Rosie and Her Daughters: Revisiting Female Labor Force Participation after World War II

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Abstract

I use new data on U.S. Army enlistments and casualties during World War II to argue that manpower mobilization and the resulting surge in female employment during the war had no detectable effect on female labor force participation in 1950. However, the war appears to have altered the labor supply decisions of the next generation: women too young to work during the war are more likely to be working in 1970 if exposed to higher mobilization rates. I show that this effect appears to operate through labor supply and is specific to daughters. I also show that previous estimates of the war's impact on female labor supply in 1950 and beyond using state-level data may be positively biased due to correlations between mobilization and other factors that influence female labor supply, as well as measurement error in key census variables for 1950.

In 1900, the female labor force participation rate in the United States was 21%. On the eve of World War II, the figure had barely budged: 23%. Over the next 40 years, however, female labor force participation more than doubled. What caused this abrupt shift in the growth of female labor force participation? Long-term structural changes in both

female labor supply and demand – especially increased female high-school graduation and the rise of clerical and sales occupations – certainly played a role. But labor economists and historians have also traditionally emphasized World War II, which put "Rosie the Riveter" and her colleagues to work, as an important catalyst for change.

This paper uses new data on manpower mobilization for World War II and a different empirical design to investigate this idea. I test for persistent effects of increased female labor force participation during the war on outcomes in 1950 and 1970 by examining women living in U.S. counties where more men served in the Army.[1] Since many women went to work when and where soldiers joined the military, these areas had higher female labor force participation during the war. I am able to examine effects across smaller geographic units than previous studies, which focused on variation across states, using data that links enlistments and casualties to soldiers' county of residence. By using casualty data, I also directly capture a new dimension of the war's impact on local labor markets: the roughly 400,000 men who died while serving.

The logical link between World War II and female labor force participation (hereinafter FLFP) is clear. Women worked in unprecedented numbers during the war, almost doubling their total participation from 1940 to 1944. The increase was particularly strong among married women whose husbands were serving in the armed forces. These women were more than twice as likely to be working in 1944 than those whose husbands were present (Goldin 1991). Single women also entered the work force to fill vacancies left by departing soldiers, especially in industries essential to the war effort. Counties that suffered more casualties experienced a double effect: temporary supply shocks as a result of enlistment became permanent.

It seems reasonable to expect these large shocks to female labor supply and demand to have left some sort of lasting legacy. On the supply side, women may have acquired skills and experience that increased their returns to employment after 1945. The war experience

[1]The Army and Army Air Forces comprised roughly 75% of the war's total fighting force.

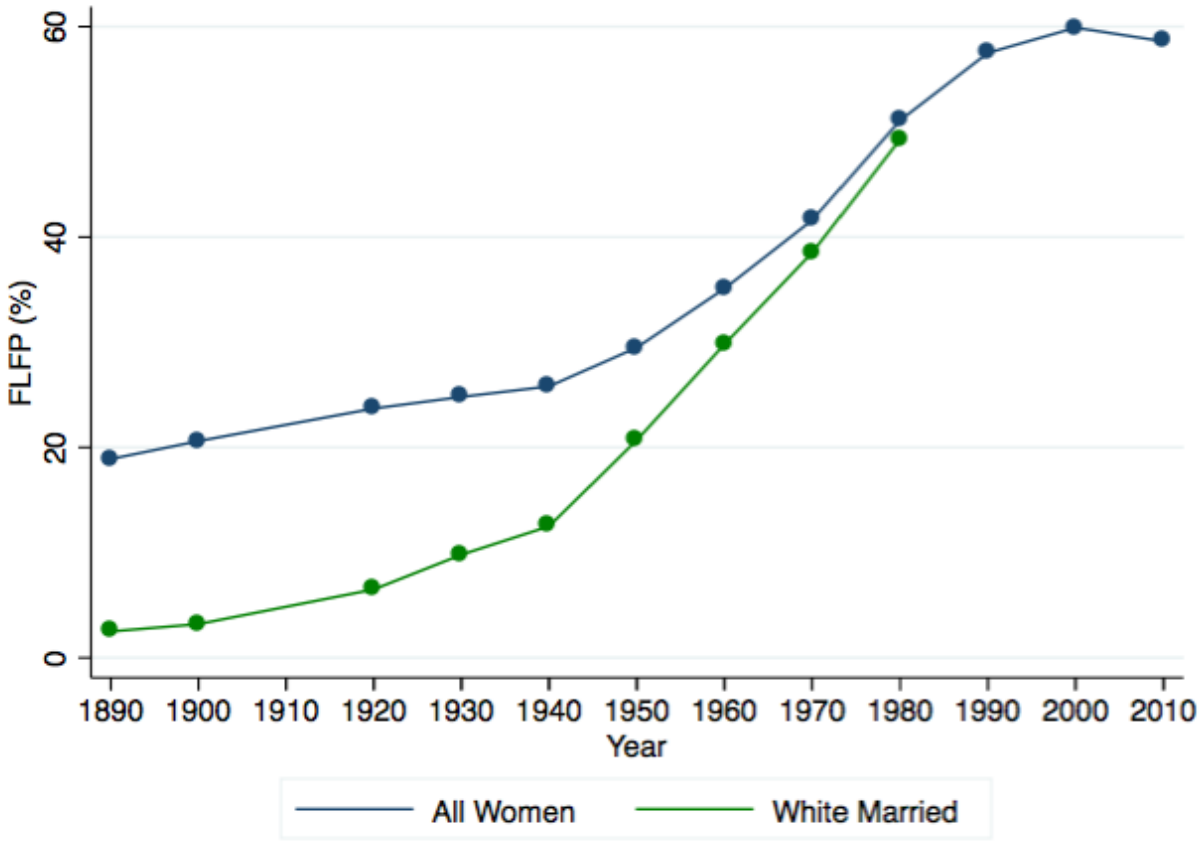

Figure 1: Evolution of Female LFP Since 1890

Figure displays the evolution of female labor force participation rates from decennial censuses. See source for additional details on data construction. Source: Goldin (1990).

also may have changed preferences for home versus market production, as suggested by Chafe (1972) and others, through altered attitudes and norms. The ubiquity of "Rosie the Riveter," who maintains currency today, is good evidence of the war's influence on the general perception of women's work. More simply, exposure to the labor market during the war may have also increased women's information about employment opportunities. There are similar stories on the demand side that feature both economic factors (e.g., investments in training programs), altered attitudes, and better information.

On the other hand, female employment during World War II was exceptional in more ways than just its unprecedented scope, and the specifics of women's war-time work make limited or negative impacts by 1950 plausible as well. Many women worked in war-specific industries

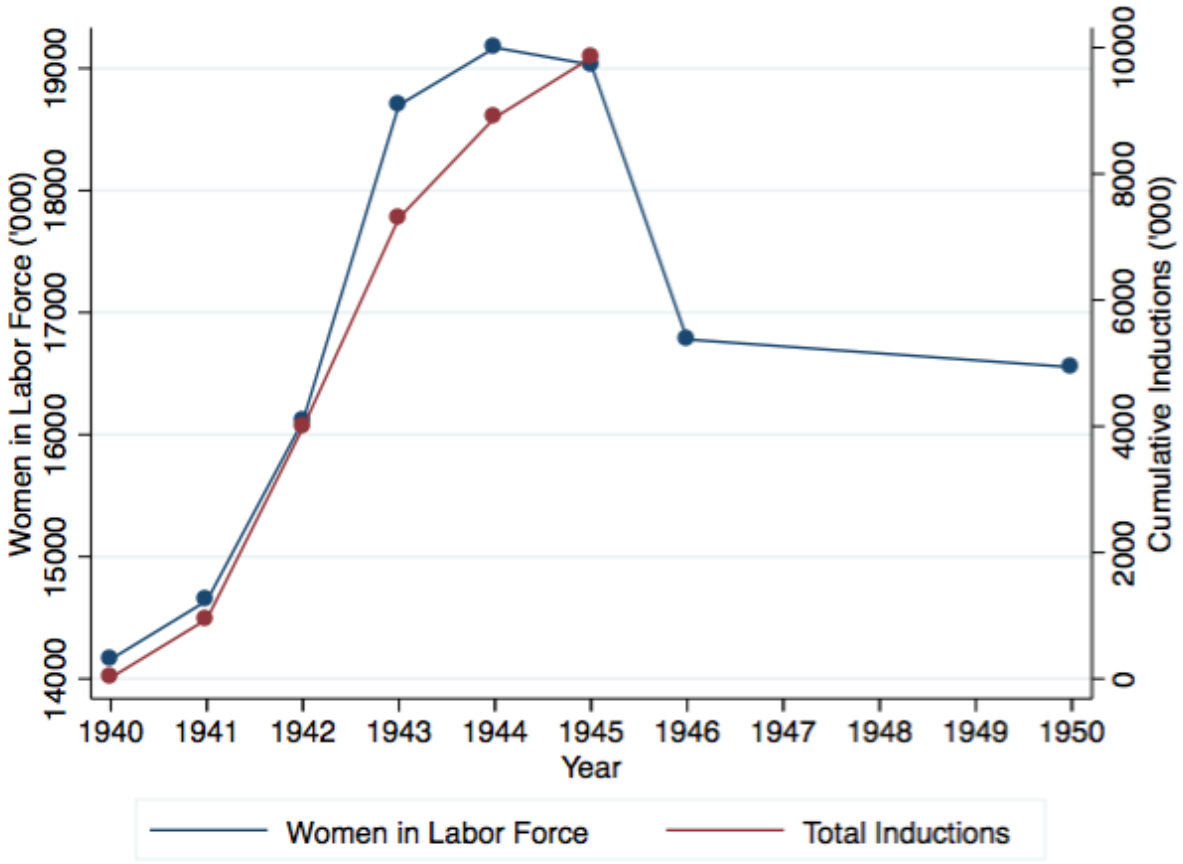

Figure 2: Female Entrance into Labor Force and Total Inductions during WWII
Figure displays the total number of women in the U.S. labor force over the war period (left axis) and the cumulative number of inductions in the continental United States (right axis). Inductions includes all draftees and volunteers entering the military through the Selective Service Administration, but not soldiers volunteering directly before 1942. Source: U.S. Census Bureau, Department of Commerce (1947) and Selective Service System (1948).

with no clear-cut path to normal production after peace was won. Rosie the Riveter, for example, was modeled after a munitions worker, perhaps the most war-specific industry of all. Even in industries that continued to thrive in the post-war period, anecdotal evidence suggests employers in traditionally male-dominated industries preferred to hire returning troops after 1945, even if women wanted to say in their jobs (Goldin 1991). Women may have chosen to work less, too, after the war's particular distortions of preferences and household formation decisions came to an end.

