest. A key challenge is that women's work was not well enumerated in these earlier censuses (Chiswick and Robinson, 2021).[21] Work by wives and daughters in family businesses, such as craft shops, was typically not recorded; instead, only the household head would be listed with the occupation. Women were generally recorded as in the labor force only if they worked for wages outside their own household or in a recognized female trade such as seamstress or textile worker, but not for domestic or household work within their own families. Our measure therefore captures market-oriented labor

[18] The Census Tree Project includes links made by the Census Linking Project (Abramitzky et al., 2020), which we use for additional empirical exercises later, as well as the IPUMS Multigenerational Longitudinal Panel (Helgertz et al., 2023).

[19] We restrict our sample to white Union Army households for three main reasons. First, Black women had substantially higher employment rates than white women during our study period, meaning that labor force entry was unlikely to be the relevant marginal response to a veteran's disability for the average Black woman (Goldin, 1977). Second, Black families' political participation was shaped by forces well beyond the economic disruptions central to our argument, including the Fifteenth Amendment's prohibition on race-based disenfranchisement and growing support for Black civil rights in several northern communities (Calder, 2025). These factors would confound our ability to isolate the specific causal relationship between labor force entry and political mobilization. Finally, because we rely on Union Army records, the number of identifiable Black soldiers in the North is very small even before our extensive linking process, making a convincing analysis disaggregated by race infeasible given our empirical design. That said, the relationship between veteran disability, women's labor supply, and political mobilization among Black families may be a fruitful avenue for future research.

[20] Younger men usually worked from the age of 14 or 15, but we restricted the sample to a minimum age of 20. These were the men who were at least 15 years old in 1865 when the war concluded. Being any younger would likely indicate a linking error as those men would have been too young to serve.

[21] The concept of labor force participation as we know it today is a more modern one. According to the Bureau of Labor Statistics definition, this encompasses work for wages, self-employment, as well as working in a family business or farm without pay if the work exceeds 15 hours per week. The first census to enumerate work according to this modern definition is generally understood to be the 1940 decennial census.

participation rather than total labor effort. While this is a narrow measure and likely an underestimate of women's work, we adopt it as our measure of women's labor force participation, as it highlights the need for women to work outside their own household for a wage.[22] Other papers have taken the same approach (see Rashid, 2025).

Our treatment variable of interest is whether the veteran household head returned from the war with a recorded disability. The *Adjutant General's Reports* provide information about soldiers' exits from service, such as being "discharged with disability." These records typically do not specify the type of disability. While one might naturally associate such discharges with amputations, the Union Army applied a much broader definition. In practice, a "disability" encompassed not only wounds and lasting injuries, but also diseases and chronic illnesses (e.g., dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy), as well as more general conditions that rendered a soldier unfit to perform military duties (Handley-Cousins, 2019). The broad definition meant that around 19 percent of Union Army soldiers were discharged due to disability (Costa et al., 2020). The term disability in this context is therefore best understood as a condition that disabled the soldier from serving during the war. Not all conditions were permanent, as cases labeled "chronic" diarrhea, ophthalmia, or scurvy might have persisted for weeks or months without implying a lifelong condition (Handley-Cousins, 2019). This implies some degree of treatment effect heterogeneity, which we will later seek to disentangle using information on a small number of soldiers for whom we observe amputations. The spatial distributions of both the outcome and treatment variables are mapped in Figure A.3 for counties in the Union and Border States, excluding the western territories.

One potential concern with our sample is that disability status may induce selection or survivorship bias. Because we have to be able to link veterans to the 1870 census, the veterans must at least be married. This conditioning on continuous marriage would introduce selection or survivorship bias if veterans with the worst disabilities died between 1865 but before the census is taken in 1870, or if their postwar marriage market prospects are negatively affected. In this case, our results will likely understate the true effect sizes. We test this by considering the full sample of veterans in 1860 and regress an indicator for not being linked on the disability treatment and a wide range of prewar observables. While statistically significant, the effect is small: disability status increases the probability of not being linked to 1870 by one percentage point, relative to a 36.7 percent baseline (Table B.1). The magnitude of this selection issue therefore appears to be reasonably small. We provide more information and robustness checks, such as inverse propensity score reweighting, and evidence for treatment effect monotonicity to justify the likely downward bias in Appendix B.1 to further show that neither type of selection or survivorship bias influences our results in a substantive way (see Table B.2).

3 Household Labor Market Consequences of Veterans' Disability Status

3.1 Veterans' Postwar Labor Market Outcomes

To motivate studying the effect of veterans' disability status on their wives' and daughters' labor force participation, we first estimate the impact of disability status on veterans' labor market outcomes in 1870. Using the sample of nearly 300,000 veterans whom we could link to the 1870 census and who

[22] We count women with recorded occupation codes in the `occ1950` variable between 0 and 970 (Laborers n.e.c.) as employed.

were married by that time, we estimate

$$y_{vcr} = \pi D_{vcr} + X'_{vcr,1860}\lambda + \alpha_{c,1860} + \Gamma_r + \epsilon_{vcr} \tag{1}$$

for veteran v, residing in county c in 1860 and serving in regiment r. The outcomes y are (i) the inverse hyperbolic sine (asinh) of the veteran's 1870 occupational income score,[23] and (ii) an indicator for not participating in the labor force despite being aged 20–65 in 1870. D_{vcr} is a binary indicator equal to one if the veteran was discharged from the Union Army with a disability, and its coefficient π quantifies the impact of a disability on the veterans' postwar income score. The vector $X_{vcr,1860}$ includes prewar observables: age and age squared; the asinh of the 1860 occupational income score; personal and real estate wealth; indicators for living on a farm, urban residence, birthplace, skill and industry group,[24] literacy, school attendance, labor force participation, and number of children in the household. We also include fixed effects for 1860 county of residence ($\alpha_{c,1860}$) and regiment (Γ_r). Together, these controls absorb a wide range of socioeconomic factors plausibly related to postwar labor market outcomes and to wartime disability risk, e.g., if soldiers from poorer backgrounds were assigned riskier tasks. Standard errors are clustered by the 1860 county of residence to account for unobserved common local shocks affecting workers in the same labor markets.[25]

Table 1 reports the regression results, which show that disabled veterans saw an approximate 1.5–2.1 percent decline in their postwar income score. Their probability of not being in the labor force rose by about 0.4 percentage points, relative to a baseline in which 6.5 percent of working-age veterans in 1870 reported not participating in the labor market. These effects appear small; however, this is due to the broad range of disability classifications employed by the Union Army as argued before. In addition, there are other important dimensions of heterogeneity. The most affected skill group is veterans who held semi-skilled occupations before the war, especially those who were of prime fighting age during the war. Relative to low- or high-skilled workers, semi-skilled veterans saw about a 4.8 percent drop in the occupational income score, increasing to roughly 7 percent among those aged 40 or younger in 1870 (see Table A.4). Their probability of exiting the labor force also rose more than in the full sample. This finding is similar to Schmick (2022), who also observes disability status as a binary variable for World War I veterans and finds a 6.5 percent decrease in wages.

Treatment effect heterogeneity: We are able to observe amputations for a subset of almost 2,000 soldiers. We match these veterans to non-disabled veterans using one-to-one nearest-neighbor matching on the prewar covariates in $X_{vcr,1860}$, and then regress the asinh occupational income score on an amputation indicator while including matched-pair fixed effects. Table A.5 shows substantially larger negative effects for amputees (panel a) and a higher probability of exiting the labor force by 1870 (panel b). On average, amputees lost around 13 percent of their 1870 occupational income compared to their matched

[23] Pre-1940 censuses do not report income. The occupational income score is a proxy that assigns the median 1950 income for the veteran's occupation (in hundreds of dollars).

[24] Skill groups are professional, technical; farmers; managers, officials, proprietors; clerical and kindred; sales workers; craftsmen; operatives; service workers; farm laborers; laborers. Industry groups are agriculture, forestry, fishing; mining; construction; manufacturing; transportation, communication, other utilities; wholesale trade; retail trade; finance, insurance, and real estate; services; public administration.

[25] Clustering by regiment would also be reasonable. Units were typically raised locally, hence for all practical purposes this does not make a difference. We show results with robust standard errors, and with clustered standard errors by regiment, state economic area, 1-by-1-degree grid cell to account for spatial autocorrelation (Bester et al., 2011), and two-way clustering by regiment and county in Table A.3.

non-disabled control veterans. When we further differentiate by type of amputation, we find that losing a right arm is associated with an approximate 26 percent drop in occupational income while losing a left arm leads to a drop of around 13.2 percent.[26] This is consistent with the majority of the population being right-handed. Losing fingers or toes did not significantly affect veterans' postwar income scores. The income losses associated with amputations are similar to the modern literature studying income losses from permanent disabilities. The magnitude of income losses for amputations aligns with modern evidence on the earnings impact of permanent disabilities: for example, Reville and Schoeni (2001) estimate a 25 percent earnings loss following permanent work-related injuries in California, and Dworski and Powell (2022) find a 19.6 percent wage decline even ten years after the onset of disability.

This first-stage relationship between disability severity and men's labor market outcomes could be partially mediated by differential pension receipt under the Civil War Pension system rather than a direct physical effect. The Act to Grant Pensions of July 14, 1862 graded pension payments primarily by rank and disability severity (Skocpol, 1993). For total disability, defined as inability to perform manual labor, enlisted men received a maximum of $8 per month while officers received between $15 and $30 per month depending on rank (Johnson, 2011). Within each rank, pensions varied by disability severity as a fraction of the maximum available to that rank; for example, an amputated finger constituted a 2/8 disability, corresponding to a $2 monthly pension for an enlisted soldier or approximately $8 for a lieutenant colonel (Linares, 2001). Thus, more severe amputations qualified for higher pension payments, which through an income effect may have reduced veterans' 1870 occupational income scores and/or labor force participation independent of physical capacity.[27]

Regardless of the exact channel driving the first stage, the implications for the second stage are reassuring. To the extent that higher-ranking and therefore higher-earning households were both more likely to receive larger pensions and less likely to send women into wage labor, any correlation between pension income and women's LFP would work against our main results rather than spuriously generating them. This interpretation is further supported by Eli (2015), who shows that higher pension income induced higher fertility, meaning that larger pensions would suppress women's labor force entry through both an income effect and additional childbearing demands. Moreover, the average pension payment was modest relative to a common laborer's wages, suggesting that the observed reduction in veterans' labor force participation is better attributed to disability raising his reservation wage through increased disutility of work than to pension income providing sufficient household resources to remove him (and his dependents) from the labor force entirely via an income effect.[28] Further, the second stage accounts for the veteran's rank at enlistment, which proxies for the maximum pension he would qualify for; conditional on rank, remaining variation in pension amount reflects disability severity, which is consistent with LATE estimates that exceed OLS. On net, differential pension receipt by disability severity is unlikely to account for our main findings, and if anything biases our estimates toward zero.

PTSD in the control group: An important question is whether non-disabled veterans are a suitable control group. Despite having been discharged without a physical disability, they may have returned

[26] Given the coefficient sizes, the approximation $\sim \beta \times 100$ becomes inaccurate. The exact effect sizes are 14 percent for any amputation, 27 percent for an amputated right arm, and 15.3 percent for an amputated left arm, computed as $100\times(\exp^{\beta}-1)$.

[27] Controlling for rank-based pension categories ($8, $15, and $30 maximum thresholds) increases both the magnitude and statistical significance of the estimated first-stage effects, indicating that unobserved pension income likely attenuates the first stage relationship.

[28] Johnson (2011) notes that as late as 1888, two thirds of disabled pension recipients received $8 or less per month and half received $6 or less per month, approximately 25% of the average coal miner's monthly wages.

from the war with Post-Traumatic Stress Disorder (PTSD). If this impacted their labor market outcomes, meaning that the control group is partially treated by an unobserved mental disability, we will underestimate the true π in equation (1). PTSD is unobservable to us in the data, but we try to test for its potential impact on non-disabled veterans' employment and wages by using proxies for PTSD exposure. Using the 19 largest battles listed by Selcer (2006), we take the sample of non-disabled veterans only and regress their labor market outcomes on the number of major battles they participated in as a proxy for PTSD.[29] The resulting coefficients are plotted in Figure 2. We find no negative effect of fighting in the largest battles on non-disabled veterans' occupational income score (panel a) or their probability of working (panel b). These findings do not imply that the potential impacts of PTSD weren't severe, but rather show that our control group's labor market outcomes are not significantly impacted by wartime trauma.[30] Even though they did not influence their wives' and daughters' labor force participation, we will later show that soldiers who participated in the deadliest battles of the war did impact women's political participation in the temperance movement, as per the flow chart shown in Figure A.1.

3.2 Impacts on the Labor Force Participation of Veterans' Wives and Daughters

Given the significant impacts of returning veterans' disability status on their income, we next test the implications for the labor force participation of their working-aged wives and daughters in 1870. Using the sample of wives and daughters of veterans, we compare those living with a disabled veteran to those living with a veteran who returned from the war without lasting physical ailments. We do not make comparisons to the wives and daughters of non-veterans to abstract from men's potentially endogenous decision to enlist in the war.

We begin by characterizing some of the data patterns observed for women in households with disabled and non-disabled veterans. Figure 3 plots unconditional differences in employment for women in both types of households. The overall share of working women is around 4 percent, reflecting both our oversampling of married women (who were typically expected to exit the labor force after marriage) and the conservative way work is measured here. Panel (a) shows that those in treated households have a one-percentage-point higher probability of working in 1870 compared to wives and daughters of non-disabled veterans, a 20 percent difference in relative terms. As with the veterans, however, comparisons of raw means mask significant heterogeneity. Panel (b) considers the extreme case in which the household head was not only disabled but also out of the labor force in 1870; in these households, wives' and daughters' labor force participation is roughly double that of women in control households. To give another example of heterogeneity, looking at the age–employment profile in 1870, younger treated wives work more but older treated wives work less frequently than their counterparts in the control group, while treated daughters are consistently more likely to work than those in the control group, with the gap narrowing as they reach typical marriage ages in their late twenties (see Figure A.5). Lastly, panel (c) of Figure 3 shows that women from households with disabled veterans were much more likely to be employed in service occupations and professional or technical jobs than those in households with

[29] The twenty largest battles listed by Selcer (2006) in chronological order are the First Bull Run, Shiloh, Seven Pines, Seven Days Battle, Second Bull Run, Antietam, Perryville, Fredericksburg, Stone's River, Chancellorsville, Vicksburg, Gettysburg, Chickamauga, Chattanooga, Wilderness, Spotsylvania Courthouse, Cold Harbor, Petersburg, Atlanta. We exclude the twentieth battle at Fort Fisher because it was primarily a naval battle which is not covered in our data.

[30] We also consider an alternative measure of potential PTSD exposure by computing the share of killed soldiers in a veteran's regiment, and regress their labor market outcomes on this mortality exposure variable in Figure A.4, which also shows no negative impacts on non-disabled veterans' occupational income score or non-participation in the labor force.

non-disabled veterans.

Next, we formalize this analysis by using the sample of wives and daughters observed in veteran-headed households in 1870, estimating the coefficients of the following equation,

$$\Pr(\text{works}_{ihc}) = \alpha_{c,1860} + \beta(\text{HH head disabled}_{hc}) + W'_{ihc}\phi + V'_{hc}\kappa + \nu_{ihc} \tag{2}$$

where works_{ihc} is an indicator for whether wife or daughter i in household h who resided in 1860 in county c had a recorded occupation in 1870 as defined in Section 2.2. The treatment is a binary indicator for whether the veteran household head was disabled or not. The coefficient β estimates the impact of living with a disabled household head on the women's probability of working outside their own household for a wage. To compare veteran families in similar labor markets and given the local nature of how regiments were raised, we include the prewar county of residence fixed effect $\alpha_{c,1860}$. We include observable characteristics of the women in the vector W_{ihc}, such as their age, age squared, and birthplace indicators, i.e., factors that are predetermined.[31] The vector V_{hc} controls for prewar characteristics of the veteran that potentially correlate both with their disability status and with their wives' and daughters' labor force participation in 1870. These 1860 census measures include the veteran's asinh of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups. In additional specifications, we control for age at enlistment and its square, rank, and enlistment date. Observations are weighted by the number of linking methods that linked the veteran household head from 1860 to 1870 to upweight high-quality links. We provide a more detailed econometric rationale for this weighting scheme in Appendix B.2. The argument is that the more methods link a person, the more confident we can be that they are correctly linked.[32] However, we also show results without any weighting. We cluster standard errors at the household-level as the unit at which treatment status is assigned.

Main results: Table 2 reports the main results. Wives and daughters of disabled veterans were 0.8 percentage points more likely to work in 1870, relative to women in control households (column 1). Once prewar characteristics of the household are included, the coefficient stabilizes around 0.55 percentage points, also when controlling for veterans' military observables and women's prewar family background characteristics (columns 2–4). The same holds when we use the post-double selection estimator proposed by Belloni et al. (2014), which selects controls based on whether they significantly predict the outcome or the treatment, i.e., controls that would be important confounders if left out from the regression. The result is remarkably robust across specifications. We also report the selection-on-unobservables test of Oster (2019).[33] As a rule of thumb, results are considered robust when δ, the implied ratio of selection on unobservables to selection on observables needed to eliminate the effect, exceeds one. Across specifications, our estimates of δ range from 2.5 to 8, indicating that unobservables would have to be several times stronger than observables to explain away the result.

Again, these results appear small in magnitude, though this is because the share of working women

[31] In some specifications, we control for the women's 1860 household head's characteristics, if they had not been married to their husband in 1870, as a measure of family background, including the asinh of the household head's occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status.

[32] These weights should be unrelated to confounders. Given the broad set of socioeconomic prewar characteristics, we capture factors such as foreign names, literacy, wealth, occupational income, skill and industry, or age, among others.

[33] The test compares how the treatment coefficient and the R^2 change when adding observed controls and infers the strength of selection on unobservables *relative to* observables needed to drive the coefficient to zero, given an assumed maximal fit $R_{\max}$. Following Oster, we set $R_{\max} = \min\{1.3 \times R^2_{\text{full}}, 1\}$ to allow for measurement error in the outcome and unobservables.

in the regression sample is 3.9 percent. Of the roughly 370,000 women in the sample, 80 percent are wives who overall had labor force participation rates as low as 2-4%. Relative to the unconditional mean, a 0.55 percentage-point increase in the probability of working is an increase of 14 percent. After adjusting for the under-enumeration problems of the 1870 census, Boustan and Collins (2014) report a change in the labor force participation of white married women of 0.45 percentage points between 1870 and 1900 — an effect size of 0.55 percentage points is therefore equivalent to over three decades of aggregate progress at the time. To provide a comparison to related work, Rashid (2025) finds that counties with increased exposure to typewriter intensive occupations saw a rise in women's labor force participation of 1.1 to 2.1 percent. Depending on the comparison, our estimates are reasonable in size and non-trivial. There are reasons to believe that they represent a conservative lower bound due to i) the conservative way women's work is measured and ii) the unobserved heterogeneity underlying the binary disability indicator, as argued in the previous section.

Heterogeneity and robustness: In terms of heterogeneity, effects are particularly driven by economic factors. If the disabled veteran also dropped out of the labor force or if they had a higher prewar income, women were more likely to work in 1870 (Table 3). We find no differential effects for women in households residing in urban areas or on farms, or women who were foreign-born. Especially the insignificant rural-farm differential is important because women's work was particularly poorly enumerated for women on farms (Chiswick and Robinson, 2021). Daughters and younger wives had significantly stronger labor market responses to living with a disabled veteran, while the number of children reduced their probability of working (Table A.6). Other working-aged men or potential family support by aunts or grandparents in the household had the expected signs but were not significant. In the worst case, when a veteran died, labor market effects for their wives and daughters are substantially larger and the wife has a significantly higher probability of being observed as the household head in 1870 (Table A.7).

The results in Table 2 are robust to the inclusion of different fixed effects for veterans' regiments, state economic area (SEA), 1870 county of residence, or any combination of these (Table A.8), unweighted regressions (Table A.9), alternative clustering of standard errors at the county, SEA, or regiment level, accounting for spatial autocorrelation as per Bester et al. (2011), and two-way clustering by household-regiment or household-county (Table A.10), and the exclusion of states one at a time (Figure A.6). We can also rule out that women in households with disabled veterans generally lived in counties with higher disability rates. In this case, such spillover effects would misattribute women's labor market response to the household treatment when in fact it was the general labor supply shock by men that influenced their work decisions. When controlling for SEA fixed effects and the share of disabled soldiers in a county, we find the same treatment coefficient as in our main results (Table A.11).

3.3 Instrumental Variables Results

The previous robustness and sensitivity checks for the ordinary least squares results provided encouraging evidence of a robust effect of veterans' disability status on the labor force participation of their female household members. We next seek to further strengthen this argument for a causal channel by employing an instrumental variables strategy. To construct the instrument, we rely on the information on which regiment a soldier served in, the duration, and the exits of other soldiers in the unit who served at the same time. A typical infantry regiment consisted of 1,000 soldiers, subdivided into ten companies of

100 soldiers each. We compute an instrument for a given soldier s who served in regiment r in company k_s for duration t_s. We consider all other soldiers in the same regiment r who served at any point during t_s, and compute the instrument as the share of these soldiers who left the regiment during t_s with a disability as,

$$\text{Disability risk}_{srk_s} = \frac{\sum_{rt_sk \neq k_s} \text{disability exits}}{\sum_{rt_sk \neq k_s} \text{soldiers}}. \tag{3}$$

Notice that we exclude the soldier himself and everyone in his own company k_s from this computation. Soldiers from the same towns and communities often enlisted together, meaning that social networks and background characteristics were particularly homogeneous within the same company (see Costa and Kahn, 2003). The cleanest possible instrument therefore not only excludes the soldier but his entire company, computing the share using soldiers in the other nine companies.

Such risk-based leave-one-out unit-level designs have been employed in studies using modern military data (Lyle, 2006; Bruhn et al., 2024; Cesur et al., 2024) and previous studies of the U.S. Civil War (Lee, 2005; Costa and Kahn, 2007a; Dupraz and Ferrara, 2025). We then assign the unit-level measure of disability risk to the female household members living with soldier s in 1870 to have an instrument for the veteran household head's disability status. The argument for the exclusion restriction to be satisfied is that, conditional on regiment-level socioeconomic controls,[34] unit-level risk depended on military strategy, which did not consider the labor market outcomes of soldiers' wives and daughters in 1870. The broad regiment-level controls for socioeconomic composition should limit concerns about low-SES units being sent to battle more frequently, for instance. Unit-level risk therefore should only impact women's labor force participation through the channel of the veteran household head's disability status.

Results from estimating equation (2) using the unit-level disability risk measure as instrument for the veteran household head's disability status are shown in Table 4. The first stage results are reported in Table A.12. The instrumental variables regressions estimate a consistently positive and significant impact of household head's disability status on women's likelihood of working in 1870. Given the strong correlation between unit-level and individual disability risk and the large sample size, the regressions produce a sizable F-statistic on the instrument coefficient in the first stage. Although unusual in most empirical settings, other studies using military data report similarly large F-statistics.[35] Since the first stage F-statistic scales with the size of the data set, our closest reference points are Bruhn et al. (2024) and Sabia and Skimmyhorn (2023) which have around 782,000 and 977,000 observations, respectively.[36] After the inclusion of military controls, the regressions estimate a 1.6 percentage-point increase in women's probability of working. This is an effect size of about 41 percent relative to the mean, and roughly three times the estimates produced by the OLS regressions. The coefficient barely changes with the inclusion of controls, using the backward linked sample of wives, or when using the LASSO to select controls.

[34] The regiment-level characteristics exclude the soldier and his company. Following the logic in equation (3), these include 1860 prewar controls such as the share of soldiers who were in the labor force, literate, household heads, urban, on farms, foreign-born, in school, low-, semi-, and high-skilled, employment by industry group, as well as the average surname length, number of children in household, age, and number of other families in the household.

[35] Examples include Bruhn et al. (2024) and Sabia and Skimmyhorn (2023) with modern military data, who have F-statistics of 31,644 and 50,512 respectively. Studies using historical military data include Angrist and Chen (2011) (F∼2,400), Ang and Chinoy (2025) (F∼1,800), Galiani et al. (2011) (F∼3,000), Goodman and Isen (2020) (F∼1,640), and Dupraz and Ferrara (2025) (F∼1,270). F-statistics are stated approximately due to differences across specifications.

[36] The most basic expression of the F-statistic is the squared t-statistic of the instrument coefficient in the first stage, i.e., $F_z = t^2 = \frac{r^2 \times df}{1-r^2}$, where r^2 is the partial R-squared for the instrument z and df are the residual degrees of freedom. This shows how the F-statistic scales with the size of the data set as the degrees of freedom increase. Two equivalent studies with partial $r^2 = 0.02$ and degrees of freedom of 1,000 and 1,000,000 will have F-statistics of 20.4 and 20,408, respectively.

Effect size differences: A natural question arising from these results is why the IV estimates should be three times the magnitude of the OLS results. This is particularly true in light of the wide range of prewar controls, robustness to selection on unobservables (Oster, 2019), and other tests that are passed by the OLS estimates. One argument is that neither OLS nor IV is "wrong" per se, but they differ because they tackle different estimands.[37] This idea is shown conceptually in panel a of Figure 4. Suppose we could measure both a veteran's inability to work and their degree of war-related disability continuously, and that the degree of disability nonlinearly influences the inability to work. Even in the absence of any endogeneity, OLS and IV might provide different estimates because IV is identified off of the compliers, i.e., the soldiers who became disabled because they served in a high-risk unit and who would not have become disabled otherwise. If riskier units participated in larger battles, and larger battles resulted in worse disabilities, then IV would capture a local average treatment effect (LATE) and estimate a steeper slope coefficient than OLS.

To empirically support this argument, we first show that large battles are associated with i) more disability exits and ii) more severe disabilities. For the first point, we use data from 618 battles and skirmishes and compute the share of participating soldiers who left the Union Army within 90 days of a battle. Wounded soldiers would typically receive treatment in a field hospital before being discharged with a disability (see Handley-Cousins, 2019), hence the 90 days cutoff. We then plot the share of these disability exits by above- vs. below-median battle size in terms of the fielded soldiers in panel b of Figure 4. The share of post-battle disability exits rises with the size of the battle. We further subdivide battles into those with above- and below-median numbers of artillery units on the field, which amplifies the effect. In Table A.13, we report the regression equivalent of this figure, where we further show that the share of post-battle disability exits in Union Army regiments is driven by the number of Confederate artillery units but not by Union Army artillery units.

Secondly, larger battles not only increased soldiers' risk of suffering a disability but also impacted the probability of sustaining a more severe disability. Those who participated in the 19 largest battles of the war were more likely to have an amputation relative to other soldiers, and even within the group of soldiers discharged for disabilities, participation in the largest battles shows an amputation discharge rate that was two and a half times higher (Figure A.7). Our previous results in Table A.5 also showed that amputations had much stronger effects on veterans' occupational income. In line with the reasoning provided in panel a of Figure 4, when we use an indicator for participation in the 19 largest battles of the war as instrument for the veteran household head's disability status in Table A.14 we find even larger effects on women's postwar labor force participation than in the baseline results reported in Table 4. This highlights how the degree of risk captured by the instrument identifies different subpopulations of compliers, leading to larger effect sizes for those who were exposed to the highest degree of disability risk.

Robustness: As before, we provide a thorough battery of robustness checks for our IV results. They persist when excluding states one by one to show that results are not driven by a specific state (Figure A.6), when accounting for spillover effects from overall disability rates at the county level (Table A.11),

[37] Other explanations are possible, including downward bias from census linking induced measurement error (see section B.2), or omitted variables. To give an example, if the relationship between daughters' probability of working (y), their fathers' disability status (d), and intergenerational poor health (h) can be represented as $y = \beta d + \delta h + u$, then OLS estimates $\widehat{\beta} = \beta + \delta \frac{Cov(d,h)}{Var(d)}$. If poor health has a negative effect on daughter's LFP, $\delta < 0$, but positively correlates with fathers' disability status, $Cov(d, h) > 0$, then this negatively biases the OLS estimate.

using battle assignment as alternative instrument (Table A.14), alternative clustering of standard errors at the county, regiment, or SEA level, two-way clustering by household-regiment or household-county, and spatial autocorrelation adjusted inference (Table A.15), no weighting of observations (Table A.16), more efficient modeling of the nonlinearities in the first stage (Table A.17), inverse propensity score reweighting to account for inclusion in the sample (Table B.3), and using a nearest neighbor matched sample of women in treated and control households (Table B.4).

4 Disabled Veterans and Women's Temperance Activism

As women entered occupations outside of their own households after the war, they grappled with the societal problem of alcoholism. Returning veterans, disabled and non-disabled, had been exposed to alcohol and opium during their years of service, oftentimes fueling postwar addiction (Jones, 2020), which translated into elevated levels of domestic violence (Carroll, 2016). In this section, we test how women's increased labor force participation impacted their incentives and ability to politically mobilize against alcohol. This setting highlights the interaction between institutions, in this case the labor market, and incentives in shaping collective action decisions. Here, women are incentivized by alcoholism to act against alcohol, but they can only do so collectively and if free-riding is limited. The ability to mobilize is then influenced by their income and the technology available for coordination. This idea is captured by theories such as threshold models of collective action (Chwe, 1999), power-sharing in response to social unrest (Acemoglu and Robinson, 2000), and information aggregation among groups (Battaglini, 2024), to give a few examples. We now test whether increased female labor force participation served as a channel linking the economic shock from veterans' disabilities to women's mobilization in the temperance movement.

We focus on the Temperance Crusades of 1873–74, which began in Hillsboro, Ohio, in response to the increasing societal burden of alcoholism. The protests spread rapidly across 29 states, mostly in the North and Midwest, as news spread via telegraph and railroad connections (García-Jimeno et al., 2022). Women in the Temperance Crusades rooted their activism in Christian belief and family duty, expressing protest through prayer vigils outside saloons, an approach that gained broader public acceptance than the contemporaneous suffrage movement or Carry Nation's later direct, violent action against saloons (Blocker, 1985). Historians commonly view these protests as the event that gave rise to the Woman's Christian Temperance Union (Bordin, 1981), the first large-scale women-led organization in the United States. We generate town-level data from the 1870 Census using crosswalks from the Census Place Project (Berkes et al., 2023) and combine them with information on Temperance Crusade protests collected by García-Jimeno et al. (2022) as well as our data on Union Army veterans. This results in a sample of 8,882 towns. We plot the average share of disabled soldiers across towns within a county together with the locations of Temperance Crusade protests in Figure 5. The map shows a strong relationship between the two variables. Summary statistics for the towns alongside the variables used in the regressions below are reported in Table C.1. The raw data are plotted in Figure C.1, which shows a positive relationship in the binned scatter plots between the share of disabled soldiers in a town and temperance crusade activity, the share of disabled soldiers and the share of women working in 1870, and between the share of working women and Temperance Crusade protests.

To quantify these relationships, we start by regressing an indicator for the occurrence of a Temper-

ance Crusade in town d in 1873–74 on the share of disabled Union Army veterans,

$$\Pr(\text{Temperance Crusade}_d) = \alpha_g + \beta \text{Pct. disabled}_d + K'_{d,1870}\gamma + X'_{d,1860}\lambda + \eta_d \tag{4}$$

where α_g are one-by-one-degree grid cell fixed effects that account for unobserved characteristics of the towns' local environments while providing a larger spatial unit than counties.[38] The main regressor of interest is the standardized share of disabled veterans in a town. We always control for log population size and its square, the share of veterans, as well as town latitude and longitude. We further control for 1870 town-level characteristics such as the share of Black population, foreign-born, share female, share urban,[39] employment share in agriculture, real estate and personal wealth per capita, churches per capita, distance to the nearest railroad, and an indicator for telegraph connection. We also control for 1860 town characteristics to account for potential pre-existing trends in these variables. The prewar 1860 variables contain the same variables as the 1870 controls as well as the share of working women. We cluster standard errors at the one-by-one-degree grid cell level to account for heteroscedasticity and spatial autocorrelation (Bester et al., 2011), and report alternative grid cell sizes and alternative clustering units in robustness checks.

The results from estimating equation (4) are reported in Table 5. The first four columns show the OLS results with baseline controls, including the 1860 controls, 1870 controls, and the full set of controls, respectively. The IV results are reported in the same way in columns 5–8, where the share of disabled veterans is instrumented by the average unit-level risk, i.e., the town-level aggregated version of the previous section. We find that a standard deviation increase in the share of disabled Union Army soldiers is associated with a 0.5 percentage-point increase in the probability of observing a Temperance Crusade protest in a town in 1873–74. This effect is significant at the 5 percent level and robust to the inclusion of any or all sets of controls. The unconditional outcome mean is 6.8 percent, hence the effect size implies a change in the outcome of 7.4 percent relative to the mean. As before, the IV coefficients are roughly two to three times larger than the corresponding OLS results with an effect size of around 19 percent relative to the outcome mean. Both OLS and IV results in Table 5 are robust to alternative inference methods. Table C.2 reports results with different cell sizes, including 0.5-by-0.5 and 2-by-2 degree grid cells, clustering at the county or the SEA level, as well as Conley (1999) standard errors with 100 and 200km distance cutoffs. Figure C.2 plots the OLS and IV coefficients when excluding states one by one to show that results are not driven by any particular state in the sample.

We next seek to relate this reduced form effect of disabled soldiers on Temperance Crusade activity to the female labor force participation (FLFP) channel. Table 6 first regresses FLFP in 1860 on the standardized share of disabled soldiers in columns 1 and 2. Neither the OLS nor the IV finds a significant effect, which serves as a placebo result that shows no pre-trend in women's employment prior to the war. We repeat this exercise in columns 3 and 4 but using FLFP in 1870. This is similar to the individual-level analysis from the previous section. Again, we find a positive and significant impact of the share of disabled Union Army veterans on the share of working women in 1870. Column 5 regresses the indicator

[38] In our sample for the North and Midwest, we have 986 counties and 202 one-by-one-degree grid cells. On average, these contain 14 and 67 towns, respectively. The advantage is consistency since we have clustered standard errors at the cell level in earlier regressions, but also to ensure that there is sufficient variation within a spatial unit.

[39] While this may seem counterintuitive since we are looking at towns and cities, individuals in smaller towns are identified by their post office in the census. This means that some people live a little outside a town and are actually rural, which is why we also have farmers and farm laborers in the sample. The share of urban population helps to distinguish more densely populated places.

for the occurrence of a Temperance Crusade protest on FLFP in 1860, which is insignificant. When regressing it on FLFP in 1870 in column 6, however, we find a positive and significant effect, meaning that Temperance Crusade protests were positively related to the share of working women in 1870 but not in 1860. To further test whether this relationship depends on women's incentives to participate in the temperance movement, proxied by the share of disabled soldiers in their town, we repeat the regression from column 6 for towns without any known disabled veterans and towns with any disabled veterans in columns 7 and 8, respectively. The entire effect is driven by towns with disabled soldiers, whereas towns without disabled veterans saw no significant relationship between women's 1870 LFP and the Temperance Crusade protests. This result suggests that when incentives to engage with a political movement are present, the labor market may be an important point of entry or coordination for higher levels of political organization. However, the labor market will not give rise to political activity in the absence of a pre-existing catalyst.

Impacts of PTSD and Death: Wounded and disabled veterans were not the only Civil War-related shock affecting these local communities. As per the conceptual framework sketched in Figure A.1, veterans with PTSD and killed soldiers may have had their own impacts on women's work and political mobilization. We previously showed that likely PTSD-affected soldiers did not significantly impact women's probability of working (Figures 2 and A.4), while deceased soldiers had an outsized impact due to the complete income loss of the former household head (Table A.7). We further hypothesized that women's political mobilization should: i) not be impacted by dead soldiers, given that domestic alcoholism and violence was less likely to affect them; ii) be impacted by disabled soldiers through the channel of women's increased labor force participation; iii) be impacted by soldiers with PTSD.

Table 5 has shown that women's temperance activism was significantly related to the share of disabled veterans, and that this was especially true in places where women also entered the labor force (Table 6). Additional supporting results from using a causal mediation estimator in Table C.3 suggest that the effect of the share of disabled veterans on crusade activity is entirely mediated by the increase in women's labor force participation in 1870. This finding provides further evidence that in the presence of a common catalyst (in our case, Civil War disability rates), the labor market can provide a venue for coordinating collective action. We will further probe this mechanism in the following section. To test the impacts of deceased and PTSD-affected soldiers, we include the share of killed soldiers and the share of soldiers who participated in the largest battles of the war as additional regressors in Table C.4. The share of soldiers who died, either by battle or disease, has no significant effect on the probability of observing a Temperance Crusade protest in town. The share of soldiers with potential PTSD, however, does have a positive and significant effect. All variables are standardized for ease of interpretation. The share of battle-exposed soldiers has an equally large effect as the share of disabled soldiers. When instrumenting the share of disabled soldiers, as before, results remain similar, though with less precisely estimated coefficients. Lastly, in columns 3–5 we interact the share of disabled soldiers with the share of deceased soldiers, the share of soldiers with potential PTSD, and the overall population share of soldiers in a town. None of these interactions is significant, suggesting that the impact of disabled and PTSD veterans on political mobilization worked independently from each other.

4.1 Mechanisms

We test two sets of mechanisms connecting Civil War disability rates and increased temperance activity: (i) labor-market channels and (ii) information-transmission and incentive channels. On the labor market side, there are at least two potential explanations. Women could be mobilizing against liquor because i) their increased relative household income improved their bargaining position or economic independence, or ii) they met other women in the workplace which enabled them to more efficiently communicate and mobilize. Using the 1870 census, we compute the share of women's household income by taking the log ratio of the sum of women's occupational income scores over the income score of the household head. We only include non-zero income score values so this continuous, intensive measure will not be conflated with the extensive decision of whether or not to work. Binned scatter plots of this variable and crusade activity, women's LFP, and the distributions by treatment status are shown in Figure C.3. To test the hypothesis that workplace interactions affected collective action, we computed a Herfindahl-Hirschman type index for women's employment concentration in different occupations. We first compute the share of employed women in each occupation over total female employment and then sum the squared shares. An index of one then implies that all women work in the same occupation, while values closer to zero imply greater dispersion.

The information transmission channel is motivated by previous work, which has shown that an important driver of collective action is the ability to mobilize via information sharing (e.g. Manacorda and Tesei, 2020; García-Jimeno et al., 2022). Grievance, as measured by the propensity to suffer from alcoholism, provides incentives for groups to mobilize politically (Chwe, 1999). We rely on standard measures of information transmission, such as connectedness to the railroad or telegraph networks, and being close to other protest towns in a 20-mile radius, as well as the number of newspapers per capita in a town, and the number of churches per capita. For instance, the protests in Hillsboro, Ohio, that sparked the wave of Temperance Crusades, were started by women after attending a local sermon (Blocker, 1985). To measure grievance against alcohol, as far as possible, we compute the employment share of workers in bartender and waiter occupations in 1870 from the census.

Labor market channel: Table 7 reports a positive and significant effect of the standardized log female-to-male household income ratio on the probability of observing a Temperance Crusade protest in a town. A standard deviation increase in this ratio is associated with a 1.3 percentage points higher probability of there being a protest. Controls are the same as the full set of controls used in Table 5. As in Table 6, which showed an impact of FLFP in 1870 on temperance activity in towns with disabled veterans, but not in towns without them, we find the same to hold true for the impact of changes in the female-to-male income ratio as shown in columns 2 and 3 of Table 7. This again shows that changes in women's relative economic position through the institution of the labor market matter, but that this channel only works when combined with incentives to mobilize. We also find a positive relationship between the share of disabled soldiers in a town and the female-to-male household income ratio in column 4. Column 5 leverages this relationship in an IV regression, treating the share of disabled soldiers as an instrument for the relative income ratio as a potential channel. Results provide evidence that disability-induced changes in women's relative incomes and economic position may have had a large effect on the probability of political mobilization. We take the results of this analysis as suggestive rather than causal due to the extreme assumption that the share of disabled soldiers is a valid instrument for the relative income ratio as a potential channel. We next test the workplace interaction channel by regressing the Temperance

Crusade indicator on the standardized women's occupational concentration index. If anything, we find a negative effect, suggesting that more women in the same occupations did not contribute to their political mobilization but actually even decreased it (column 6). A null finding would imply that the share of women in an occupation did not matter as much as women's labor force participation at large. A negative finding would imply occupational crowding and within-workplace competition between women could suppress collective organizing through labor market channels. We find no significant relationship between occupational concentration and the share of disabled soldiers.