Assessing these competing claims requires overcoming two important challenges. The first

is the lack of an appropriate counterfactual or experiment. There is no "control" America that did not go to war. Consequently, my strategy will be to exploit the fact that different parts of the U.S. sent more young young men to fight overseas, and therefore experienced different intensities of "direct effects" through manpower mobilization.[2] Since mobilization was a central component of the World War II experience – more than 17 million American men would serve – this exercise likely captures a significant component of the war's effect on FLFP. My primary explanatory variable of interest will therefore be the fraction of military-aged men in a given area who served in the war. Due to data issues discussed further below, I often proxy for this variable with the fraction of military-aged men who died in the war.

The second challenge lies in the fact that mobilization was not randomly assigned across the U.S. The more than 6,000 local draft boards that implemented the Selective Service Act of 1940 operated with substantial discretion, but ultimately answered to national guidelines that exempted some from the draft. These guidelines were designed to limit disruptions to the domestic economy and local communities. Early in the war, for example, exemptions for new fathers were widespread. Farmers went to war less often. And fewer blacks were called to serve, especially in combat units, in a still-segregated Army.

Many of the specifics of draft policy are plausibly correlated with levels and changes in FLFP. For example, farm employment did not drive early increases in women's work, which was concentrated in white-collar jobs. Male and female demographics are closely related, suggesting that draft policies designed around men's age and martial status may also correlate with women's age and martial status, which are important correlates of FLFP. White women were less likely to work in 1940, and consequently saw the largest increases in participation rates over the next few decades.

One answer to this challenge is to attempt to control for the vague formulas used by the Selective Service Administration to implement the draft. County-level demographic and

[2]World War II also likely had multiple indirect effects on female labor force participation through channels such as changes in industrial composition, male education, and so on. To the extent that these indirect effects are also correlated with the number of troops who served from a particular area in the U.S., they will be captured in my results.

economic statistics like those mentioned above, for example, can explain roughly 60% of the variation in enlistments during the war. But it remains unclear how the remaining variation should be treated. After all, even if a full set of controls account for the relationship between draft policies and women's work, 40% of troops were never drafted – they volunteered. We have little understanding of how selection into volunteering may also correlate with FLFP.

I argue that a good-control strategy may be misleading. Basic results about the war's impact on FLFP in 1950 are highly sensitive to controls for local demographic and employment variables, particularly changes in female demographics and employment and occupational mix. In my preferred specification, the baseline impact of mobilization on FLFP in 1950 is negative, a sharp departure from several previous estimates that found positive effects. These previous results may be driven by changes in definitions of key census variables from 1940 to 1950, which could create positive bias in the estimated effect of mobilization on FLFP.

Since it is clearly difficult to successfully identify and measure the "right" controls, I employ a quasi-treatment and control design using the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2010) sample from the 1950 census.[3] Specifically, I compare the labor force outcomes of women living in high mobilization counties (the treatment group) to women who moved into the area from lower mobilization counties after the war (the control), and vice versa. After conditioning on age, martial status, and education, I find that mobilization decreases the labor force participation of women in high mobilization areas relative to women who moved from low mobilization areas, though the coefficients are imprecisely estimated. This design ensures that we compare women facing the same market demand in 1950, making it unnecessary to control for the relationship between mobilization and county characteristics that may impact labor demand. Indeed, I show these controls no longer impact the results. Importantly, causal identification also does not require that

[3] "Quasi" in the sense that selection into the treatment and control groups is the result of general equilibrium behaviors and not the researcher's experimental design, and in the sense that the treatment is applied to all subjects to varying degrees.

women who move have the same labor force outcomes as those who do not. Instead, it requires that mobilization rates are not also correlated with other factors that differentially impact the labor force outcomes of women who move.

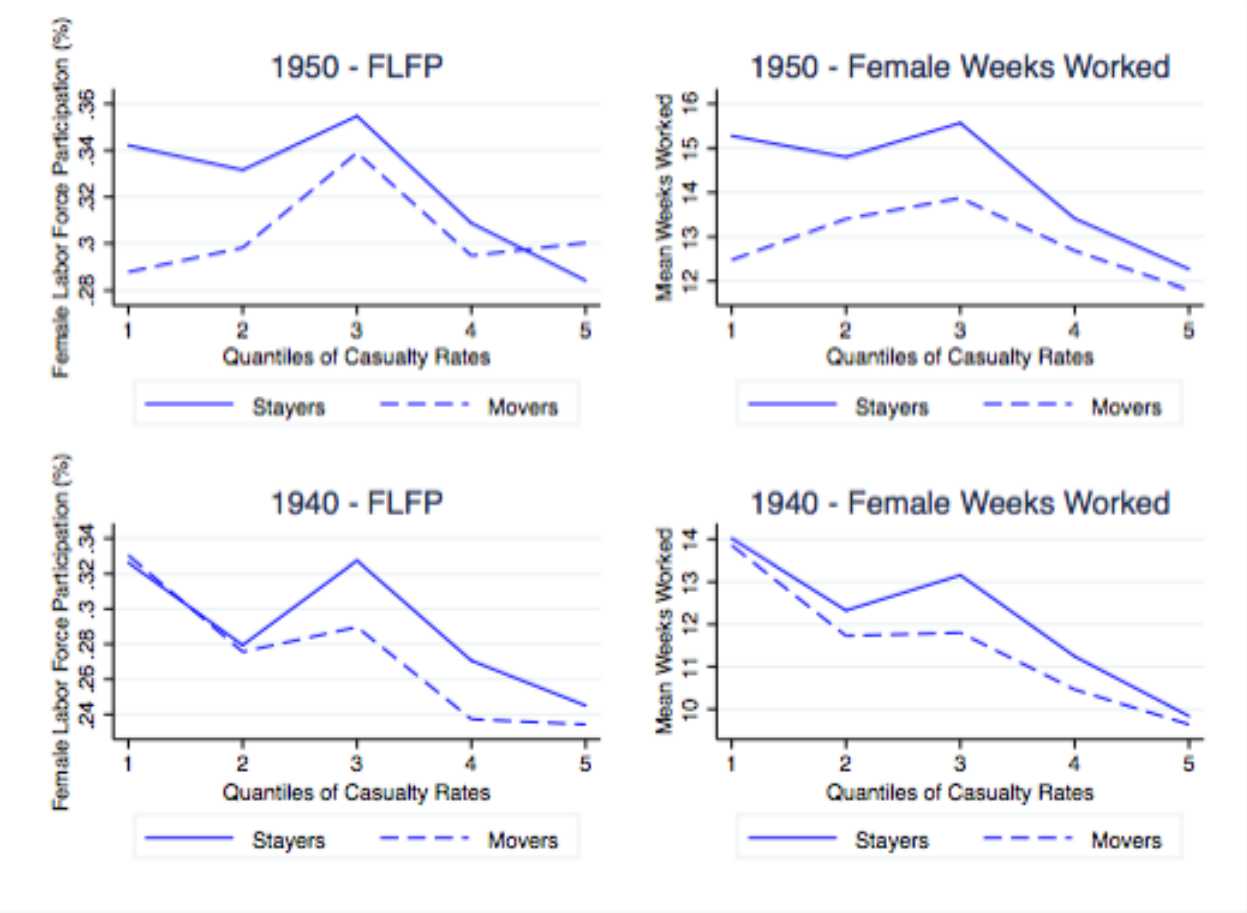

Figure 3: Female Movers vs. Stayers by Mobilization Rate

The top left panel displays the mean 1949 FLFP rate for women living their state of birth and who did not move between counties in 1948 and 1949 ("Stayers") and the mean FLFP rate for women who moved into their 1949 county of residence after 1945 ("Movers") for 5 quantiles of casualty rates. The top right panel displays the same disaggregation for mean female weeks worked. The bottom two panels display the same analysis for 1940. For the 1940 census, movers include those who moved into their 1939 residence after 1935. Includes women 15 to 60 only. Source: IPUMS, Army casualty data, National Archives, author's analysis.