Information and incentives channel: Lastly, we re-estimated equation (4) including the main effects for the different information transmission and incentive channel variables, as well as their interaction with the share of disabled Union Army veterans. All continuous variables were standardized to have mean zero and variance one. Figure 6 plots the OLS and IV interaction coefficients. We find a strong positive amplifying effect from the share of bartenders and waiters in a town. While it is significant in the OLS, it is much more noisily estimated in the IV regression but still goes in the right direction and cannot be statistically distinguished from the OLS coefficient. We also find some positive impact of being close to other protest towns within 20 miles, supporting the network effects results by García-Jimeno et al. (2022). The standardized railroad distance interacts with the disability rate as expected, but the effect is very small, and also connections to the telegraph network do not amplify the disability treatment effect.[40] Churches per capita also had no significant effect in either regression, whereas newspapers per capita did have a significant positive amplification effect of the disability treatment. Information flows therefore were important, however, the medium of communication did matter with newspapers being a more important channel than railroads or telegraphs.

5 Conclusion

This paper has shown that the U.S. Civil War affected the economic and political participation of women through the postwar consequences of veterans' disabilities. Using detailed micro-level data linking Union Army soldiers to the 1860 and 1870 censuses, we document that households with disabled veterans experienced a significant shift in women's labor market participation. The wives and daughters of disabled soldiers were more likely to work for wages outside their household, a finding that is robust across a wide range of empirical specifications and identification strategies, including an instrumental variables approach based on regiment-level disability risk.

We further provide evidence that this disability-induced rise in women's economic activity was connected to later political mobilization through the Temperance Crusades of 1873–74, the first large-scale, women-led political movement in U.S. history. Towns with a higher share of disabled veterans saw more women enter the labor market by 1870, and those same towns were subsequently more likely to experience organized temperance protests. The mediation estimates are consistent with women's labor force participation accounting for most, and possibly all, of the link between veterans' disabilities and the emergence of these protests. At the same time, other war-related shocks, especially exposure to major battles, appear to have affected temperance activity without changing women's work. The results therefore point to distinct channels through which the Civil War shaped postwar political mobilization.

[40] This is not to say that railroad and telegraph networks did not matter for political mobilization. Their main effects are significant, as shown in the tabular version of this figure in Table C.5.

Our findings suggest that economic participation can facilitate political mobilization in several ways. The evidence is strongest for changes in women's relative economic position within the household and for broader information-transmission channels, especially newspapers and proximity to other protest towns. We find less support for the narrower workplace-contact mechanism measured by occupational concentration. In our empirical setting, these forces operated alongside shared grievances related to alcohol abuse and domestic violence, problems that historical accounts link closely to wartime trauma, pain management, and disability. Political mobilization in this context was therefore not only ideological. It also reflected economic necessity, changing household roles, and the ability of women to coordinate around problems that directly affected family life.

Taken together, the results contribute to a broader understanding of the historical roots of women's political empowerment. They provide new evidence that economic shocks, even those originating from war, can alter gender roles and create conditions under which excluded groups become more likely to organize politically. In doing so, the paper bridges literatures on the political economy of enfranchisement, the consequences of war, and the evolution of female labor markets. The Civil War inadvertently helped create the conditions for an early and consequential step in the long march toward women's political agency in the United States.

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Tables

Table 1: Disability Effect on Veterans' Postwar Wages and Employment

	Asinh(occupational income score$_{1870}$)			Pr(left labor force$_{1870}$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Soldier disabled	-0.021***	-0.015**	-0.016***	0.003**	0.004**	0.004***
	(0.006)	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)
County FE	yes	yes	yes	yes	yes	yes
1860 ctrl		yes	yes		yes	yes
Regiment FE			yes			yes
Observations	294,587	294,587	294,453	294,587	294,587	294,453
Outcome mean	3.412	3.412	3.411	0.065	0.065	0.065
Adj. R^2	0.114	0.133	0.137	0.138	0.146	0.150

Note: OLS regressions of the inverse hyperbolic sine (asinh) of Union Army veterans' occupational income score (columns 1-3) and an indicator for not being in the labor force (columns 4-6) on an indicator for whether a veteran left the Union Army due to a disability. The sample includes veterans in 1870 who are between 20 and 65 years of age. Veterans were linked to the 1860 census based on name, age, and residence, and to 1870 using the Census Tree crosswalks by Price et al. (2021). The disability indicator includes all types of disability-related exits from service, such as wounds, physical disabilities, but also diseases and chronic illnesses (e.g. dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy, among others), and general conditions that made a soldier unfit to perform their military duties. All regressions include 1860 county of residence fixed effects. Prewar controls measured in 1860 include age, age-squared, number of own children, the asinh of the veteran's occupational income score, the asinh of their personal and real estate wealth, as well as indicators for birthplace, skill and industry groups based on the 1950 Census Bureau definitions, school attendance in 1860, literacy, urban residence, farm status, group quarter residence, and labor force participation status. Columns 3 and 6 further control for fixed effects for the regiment a soldier served in during the war. Observations are weighted by the number of census linking methods that successfully linked each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the 1860 county of residence and are reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Disabled Veterans' Effect on the Postwar Labor Force Participation of their Wives and Daughters

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0081***	0.0049***	0.0054***	0.0056***	0.0055***	0.0063***
	(0.0015)	(0.0014)	(0.0014)	(0.0016)	(0.0014)	(0.0016)
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl		yes	yes	yes	yes	yes
Military ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,918
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
Adj. R-squared	0.038	0.057	0.066	0.065	0.067	0.065
Oster (2019) delta	8.032	2.488	2.982	2.735	3.091	2.967

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. The final row of the table reports the delta statistic by Oster (2019) to test for sensitivity with respect to unobservable characteristics. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneity in the Disability Effect on Wives' and Daughters' Post-Civil War Work

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0043***	0.0052***	0.0055***	0.0059***	0.0058***	0.0053***
	(0.0015)	(0.0014)	(0.0014)	(0.0015)	(0.0020)	(0.0015)
.. × no LFP$_{1870}$	0.0178***					
	(0.0067)					
.. × ln occ score$_{1860}$		0.0022*				
		(0.0013)				
.. × ln wealth$_{1860}$			0.0007			
			(0.0013)			
.. × urban$_{1860}$				-0.0022		
				(0.0038)		
.. × farm$_{1860}$					-0.0010	
					(0.0028)	
.. × foreign$_{1860}$						0.0012
						(0.0037)
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Military ctrl	yes	yes	yes	yes	yes	yes
Observations	370,067	370,067	370,067	370,067	370,067	370,067
Outcome mean	0.039	0.039	0.039	0.039	0.039	0.039
Adj. R-squared	0.066	0.066	0.066	0.066	0.066	0.066

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The disability treatment is interacted with different individual and family characteristics, including the respective main effects. The interaction variables are the veteran's labor market status in 1870 (column 1), prewar characteristics such as the inverse hyperbolic sine (asinh) of the veteran's 1860 occupational income score (column 2), the asinh of personal and real estate wealth (column 3), and indicators for urban residence (column 4), farm status (column 5), and foreign birthplace (column 6). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace), additional 1860 veteran household head controls (asinh of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions), and military controls (age at enlistment and its square, rank, and enlistment date). Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Postwar LFP of Women with Disabled Veterans at Home - Instrumental Variables Results

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0070**	0.0166***	0.0162***	0.0149***	0.0162***	0.0162***
	(0.0032)	(0.0031)	(0.0033)	(0.0038)	(0.0033)	(0.0038)
County FE	yes	yes	yes	yes	yes	yes
Individual ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Veteran ctrl		yes	yes	yes	yes	yes
Regiment ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,912	370,067	250,912
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
K-P F-statistic	19,348.1	20,252.2	18,052.6	13,829.5	18,052.6	13,829.5

Note: Instrumental variables regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The disability indicator is instrumented with the share of other soldiers who left the veteran's regiment due to disability, excluding the veteran's own company. Regiments (∼1,000 soldiers) nest companies (∼100 soldiers). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Disabled Civil War Veterans and Temperance Crusades Across Towns, 1873–74

	Outcome: Pr(Temperance Crusade$_{1873--74}$)							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share disabled	0.005**	0.005**	0.005**	0.005**	0.019***	0.016**	0.013**	0.012*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)	(0.006)	(0.006)
1860 controls		yes		yes		yes		yes
1870 controls			yes	yes			yes	yes
Observations	8,882	8,882	8,882	8,882	8,882	8,882	8,882	8,882
R^2	0.120	0.135	0.194	0.201				
K-P F-Stat					230.363	227.981	229.047	226.455
Outcome mean	0.068	0.068	0.068	0.068	0.068	0.068	0.068	0.068

Note: OLS and IV regressions of an indicator for the occurrence of Temperance Crusade activity in 1873–74 on the share of disabled Union Army veterans residing in the town in 1870. The sample consists of towns in the North and Midwest. Columns 1-4 report the ordinary least squares results and columns 5-8 report the instrumental variables results where the share of disabled Civil War veterans in a town is instrumented with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own companies. Baseline controls include the log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). 1860 controls include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls, as well as the distance to the nearest railroad and an indicator for telegraph connection to account for potential information networks across towns. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Results using alternative standard error estimation methods are reported in Table C.2. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Disabled Soldiers, Temperance Crusades, and Women's LFP

	FLFP_{1860}		FLFP_{1870}		$\text{Pr(Temperance Crusade}_{1873--74}\text{)}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share disabled	0.013	0.014	0.019**	0.042*				
	(0.014)	(0.034)	(0.008)	(0.024)				
FLFP 1860					0.001			
					(0.002)			
FLFP 1870						0.013**	-0.004	0.013*
						(0.006)	(0.005)	(0.007)
Estimator	OLS	IV	OLS	IV	OLS	OLS	OLS	OLS
Sample							no disab	any disab
Observations	8,882	8,882	8,882	8,882	8,882	8,882	1,601	7,247
R^2	0.013		0.198		0.200	0.201	0.119	0.208
K-P F-Stat		228.433		228.433				

Note: OLS and IV regressions of the female labor force participation (FLFP) rate in 1860 (columns 1-2) and 1870 (columns 3-4) on the share of disabled Union Army veterans residing in the town in 1870. Columns 5-8 report the results from OLS regressions of an indicator for Temperance Crusade activity in 1873–74 on the FLFP rate in 1860 and 1870 (columns 5-6). Columns 2 and 4 instrument the share of disabled Union Army veterans with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own companies. Columns 7 and 8 split the sample into towns that had no recorded disabled Union Army veterans in 1870 and towns with any such veterans. All regressions include the following controls: log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Additional controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). For 1860, these include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls, as well as the distance to the nearest railroad and an indicator for telegraph connection to account for potential information networks across towns. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

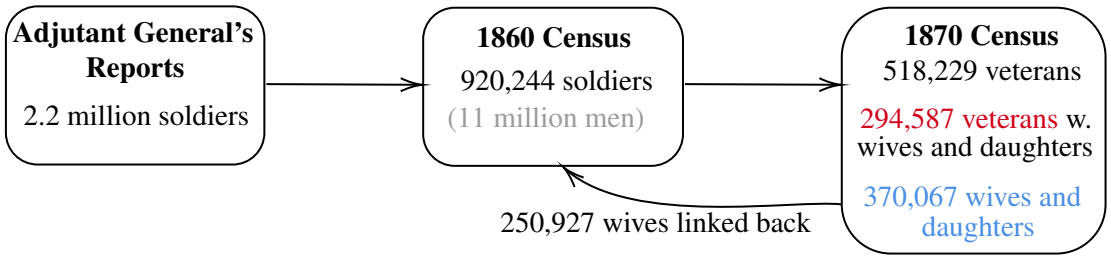
Table 7: Disability Effects on Temperance through Intra-Household Income and Workplace Interaction

	Pr(Temperance Crusade)			$\ln\left(\frac{\text{fem. inc.}}{\text{male inc.}}\right)$	Pr(Temp. Crusade)		HHI female employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\ln\left(\frac{\text{fem. inc.}}{\text{male inc.}}\right)$	0.013***	0.002	0.014***		0.147*		
	(0.003)	(0.003)	(0.004)		(0.086)		
Share disabled				0.389***			0.015
				(0.138)			(0.035)
HHI fem. empl.						-0.038***	
						(0.009)	
Estimator	OLS	OLS	OLS	OLS	IV	OLS	OLS
Sample		no disab	any disab				
Observations	8,882	1,601	7,247	8,882	8,882	8,576	8,576
Outcome mean	0.068	0.009	0.081	2.923	0.068	0.068	0.147
R^2	0.202	0.119	0.209	0.495		0.209	0.172
K-P F-Stat					7.950		

Note: Columns 1 to 3 regress a town-level indicator for Temperance Crusade activity in 1873–74 on the log of the average household female-to-male income ratio, i.e. the ratio of occupational income scores by women over those of the male household head in the same household, which are measured in 1870. Columns 2 and 3 consider towns without and with any number of disabled veterans, respectively. Column 4 regresses this ratio on the share of disabled Union Army veterans in a town. This is the first stage for the result in column 5, which regresses temperance activity on the log average female-to-male income ratio, instrumenting the latter with the share of disabled soldiers under strong assumptions regarding the exclusion restriction. Column 6 regresses temperance activity on the Herfindahl-Hirschman (HHI) concentration index that measures how concentrated women are across industries. Column 7 regresses that index on the share of disabled Union Army soldiers. The sample consists of towns in the North and Midwest. Military and census data were aggregated to the town-level using the crosswalks provided by the Census Place Project (Berkes et al., 2023). All regressions control for the 1870 and 1860 town-level characteristics detailed in Table 5. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

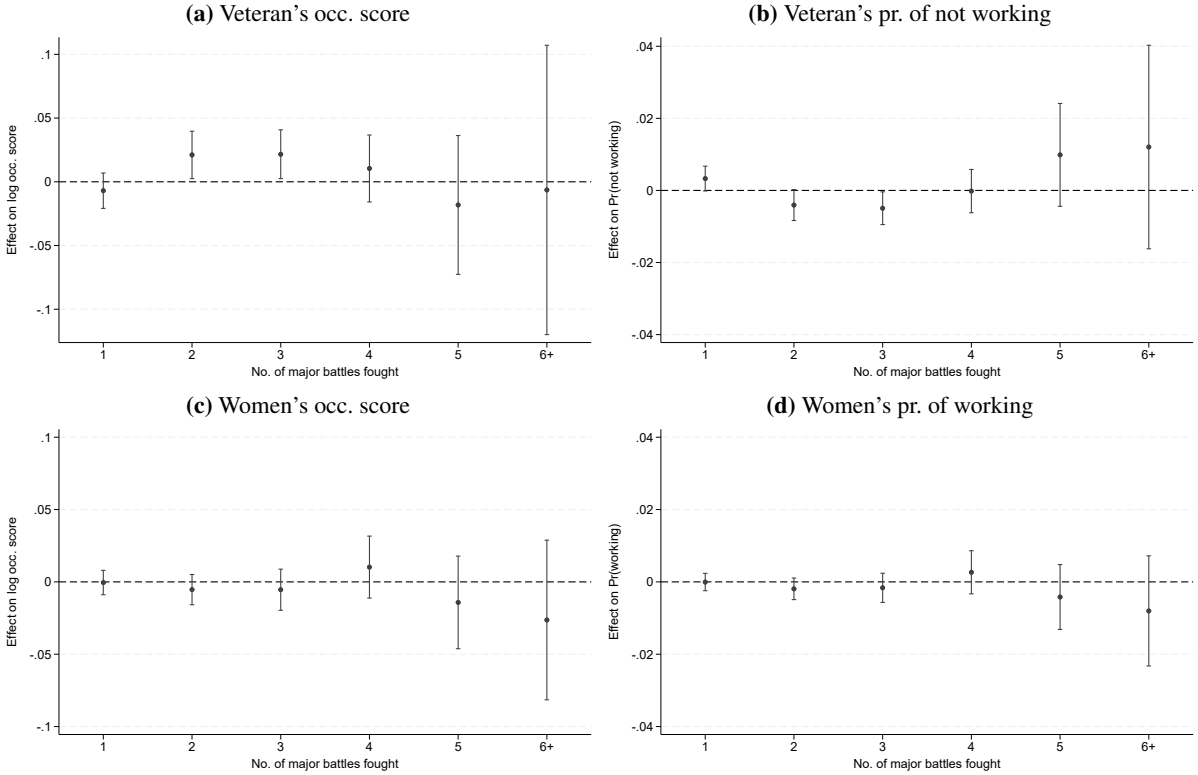
Figures

Figure 1: Linking Soldiers and Families to the 1860 and 1870 Census



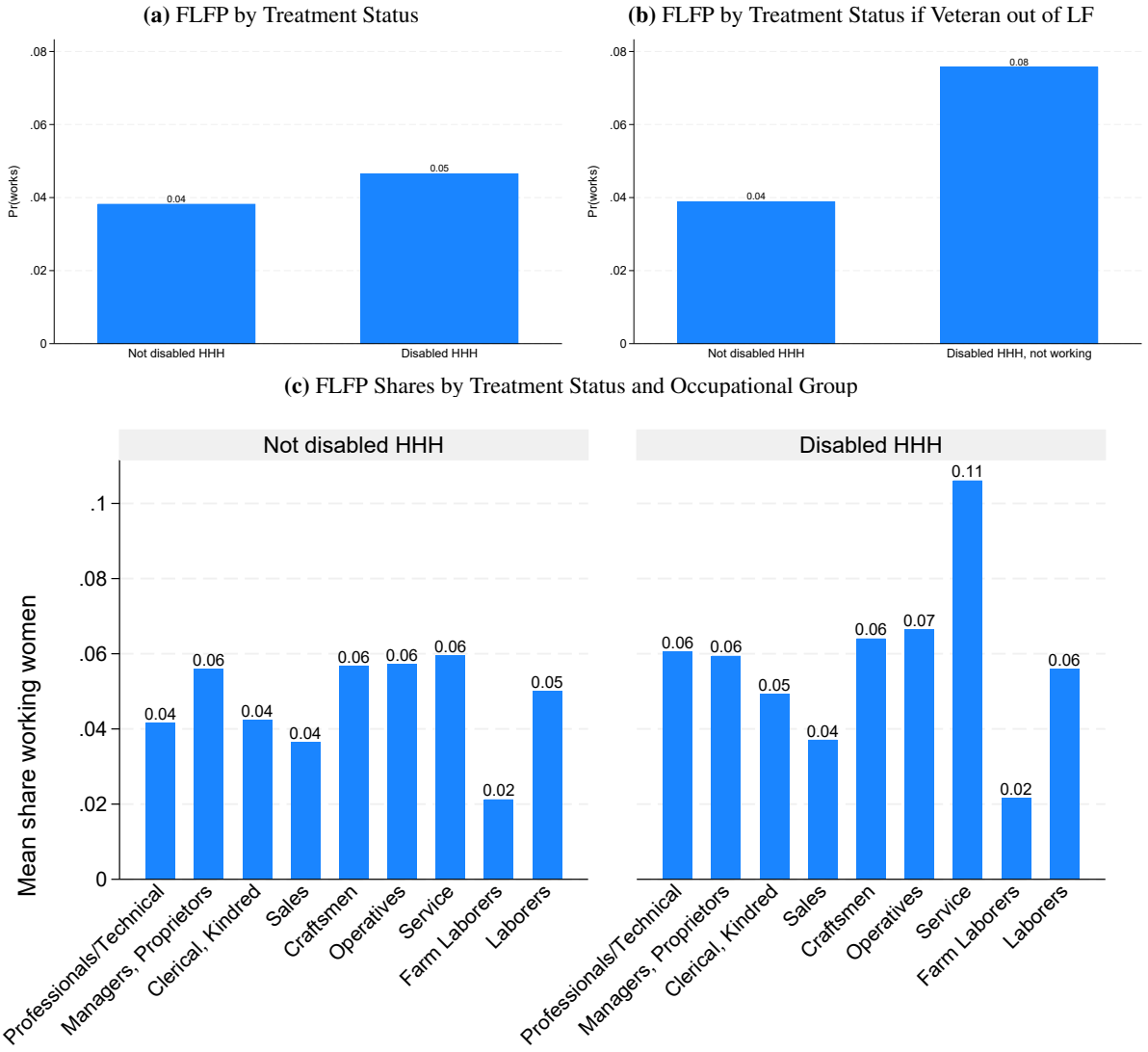
Note: The figure illustrates the linking approach that was used to identify veteran-headed households in the 1870 census. Using data on the 2.2 million Union Army soldiers, we link them to the 1860 full-count census using their names, county of residence, and age. The gray figure in the second box reports the total number of men in Union states in 1860 as reference point. Soldiers were then linked to the 1870 census using the crosswalks provided by the Census Tree Project (Price et al., 2021). In 1870, we observe soldiers and their family members. The relevant samples are the veterans with wives and daughters of working age, as well as the wives and daughters of veterans who lived in veteran-headed households in 1870. For robustness checks, we also linked the wives back to the previous census to obtain their prewar family characteristics.