The basic logic of this approach is clear from Figure 3, which shows the mean FLFP rates and weeks worked for movers and stayers across 5 quantiles of mobilization rates. In low mobilization areas in 1950, women who lived in the same area during the war work more than women who moved into the area from other places sometime afterwards. These "movers" on

average come from areas with higher mobilization rates by construction, and thus have higher treatment levels. The gap between movers and stayers narrows as mobilization increases. In the highest mobilization areas, women moving from other counties actually work more than those who lived there during the war. The same trend does not hold for movers and stayers in 1940. I interpret these patterns as evidence that the true relationship between mobilization and female labor supply in 1950 may have been negative. As shown in Section 3, this correlation is robust to a large set of individual level controls such as age, marital status and race as well as county-level demographic and economic characteristics. As a further robustness check, I also compare stayers with the subset of movers coming from areas with significantly different mobilization rates to their 1950 area of residence instead of all movers. The results are the same.

I then take this same design to the 1970 census to investigate whether women exposed to World War II were more likely to work later. I find no significant effects. However, women who were too young to work during the war, but whose mothers may have, are more likely to be in the labor force. A one standard deviation increase in mobilization is associated with a 2% increase in the likelihood of a 30 year-old participating in the labor force. Since this effect is identified using the variation in FLFP between women in the same county, I argue that this represents increased female labor supply among the children of female war-time workers – "Rosie's daughters" – and not changes in demand. Interestingly, the effect appears only among women, suggesting a variety of mechanisms, including role-model effects, family formation decisions, spousal choice, and inter-generational transmission of preferences, that are specific to daughters. I do not investigate the mechanism here, simply focusing on reduced form relationships.

This paper adds to a large literature on FLFP and its intersection with World War II. Female labor supply itself has been studied extensively, especially since the 1960's (see Killingsworth & Heckman (1986) for a survey), in a variety of contexts. World War II was initially seen as a "watershed" moment in this literature (Chafe 1972). The temporary surge

in war-time employment was thought to have increased both female labor supply, as women invested in skills and redefined their roles in the household, and demand, as employers became more willing to hire women after positive experiences during the war. This view was challenged in the 1980s and 1990s by historians and economists, who showed that the majority of Rosies – women who entered the workforce from 1940 to 1945 – did not become permanent participants. Goldin (1991) uses a retrospective survey to demonstrate that among among women working in 1950, the rate of entrance into the labor force was roughly constant over the previous decade.

More recently, Acemoglu, Autor & Lyle (2004) and Goldin & Olivetti (2013), exploit cross-state variation in mobilization rates to argue that the war increased FLFP in 1950, despite the fact that many war-time workers left the labor force. Acemoglu et al. find that women worked 1.1 more weeks on average in states with 10 percentage points higher mobilization rates. Goldin et al. finds that the effect is concentrated among white, married women from the top half of the education distribution. Fernandez, Fogli & Olivetti (2004) use the same data to argue that war also changed work preferences for the subsequent generation. I argue that some of these analyses do not adequately account for endogeneity in mobilization rates and changes in census variable definitions, possibly introducing positive bias. My results are more consistent with Jaworski (2014), who uses within-state variation in mobilization across time to argue that exposure to World War II decreased educational attainment among high-school aged women and reduced their employment and earnings. See Section 3.2 for further analysis of these previous analyses.

Though I do not study specific mechanisms, my results are also related to the literature on the role of beliefs and preferences in determining FLFP. Much of this work suggests that factors not determined by the market for labor, such as long-standing cultural norms about gender roles, drive substantial variation in female labor force outcomes. See, for example, work by Alesina, Giuliano & Nunn (2013) on the influence of the adoption of the plough on contemporary gender attitudes. When examining effects within the U.S., this literature has

focused on immigrants in order to isolate the effects of "culture" (broadly defined). Blau et al. (2013), for example, find that the fertility, education and labor supply decisions of second-generation immigrants are positively related to their parents' and argue that the results are partly due to intergenerational transmission of gender roles (see also Reimers (1985), Fernandez & Fogli (2009) and others; Fernandez (2011) for a survey). My results are consistent with these findings, though they suggest that even brief employment spells can having lasting impacts on subsequent generations' labor force decisions.

This paper is organized as follows: Section 1 describes the data on enlistments and casualties available and my basic empirical strategy. Section 2 provides historical context on the Armed Forces enlistment process, casualties in the Army and Army Air Forces, and the nature of female employment during World War II. Section 3 presents my main results, including results for 1950 (Section 3.1), a reconciliation with the previous literature (Section 3.2), and the results for 1970 (Section 3.3). Section 4 concludes.

1 Data and Empirical Strategy

1.1 Sources

Enlistment records come from a National Archives database of about nine million individual records for the U.S. Army and Army Air Forces. The data were created from the Army's original enlistment "punch cards" that recorded basic information about inductees, including serial number, name, address, rank, height, weight and other information on paper index cards. In 1994, the National Archives and the Census Bureau converted over a thousand microfilm rolls of punch cards images into a digital format, which is the core file used in this study. Substantial information on total enlistments by state and branch are also drawn from the Selective Service Administration's special monograph series on the war.

These records do not capture the full universe of Army and Army Air Force enlistees. Because some microfilm roles were unreadable, several blocks of known Army serial numbers

are missing. Unfortunately, serial numbers began with two digits that denoted the solider's state of origin in clusters of three to nine states. The result is that several states are missing significant shares of total enlistments as reported in other Army documents. In 14 states, enlistment records cover 90% or more of the known total.[4] Twenty-three states have 80% coverage or more. Eight states have less than 50% coverage. Importantly, the data also do not include officers.

Casualty data comes from War Department documents hosted at the National Archives and known as the "Honor Roles of Dead and Missing." These documents list the total Army and Army Air Force deaths in the line of duty for each state and county and cover the period May 27, 1941 through January 31, 1946. These figures include those killed in action, deaths from wounds or injuries, non-battle deaths, missing men, and all others determined dead by law. For each death, the War Department also reported a location. This location corresponds to either the solider's home upon enlistment or, if he gave no address when inducted, the address of his next-of-kin. If neither of these addresses were available, the reports list the location of the solider's draft board. Failing that, soldiers are listed as "State at Large" (War Department: The Adjutant Generals' Office 1946).[5] I digitized these records to obtain county-level sums.

After throwing out corrupted and missing entries, the combined enlistment and casualty data record information for 3,064 counties. This includes the distribution of 7,455,759 enlistees and 305,245 casualties.

Demographic and labor force information is drawn from decennial censuses hosted at the Inter-university Consortium for Political and Social Reserach (ICPSR). These files contain county-level averages and aggregates of census information on several hundred variables in each decade in the 20th century. Where necessary, I supplement with Current Population Survey data from IPUMS, as well as the IPUMS micro-samples from the 1940, 1950 and 1970

[4]These states are Alabama, California, Connecticut, Florida, Georgia, Massachusetts, Maine, Mississippi, Nevada, North Carolina, New Hampshire, Rhode Island, Utah, and Vermont.

[5]A negligible number of soldiers fall into this category.

censuses. These data are merged with enlistment and casualty records at the county level. In all analyses, I use only counties in continuous existence over the relevant sample period. I also exclude Virginia, which experienced substantial changes in county boundaries over this period, and New Mexico and Nevada, which experienced dramatic population changes. I also exclude Washington, D.C., since it contains just one county (itself). All results are robust to their inclusion as well.

1.2 Empirical Strategy

My primary covariate of interest is the county-level exposure to mobilization during World War II. I define this variable as the total enlistments from county i from 1940 to 1945 divided by the total male population aged 15 to 44 in 1940, which corresponds to the population that would have been eligible for service at some point during the war. The precise definition is:

$$enlist_i = \frac{\sum_{t=1940}^{1945} enlist_t}{male - pop - 15 - 44_{1940}} \tag{1}$$

I use the term "mobilization" to refer to this variable. As mentioned above, holes in the enlistment data appear in blocks of serial numbers, which correlate spatially. The data are clearly not missing-at-random at the state and regional level. Given these issues, it also seems unlikely that the county-level data within these states is representative of their actual distributions. If this is the case, using enlistment data as given may bias my results (for example, if smaller counties are under-represented).

I correct for these issues in several ways. First, I use variation in total Army and Air Forces casualties by county, for which I have complete data, as a proxy for total enlistments. I do this directly by using the analogously defined covariate:

$$cas_i = \frac{\sum_{t=1940}^{1945} casualties_t}{male - pop - 15 - 44_{1940}} \tag{2}$$

As shown in Table 5 and discussed further below, the fraction of soldiers who were killed

in the war is not strongly correlated with factors likely to also be related to labor force outcomes in states where the quality of enlistment data is high (at least 90% of known totals captured). This suggests that casualty rates can provide a noisy, but unbiased, measure of the actual county-level enlistment rates. Figure 4 demonstrates this visually with a binned scatterplot of $enlist_i$ against cas_i in 90% states.

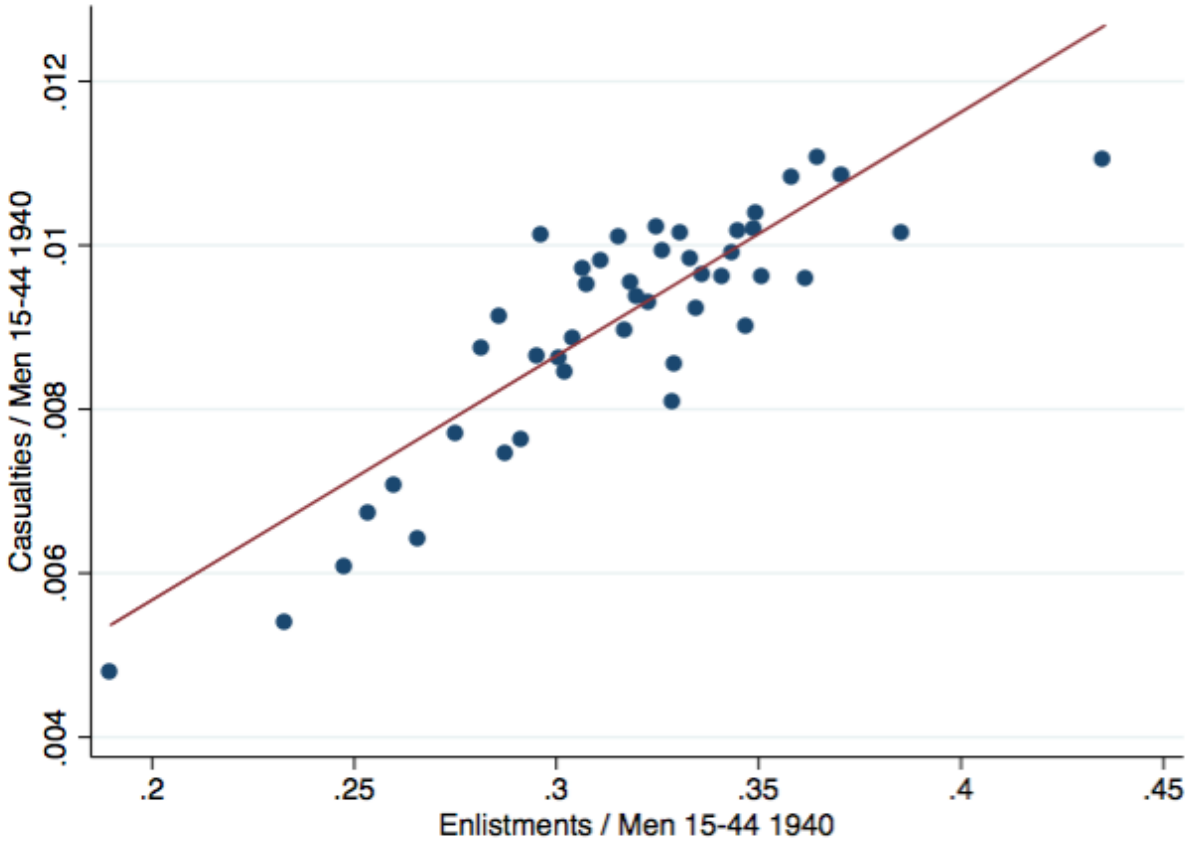

Figure 4: Linear Relationship Between Casualties and Enlistments
Figure is a binned scatter plot of total war-time casualties as a fraction of military-aged men in 1940 vs. enlistments as a fraction of military-aged men in 1940. Dots represent the means of 50 quantiles in enlistments. The line is a best linear fit. Its coefficient is 0.029 (se: 0.0015), suggesting an average casualty rate of 3%, which is in line with the Army average. The constant is not significantly different from 0. Source: Army enlistment data, National Archives, U.S. Census 1940, author's analysis.

Of course, casualty rates also capture another dimension of the war's impact on American counties: troops who didn't come home. This dimension is also important to the war's impact

on FLFP. Women whose husbands did not return from the war may have been more likely to continue working to support their families, and counties that experienced relatively high casualty rates my have been compelled to continue to employ more women overall. Because enlistments and casualties are highly correlated, using the former variable would also capture some of this effect.

In some analyses I instrument for enlistment rates using casualty rates, which purges the enlistment measure of non-classical measurement error not present in the casualty data. As a final check, I scale the county-level enlistment data so that state totals match the actual figures and report results for the set of 90% coverage states. My results are robust to not scaling as well.

Since this is the first time both datasets have been used in the literature to date (to my knowledge), I perform several checks to ensure the data is capturing similar variation to the state-wide aggregates used previously and provide support that my estimation strategy is valid. The results in Table 4, which describe the county-level determinants of enlistment rates and are discussed further below, provide the first check. These results are the same sign as the coefficients reported in OLS regression using state-level aggregates in Jaworski (2014) and Acemoglu, Autor & Lyle (2004). The coefficients are also of similar relative magnitude. It would be surprising, however, if all coefficients were the same, since my regressions measure within-state variation.

Table 1: 90% States: Characteristics of Counties by High-Medium-Low Casualty Rates

	(1)	(2)	(3)
	1	2	3
	mean	mean	mean
Casualties / men 15-44	0.005	0.008	0.011
Share pop white	0.531	0.803	0.947
Share pop urban	0.198	0.580	0.629
Share male employment farm	0.571	0.224	0.151
Share pop 25+ 4+ years HS	0.083	0.152	0.184

Means of covariates are reported for counties in the 14 states where enlistment data captures 90% or more of the known totals and are split by quantiles of cas_i. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

As another check, I follow Acemoglu, Autor & Lyle (2004) in reporting basic demographic characteristics for low, medium, and high casualty counties and likewise for enlistments in states for which the data capture at least 90% of known totals. I also report the same statistics for quantiles of casualties across the full sample. The results are reported in Table 1, Table 2 and Table 3 respectively. The county-level means follow the same pattern as Acemoglu, Autor & Lyle (2004) and Jaworski (2014) and are at similar levels. Note also that comparing Table 1 and Table 2 provides further evidence that casualties can serve as a useful proxy for enlistments, since the patterns are similar. Comparing the same figures to Table 3, however, suggests that the sub-sample of high enlistment data quality states is not necessarily representative of enlistment and casualty patterns nationwide.

Table 2: 90% States: Characteristics of Counties by High-Medium-Low Enlistment Rates

	(1)	(2)	(3)
	1	2	3
	mean	mean	mean
Enlistments / men 15-44	0.242	0.305	0.347
Share pop white	0.578	0.864	0.880
Share pop urban	0.203	0.589	0.618
Share male employment farm	0.547	0.210	0.168
Share pop 25+ 4+ years HS	0.095	0.178	0.158

Means of covariates are reported for counties in the 14 states where enlistment data captures 90% or more of the known totals and are split by quantiles of $enlist_i$. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

Table 3: All States: Characteristics of Counties by High-Medium-Low Casualty Rates

	(1)	(2)	(3)
	1	2	3
	mean	mean	mean
Casualties / men 15-44	0.007	0.010	0.012
Share pop white	0.780	0.932	0.966
Share pop urban	0.457	0.670	0.470
Share male employment farm	0.340	0.157	0.260
Share pop 25+ 4+ years HS	0.117	0.155	0.135

Means of covariates are reported for full sample and are split by quantiles of cas_i. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

As a final check, I test the finding in Jaworski (2014) that exposure to WWII enlistments

decreased the educational attainment of high school-aged women in 1960. The results show similar patterns and are presented in appendix section A.1.

2 Institutional Context

2.1 Mobilization for WWII

Though millions of American men fought overseas in World War II, the intensity of manpower mobilization varied across the country. Other researchers have argued that as a result of the Selective Training and Service Act of 1940, which supplied 66% of America's fighting force through a complex draft system, a large component of this variation was essentially random. I argue that it is difficult to identify "exogenous" variation in mobilization, even though draft policies are well known. This section makes this case.[6]

In principle, the draft process was designed to equitably draw the requisite manpower from across the country. The Selective Service Administration achieved this by distributing the national "quota," which represented the total desired military strength, among the states in proportion to the number of men available for military service in that state.[7] States were obliged to supply draftees if voluntary enlistments (men who volunteered at one of the three military branches' recruitment centers), did not meet their quota. Voluntary enlistments eventually came to be seen as counterproductive to the careful induction process the Selective Service Administration designed;[8] after February 1942, induction occurred solely through local draft boards, though men could still volunteer with their approval, and many did.

The number of men available for service in each state was determined by sorting registrants into class I (qualified for military service) or one of three main exempted classes

[6]The details of the section are drawn from a series of Selective Service Administration Monographs, especially Selective Service System (1948) and Selective Service System (1954).

[7]Among those eligible for service, priority for conscription was determined by lottery before 1942 (registrations one through four), and by date of birth thereafter (registrations five and six).

[8]"A registrant might be the only man left on a dairy farm who could tend the herd, and his local board might have deferred him for that purpose; but if some recruiter made him uneasy about his future, he would enlist and disrupt the manpower situation to that extent." (Selective Service System 1955)