Figure 2: Impact of PTSD on Employment Outcomes - Exposure to Large Battles



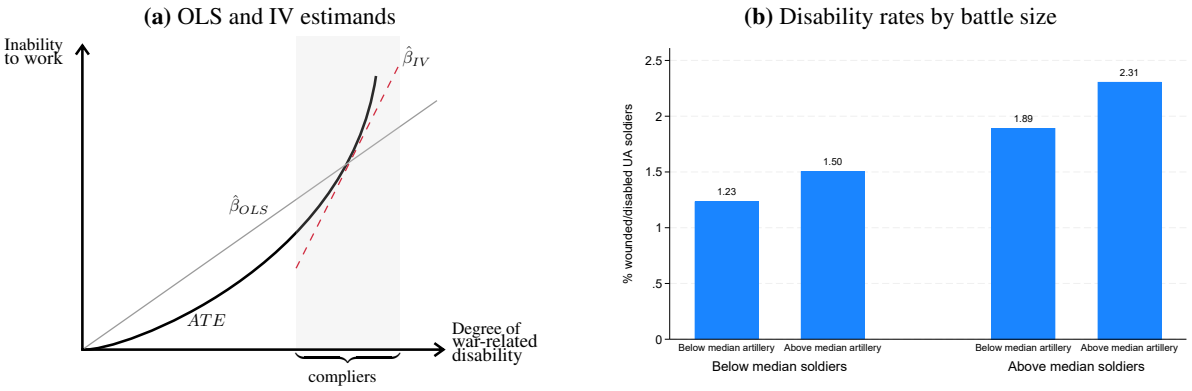
Note: Coefficients from OLS regressions of the inverse hyperbolic sine (asinh) of a veteran's occupational income score (panel a), a veteran-specific indicator for not working (panel b), the asinh of a veteran's wife's or daughter's occupational income score (panel c), and an indicator for a veteran's wife or daughter working (panel d) in 1870 regressed on how many major battles the veteran participated in during the Civil War as a proxy for post-traumatic stress disorder. The list of 19 major battles was taken from Selcer (2006) — the 20th battle was a naval engagement and is not covered in our data. Examples include Shiloh, Gettysburg, Antietam, the 1st and 2nd Bull Run, among others. The sample only includes soldiers who returned from the war without a recorded disability (panels a and c), as well as wives and daughters of veterans without a recorded disability (panels b and d). The baseline comparison group are soldiers who did not participate in any of these major battles. Regression specifications are the same as in Table 1 (columns 2 and 4) for veterans, as well as those in Table 2 (column 2) for wives and daughters. Standard errors are clustered at the 1860 county of residence for veterans and at the household level, following the main regression specification. Resulting confidence intervals reported at the 95 percent level.

Figure 3: Women's Employment by Treatment Status and Occupational Group in 1870



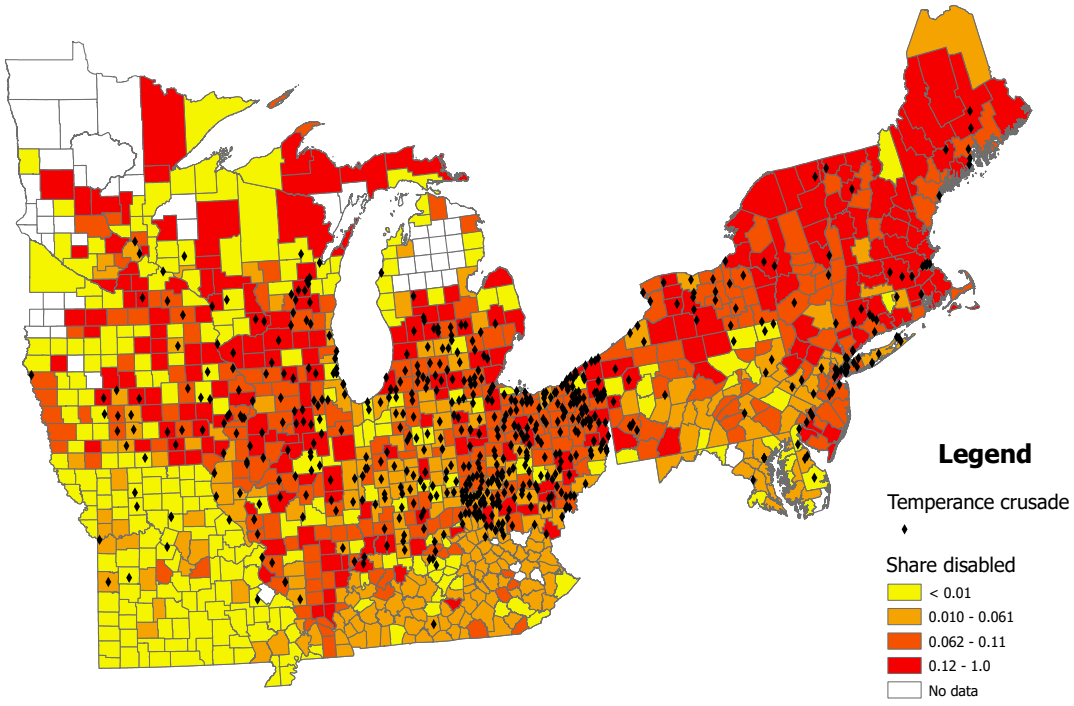
Note: Panel a shows the unconditional probability of working for women in 1870 by whether or not their veteran household head sustained a disability from the U.S. Civil War. The share of working women was computed from the full-count 1870 census file for women aged between 15 and 65, averaging an indicator for whether they had a recorded occupation excluding missing, N/A, and non-occupational responses in the occ1950 variable (codes 0 to 970, inclusive). Disability is defined as exits due to wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. Panel b reports the share of working women for the two groups, changing the treatment group to disabled veteran household heads who also reported not working in 1870. Panel c shows employment shares by treatment status and occupational group as defined by the 1950 Census Bureau occupational classification.

Figure 4: Conceptualizing the Difference Between OLS and IV Effect Sizes



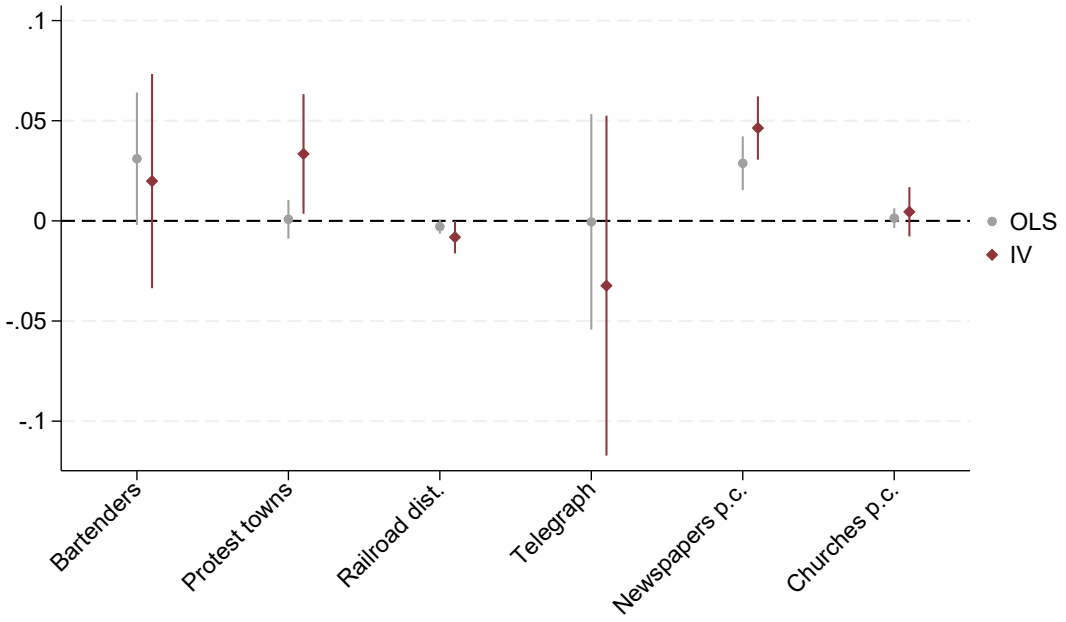
Note: Panel a shows the conceptual differences between OLS and IV when estimating the average treatment effect (ATE) with linear models and unobserved degrees of treatment intensity. The bold black line is the ATE, the thin gray line is the OLS, and the dashed red line is the IV target estimand. While we only observe a binary variable for disability status, war-related disabilities and their impact on veterans' ability to work may be nonlinear, e.g. losing a finger compared to losing the entire arm will have nonlinear effects on labor market ability. The set of compliers are those who were induced to have a war-related disability due to serving in a high-risk unit and who would otherwise not have become disabled. If soldiers in the riskiest units also suffered the worst disabilities, then the IV coefficient will estimate a steeper slope as a linear approximation to the ATE for the subset of compliers. Panel b displays battle-level statistics from 618 battles and skirmishes between the Union Army and the Confederate Army. Bars plot the share of Union Army soldiers involved in a battle who left the Union Army within the following 90 days due to wounds or disabilities after the battle. This threshold was chosen due to soldiers receiving treatment in field hospitals prior to discharge. The left two bars split battles by below median numbers of soldiers, the right two bars split the sample by above median soldiers on the battle field. Within each group, the left bar plots the share of disabled soldiers in battles with below median deployment of artillery units, and the right bar plots it for above median deployment.

Figure 5: Temperance Crusade Activity in Places with Different Shares of Disabled Veterans



Note: Maps show the share of disabled Union Army veterans and the location of towns with Temperance Crusade activity in 1873–74 using the data from García-Jimeno et al. (2022). For better visibility, the town-level share of disabled Union Army veterans is reported as average at the county level with Temperance Crusade activity reported at the town level. The share of disabled veterans was computed as the number of soldiers who left the Union Army with a recorded disability over the total number of soldiers from a county using the data from the Adjutant General's Reports. Disability includes exits due to wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. Temperance Crusade activity includes protests, marches, meetings, and petitions. County boundaries are as of 1870.

Figure 6: Disabled Veterans Interaction Effects on Crusade Activity



Note: OLS and IV regressions of an indicator for Temperance Crusade activity in 1873–74 on the share of disabled Union Army veterans residing in the town in 1870. Regressions also include the interaction between the share of disabled soldiers and variables capturing different mechanisms together with their main effects. The interaction effects from the OLS and IV regressions are plotted in this figure. They include the interaction with a decile for being in the top decile of bartender and wait staff employment, the number of protest towns in a 20-mile radius, distance to the nearest railroad, telegraph connection, newspapers per capita, and churches per capita. Continuous variables are standardized to have mean zero and variance one. In the IV regressions, the share of disabled Civil War veterans is instrumented with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own companies. Regressions control for 1860 and 1870 town-level characteristics described in detail in Table 5. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

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A Individual Labor Market Effects Appendix

Tables

Table A.1: Veteran Summary Statistics

	Obs.	Mean	St. Dev.	Min.	Max.
Disabled	294,587	0.12	0.32	0.00	1.00
Fought in major battle	294,587	0.31	0.46	0.00	1.00
Rank = Private	294,587	0.83	0.37	0.00	1.00
Occupational income score (asinh), 1870	294,587	3.41	0.99	0.00	5.08
Age in 1870	294,587	36.85	9.66	21.00	64.00
Foreign-born	294,587	0.20	0.40	0.00	1.00
In school, 1860	294,587	0.16	0.37	0.00	1.00
Literate, 1860	294,587	0.70	0.46	0.00	1.00
In labor force, 1860	294,587	0.71	0.46	0.00	1.00
Urban residence, 1860	294,587	0.25	0.43	0.00	1.00
Farm status, 1860	294,587	0.40	0.49	0.00	1.00
Regular group quarter, 1860	294,587	0.94	0.24	0.00	1.00
No. of own children, 1860	294,587	1.31	1.92	0.00	9.00
Occupational income score (asinh), 1860	294,587	2.59	1.67	0.00	5.08
Personal and real estate wealth (asinh), 1860	294,587	3.21	3.69	0.00	15.20

Note: Sample of Union Army veterans who were linked from the Union Army roster records to the 1860 census, and from the 1860 to 1870 census using the Census Tree links (Price et al., 2021). Veterans are observed in 1870 and have at least a working-age wife or daughter in their household. The disability indicator includes all types of disability-related exits from service, such as wounds, physical disabilities, but also diseases and chronic illnesses (e.g. dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy, among others), and general conditions that made a soldier unfit to perform their military duties. Occupational income scores and wealth variables were transformed using the inverse hyperbolic sine (asinh).

Table A.2: Summary Statistics for Women by Treatment Status

	Untreated N = 329,223		Treated N = 40,844		
	Mean	St. Dev.	Mean	St. Dev.	Diff.
Wife/daughter works, 1870	0.038	0.192	0.047	0.211	-0.008
Wife/daughter age, 1870	30.142	10.054	31.086	10.556	-0.945
Wife/daughter foreign-born, 1870	0.169	0.375	0.165	0.372	0.003
Wife/daughter occ. income score (asinh), 1870	0.131	0.667	0.161	0.738	-0.030
HH head fought in major battle	0.290	0.454	0.447	0.497	-0.157
HH head rank = Private	0.830	0.375	0.838	0.368	-0.008
HH head age at enlistment	35.867	12.139	34.394	11.061	1.473
HH head foreign-born	0.213	0.409	0.209	0.407	0.003
HH head in school, 1860	0.138	0.344	0.103	0.305	0.034
HH head literate, 1860	0.733	0.442	0.797	0.402	-0.064
HH head in labor force, 1860	0.733	0.443	0.773	0.419	-0.040
HH head occ. income score (asinh), 1860	2.689	1.632	2.819	1.551	-0.131
HH head personal and real estate wealth (asinh), 1860	3.725	3.787	3.831	3.699	-0.107
Family urban residence, 1860	0.252	0.434	0.260	0.439	-0.008
Family farm status, 1860	0.407	0.491	0.378	0.485	0.029
Family regular group quarter, 1860	0.943	0.232	0.946	0.226	-0.003
Family no. of own children, 1860	1.720	2.187	1.904	2.226	-0.184

Note: Mean, standard deviation, and differences between wives and daughters living in households without disabled household (HH) head (control), and wives and daughters who lived with a disabled HH head (treated) in 1870. Work is defined as having any recorded occupation in 1870 excluding missing, N/A, and non-occupational responses. All women are aged between 16 and 65 and live with a Union Army veteran in 1870. Occupational income scores and wealth variables were transformed using the inverse hyperbolic sine (asinh). Regular group quarters indicate that the household members lived in a standard family as defined by the Census Bureau, as opposed to military barracks, prisons, student dorms, or mental institutions.

Table A.3: Disability Effect on Veterans' Wages and Employment - Alternative S.E. Estimation

	Asinh(occupational income score$_{1870}$)			Pr(left labor force$_{1870}$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Soldier disabled	-0.0211***	-0.0154**	-0.0158***	0.0034**	0.0037**	0.0041***
	(0.0063)	(0.0061)	(0.0060)	(0.0015)	(0.0015)	(0.0015)
S.E. clustered by:						
Robust	0.0060	0.0060	0.0062	0.0015	0.0015	0.0015
Regiment id	0.0061	0.0060	0.0062	0.0015	0.0015	0.0015
SEA	0.0059	0.0058	0.0059	0.0014	0.0014	0.0014
1x1 degree grid	0.0070	0.0069	0.0065	0.0016	0.0017	0.0016
Regiment-county	0.0063	0.0061	0.0060	0.0015	0.0015	0.0015
County FE	yes	yes	yes	yes	yes	yes
1860 ctrl		yes	yes		yes	yes
Regiment FE			yes			yes
Obs.	294,587	294,587	294,453	294,587	294,587	294,453
Outcome mean	3.412	3.412	3.411	0.065	0.065	0.065
Adj. R^2	-0.000	0.003	0.003	-0.000	0.002	0.002

Note: OLS regressions of the inverse hyperbolic sine (asinh) of Union Army veterans' occupational income score (columns 1-3) and an indicator for not being in the labor force (columns 4-6) on an indicator for whether a veteran left the Union Army due to a disability. The sample includes veterans in 1870 who are between 20 and 65 years of age. Veterans were linked to the 1860 census based on name, age, and residence, and to 1870 using the Census Tree crosswalks by Price et al. (2021). Prewar controls measured in 1860 include age, age-squared, number of own children, the asinh of the veteran's occupational income score, the asinh of their personal and real estate wealth, as well as indicators for birthplace, skill and industry groups based on the 1950 Census Bureau definitions, school attendance in 1860, literacy, urban residence, farm status, group quarter residence, and labor force participation status. Columns 3 and 6 further control for fixed effects for the regiment a soldier served in during the war. Observations are weighted by the number of census linking methods that successfully linked each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the 1860 county of residence and are reported in parentheses. Alternative standard errors reported below, including robust standard errors, and standard errors clustered at the regiment level, state economic area (SEA), 1-by-1-degree grid cell to account for spatial autocorrelation (Bester et al., 2011), and two-way clustering by regiment and county. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Disabled Veterans' Postwar Labor Market Outcomes - Age and Skill Heterogeneity

	Panel a: Asinh(occupational income score$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Soldier disabled	-0.012*	-0.018**	-0.003	-0.003	-0.016***	-0.019**
	(0.007)	(0.009)	(0.007)	(0.009)	(0.006)	(0.008)
Disabled × lowskilled$_{1860}$	-0.017	-0.007				
	(0.014)	(0.017)				
Disabled × semiskilled$_{1860}$			-0.045***	-0.067***		
			(0.015)	(0.021)		
Disabled × highskilled$_{1860}$					0.019	-0.019
					(0.033)	(0.056)
1860 ctrl	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes
Sample	all	age<40	all	age<40	all	age<40
Observations	294,587	189,813	294,587	189,813	294,587	189,813
Outcome mean	3.412	3.420	3.412	3.420	3.412	3.420
Adj. R^2	0.133	0.122	0.133	0.122	0.133	0.122
	Panel b: Pr(left labor force$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Soldier disabled	0.004**	0.007***	0.001	0.003	0.004***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Disabled × lowskilled$_{1860}$	-0.000	-0.002				
	(0.003)	(0.004)				
Disabled × semiskilled$_{1860}$			0.008**	0.015***		
			(0.003)	(0.005)		
Disabled × highskilled$_{1860}$					-0.012*	-0.004
					(0.007)	(0.012)
1860 ctrl	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes
Sample	all	age<40	all	age<40	all	age<40
Observations	294,587	189,813	294,587	189,813	294,587	189,813
Outcome mean	0.065	0.060	0.065	0.060	0.065	0.060
Adj. R^2	0.146	0.135	0.146	0.135	0.146	0.135