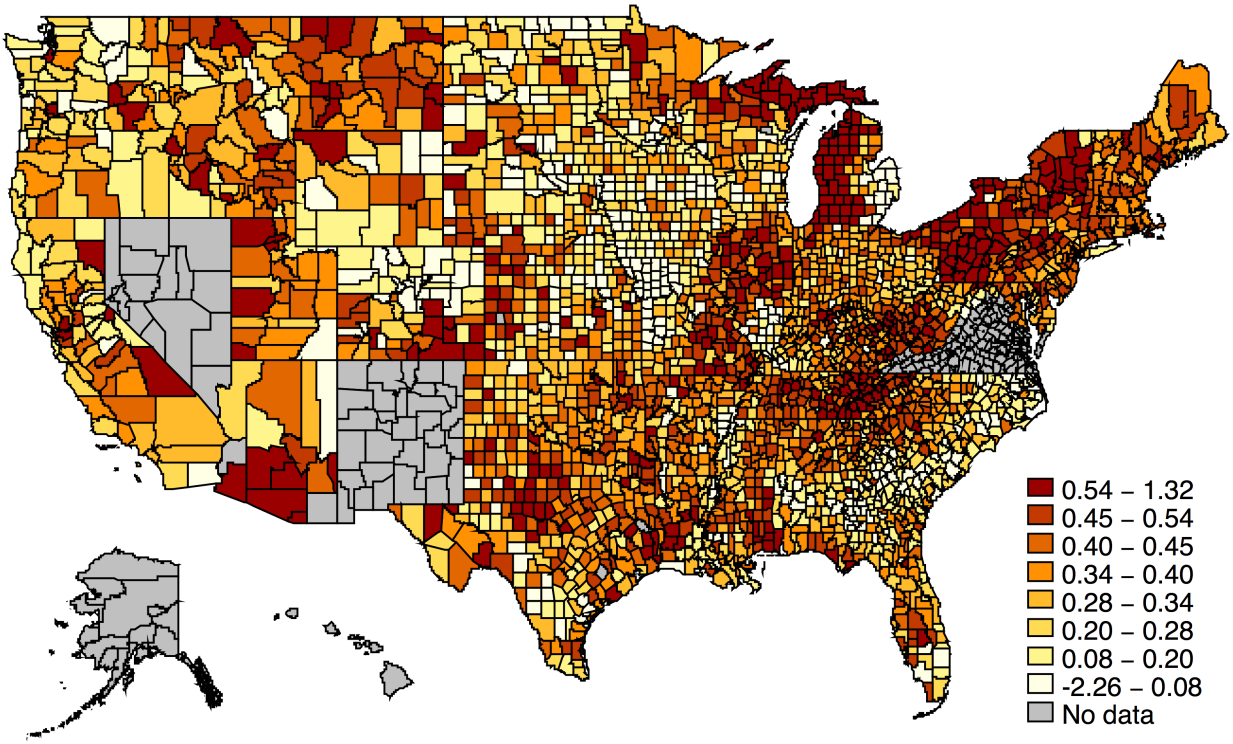

Figure 5: Distribution of Enlistments 1940-1945

Figure displays log deviations in enlistments as a fraction of 1940 male population aged 15-44 from the national average of 22.1%. A zero therefore refers to counties with average enlistment rates, whereas 0.2 denotes counties with approximately 20% more enlistments. No counties had zero enlistments. County-level enlistments are scaled in each state so that the state total matches figures in Selective Service Administration documents. Source: Army enlistment data, National Archives, U.S. Census 1940, author's analysis.

based on family status, occupation, and physical and legal fitness.[9] Roughly 6,000 local draft boards accomplished this sorting with substantial local discretion. These boards were staffed by leading citizens from their jurisdictions and had authority over all questions of eligibility for service, subject to appeal.[10] As the Selective Service Administration would later note, local authority and discretion was the *point* of the board system: "The Selective Training and Service Act...which evoked the gravest obligations and responsibilities,

[9]As noted in Acemoglu, Autor & Lyle (2004), exemption reflects both "economic" differences across areas, such as the share of the population working in agriculture, and "non-economic" factors, such as the number of clergymen or residents of German descent. Though some factors have an obvious connection to the character of local economic activity, there is no reason to conclude that "non-economic" factors are not also correlated with important determinants of local economic activity.

[10]Boards' jurisdictions roughly corresponded to counties. 75% of counties had one board, while a handful of larger counties had up to nine.

[was] left to the people themselves to administer. This was democracy in its purest form" (Selective Service System 1951).

These boards based their decisions on very flexible classification rules. Before Pearl Harbor, for example, deferment for dependents covered wives, divorced wives, children, parents, grandparents, brothers, sisters or any other persons who "depend in fact for support in a reasonable manner...on income earned by the registrant." Indeed, the original guidelines informed local boards that "no hard-and-fast rules will work...what is reasonable in one locality may not be in others." Occupational deferment categories were similarly broad, allowing local boards to defer "most any registrant they saw fit"[11] (Selective Service System 1951). Classification criteria changed over the course of the war, though the three main deferment categories – occupation, dependents, and deferred by law or unfit for service – remained the same. Agricultural workers continued to be protected throughout the war, however, along with workers in select firms critical to the effort, such as the cluster of aircraft manufacturers in Oregon, California and Washington.

These policies produced substantial variation in enlistment rates. In Rhode Island, where less than 3% of employed men worked in agriculture in 1940, 42% of registrants were serving in the military by January 1946. In North Dakota, where more men worked on farms than not, that figure was 33% (Selective Service System 1948). Within states, the standard deviation of enlistments as a fraction of military aged male population ($enlist_i$) lies between 5 and 8 percent. Figure 5 shows the log differences in $enlist_i$ to the national average. Table 4 describes the correlates of this variation. In columns (1) and (2), I fit $enlist_i$ onto state-specific constants and factors likely correlated with the classification process discussed above. Column (1) includes the set of 90% enlistment data coverage states; column (2) is the same sample but limitied to counties with a 1940 population greater than 10,000 (the top three quartiles) to exclude any small county outliers. Not surprisingly, counties with more agricultural workers experienced fewer enlistments. White men were more likely to

[11] This category included students.

serve; the armed forces were segregated and most black men were relegated to support and supply roles. Men 20-24 were most likely to be newly married or young fathers and were consequently less likely to serve.

These regressions explain roughly 60% of the within-state variation in enlistment rates. There is no reason to conclude, however, that the residual variation, presumably due to idiosyncrasies in local draft policy, is exogenous to later female labor force outcomes. Draft policies were sufficiently vague to produce correlations along a number of measures not included here.

In columns (3)-(5) I repeat similar regressions using cas_i as the outcome variable, which I have scaled to have the same mean as $enlist_i$ in this table so that the coefficients can be more easily compared. The results again provide evidence both that cas_i is a useful proxy for $enlist_i$ and that the sub-sample of 90% states is not necessarily representative. For example, men 15-19 appear more likely to serve in enlistment data in these states, but not the full sample. The regressions also highlight some important differences in the correlates of cas_i and $enlist_i$. Men with more education appear less likely to enlist, but more likely to die, though the coefficients are imprecisely estimated. Similarly, the share of manufacturing employment does not predict enlistment, but does correlate with casualties. The influence of race also appears stronger in casualty data. These patterns may reflect the unit assignment process used in the Army, which is discussed in more detail below, as well as the fact that officers are not included in the enlistment data.

2.2 Service and Casualties

Before proceeding with our casualty data, it is important to understand how soldiers were assigned to their military branches and units, which experienced different casualty rates during the war. If casualties as a fraction of enlistments is correlated with other factors that influence FLFP, this may introduce new bias into my results. The data and institutional history suggest that unit assignment was not entirely random and was likely correlated with

individual characteristics such as education and occupational training, though not county of residence. I discuss the process of unit assignment here and analyze the implications for my results.

For roughly a third of American troops, branch assignment was decided by soldiers themselves by volunteering at local recruiting stations for the branch of their choice. This was the sole source of manpower for the Navy, Marine Corps, and Coast Guard before the end of 1942, after which all enlistments were routed through the Selective Service Administration. Between 1942 and 1944, men were assigned to the Navy or the Army after induction based on each branch's total needs. After 1944, the Selective Service Administration administered separate calls for the two primary branches (Selective Service System 1955).

Both branches were in broad competition for manpower throughout the war. Army administrators worried early that volunteerism left the ground forces with men of "lower quality," and later that stricter Navy induction standards channeled the best potential soldiers into other branches, especially during manpower crises in 1943 and 1944. Measures of quality, however, referred to physical, psychiatric and intelligence standards that were specific to the military's purposes and do not map directly into characteristics relevant to civilian life. In addition, the relative size of the Army – 75% of the cumulative U.S. fighting force from 1940 to 1945 – meant that large numbers of men determined to be high quality still served in the infantry (Palmer, Wiley & Keast 1948).

Once in the Army, unit assignment was based on a battery of tests, as well as the soldier's work history. Troops were assigned to general service or limited service based on their physical capabilities first. Unit assignment was then determined using the Army General Classification Test, which measured natural intelligence and existing knowledge relevant to the Army. Scores were ranked in five classes; officers came from classes one and two.[12] Based on their work histories, troops were given "specification serial numbers" ranging from 1 to 999 that delineated their relevant skills. For jobs with no obvious civilian counterpart, such

[12]Unlike other branches, the Army did not directly induct commissioned officers for most of the war. These leaders came through regular induction and basic training along with enlisted men.

as tank driver (736) or rifleman (745), classification officers used their individual judgement. One of the most common serial numbers was 521, which referred to men fit for any job (Palmer, Wiley & Keast 1948).

While individual characteristics mattered, the Army's demand for each specialty was also a factor. Each service in the Army published desired classification rates per 1,000 men, and induction centers sought to supply troops at these rates. The infantry in 1943, for example, required 112 basic men (serial number 521) out of every thousand, and 780 men with various combat-specific serial numbers. In practice, this meant that education, occupation and skills were often ignored in order to meet the Army's specific needs. The Army also made an effort to distribute men of different education and intelligence levels evenly across units, subject to need (Lerwill 1954). The result of the entire process was that two soldiers from the same hometown, of similar age and backgrounds, and similar work histories, were often assigned to different segments of the army.

Not all jobs in the army were equally deadly. Though officers made up about 11% of the Army, for example, many served as Army Air Force pilots, a particularly dangerous job. Overall officers were close to twice as likely to be killed as enlisted men. Combat roles, such as riflemen and machine gunners, were more dangerous than support roles. Casualty rates also varied over time. Almost four times as many Army troops died in 1944, the deadliest year for the U.S. in the war, than 1943. Overall, the Army accounted for more than 75% of total U.S. casualties (Statistical and Accounting Branch of the Office of the Adjutant General 1953).

Despite the differential casualty rates experienced across branches and military occupations, the random component of war deaths and the diversity of the pool of eligible men in each county contributed to substantial spatial variation in casualty rates. Figure 6 shows log deviations of cas_i from the national average of roughly 1% of men aged 15 to 44 in 1940. The figure shows a mix of spatial correlations, driven by the patterns in enlistment described above, and seemingly random variation. For example, there were fewer casualties

in some southern states, likely as a result of having larger black populations.[13] The standard deviation in casualty rates is 0.3%.[14]

I also test whether casualties, conditional on enlistments, are correlated with the same county-level covariates examined earlier. I do this by regressing total war casualties over enlistments ($cas_i/enlist_i$) on county aggregate variables. This results are shown Table 5. Only the number of men who are white in 1940 is a significant predictor of casualties as a fraction of enlistments across all sub-samples, which was expected given that blacks were largely relegated to non-combatant roles in the war. The share of men with some college education is also significant outside of the South, which likely reflects higher casualty rates for officers. The coefficients are small, however. A one standard deviation difference in the share of white men, for example, is associated with a 0.1 standard deviation increase in casualty rates.

2.3 FLFP and WWII

Understanding the character of women's work during the war is important to interpreting its impact in 1950. As noted in the introduction, women entered the work force in unprecedented numbers as the country mobilized for war. Figure 2 shows that the total number of women in the labor force increased by nearly 40% from pre-war levels to the 1944 peak. The two primary drivers of this increase were the elevated need for labor to support essential war-time industries and the increased labor force participation of married women, particularly those whose husbands served in the military. As noted above, 53% of married women of all ages whose husband were absent in the armed forces were in the labor force in 1944, compared to 22% of women whose husbands were present. Reflecting this same trend, more than half of new female labor force entrants during the war were previously engaged in housework. A

[13]The same figure produced using casualties as a share of the white population 1940 does not show a similar pattern.

[14]Three counties – Banner, NE, King, TX, and Esmeralda, NV – experienced no Army casualties. Each of these counties had a population of less than 1,500 in 1940.

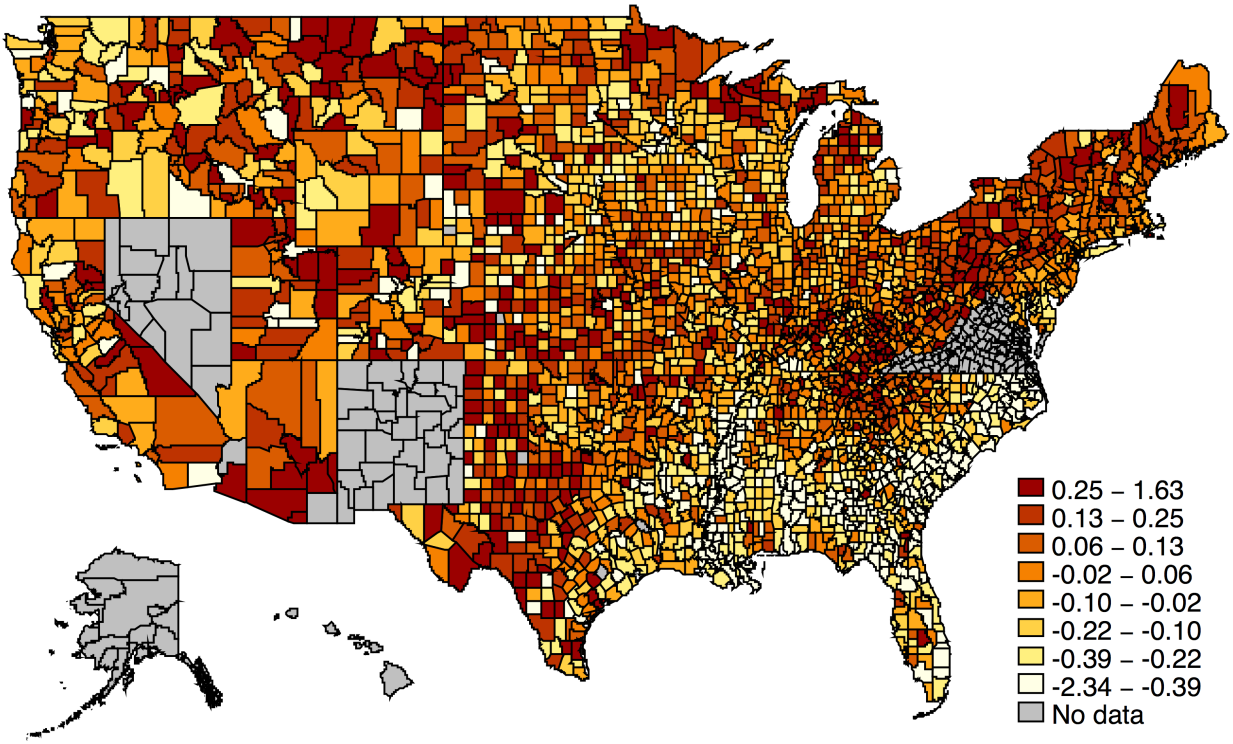

Figure 6: Distribution of Casualties 1940-1945

Figure displays log deviations in casualties as a fraction of 1940 male population aged 15-44 from the national average of 0.99%. A zero therefore refers to counties with average casualty rates, whereas 0.2 denotes counties with 20% more casualties. Casualties refer to the dead and missing. The three counties with zero casualties are excluded. Source: Army casualty data, National Archives, U.S. Census 1940, author's analysis.

third were students.[15]

War related industries were the primary beneficiaries of this surge in female work. Manufacturing industries accounted for 61% of the total net increase in female employment from 1940 to March 1944. Figure 7 shows that most of this work was in war materials industries, such as metal, chemical and rubber industries, and war-essential industries, such as textiles and leathers. Mining, construction and government industries saw the second highest proportional increase in female employment. Though many women entered these industries from the home, they also drew a substantial number of female workers away from other sectors, such as trade and clerical jobs. These women may have struggled to return to their old jobs

[15]Unless otherwise noted, the figures in this section are drawn from U.S. Department of Labor (1944) and U.S. Department of Labor (1946), both of which are special Department of Labor analyses of the CPS.

after the war and opted not to work instead, potentially driving the impact of mobilization in 1950 down.

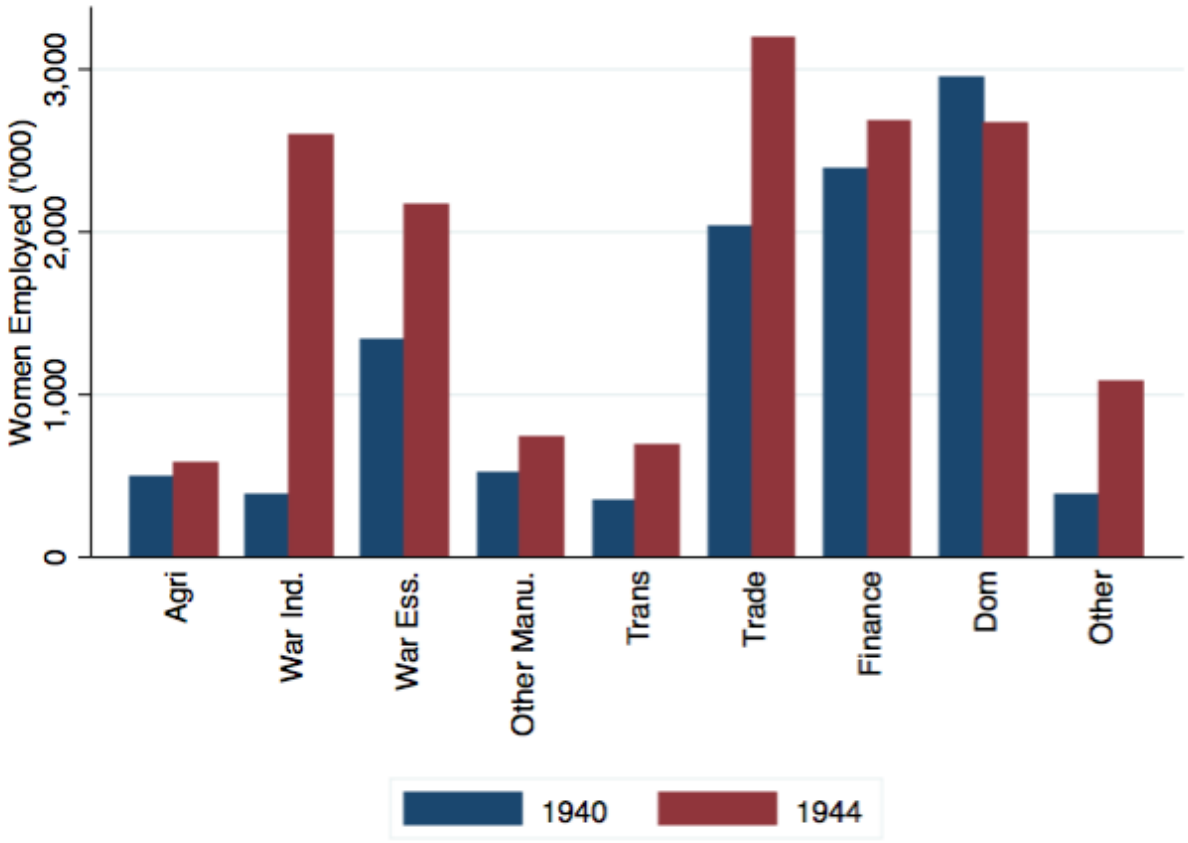

Figure 7: Change in Female Employment 1940-1944 by Industry Group
Figure displays the total number of women employed in March 1940 vs. March 1944. "Agri" refers to agriculture. "War ind" includes war material industries, such as metal, chemical and rubber sectors. "War ess" includes war essential industries, such as textiles and leathers. "Other manu" includes manufacturing industries not directly related to the war effort. "Trans" includes transportation, communication and public utilities. "Trade" includes trade, wholesale and retail. "Finance" includes finance, business, repair and professional. "Dom" includes domestic, personal, recreation services. "Other" includes all other industries. All industry categories are taken directly from the DOL at given level of aggregation. Source: U.S. Department of Labor (1944).