Note: OLS regressions of the inverse hyperbolic sine (asinh) of veterans' occupational income score (panel a) and of an indicator for whether a veteran was not in the labor force in 1870 (panel b) on an indicator for whether a veteran left the Union Army due to a disability. The sample includes veterans in 1870 who are between 20 and 65 years of age. The disability treatment is interacted with a skill group indicator (low-skilled = laborers, farm laborers, service workers; semi-skilled = craftsmen, operatives, sales workers; high-skilled = professional/technical, managers, officials, proprietors, and clerical and kindred workers). Even columns report full-sample results, odd columns use the sample of *young* veterans who are less than 40 years old in 1870. The disability indicator includes all types of disability-related exits from service, such as wounds, physical disabilities, but also diseases and chronic illnesses (e.g. dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy, among others), and general conditions that made a soldier unfit to perform their military duties. Veterans were linked to the 1860 census based on name, age, and residence, and to 1870 using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county of residence fixed effects. Prewar controls measured in 1860 include age, age-squared, number of own children, the asinh of the veteran's occupational income score, the asinh of their personal and real estate wealth, as well as indicators for birthplace, skill and industry groups based on the 1950 Census Bureau definitions, school attendance in 1860, literacy, urban residence, farm status, group quarter residence, and labor force participation status. Columns 3 and 6 further control for fixed effects for the regiment a soldier served in during the war. Observations are weighted by the number of census linking methods that successfully linked each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the 1860 county of residence and are reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Labor Market Effects of Amputations - Matched Sample Analysis

	Panel a: Asinh(occupational income score$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Amputee	-0.130***	-0.073	-0.089*	-0.130**	-0.158***	-0.153***
	(0.048)	(0.059)	(0.053)	(0.052)	(0.057)	(0.050)
Arm		-0.135*				
		(0.074)				
Right arm			-0.171**			
			(0.080)			
Left arm				0.002		
				(0.081)		
Leg					0.030	
					(0.037)	
Fingers/toes						0.180*
						(0.093)
Baseline ctrl	yes	yes	yes	yes	yes	yes
Joint beta		-0.208	-0.260	-0.129	-0.127	0.026
t-stat		-3.286	-3.403	-1.654	-2.603	0.286
Observations	3,937	3,937	3,937	3,937	3,937	3,937
Outcome mean	3.274	3.274	3.274	3.274	3.274	3.274
Adj. R-squared	0.009	0.010	0.010	0.008	0.009	0.010
	Panel b: Pr(left labor force$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Amputee	0.043***	0.037**	0.036***	0.046***	0.041***	0.049***
	(0.013)	(0.016)	(0.014)	(0.014)	(0.015)	(0.013)
Arm		0.014				
		(0.019)				
Right arm			0.030			
			(0.021)			
Left arm				-0.011		
				(0.021)		
Leg					0.002	
					(0.010)	
Fingers/toes						-0.046*
						(0.025)
Baseline ctrl	yes	yes	yes	yes	yes	yes
Joint beta		0.051	0.066	0.035	0.043	0.003
t-stat		3.110	3.300	1.727	3.355	0.136
Observations	3,937	3,937	3,937	3,937	3,937	3,937
Outcome mean	0.116	0.116	0.116	0.116	0.116	0.116
Adj. R-squared	0.009	0.009	0.009	0.008	0.008	0.010

Note: OLS regressions of veterans' inverse hyperbolic sine (asinh) transformed occupational income score in 1870 (panel a) and an indicator for whether the veteran left the labor force by 1870 (panel b) on indicators for whether a veteran was an amputee and what kind of amputation they sustained. The sample is a matched sample of amputees and non-amputees who exited service without any recorded physical injury, wounds, or disability. Regressions compare treated and control units within each matched pair. Column 1 estimates the overall effect of being an amputee, while other columns further break the effect down by type of amputation. For columns 2-6, we report the linear combination of the main effect (amputation) and the type of amputation as joint beta with the resulting t-statistic. Arm and leg amputations include any kind of related amputation, e.g. forearm, elbow, shoulder, etc. Veterans were matched based on one-to-one nearest neighbor matching with replacement. Matching was based on prewar characteristics measured in 1860, including veterans' labor force participation status, asinh transformed occupational income score, age, asinh transformed personal and real estate wealth, literacy, school attendance, state of residence indicators, as well as indicators for their occupational skill and industry group based on the 1950 classification provided by the Census Bureau. There were 2,313 amputees who were matched to 1,624 control veterans. If a control unit was used more than once, this is accounted for by weighting by the frequency with which they enter the sample. To ensure common support, treated units were dropped from the regression if their propensity score was not within the minimum and maximum of the propensity scores in the control group. Regressions control for these baseline characteristics, the estimated propensity score, and the matched-pair fixed effect. Standard errors are clustered at the matched-pair fixed effect. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: OLS Result Heterogeneity by Demographic and Household Characteristics

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	-0.0062***	0.0141***	0.0086***	0.0067**	0.0055***	0.0049***
	(0.0007)	(0.0026)	(0.0023)	(0.0027)	(0.0015)	(0.0014)
.. × daughter	0.0322***					
	(0.0059)					
.. × wife aged >30		-0.0186***				
		(0.0028)				
.. × no. children			-0.0015*			
			(0.0008)			
.. × any men in HH				-0.0016		
				(0.0031)		
.. × grandparents					-0.0005	
					(0.0048)	
.. × uncles						-0.0019
						(0.0092)
.. × aunts						0.0137
						(0.0094)
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
Husband 1860 ctrl	yes	yes	yes	yes	yes	yes
Military ctrl	yes	yes	yes	yes	yes	yes
Observations	370,067	370,067	370,067	370,067	370,067	370,067
Outcome mean	0.039	0.039	0.039	0.039	0.039	0.039
Adj. R-squared	0.101	0.067	0.066	0.066	0.066	0.066

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability interacted with different demographic and household characteristics. These include a daughter indicator, an indicator for whether the wife is older than 30 years, the number of children in the household, and indicators for whether there are any men, grandparents, aunts or uncles in the household to measure the role of family support structures. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace), additional 1860 veteran household head controls (inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions), and military controls (age at enlistment and its square, rank, and enlistment date). Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Deceased Soldiers Effect on Women's Labor Force Participation

	Pr(wife/daughter works$_{1870}$)						Pr(wife is HHH$_{1870}$)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HHH dead	0.019***	0.040***	0.051***	0.190***	0.020***	0.052***	0.214***	0.202***
	(0.002)	(0.007)	(0.004)	(0.042)	(0.002)	(0.007)	(0.004)	(0.011)
HHH dead × daughter					-0.007*	-0.045***		
					(0.004)	(0.015)		
County FE	yes	yes	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes	yes	yes
Military ctrl	yes	yes	yes	yes	yes	yes	yes	yes
Wife is HHH			yes	yes				
Observations	226,465	226,465	199,722	199,722	226,465	226,465	124,367	124,367
Outcome mean	0.051	0.051	0.050	0.050	0.051	0.051	0.084	0.084
Adj. R^2	0.019		0.018		0.062		0.088	
K-P F-statistic		3,074.11		595.09		1,790.90		3,543.39

Note: OLS and IV regressions estimating the effect of a deceased soldier on their family in 1870. The outcomes are an indicator for women's labor force participation in 1870 (columns 1 to 6) and an indicator for the deceased soldier's wife being the household head (HHH). The treatment is an indicator for the Civil War-related death of the 1860 male household head. The sample consists of wives and daughters of Union Army veterans whose 1860 household head either returned from the war (without wounds or disabilities) or died in the war using a linked sample of women in 1870 who were linked with the Census Tree crosswalks by Price et al. (2021). Columns 5 and 6 interact the death indicator of the 1860 HHH with an indicator for daughters. The IV regressions instrument the death indicator with the share of soldiers who died in the soldier's regiment following the empirical strategy in Dupraz and Ferrara (2025). In column 5, the interaction term is instrumented with the interaction of the daughter dummy and the share of regimental deaths. Columns 7 and 8 use the sample of wives only. All regressions include 1860 county fixed effects, baseline controls (age, age-squared, birthplace), 1860 veteran HHH controls (asinh of occupational income and wealth, and indicators for literacy, schooling, labor force, urban status, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions), military controls (rank, age at enlistment (and squared), and enlistment date). Observations are weighted by the number of linking methods that successfully link women from the 1860 to 1870 census to upweight high-quality linked observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: OLS FLFP Effect of Disabled Veteran HH Head - Robustness to Different Fixed Effects

	Outcome: Pr(wife/daughter works$_{1870}$)				
	(1)	(2)	(3)	(4)	(5)
HH head disabled	0.0038***	0.0036**	0.0036**	0.0062***	0.0055***
	(0.0015)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Regiment FE	yes	yes	yes		
1860 County FE		yes			yes
SEA FE			yes	yes	
1870 County FE					yes
Observations	369,959	369,959	369,959	370,067	370,051
Outcome mean	0.039	0.039	0.039	0.039	0.039
Adj. R-squared	0.062	0.059	0.060	0.067	0.063

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability while controlling for different sets of fixed effects, such as fixed effects for the veteran's regiment, 1860 and 1870 county of residence fixed effects, and state economic area (SEA) fixed effects. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace), additional 1860 veteran household head controls (inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions), and military controls (age at enlistment and its square, rank, and enlistment date). Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Labor Force Participation of Women with Disabled Veterans at Home - OLS Unweighted

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0062***	0.0028**	0.0036***	0.0035**	0.0037***	0.0044***
	(0.0012)	(0.0012)	(0.0012)	(0.0014)	(0.0012)	(0.0014)
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl		yes	yes	yes	yes	yes
Military ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,918
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
Adj. R-squared	0.036	0.057	0.066	0.068	0.066	0.067
Oster (2019) delta	8.535	1.581	2.375	1.937	2.523	2.208

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. The final row of the table reports the delta statistic by Oster (2019) to test for sensitivity with respect to unobservable characteristics. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: OLS FLFP Effect of Disabled Veteran HH Head - Alternative Standard Errors

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0081***	0.0049***	0.0054***	0.0056***	0.0055***	0.0063***
	(0.0015)	(0.0014)	(0.0014)	(0.0016)	(0.0014)	(0.0016)
S.E. clustered by:						
County	0.0015	0.0014	0.0013	0.0016	0.0013	0.0016
SEA	0.0015	0.0014	0.0013	0.0014	0.0013	0.0014
Regiment id	0.0015	0.0015	0.0015	0.0017	0.0015	0.0017
1x1 degree grid	0.0019	0.0017	0.0016	0.0017	0.0016	0.0017
HH-Regiment	0.0015	0.0015	0.0015	0.0017	0.0015	0.0017
HH-County	0.0015	0.0014	0.0013	0.0016	0.0013	0.0016
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl		yes	yes	yes	yes	yes
Military ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,918
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
Adj. R-squared	0.038	0.057	0.066	0.065	0.067	0.065

Note: OLS regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability with different variants of estimating standard errors including clustering at the county of residence, the state economic area, the veteran's regiment identifier, or the 1-by-1-degree grid cell to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011), and two-way clustering by household and regiment, and by household and county of residence. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. In the top row, standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Accounting for Spillover Effects via Aggregate Disability Rates

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0044***	0.0045***	0.0045***	0.0063**	0.0059*	0.0059*
	(0.0014)	(0.0014)	(0.0014)	(0.0032)	(0.0031)	(0.0032)
Pct. disabled (county)			0.0376			0.0366
			(0.0262)			(0.0262)
County FE	yes			yes		
SEA FE		yes	yes		yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Observations	379,758	379,758	372,132	379,758	379,758	372,132
Outcome mean	0.040	0.040	0.040	0.040	0.040	0.040
Adj. R-squared	0.056	0.058	0.059			
Oster (2019) delta	2.356	2.512	2.432			
K-P F-statistic				19,378.4	19,532.8	19,196.5

Note: Columns 1 and 4 replicate the OLS and IV results in columns 2 and 1 of Tables 2 and 4, respectively. Columns 2 and 5 here then replace county fixed effects with SEA fixed effects. Since we want to control for county-level disability rates, we cannot use the county fixed effects. The county-level disability rates are then controlled for in columns 3 and 6 to show that aggregate disability rates do not explain away the household-level treatment effect. The notes of Tables 2 and 4 detail the control sets. Observations are weighted by the number of links that connected them from the 1860 to the 1870 census to give more weight to higher-quality links. Standard errors are clustered at the household-level and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Effect of Regiment-Level Disability Exits on Soldier's Disability Risk - First Stage

	Outcome: Pr(veteran disabled)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disability risk	1.1336***	1.1519***	1.1321***	1.1463***	1.1519***	1.1648***	1.2883***	1.1694***
	(0.0081)	(0.0081)	(0.0084)	(0.0097)	(0.0081)	(0.0093)	(0.0172)	(0.0121)
County FE	yes	yes	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes	yes	yes
Military ctrl		yes	yes	yes	yes	yes	yes	yes
Regiment ctrl			yes	yes	yes	yes	yes	yes
Women's 1860 ctrl				yes		yes		
Post-double selection					yes	yes		
High-risk							personal	unit
Observations	370,067	370,067	370,067	250,912	370,067	250,912	113,746	177,666
Outcome mean	0.110	0.110	0.110	0.114	0.110	0.114	0.160	0.162
K-P F-statistic	19,348.1	20,252.2	18,052.6	13,829.5	20,252.2	15,506.2	5,636.5	9,388.9
Adj. R-squared	0.219	0.227	0.230	0.234	0.227	0.232	0.174	0.196

Note: First-stage OLS regressions of Union Army veterans' disability status in 1870 on the overall disability risk in their military unit. The regimental disability risk is computed as the share of other soldiers who left the veteran's regiment r due to disability, excluding the veteran's own company. Regiments (∼1,000 soldiers) nest companies (∼100 soldiers). The sample consists of Union Army veterans who were the heads of households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects. Column 1 adds baseline controls (age, age-squared, birthplace) and 1860 household head characteristics (inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions). Column 2 adds military controls (age at enlistment and its square, rank, and enlistment date). Column 3 adds 1860 regiment composition controls based on the other soldiers in a veteran's regiment (share in labor force, literacy, household head status, urban residence, farm status, school attendance, average surname length, number of children, families in household, age, and prewar employment shares by skill and industry). Column 4 adds 1860 characteristics of women's household head (if different from their 1870 household head), including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5–6 use the LASSO-based post-double selection estimator Belloni et al. (2014), with and without column 4 controls. Columns 7–8 restrict the sample to soldiers at higher risk of becoming disabled: column 7 includes only those having ever participated in any of the major battles listed in Selcer (2006); column 8 includes regiments with above-median battle participation. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Battle Scale Effects on Union Army Disability Exit Rates

	Outcome: Pct. of UA Soldiers Exiting w Disability 90 Days Post-Battle			
	(1)	(2)	(3)	(4)
No. soldiers (000s)	0.009***	0.021**	0.020**	
	(0.003)	(0.009)	(0.008)	
Total artillery		0.035***		
		(0.011)		
No. Union Army artillery			-0.011	-0.017
			(0.016)	(0.018)
No. Confederate Army artillery			0.058***	0.063***
			(0.016)	(0.018)
Union Army soldiers (000s)				0.023***
				(0.008)
Confederate Army soldiers (000s)				0.014
				(0.013)
Observations	618	618	618	618
Outcome mean	1.710	1.710	1.710	1.710
R^2	0.017	0.026	0.032	0.033

Note: OLS regressions of the share of Union Army soldiers who left service with a recorded wound or disability within 90 days of a given engagement. The sample consists of 618 battles and skirmishes between the Union and Confederate Armies. All regressions include year and month fixed effects, as well as the variables for which coefficients are reported in the table. The number of soldiers is reported in thousands. Robust standard errors in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Using Battle Assignment as Alternative Instrument

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0016	0.0126***	0.0120***	0.0092***	0.0120***	0.0112***
	(0.0026)	(0.0025)	(0.0027)	(0.0033)	(0.0027)	(0.0032)
County FE	yes	yes	yes	yes	yes	yes
Individual ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Veteran ctrl		yes	yes	yes	yes	yes
Regiment ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,912
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
K-P F-statistic	26,660.5	27,489.1	24,438.5	17,381.2	24,438.5	17,590.8

Note: Instrumental variables regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. The disability indicator is instrumented with an indicator for whether the soldier had participated in one of the 19 largest battles of the war from the list of 20 battles provided by Selcer (2006). The 20th battle, Fort Fisher, was mainly a naval engagement which is not covered in our data. The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: IV FLFP Effect of Disabled Veteran HH Head - Alternative Standard Errors

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0070**	0.0166***	0.0162***	0.0149***	0.0162***	0.0162***
	(0.0032)	(0.0031)	(0.0033)	(0.0038)	(0.0033)	(0.0038)
S.E. clustered by:						
County	0.0034	0.0034	0.0035	0.0042	0.0035	0.0042
SEA	0.0040	0.0037	0.0035	0.0042	0.0035	0.0042
Regiment id	0.0034	0.0033	0.0035	0.0039	0.0035	0.0039
1x1 degree grid	0.0033	0.0031	0.0032	0.0036	0.0032	0.0036
HH-Regiment	0.0034	0.0033	0.0035	0.0039	0.0035	0.0039
HH-County	0.0034	0.0034	0.0035	0.0042	0.0035	0.0042
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl		yes	yes	yes	yes	yes
Military ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,912
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
K-P F-statistic	19,348.1	20,252.2	18,052.6	13,719.6	18,052.6	13,829.5

Note: Instrumental variables regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability with different variants of estimating standard errors including clustering at the county of residence, the state economic area, the veteran's regiment identifier, or the 1-by-1-degree grid cell to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011), and two-way clustering by household and regiment, and by household and county of residence. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The disability indicator is instrumented with the share of other soldiers who left the veteran's regiment due to disability, excluding the veteran's own company. Regiments (∼1,000 soldiers) nest companies (∼100 soldiers). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. In the top row, standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: LFP Effects for Women with Disabled Veterans at Home - Unweighted IV

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0016	0.0126***	0.0120***	0.0092***	0.0120***	0.0112***
	(0.0026)	(0.0025)	(0.0027)	(0.0033)	(0.0027)	(0.0032)
County FE	yes	yes	yes	yes	yes	yes
Individual ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Veteran ctrl		yes	yes	yes	yes	yes
Regiment ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,927	370,067	250,912
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
K-P F-statistic	26,660.5	27,489.1	24,438.5	17,381.2	24,438.5	17,590.8

Note: Instrumental variables regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The disability indicator is instrumented with the share of other soldiers who left the veteran's regiment due to disability, excluding the veteran's own company. Regiments ($\sim$1,000 soldiers) nest companies ($\sim$100 soldiers). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

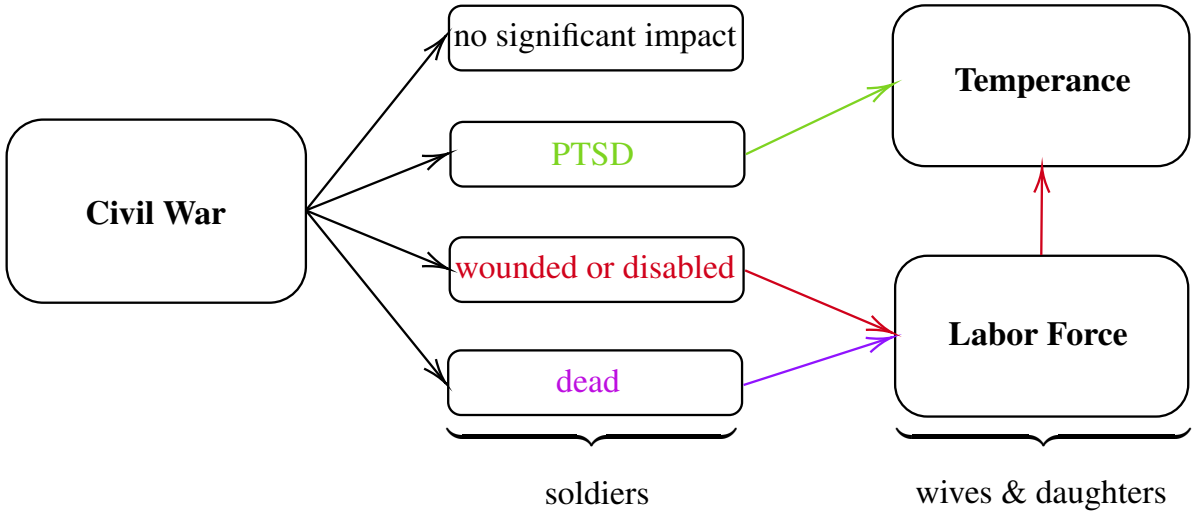
Table A.17: LFP Effects for Women with Disabled Veterans at Home - Efficient IV