Not all women who entered the work force during World War II went to work on war materials, however. Trade, wholesale and retail industries saw substantial increases along with transportation, communication and public utilities. This reflects the fact that many women worked during the war to supplement their spouse's military salaries, as well as the

decreased competition in these sectors as the military absorbed a large share of the male labor force.[16]

Despite the war-specific nature of their employment, many women expected to remain in the labor force after 1945. DOL interviews with 13,000 employed women in 1944 and 1945 in 10 major geographic areas revealed that 75% expected to continue working after the war. The majority of these women were already working before the war, but 57% of women who entered the workforce from the home during the war also reported a desire to continue working. As made clear by Goldin, however, many war-time entrants were no longer working by 1950. Only 46% of married entrants were employed in January 1951. The figure for all female war-time entrants is 65% (Goldin 1991).

This evidence supports the view that the war's impact on FLFP in 1950 should have been small. It is not enough to give a full accounting of the war's impact on female labor force participation, however, since the summary evidence lacks a compelling counterfactual. Although the composition of female workers in 1950 featured no mass of women who joined the workforce during the war, it is difficult to know if even fewer (or more) women would have joined or dropped out without it. It is also difficult to know how many women who joined the labor force between 1945 and 1949 may have done so as a result of changes in labor supply or demand induced by the war. By systematically examining labor force patterns among areas differentially exposed to the war through mobilization rates, we can try to asses these counterfactuals.

[16]Women also served in the military as nurses, auxiliaries and reservists.

3 Results

3.1 Impacts in 1950

3.1.1 Baseline County-Level Results

I first demonstrate that enlistments and casualties are associated with at most negligible increases in the growth of female labor force participation from 1940 to 1950, though the results are highly sensitive to controls. Figure 8 plots the mean change in female labor force participation from 1940 to 1950 across 50 quantile bins of mobilization rates with no controls. There is a clear positive trend: the coefficient on the regression line is 3.00, with a standard error of 0.80. This suggests that a one standard deviation increase in casualties is associated with 3% additional growth of female labor force participation from 1940 to 1950.

Given that enlistment was not randomly assigned, one might be concerned that this relationship is the result of correlation between mobilization and other factors that affected the growth of female labor force participation. If omitted variables that are positively (negatively) correlated with mobilization are positively (negatively) correlated with change in FLFP, the results will be biased upwards. If farming communities, for example, were more likely to experience lower growth in FLFP, for example, this may confound the results. This appears to be the case, as the the partial association scatter plot in Figure 9 makes clear. After controlling for the growth in employment, urban population, white population, white collar jobs, female high-school education, and female demographics, the relationship between casualties and growth in female labor force participation is actually *negative*. The coefficient in the plotted regression line is -0.90 with a standard error of 0.26.

Which controls are driving this reversal? To address this question explicitly, I estimate a differences-in-differences regression of the following form:

$$\triangle y_{s,i,t} = \delta_s + \triangle X'_{s,i,t} \cdot \beta + \gamma \cdot cas_{s,i} + \epsilon_{s,i,t} \tag{3}$$

Figure 8: Change in Female Labor Force Participation vs. Mobilization

Figure displays a binned scatter plot with 50 quantiles of enlistments / 1940 male population 15-44 plotted against each quantile's mean change in female labor force participation 1940 to 1950 (in percentage points). Also plotted is a regression line fit to the underlying data and weighted by 1950 female population. The point estimate of the coefficient is 3.0 with a standard error of 0.8 clustered at the state level. Source: Army enlistment data, National Archives, U.S. Census 1940 and 1950, author's analysis.

where the $\triangle$ operator denotes differences from the previous census, $y_{s,i,t}$ is the female labor force participation rate in county i in state s at time t; δ_s is a state-specific fixed effect; and $X_{s,i,t}$ is a vector of county observables including employment, population, racial composition, educational, occupational mix, and demographics. The coefficient of interest is γ, which describes the effect of mobilization rates, proxied by cas_i, on the change in female labor force participation rates in 1950.

Table 4: 1940 County-Level Determinants of Mobilization Rates

	(1) enlist in 90% states	(2) enlist 90% and pop > 10k	(3) cas in 90% states	(4) cas all states	(5) cas all states and > 10k
	Coef./se	Coef./se	Coef./se	Coef./se	Coef./se
Share farmers	-0.10	-0.11	-0.04	-0.05	-0.04
	(0.02)***	(0.02)***	(0.01)**	(0.03)	(0.03)
Share manu.	-0.00	-0.01	0.10	0.09	0.09
	(0.02)	(0.02)	(0.02)***	(0.04)*	(0.04)*
Share pop urban	0.03	0.03	0.01	0.01	0.02
	(0.01)*	(0.01)	(0.02)	(0.01)	(0.01)
Share men 1+ years	-0.17	-0.18	0.19	0.25	0.27
college	(0.08)	(0.07)*	(0.09)	(0.09)**	(0.10)**
Share white men	0.13	0.13	0.21	0.21	0.21
	(0.01)***	(0.01)***	(0.02)***	(0.02)***	(0.02)***
Share German	-1.99	-1.75	-1.63	-0.10	-0.07
	(0.68)*	(0.59)*	(1.01)	(0.32)	(0.32)
Share Italian	0.32	0.35	-0.11	-0.44	-0.45
	(0.12)*	(0.11)**	(0.23)	(0.33)	(0.34)
Share men 15-19	0.88	1.27	0.49	-0.37	-0.31
	(0.26)**	(0.48)*	(0.42)	(0.53)	(0.60)
Share men 20-24	-1.78	-2.00	-1.07	-0.63	-0.74
	(0.23)***	(0.40)***	(0.24)***	(0.23)*	(0.23)**
Share men 25-29	0.98	1.24	0.09	0.60	0.77
	(0.62)	(0.87)	(0.39)	(0.58)	(0.64)
Share men 30-34	0.67	0.68	0.21	-2.15	-2.48
	(0.52)	(0.64)	(0.61)	(0.96)*	(1.11)*
Share men 35-39	-0.69	-0.59	-0.06	-0.98	-0.74
	(0.83)	(0.93)	(0.74)	(0.45)*	(0.56)
Share men 40-44	-0.77	-0.73	-1.65	-0.60	-0.72
	(0.87)	(1.00)	(0.67)*	(0.46)	(0.51)
R2	0.59	0.62	0.65	0.46	0.49
Obs.	629	498	629	2918	2269

The outcome variable for columns (1) and (2) is total county enlistments as a share of men aged 15-44 in 1940. The outcome variable for columns (3), (4) and (5) is total county dead and missing as a share of men aged 15-44 in 1940, rescaled so that the mean is equal to the mean of the enlistment outcome to facilitate comparison of coefficients. Covariates are all 1940 census data. Share farmers is total men employed in farm work / total men employed. Share manu. is share of employment in manufacturing. Share pop urban is share of population living in urban areas. Share men 1+ years college is share of men with at least some college. Share white is fraction of men who are white. Share German and Italian are fractions of population born in Germany and Italy in that country. Each regression is weighted by total 1940 population and includes a state-specific constant. Standard errors are clustered at the state level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

Table 5: 1940 County-Level Determinants of Casualty Rates

	(1) 90% enlist data states Coef./se	(2) 90% and pop > 10k Coef./se	(3) 90% excl. South Coef./se	(4) 90% South only Coef./se
Share farmers	0.00	0.00	0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Share manu.	0.01	0.01	0.02	0.00
	(0.00)**	(0.00)*	(0.00)**	(0.00)
Share pop urban	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Share men 1+ years	0.03	0.03	0.04	0.02
college	(0.01)**	(0.01)**	(0.00)***	(0.01)
Share white men	0.01	0.01	0.02	0.02
	(0.00)***	(0.00)***	(0.02)	(0.00)**
Share German	0.00	0.01	0.07	-0.19
	(0.07)	(0.07)	(0.09)	(0.10)
Share Italian	-0.04	-0.04	-0.03	0.04
	(0.02)*	(0.01)*	(0.01)*	(0.06)
Share men 15-19	-0.03	-0.01	-0.05	-0.07
	(0.03)	(0.04)	(0.05)	(0.02)*
Share men 20-24	0.08	0.09	0.16	0.02
	(0.04)	(0.05)	(0.05)*	(0.03)
Share men 25-29	-0.10	-0.14	-0.24	0.03
	(0.08)	(0.10)	(0.14)	(0.04)
Share men 30-34	-0.04	-0.02	0.07	-0.13
	(0.04)	(0.05)	(0.13)	(0.04)*
Share men 35-39	0.08	0.09	0.04	0.04
	(0.09)	(0.11)	(0.29)	(0.05)
Share men 40-44	-0.13	-0.13	-0.12	-0.07
	(0.09)	(0.11)	(0.25)	(0.06)
R2	0.56	0.59	0.40	0.44
Obs.	629	498	154	690