	Outcome: Pr(wife/daughter works$_{1870}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0070**	0.0166***	0.0162***	0.0149***	0.0162***	0.0162***
	(0.0032)	(0.0031)	(0.0033)	(0.0038)	(0.0033)	(0.0038)
County FE	yes	yes	yes	yes	yes	yes
Individual ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Veteran ctrl		yes	yes	yes	yes	yes
Regiment ctrl			yes	yes	yes	yes
Women's 1860 ctrl				yes		yes
Post-double selection					yes	yes
Observations	370,067	370,067	370,067	250,912	370,067	250,912
Outcome mean	0.039	0.039	0.039	0.041	0.039	0.041
K-P F-statistic	19,348.1	20,252.2	18,052.6	13,829.5	18,052.6	13,829.5

Note: Instrumental variables regressions of the 1870 labor force participation status of wives and daughters of Union Army veterans on an indicator for a veteran's war-related disability. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability status includes wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The disability indicator is instrumented with a predicted disability risk measure which is predicted via a Probit regression by regressing the disability indicator on the share of disabled soldiers in the veteran's regiment and regiment-level controls, excluding the veteran's own company. The predicted probability of disability is then used as instrument for observed disability status. Notice that these are *not* the fitted values from the Probit regression (which would make this 2SLS regression a forbidden regression), but the predicted probabilities (see Angrist and Pischke, 2009). Regiments (∼1,000 soldiers) nest companies (∼100 soldiers). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to the 1870 census using the Census Tree crosswalks by Price et al. (2021). All regressions include 1860 county fixed effects and baseline controls (age, age-squared, birthplace). Additional 1860 veteran household head controls include the inverse hyperbolic sine (asinh) of occupational income and wealth, indicators for literacy, school attendance, labor force participation, urban residence, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions. Military controls include age at enlistment and its square, rank, and enlistment date. Women's 1860 controls include characteristics of their household head in 1860 (if different from 1870 household head) including the asinh of their occupational income score, asinh of personal and real estate wealth, and indicators for literacy, urban residence, and farm status. These controls require women to be linked back to 1860, which loses observations due to the double-linking step. Columns 5 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014) with and without the backward linked characteristics from column 4. Observations are weighted by the number of census linking methods that successfully matched each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the household level and reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figures

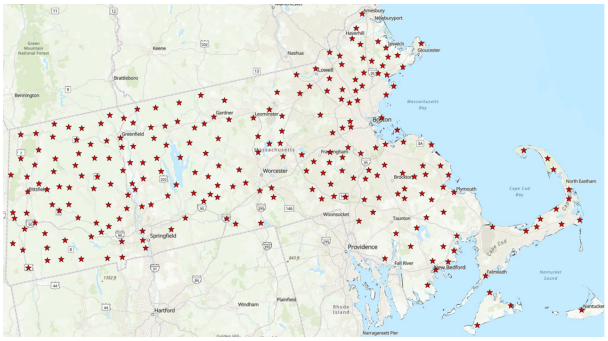
Figure A.1: Conceptual Framework Linking Soldiers' Postwar Status to Women's Outcomes



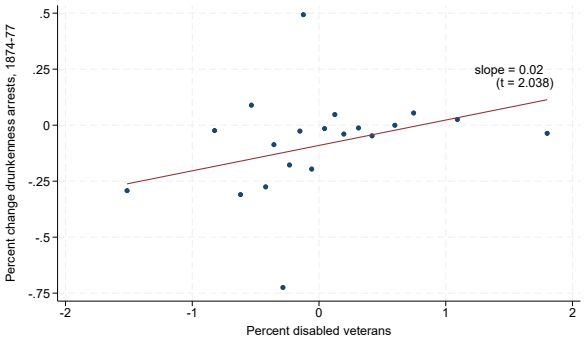
Note: Conceptual framework of the relationship between different Civil War-related shocks affecting veterans and their wives' and daughters' participation in the labor force as well as in the temperance movement. While war is a life-altering experience, no significant impact is meant to say that a soldier returned without physical or mental issues that would affect their work or family. We posit that post-traumatic stress disorder (PTSD) impacts women's participation in the temperance movement but leaves their labor force participation unchanged. There could be a direct effect on women's labor force participation but we rule this out in our data. Likewise, dead soldiers should only affect the women's labor force status but without incentivizing them otherwise to join the temperance movement. Wives and daughters living with wounded or disabled veterans are assumed to be affected in terms of their labor force participation, and that their entering the workforce impacts their political participation. In theory, there could be a direct effect, however, we find no empirical evidence for this.

Figure A.2: Drunkenness Arrests 1874-77 and Disabled Veterans in Massachusetts

(a) Towns with Arrest Data in Massachusetts



(b) Regressing Drunkenness Arrests on Disab. Veteran Shares

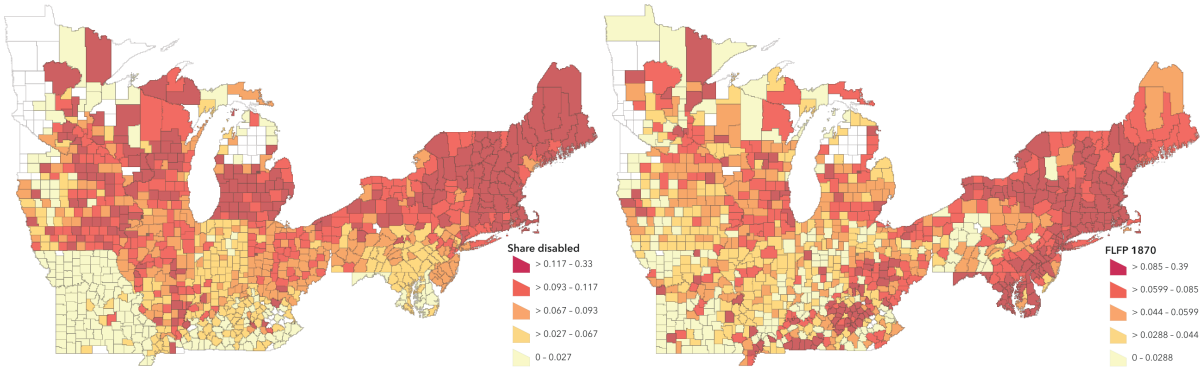


Note: Panel a plots the 239 towns with information on arrests for drunkenness which we digitized from Carroll D. Wright (1879) "*Statistics of Drunkenness and Liquor Selling Under Prohibitory License Legislation, 1874 and 1877*" in the 10th Annual Report of the Massachusetts Bureau of Statistics of Labor, published by Rand, Avery & Co., Printers of the Commonwealth, Boston, MA. The data were geolocated and matched with town-level information using the Census Place Project (Berkes et al., 2023). Panel b reports a binned scatter plot of the relationship between the change in the number of drunkenness arrests per capita between 1874 and 1877 over the percentage of disabled Civil War veterans in each town with the corresponding regression line, controlling for the share of veterans and total population. The slope and t-statistic of the slope coefficient are reported in the graph, based on Conley (1999) standard errors with a 100km distance cutoff. The regression shows an increase in the change in drunkenness arrests of 2 percentage points for each percentage-point increase in the disabled veteran share. For a one standard deviation increase in the disabled veteran share (5.79 percent), this implies an 11.2 percent increase in the outcome. It is important to note that this is a correlation and that individual or group-level behavior cannot be inferred from aggregate data. This exercise serves as correlational suggestive evidence for the link between drunkenness arrests and the share of disabled Civil War veterans.

Figure A.3: Mapping the Share of Disabled Veterans and Women's LFP in 1870

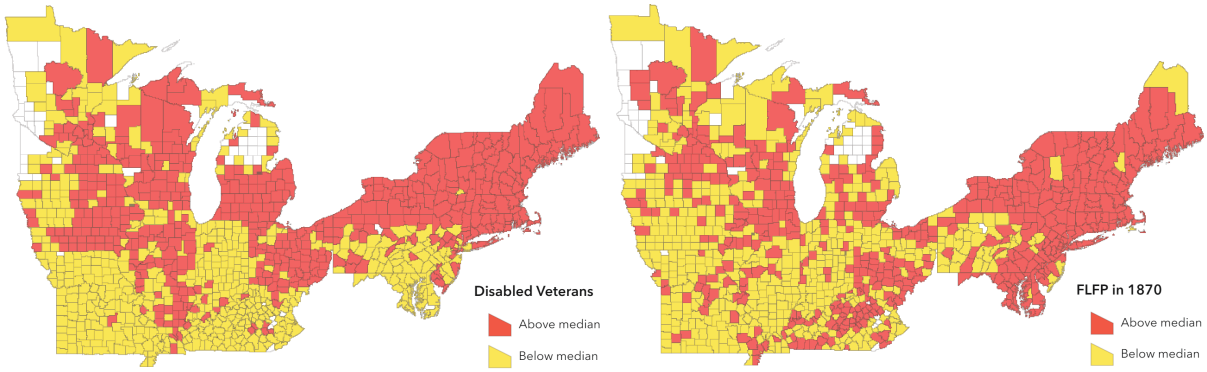
(a) Share disabled soldiers

(b) FLFP 1870



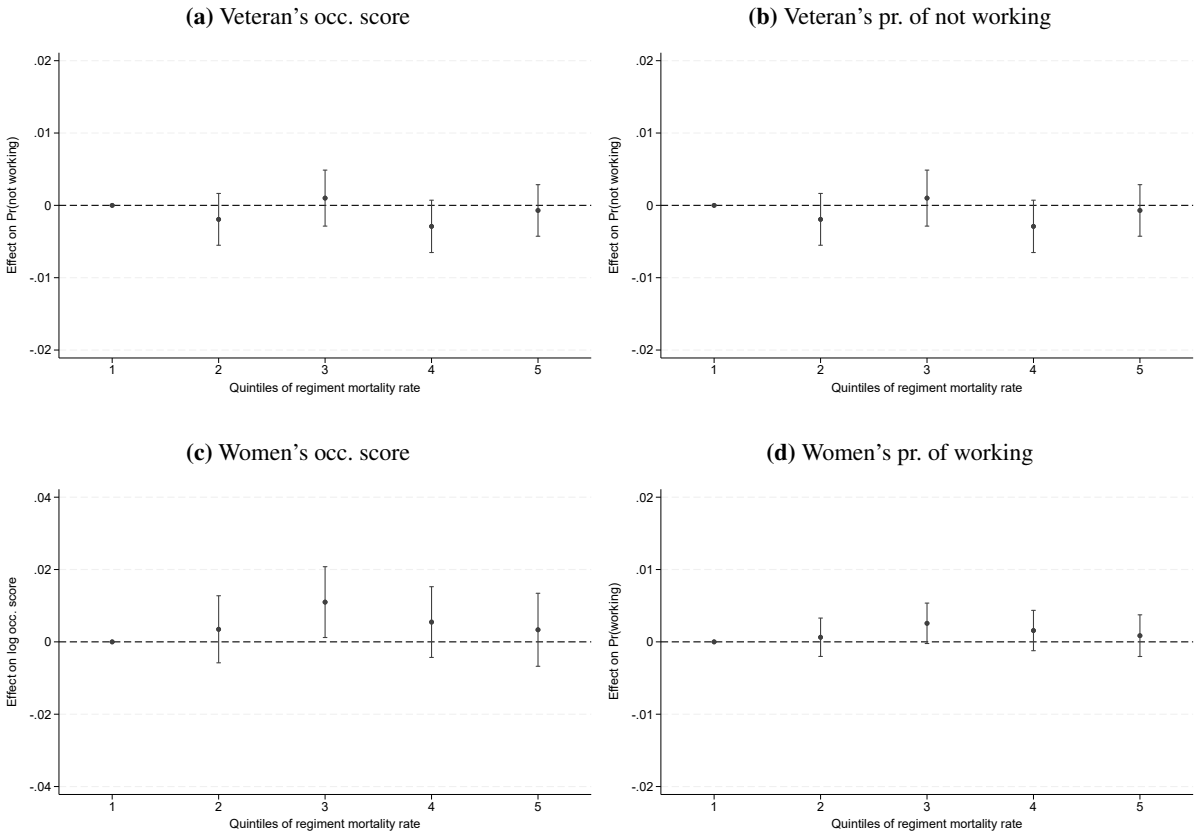
(c) Share disabled soldiers - median split

(d) FLFP 1870 - median split



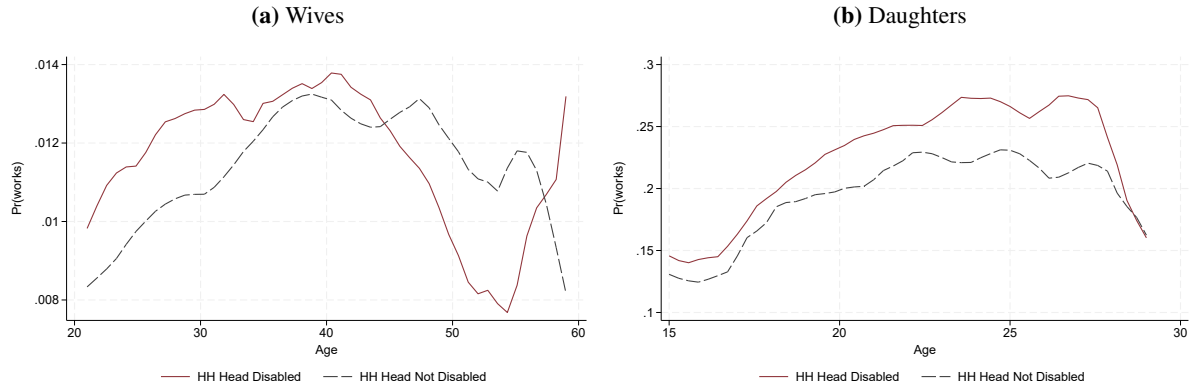
Note: Maps show the share of disabled Union Army veterans (panel a) and the share of working women (panel b) in the respective 1870 counties. Values for both variables were subdivided by quintiles. The share of disabled veterans was computed as the number of soldiers who left the Union Army with a recorded disability over the total number of soldiers from a county using the data from the Adjutant General's Reports. Disability includes exits due to wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. The share of working women was computed from the full-count 1870 census file for women aged between 15 and 65, averaging an indicator for whether they had a recorded occupation excluding missing, N/A, and non-occupational responses in the occ1950 variable (codes 0 to 970, inclusive). Panels c and d show the same information as in the top two panels but splitting each variable above and below their respective median values.

Figure A.4: Impact of PTSD on Employment Outcomes - Exposure to Casualties



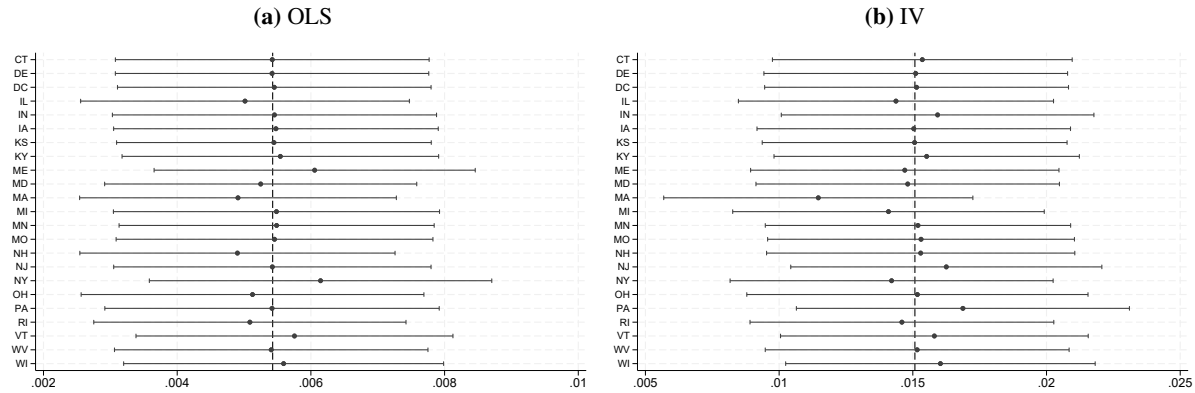
Note: Coefficients from OLS regressions of the inverse hyperbolic sine (asinh) of a veteran's occupational income score (panel a), a veteran-specific indicator for not working (panel b), the asinh of a veteran's wife's or daughter's occupational income score (panel c), and an indicator for a veteran's wife or daughter working (panel d) in 1870 regressed on the quintiles of the share of soldiers killed in a veteran's regiment, excluding the veteran's own company, a proxy for post-traumatic stress disorder. The sample only includes soldiers who returned from the war without a recorded disability (panels a and c), as well as wives and daughters of veterans without a recorded disability (panels b and d). The baseline comparison group are veterans in units in the bottom quintile of the regimental mortality rate. Regression specifications are the same as in Table 1 (columns 2 and 4) for veterans, as well as those in Table 2 (column 2) for wives and daughters. Standard errors are clustered at the 1860 county of residence for veterans and at the household level, following the main regression specification. Resulting confidence intervals reported at the 95 percent level.

Figure A.5: Age-Employment Profile for Wives and Daughters by Treatment Status



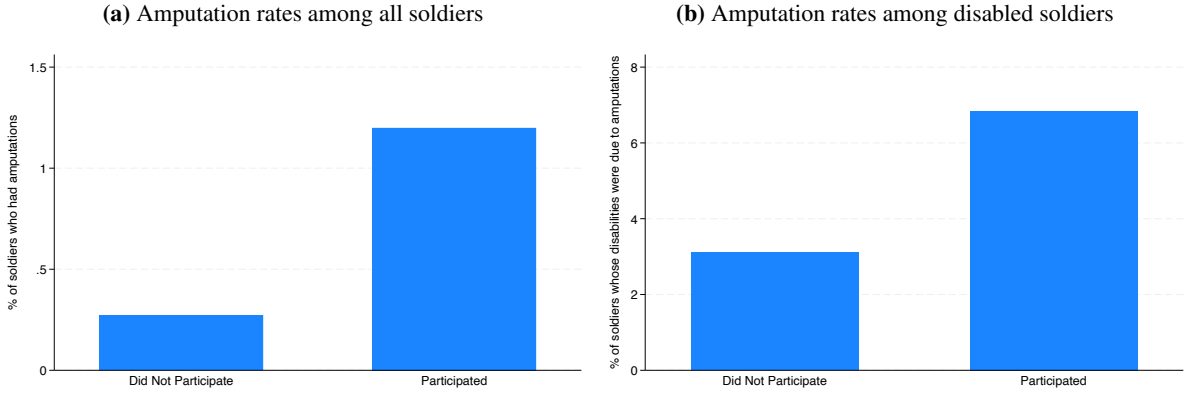
Note: Probabilities of being gainfully employed over age for wives and daughters of Union Army veterans living with a veteran household head in 1870. Panel a plots the age-employment profile for wives of veterans, panel b plots the same for daughters of veterans. The graphs further distinguish whether the veteran household head returned from the war with or without a disability. Employment is defined as having a recorded occupation in the census, excluding missing, N/A, and non-occupational responses. Graphs are based on the 1870 full-count census and the linked Union Army roster data introduced in section 2.2.

Figure A.6: Veteran Disability Effect on FLFP - Excluding States One-by-One



Note: Graphs show results from replicating column 3 of the OLS (Table 2) and IV (Table 4) estimates of the effect of living with a disabled veteran household head on the wives' and daughters' labor force participation in 1870 excluding states one-by-one. For each regression, the point estimate is plotted with the 95 percent confidence interval which was computed from cluster robust standard errors that were clustered by unique household identifier. Panel a shows the OLS estimates and panel b reports coefficients from the IV regressions. The dashed black line in each figure marks the OLS or IV estimate from using the full sample. The excluded state in a given regression is reported on the vertical axis. All point estimates are significantly different from zero.

Figure A.7: Disability Severity by Participation in Major Battles



Note: Share of soldiers with recorded amputations among all soldiers (panel a) and share of soldiers with amputations among soldiers who left the Union Army due to disability by whether they participated in one of the 19 largest battles of the war. The list of 19 major battles was taken from Selcer (2006), excluding the twentieth battle which was a naval engagement and is therefore not covered in our data. Amputation rates among disabled soldiers in panel b are not tautological since disabilities include a broad range of conditions that disqualified soldiers from service, such as wounds, physical disabilities and amputations, but also diseases and chronic illnesses (e.g. dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy, among others), and general conditions that made a soldier unfit to perform their military duties.

B.1 Sample Selection and Survivorship Bias

Sample selectivity: Given the way we constructed our sample, two things must hold for us to observe a veteran and their family members: while we observe deaths during the war years, we do not observe deaths between 1865 and when the census is taken in 1870. Hence, the soldier has to i) survive until 1870 and ii), if he was not married in 1860, his disability must not have been severe enough to impact his marriage outcomes. Survival to the next census year and marriage thus may be an outcome of the severity of the treatment.

To test this, we take the sample of all soldiers linked to the 1860 census and link them to 1870 using the Census Tree Project crosswalks, after which we generate an indicator for whether a soldier was not linked.[1] Table B.1 reports results from this exercise, where we regress this indicator on soldiers' disability status and the standardized instrument in columns 1 and 2 — the instrument, which is introduced in section 3.3, is the share of other soldiers in a given soldier's regiment who left the unit with a disability while excluding the soldier and his own company. While we find a positive and significant effect of both variables, the effect is relatively small. Being disabled increases the probability of not being linked to 1870 by 1.1 percentage points conditional on a broad range of prewar controls. This is relative to an average share of 36.7 percent of soldiers who were not linked to 1870. Likewise, a standard deviation increase in the instrument is associated with a 0.59 percentage-point increase in the probability of not being linked.

We also find small and significant negative effects on marriage with disability and a standard deviation increase in the instrument reducing the marriage probability by 0.8 and 0.7 percentage points, respectively, compared to 20 percent of soldiers who are married (columns 3 and 4). The marriage result is conditional on surviving to 1870. This indicates that survivorship and selection may depend on disability status and the severity of the disability, however, the effect is relatively modest. We also find that disability status negatively correlates with the probability that wives and daughters lived with the same veteran in 1870 who they had lived with in 1860. This might reflect new household formations by daughters who marry, wives leaving their husbands, or soldiers dying between 1865-70. When we repeat the same exercise with the instrument, we find a positive but much smaller effect, which is 0.4 and 0.9 percentage points for the wives and daughters, respectively, relative to an unconditional probability of around 62 percent.

Bias direction: The direction of the selection and survivor biases discussed above are likely going to go against our main results. Veterans with the worst disabilities are the ones most likely to not survive until 1870 and, conditional on survival, to not marry. Had these veterans lived and married, they arguably would also have had the largest effect on their wives' and daughters' labor market participation and temperance activism. In this case, we do not observe the individuals at the highest treatment levels, meaning that our estimates will understate the true average treatment effect. We provide evidence of such treatment effect monotonicity using a matched sample of non-disabled soldiers and disabled soldiers for whom we can observe different types of amputations (see Table A.5). To give an example of this downward bias, we re-estimate the results in Table 1 of disability status on veterans' labor market outcomes but using a sample of veterans who were already married in 1860. Since divorce rates were

[1]The total number of soldiers linked to the 1860 census with non-missing values in observable characteristics in 1860 is 746,714 individuals.

very low at the time, these veterans' marriage markets had resolved prior to becoming disabled. The results are reported in Table B.2 and show a stronger negative effect of disability on occupational income scores. Using the full set of controls and regiment fixed effects, relative to a non-disabled veteran, a disability reduced veterans' income scores by 2.4 percent instead of 1.6 percent in the main table.

Potential remedies: We address selection bias in various ways. First, even though the instrumental variables approach itself does not directly fix sample selection and survivorship bias, Table B.1 shows that while there is a correlation with measures of selectivity, these tend to be significantly smaller for the instrument. Therefore we would expect the bias in the IV estimates to be smaller as well but it will not eliminate the bias. Second, we re-estimate the main results using inverse propensity score reweighting in Table B.3. The inverse propensity score weights (IPW) were estimated using the full sample of veterans linked to 1860, regressing an indicator for whether a veteran was linked to 1870 on a wide range of prewar characteristics. Dividing the share of linked individuals by the predicted probabilities of linking then yields the IPW. If the severity of disabilities depends on soldiers' prewar socioeconomic characteristics, then this approach will further reduce and possibly eliminate the issue of differential linkage by treatment intensity. Table B.3 shows that results are largely unchanged by the IPW reweighting approach.

Table B.1: Estimating the Potential Importance of Sample Selection

	Pr(not linked$_{1870}$)		Pr(married$_{1870}$)		Pr(lives in a different HH$_{1870}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Veteran disabled	0.0105***		-0.0079***		-0.0507***		-0.0671***	
	(0.0018)		(0.0012)		(0.0023)		(0.0026)	
Disability risk		0.0059***		-0.0067***		0.0041***		0.0085***
		(0.0007)		(0.0004)		(0.0008)		(0.0008)
Sample	Soldiers	Soldiers	Soldiers	Soldiers	Wives	Wives	Daughters	Daughters
Observations	746,714	746,714	746,714	746,714	386,115	386,115	327,483	327,483
Outcome mean	0.367	0.367	0.200	0.200	0.613	0.613	0.629	0.629
Adj. R-squared	0.026	0.026	0.249	0.249	0.143	0.142	0.078	0.076

Note: The table reports tests for soldiers' sample attrition between 1860 and 1870, and for selection into marriage in 1870 (columns 1-4), as well as tests for whether the wives and daughters of veterans remain in the same household (columns 5-8). The sample of soldiers consists of all soldiers linked to the 1860 census. Soldiers were then linked to the 1870 census using the crosswalks provided by the Census Tree Project (Price et al., 2021). Soldiers and their families were dropped from the sample if the soldier died by the end of the war in 1865 (such that we observe the death record), however, remaining soldiers may have succumbed to their wounds or disabilities between 1865-70 before the census was taken. The outcome in columns 1 and 2 is a dummy that equals one for soldiers who were not linked to 1870, which is regressed on the main disability indicator as well as on the instrument (the share of other soldiers who left the veteran's regiment due to disability, excluding the veteran's own company). The outcome in columns 3 and 4 is an indicator for whether a veteran is married in 1870 conditional on survival and being linked. Columns 5-8 consider the wives and daughters of veterans, testing whether they lived in the same household in 1870 as the soldier with whom they had lived in 1860. All individuals are of working age in 1870, i.e. age 15 to 65. Not living in the same household as the veteran may be due to new household formation (e.g. if a daughter marries), wives leaving their husbands, or because the husband died between 1865-70. All regressions control for individuals' prewar characteristics, including age, age squared, birthplace and county of residence fixed effects, occupational skill and industry group indicators, literacy, school attendance in 1860, farm status, urban status, group quarter indicators, the inverse hyperbolic sine of their occupational income score, and of their real estate and personal wealth, and the number of children in their household. Standard errors are clustered at the regiment the soldier served in during the war. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Labor Market Outcomes of Disabled Veterans Already Married in 1860

	Asinh(occupational income score$_{1870}$)			Pr(left labor force$_{1870}$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Soldier disabled	-0.035***	-0.022**	-0.024**	0.003	0.004*	0.004*
	(0.009)	(0.009)	(0.010)	(0.002)	(0.002)	(0.002)
County FE	yes	yes	yes	yes	yes	yes
1860 ctrl		yes	yes		yes	yes
Regiment FE			yes			yes
Observations	104,453	104,453	104,251	104,453	104,453	104,251
Outcome mean	3.415	3.415	3.415	0.067	0.067	0.067
Adj. R^2	0.131	0.163	0.168	0.159	0.171	0.176

Note: OLS regressions of the inverse hyperbolic sine (asinh) of Union Army veterans' occupational income score (columns 1-3) and an indicator for not being in the labor force (columns 4-6) on an indicator for whether a veteran left the Union Army due to a disability. The sample includes Civil War veterans in 1870 who are between 20 and 65 years of age, and who were married to the same spouse in 1860. Veterans were linked to the 1860 census based on name, age, and residence, and to 1870 using the Census Tree crosswalks by Price et al. (2021). The disability indicator includes all types of disability-related exits from service, such as wounds, physical disabilities, but also diseases and chronic illnesses (e.g. dysentery, chronic diarrhea, rheumatism, consumption/TB, malaria, ophthalmia, scurvy, among others), and general conditions that made a soldier unfit to perform their military duties. All regressions include 1860 county of residence fixed effects. Prewar controls measured in 1860 include age, age-squared, number of own children, the asinh of the veteran's occupational income score, the asinh of their personal and real estate wealth, as well as indicators for birthplace, skill and industry groups based on the 1950 Census Bureau definitions, school attendance in 1860, literacy, urban residence, farm status, group quarter residence, and labor force participation status. Columns 3 and 6 further control for fixed effects for the regiment a soldier served in during the war. Observations are weighted by the number of census linking methods that successfully linked each individual from the 1860 to the 1870 census to give more weight to high-confidence links. Standard errors are clustered at the 1860 county of residence and are reported in parentheses. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Addressing Potential Sample Selection via IPW Reweighting

	Veteran sample		Wives and daughters sample			
	asinh occ. score$_{1870}$	Left labor force$_{1870}$	Works$_{1870}$		HH head disab.	Works$_{1870}$
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	-0.0131**	0.0038***	0.0039***			0.0115***
	(0.0057)	(0.0014)	(0.0011)			(0.0026)
Disability risk				0.0124***	1.0836***	
				(0.0028)	(0.0073)	
Estimator	OLS	OLS	OLS	OLS (RF)	OLS (1st stage)	IV
Observations	294,453	294,453	370,067	370,067	370,067	370,067
Outcome mean	3.411	0.065	0.039	0.039	0.110	0.039
Adj. R-squared	0.092	0.101	0.062	0.062	0.216	
K-P F-statistic						21,925.4

Note: Results report estimations of columns 3 and 6 from Table 1, column 3 from Table 2, the reduced form, column 3 from Table A.12, and column 3 from Table 4 reweighting observations by their inverse propensity score weight (IPW) of being observed in the 1870 census. Estimates are therefore adjusted by the probability of survival to the 1870 census based on prewar characteristics. The IPW weights were computed by estimating a Probit regression using all soldiers observed in the 1860 census and where the outcome was an indicator for being observed in the 1870 census. Controls included a broad range of prewar observables, including age, age squared, indicators for being in school, literate, in the labor force, for urban residency, farm status, group quarters, state and birthplace fixed effects, skill and industry groups, as well as the number of children in the household, and the inverse hyperbolic sine of the occupational income score and wealth (personal and real estate wealth). The IPW was then computed by dividing the share of linked individuals q by the predicted probabilities $\widehat{p}$ from the Probit regression after trimming the top and bottom one percent to avoid extreme outliers. Controls and treatments of standard errors are described in the relevant table notes.

Table B.4: OLS and IV Estimates for Wives' and Daughters' LFP - Matched Sample

	Outcome: Pr(wife/daughter works$_{1870}$)					
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
HH head disabled	0.0056***	0.0057***	0.0058***	0.0095***	0.0106***	0.0106***
	(0.0014)	(0.0014)	(0.0014)	(0.0022)	(0.0026)	(0.0026)
County FE	yes	yes	yes	yes	yes	yes
Baseline ctrl	yes	yes	yes	yes	yes	yes
HH head 1860 ctrl	yes	yes	yes	yes	yes	yes
Military ctrl		yes	yes	yes	yes	yes
Post-double selection			yes			yes
Observations	77,255	77,255	77,276	77,255	77,255	77,255
Outcome mean	0.044	0.044	0.044	0.044	0.044	0.044
Adj. R-squared	0.075	0.085	0.087			
Oster (2019) delta	53.9	83.6	221.7			
K-P F-statistic				48,836.4	30,997.0	30,997.0

Note: OLS and IV regressions of an indicator for women's labor force participation in 1870 on an indicator for living in a household headed by a disabled Union Army veteran using a matched sample of women. The dependent variable is an indicator for having a recorded occupation in the 1870 census, excluding missing, N/A, and non-occupational responses. Disability includes exits due to wounds, diseases (e.g., dysentery, TB, malaria, scurvy), and general chronic conditions. In columns 4 to 6, the disability indicator is instrumented with the share of other soldiers who left the veteran's regiment due to disability, excluding the veteran's own company. Regiments ($\sim$1,000 soldiers) nest companies ($\sim$100 soldiers). The sample includes wives and daughters aged 15–65 in veteran-headed households in 1870. Veterans were linked to the 1860 census based on name, age, and residence, and to 1870 using the Census Tree crosswalks by Price et al. (2021). Those women whose household was headed by a disabled veteran were matched to women in households with non-disabled veterans based on their age, age squared, birthplace, the veteran household head's prewar characteristics in 1860 (asinh of occupational income and wealth, and indicators for literacy, schooling, labor force, urban status, group quarters, foreign birth, skill and industry groups based on the 1950 Census Bureau definitions), and the veteran's military observables (rank, age at enlistment and its square, and enlistment date). All regressions control for matched-pair fixed effects. Columns 1 to 3 report the delta statistic by Oster (2019) to test for sensitivity with respect to unobservable characteristics. Columns 3 and 6 use the LASSO-based post-double selection estimator by Belloni et al. (2014). Standard errors are clustered at the matched-pair level and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Rationale for Weighting by Link Quality

When individuals are linked across census years, some links are inevitably incorrect. If treatment status is constructed from linked records, incorrect links introduce measurement error in the treatment variable, attenuating OLS estimates toward zero. This section shows that weighting observations by the number of linking methods that agree on a match can reduce this attenuation bias under certain conditions. We measure this variable, W, as the number of linking methods that successfully linked a given person from the 1860 to 1870 census, which are provided in the Census Tree crosswalks (Price et al., 2021).

Setup. Suppose we want to estimate $Y = \alpha + \beta T^* + \epsilon$, where T^* is a binary treatment that we only observe through a mismeasured version T. An observation is incorrectly linked with probability κ, and incorrect links can change observed treatment status: a truly treated individual ($T^* = 1$) is misclassified as $T = 0$ with probability τ, while a truly untreated individual ($T^* = 0$) is misclassified as $T = 1$ with probability ϕ, where τ and ϕ need not be equal.[1] When no incorrect links occur ($\kappa = 0$) or incorrect links do not alter treatment status ($\tau = \phi = 0$), we have $T = T^*$. More generally, the relationship between observed and true treatment is $T = \kappa\phi + \Delta T^* + \nu$, with $E(\nu|T^*) = 0$ and where $\Delta = 1 - \kappa(\tau + \phi) \in (0, 1]$ measures the degree of alignment between T and T^*.[2] We assume $\kappa(\tau + \phi) < 1$, so that incorrect links do not affect the majority of observations.

OLS attenuation. Let $p^* = \Pr(T^* = 1)$, so that $Var(T^*) = p^*(1 - p^*)$. The observed treatment has mean $\mu = \kappa\phi + \Delta p^*$ and variance $Var(T) = \mu(1 - \mu)$, where misclassification shifts μ away from p^* and distorts the variance. Since $Cov(T^*, T) = Var(T^*)\Delta$, the OLS estimator is attenuated: $\text{plim}\widehat{\beta}_{\text{OLS}} = \beta \cdot AF(\Delta)$, where

$$AF(\Delta) \equiv \frac{p^*(1 - p^*)\,\Delta}{\mu(\Delta)\big(1 - \mu(\Delta)\big)} \tag{B.1}$$

and $\mu(\Delta) = (1 - \Delta)\frac{\phi}{\tau+\phi} + \Delta p^*$ is linear in Δ. The attenuation factor captures both the direct effect of misclassification on the covariance between T and T^* (the numerator) and its indirect effect on the variance of the observed treatment (the denominator). By construction, $AF(1) = 1$ when $T = T^*$ and $AF(0) = 0$ when T is pure noise. In the special case where misclassification preserves $Var(T)$,[3] the attenuation factor simplifies to $\Delta = 1 - \kappa(\tau + \phi)$, the standard result for a misclassified binary treatment (see Bingley and Martinello, 2017).

The key property of $AF(\Delta)$ is that it is strictly increasing in Δ. Letting $a = \frac{\phi}{\tau+\phi}$, the sign of $\frac{dAF}{d\Delta}$ is determined by $(\mu - a)^2 + a(1 - a)$, which is strictly positive since both terms are non-negative and cannot simultaneously equal zero. Intuitively, a higher alignment rate simultaneously strengthens the covariance between T and T^* and brings $Var(T)$ closer to $Var(T^*)$, so both components of the bias improve together.

Weighting by link quality. The monotonicity of AF implies that any weighting scheme which effectively raises Δ will reduce the attenuation bias. To formalize this, let W denote the number of linking

[1] The asymmetry between τ and ϕ is natural in historical census linking: certain treatment statuses may be more or less stable to perturbations in the linked individual's characteristics.

[2] An alternative interpretation is $\Delta =$ sensitivity + specificity $- 1$, which measures the separation between T and T^*.

[3] This requires $p^* = \mu$, i.e., $p^* = \frac{\phi}{\tau+\phi}$, such that $Var(T) = Var(T^*)$.

methods that agree on a match, and allow the incorrect linking probability to depend on W, so that $\Delta(W) = 1 - \kappa(W)(\tau + \phi)$. This requires two substantive assumptions. First, the misclassification probabilities τ and ϕ are constant across W: the number of agreeing methods affects the *probability of a wrong link* but not the *consequences of a wrong link* for treatment status. This is plausible when misclassification conditional on an incorrect link depends on the structure of the treatment variable rather than on linking method agreement. Second, true treatment T^* is independent of W: the number of methods that successfully link an individual does not correlate with treatment status. In practice, both conditions need to hold conditional on covariates that determine the linking probability, such as age, foreign birthplace, education, or income.

Under these assumptions, because T is binary the moments aggregate across heterogeneous $\kappa(W)$ in the same functional form as the homogeneous case, and the unweighted OLS estimator satisfies $\text{plim}\widehat{\beta}_{\text{OLS}} = \beta \cdot AF(\bar{\Delta})$, where $\bar{\Delta} = 1 - E[\kappa(W)](\tau + \phi)$. The weighted OLS (WOLS) estimator, which weights observations by W, satisfies

$$\text{plim}\widehat{\beta}_{\text{WOLS}} = \beta \cdot AF(\Delta_w) \tag{B.2}$$

where $\Delta_w = 1 - E_w[\kappa(W)](\tau + \phi)$ and $E_w(\cdot)$ denotes the W-weighted expectation. If $\kappa(W)$ is weakly decreasing in W, i.e., observations linked by more methods are less likely to be incorrectly linked, then $\Delta(W)$ is weakly increasing in W, and the gain from weighting is

$$\Delta_w - \bar{\Delta} = \frac{Cov(W, \Delta(W))}{E(W)} \geq 0.$$

Since $AF(\cdot)$ is strictly increasing, $AF(\Delta_w) \geq AF(\bar{\Delta})$, and WOLS is less attenuated than OLS. This is reflected in the comparison of the weighted main results in Table 2 with the unweighted results in Table A.9. While weighting by link quality does not fully remove the bias, it is an easy and transparent way to reduce it.

C Temperance Crusade Appendix

Tables

Table C.1: Town-Level Summary Statistics

	Obs.	Mean	St. Dev.	Min.	Max.
Temperance crusade protest in 1873 (binary)	8,882	0.07	0.25	0.00	1.00
Temperance crusade protest in 1873 (count)	8,882	0.55	1.10	0.00	11.00
No. of protest towns within 20mi	8,875	2.13	3.19	0.00	27.00
FLFP, 1870	8,882	0.02	0.02	0.00	0.25
FLFP, 1860	8,882	0.03	0.04	0.00	0.33
Pct. disabled veterans	8,882	0.09	0.08	0.00	1.00
Pct. veteran men	8,882	0.05	0.17	0.00	12.77
Pct. of disability exits	8,882	0.13	0.06	0.00	0.74
Pct. deceased soldiers	8,882	0.13	0.10	0.00	1.00
Pct. soldiers participated in large battle	8,882	0.30	0.15	0.00	1.00
Distance to nearest railroad, 1870	8,882	7.98	10.56	0.00	174.95
Telegraph connection, 1870	8,882	0.06	0.24	0.00	1.00
Newspapers per capita, 1870	8,881	0.00	0.00	0.00	0.07
Churches per capita, 1870	8,882	0.00	0.00	0.00	0.19
Pct. bartenders and waiters, 1870	8,882	0.03	0.09	0.00	1.98
Log population size, 1870	8,882	7.24	0.88	2.30	13.74
Mean real estate wealth per capita, 1870	8,882	525.70	410.45	0.00	21473.90
Mean personal wealth per capita, 1870	8,882	218.61	284.25	0.00	13418.54
Pct. Black population, 1870	8,882	0.01	0.04	0.00	0.83
Pct. urban population, 1870	8,882	0.05	0.20	0.00	1.00
Pct. female population, 1870	8,882	0.49	0.02	0.03	0.60
Pct. foreign-born, 1870	8,882	408.11	5622.14	0.00	419094.00
Pct. agricultural workers, 1870	8,882	0.18	0.08	0.00	0.48
Mean real estate wealth per capita, 1860	8,882	430.10	1417.30	0.00	54376.48
Mean personal wealth per capita, 1860	8,882	188.84	536.21	0.00	36805.46
Pct. Black population, 1860	8,882	0.01	0.02	0.00	0.93
Pct. urban population, 1860	8,882	0.03	0.14	0.00	1.00
Pct. female population, 1860	8,882	0.48	0.03	0.10	0.64
Pct. foreign-born, 1860	8,882	0.12	0.13	0.00	0.93
Pct. agricultural workers, 1860	8,882	0.59	0.23	0.00	1.00
Latitude	8,882	41.37	1.93	36.06	47.26
Longitude	8,882	-83.20	7.32	-96.39	-67.01

Note: Town-level summary statistics for 8,882 towns in former Union Army states excluding the Western U.S. Population data and characteristics for 1860 and 1870 were computed from the full-count decennial census of each year and matched to the town-level Temperance Crusade activity data by García-Jimeno et al. (2022) using the crosswalks provided by the Census Place Project (Berkes et al., 2023). The number of Temperance Crusade towns within a 20-mile radius was computed using the latitude and longitude information. Data on distance to the nearest railroad, newspapers per capita, and telegraph connection also come from García-Jimeno et al. (2022).

Table C.2: Disabled Civil War Veterans and Temperance Crusades - Alternative S.E. Estimation

	Outcome: Pr(Temperance Crusade$_{1873--74}$)							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share disabled	0.0052**	0.0046**	0.0052**	0.0046**	0.0190***	0.0159**	0.0132**	0.0123*
	(0.0023)	(0.0022)	(0.0023)	(0.0023)	(0.0066)	(0.0066)	(0.0064)	(0.0065)
0.5x0.5mi grid	0.0022	0.0022	0.0022	0.0022	0.0064	0.0064	0.0061	0.0061
2x2mi grid	0.0026	0.0026	0.0021	0.0026	0.0076	0.0076	0.0074	0.0073
County	0.0021	0.0021	0.0023	0.0021	0.0060	0.0061	0.0057	0.0058
SEA	0.0021	0.0020	0.0026	0.0021	0.0062	0.0061	0.0059	0.0059
Conley (100km)	0.0020	0.0022	0.0022	0.0023	0.0053	0.0052	0.0056	0.0054
Conley (200km)	0.0011	0.0010	0.0011	0.0013	0.0054	0.0060	0.0056	0.0060
Observations	8,882	8,882	8,882	8,882	8,882	8,882	8,882	8,882

Note: OLS and IV regressions of an indicator for the occurrence of Temperance Crusade activity in 1873–74 on the share of disabled Union Army veterans residing in the town in 1870 with different variants of estimating standard errors. These include changing grid cell sizes from 1-by-1-degree cells to .5-by-.5 and 2-by-2-degree cells, clustering at the county level, clustering at the state economic area (SEA) level, as well as (Conley, 1999) standard errors with 100 and 200km distance cut-offs. The sample consists of towns in the former Union states excluding Western states and territories. Columns 1-4 report the ordinary least squares results and columns 5-8 report the instrumental variables results where the share of disabled Civil War veterans in a town is instrumented with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own companies. Baseline controls include the log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). 1860 controls include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls, as well as the distance to the nearest railroad and an indicator for telegraph connection to account for potential information networks across towns. In the top row, standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011), and significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: IV Mediation Effects of Disabled Veterans on Protests through FLFP

	Outcome: Pr(Temperance Crusade)				
	(1)	(2)	(3)	(4)	(5)
total effect	0.044***	0.044***	0.039***	0.037***	0.038***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
direct effect	-0.002	-0.001	-0.002	-0.000	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
indirect effect	0.045**	0.045**	0.041**	0.037**	0.040**
	(0.020)	(0.020)	(0.018)	(0.017)	(0.018)
Baseline controls	yes	yes	yes	yes	yes
FLFP 1860		yes	yes	yes	yes
1860 controls			yes		yes
1870 controls				yes	yes
Observations	8,882	8,882	8,882	8,882	8,882
Mediation effect	103.46	103.03	104.06	100.15	104.12
F-Stat Treatment	467.48	468.61	460.03	422.22	422.02
F-Stat Mediator	14.81	14.65	13.55	15.21	12.65

Note: Town-level results from instrumental variables mediation analysis regressing the Temperance Crusade protest indicator on the share of disabled soldiers, instrumented with the average disability exit rate in soldiers' regiments during their time of service. The treatment is the share of disabled Civil War veterans in a town and the mediation variable is the share of working women in 1870. The estimator provided by Dippel et al. (2020) decomposes the overall effect into the direct effect of the share of disabled veterans on the outcome, and the indirect effect which measures how much of the disability impact on the outcome is due to its effect on women's labor force participation. There are two first-stage F-statistics from the regression of T on Z (treatment first stage), and from the regression of M on Z controlling for T (see Dippel et al. (2020) for details). The percentage of the overall treatment effect explained by FLFP is reported in the bottom part of the table labeled as mediation effect. All regressions include the following controls: log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Additional controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). For 1860, these include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Temperance Crusade Responses to Different Civil War-Related Shocks

	Outcome: Pr(Temperance Crusade$_{1873--74}$)				
	(1)	(2)	(3)	(4)	(5)
Share disabled	0.004*	0.010	0.005**	0.004*	0.005**
	(0.002)	(0.006)	(0.002)	(0.002)	(0.002)
Share killed	-0.000	0.000	0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Share battle exposure	0.004**	0.003	0.004**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Share disab. × killed			0.002		
			(0.002)		
Share disab. × battle exp.				0.001	
				(0.001)	
Share disab. × share veterans					-0.032
					(0.038)
Estimator	OLS	IV	OLS	OLS	OLS
Observations	8,882	8,882	8,882	8,882	8,882
R^2	0.201		0.201	0.201	0.201
K-P F-Stat		199.958			
Outcome mean	0.068	0.068	0.068	0.068	0.068

Note: OLS and IV regressions of an indicator for the occurrence of Temperance Crusade activity in 1873–74 on the share of disabled Union Army veterans residing in the town in 1870, the share of soldiers from the town who died during the Civil War, and the share of soldiers who were exposed to one of the largest battles of the war listed in Selcer (2006) as proxy for soldiers who potentially returned with PTSD. The sample consists of towns in the North and Midwest. Column 2 reports the instrumental variables results where the share of disabled Civil War veterans in a town is instrumented with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own companies. Columns 3 to 5 interact the share of disabled soldiers with the other Civil War treatments, as well as the share of veterans in the town. Baseline controls include the log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). 1860 controls include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls, as well as the distance to the nearest railroad and an indicator for telegraph connection to account for potential information networks across towns. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Disabled Soldiers' Effect on Temperance Crusades via Information Transmission

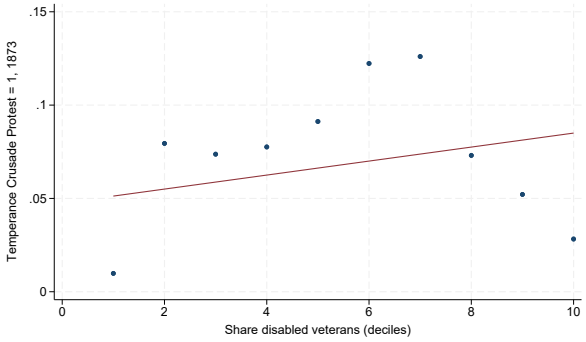
	Outcome: Pr(Temperance Crusade$_{1873--74}$)					
	Panel a: OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
Share disabled	0.002	0.004*	0.005**	0.005**	0.006**	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Main effect	0.037**	0.015	-0.008***	0.115***	0.039***	-0.004*
	(0.016)	(0.009)	(0.003)	(0.021)	(0.008)	(0.002)
Interaction effect	0.031*	0.001	-0.003	-0.000	0.028***	0.002
	(0.017)	(0.004)	(0.002)	(0.027)	(0.007)	(0.003)
Interaction with:	Bartenders	Protest towns in 20mi	Dist. railroad	Telegraph	Newspapers p.c.	Churches p.c.
Joint beta	0.033	0.005	0.002	0.004	0.035	0.006
t-stat	1.975	1.391	1.067	0.153	4.114	1.749
Observations	8,882	8,874	8,882	8,882	8,881	8,882
R^2	0.203	0.202	0.201	0.201	0.214	0.201
Outcome mean	0.068	0.068	0.068	0.068	0.068	0.068
	Panel b: IV					
	(1)	(2)	(3)	(4)	(5)	(6)
Share disabled	0.010	-0.007	0.013*	0.013**	0.013**	0.011
	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
Main effect	0.038**	0.015	-0.009***	0.115***	0.050***	-0.004
	(0.016)	(0.010)	(0.003)	(0.021)	(0.009)	(0.004)
Interaction effect	0.020	0.028**	-0.009**	-0.032	0.046***	0.006
	(0.027)	(0.013)	(0.004)	(0.043)	(0.008)	(0.008)
Interaction with:	Bartenders	Protest towns in 20mi	Dist. railroad	Telegraph	Newspapers p.c.	Churches p.c.
Joint beta	0.030	0.021	0.004	-0.019	0.058	0.017
t-stat	1.142	2.213	0.767	-0.455	5.647	1.754
Observations	8,882	8,874	8,882	8,882	8,881	8,882
K-P F-Stat	115.168	76.061	115.122	112.845	112.114	5.295
Outcome mean	0.068	0.068	0.068	0.068	0.068	0.068

Note: OLS (panel a) and IV (panel b) regressions of an indicator for Temperance Crusade activity in 1873–74 on the share of disabled Civil War veterans residing in a town in 1870. The share of disabled veterans is interacted with different factors capturing alcohol consumption or information transmission channels. Regressions include the corresponding main effects. The information channel variables ordered by column are an indicator for having a top-decile share of bartenders or waiters in the town, the number of towns with Temperance Crusade activity within 20 miles, the distance to the nearest railroad line, telegraph connection, newspapers per capita, and churches per capita. All continuous variables are standardized for comparability. In panel b, the share of disabled veterans is instrumented with the average disability exit rate in soldiers' regiments during their time of service, excluding the veteran's own company. The interaction terms are instrumented with the interaction of the main effect variable and the instrument. All regressions include the following controls: log population size in 1870 and its square, the share of veterans in the town, latitude and longitude, and 1-by-1-degree grid cell fixed effects. Additional controls were constructed from the individual censuses for 1860 and 1870 using the town-level crosswalks from the Census Place Project (Berkes et al., 2023). For 1860, these include the average real estate and personal wealth per capita, share Black population, share urban, share foreign population, share women, share women employed, share farm employment, and churches per capita. 1870 controls include the same variables as the 1860 controls. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

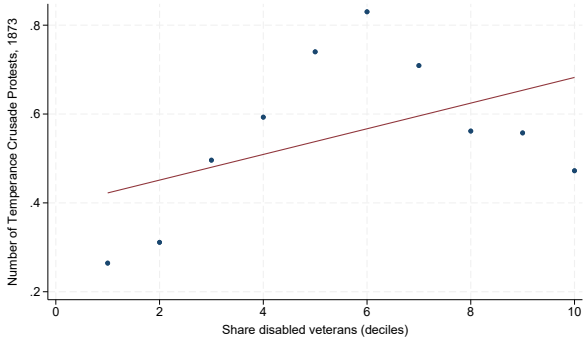
Figures

Figure C.1: Raw Data Visualization of Temperance Crusades, FLFP, and Disabled Veterans

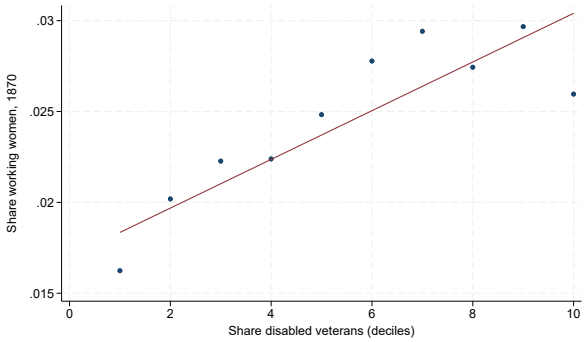
(a) Temperance crusades (bin) and disabled veterans



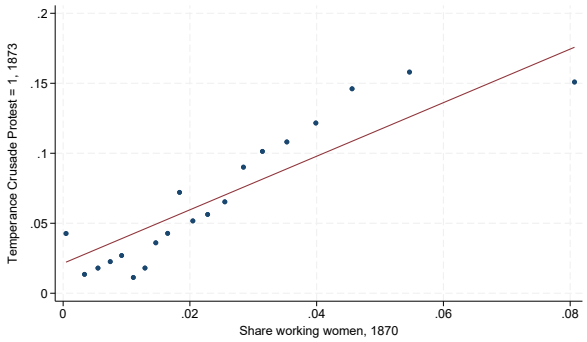
(b) Temperance crusades (num) and disabled veterans



(c) Female LFP 1870 and disabled veterans

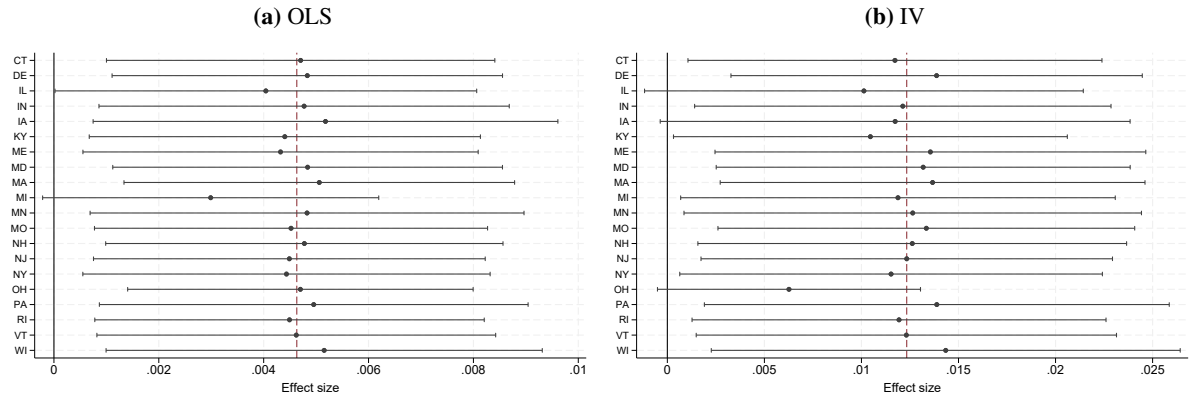


(d) Temperance crusades (bin) and Female LFP 1870



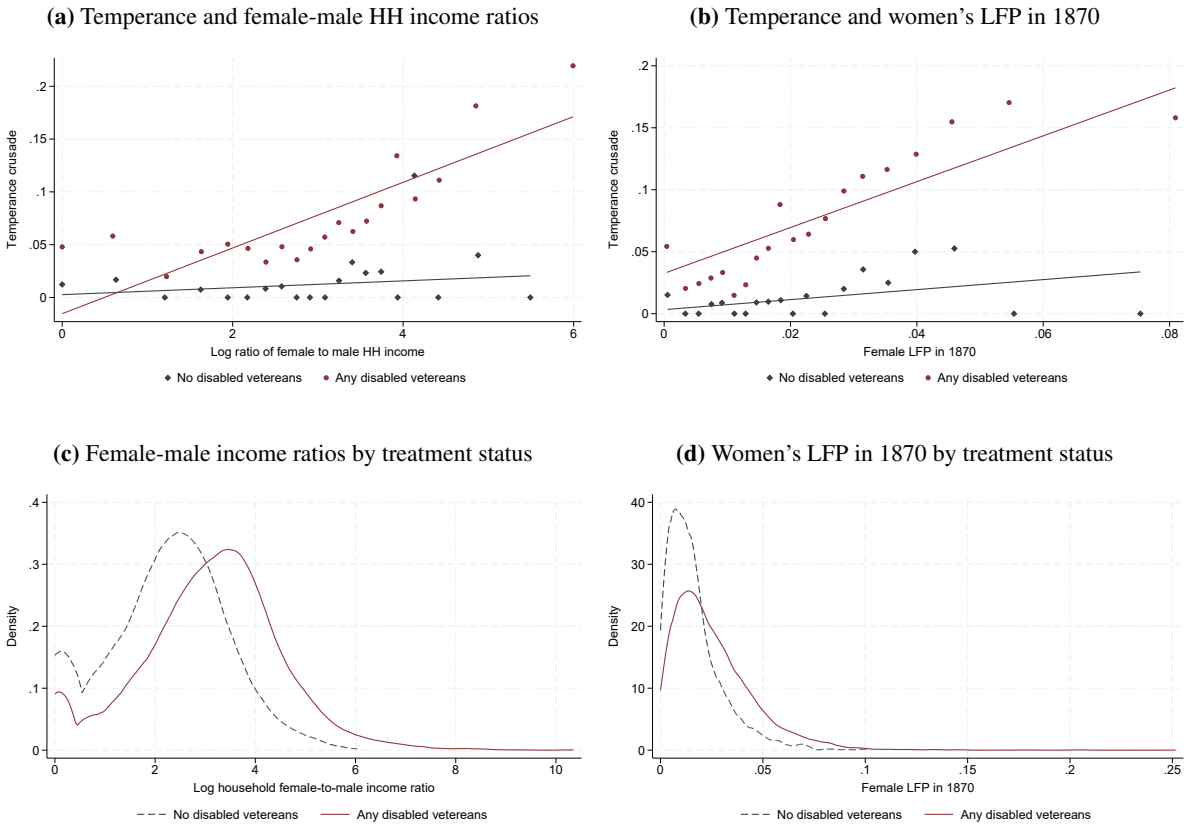
Note: Binned scatter plots using town-level data (N = 8,882) to show the raw data relationship between an indicator for the occurrence of a Temperance crusade protest (panel a), the number of Temperance crusade protests (panel b), and the share of working women in 1870 (panel c) with the deciles of the share of disabled Civil War veterans in a town. Panel d shows the relationship between the Temperance crusade indicator and the share of working women in 1870.

Figure C.2: Temperance Crusade Results - Excluding States One-by-One



Note: Panels a and b replicate the results from columns 4 and 8 with baseline, 1860, and 1870 controls from Table 5, respectively, but excluding one state at a time. The regressions estimate the impact of the share of disabled Civil War veterans in a town on the probability that a Temperance Crusade occurs in 1873–74. In panel b, the share of disabled Civil War veterans is instrumented with the average disability exit rate in soldiers' regiments during their time of service. The original point estimate from using the full sample is indicated by the dashed red line. Zero is marked by the black solid line. Each estimate is from a separate regression, the excluded state is indicated on the vertical axes. See the note of Table 5 for a full description of the included controls. Standard errors are clustered at the 1-by-1-degree grid cell level to account for spatial autocorrelation and heteroscedasticity (Bester et al., 2011). Error bars report 95 percent confidence intervals.

Figure C.3: Temperance Crusades, Women's Work, and Household Income Ratios



Note: Panel a plots the town-level share of Temperance Crusades over the log average female-to-male occupational income score ratio computed from the 1870 census and aggregated to the town level for towns with zero and towns with any disabled Union Army veterans using the crosswalks provided by the Census Place Project (Berkes et al., 2023). Panel b plots the town-level share of Temperance Crusades over the share of working women in 1870 for the same sets of treated and untreated towns. Panels c and d plot the distributions of the standardized female-to-male occupational income score ratio and women's labor force participation by treatment status, respectively.