The outcome variable is total Army and Air Force deaths as a share of total enlistments. Covariates are all 1940 census data. Share farmers is total men employed in farm work / total men employed. Share manu. is share of employment in manufacturing. Share pop urban is share of population living in urban areas. Share men 1+ years college is share of men with at least some college. Share white is fraction of men who are white. Share German and Italian are fractions of population born in Germany and Italy in that country. Each regression is weighted by total enlistments and includes a state-specific constant. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

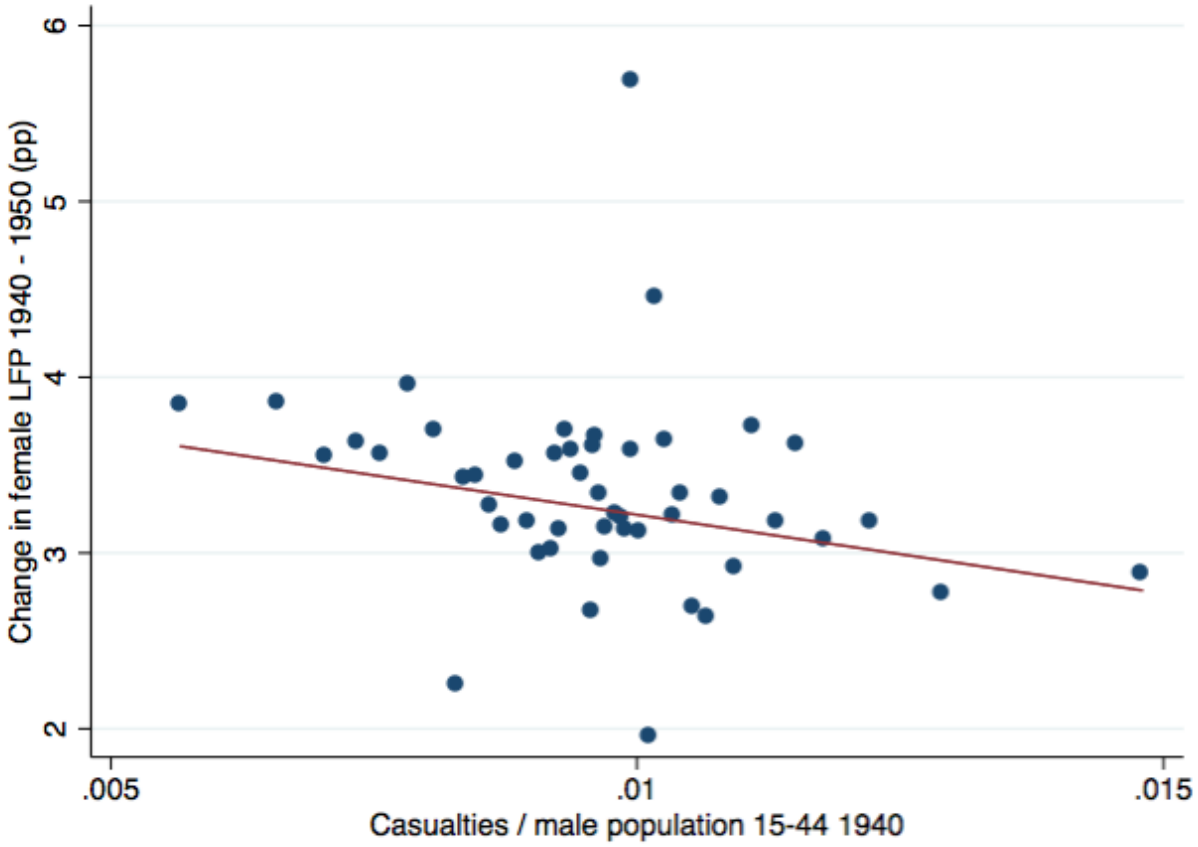

Figure 9: With Controls: Female LFP vs. Mobilization

Figure displays a binned scatter plot with 50 quantiles of enlistments / 1940 male population 15-44 plotted against each quantile's mean change in female labor force participation 1940 to 1950 (in percentage points). The outcome variable is the residual after controlling for state fixed effects, log growth in employment, p.p growth in the share of the population living in urban areas, p.p. growth in the fraction of the population that is white, p.p. growth in the fraction of women over 25 with a high school education, p.p. growth in the share of employment in clerical jobs, and p.p. growth in the shares of women 15 through 34 and 25 through 59. Also plotted is a regression line fit to the underlying data and weighted by 1950 female population. The point estimate of the coefficient is -0.9 with a standard error of 0.26 clustered at the state level. Source: Army enlistment data, National Archives, U.S. Census 1940 and 1950, author's analysis

Table 6: OLS: Determinants of 1940 to 1950 Change in Female LFP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	β/se	β/se	β/se	β/se	β/se	β/se	β/se
Casualties / men 15-44	-0.90	3.00	1.32	1.39	0.80	0.76	
	(0.26)**	(0.80)***	(0.56)*	(0.54)*	(0.46)	(0.48)	
Log pop. growth	-0.16			-0.01			-0.16
	(0.02)***			(0.01)			(0.02)***
Change % urban	-0.01			0.02			-0.01
	(0.01)			(0.01)*			(0.01)
Change % white	-0.12			0.01			-0.11
	(0.06)			(0.08)			(0.06)
Log emp. growth	0.16				0.02		0.17
	(0.02)***				(0.01)***		(0.02)***
Change % emp. clerical	0.56				0.31		0.57
	(0.07)***				(0.06)***		(0.07)***
Change % women 4 years HS	-0.01					-0.00	-0.01
	(0.04)					(0.03)	(0.03)
Change % women 15-34	0.25					0.52	0.23
	(0.07)**					(0.08)***	(0.08)**
Change % women 35-59	-0.00					0.47	-0.03
	(0.07)					(0.12)***	(0.07)
Instrumented enlistments							-0.07
							(0.02)**
State fixed effects	Yes	No	Yes	Yes	Yes	Yes	Yes
R2	0.53	0.04	0.35	0.36	0.39	0.39	0.52
Obs.	2918	2918	2918	2918	2918	2918	2918

The outcome variable is the percentage point change in FLFP from 1940 to 1950. Covariates are all 1940 and 1950 census data. Each regression is weighted by total 1950 population and includes a state-specific constant where noted. Standard errors are clustered at the state level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

The results for $t = 1950$ are reported in Table 6. Column (1) reports the estimating equation on the full sample with the full set of controls and is the basis of the regression line in Figure 9. This specification includes a set of controls known to be correlated with mobilization and that appear most likely to have also influenced the growth in female labor supply. The coefficient estimate of -0.9 implies that a 1 p.p. increase in casualties was associated with 0.9% less growth in FLFP from 1940 to 1950.

Column (2) reports the naive regression of casualties on the growth in female labor force participation, which is the regression shown in Figure 8. Column (3) shows that the addition of state fixed effects alone reduces the coefficient by more than a half. This represents the fact states experienced different mean growth in female labor supply from 1940 to 1950 as a result of different economic and demographic characteristics that also correlate with mobilization at the state level. This was expected – states differ widely in the prevalence of agricultural production, for example, which also drives variation in mobilization.

Column (4) demonstrates that changes in demographic factors including total, urban and white population sizes do not significantly affect the estimated coefficient on mobilization when entered alone, though population growth is important in the full specification. All three of these controls are known correlates of mobilization and also plausibly associated with changes in FLFP.

Column (5) shows that including variables for total employment growth and growth in white collar (clerical and sales) jobs reduces the coefficient by close to half again. This result highlights another important issue not explicitly addressed in the literature to date. For the 1950 census, households reported employment statistics for March 1949, when the U.S. was suffering from the brief but significant 1948-1949 recession, which was caused by downturns in private fixed investment and strong downward pressure on prices (Caplan 1956). If women's labor force participation was more affected by this recession (if women were the first to be laid off, for example), then the 1950 census measures may be pushed down in counties most exposed to it. Given the results shown in column (5), it seems plausible that this

caused positive bias in mobilization coefficients. The recession was worse in slow-growth, agricultural areas, where enlistments were also likely to be lower, and less women may have been working in those places in March 1949 as a result.[17]

Columns (6) shows that controlling for changes in the number of young women also reduces the coefficient on mobilization by roughly one half. These demographic variables are correlated with their male counterparts, which are important drivers of mobilization. These women represent the working-age female population and therefore also correlate with counties' FLFP rates.

Lastly, column (7) shows that instrumenting for $enlist_i$ using cas_i to attempt to correct for measurement error in enlistment data yields similar results. The coefficient estimate of -0.07 is smaller, but there is also more variation in the $enlist_i$ variable. The coefficient in column (7) suggests that a 1 standard deviation increase in enlistments (7%) is associated with a 0.5% decrease in the growth of FLFP. The analogous calculation for casualties from column (1) suggests a 0.3% decrease in the growth of FLFP. In appendix table 11, I report the same table estimated using enlistments on the sub-sample of states where enlistments capture 90% of the known totals or more. We cannot reject small positive or negative effects across all specifications. However, these result should be interpreted with caution given the enlistment data quality.

To account for endogeneity in mobilization rates, Acemoglu, Autor & Lyle (2004) deploy an instrument variables approach. This approach uses 1940 male demographic variables and the share of the population born in Germany in a first stage regression for mobilization. The identifying assumption is that these demographic variables are not also correlated with other omitted variables that impacted the growth in female labor force participation from 1940 to 1950. Because male demographics may impact female labor force participation through family-formation and marriage, I argue these instruments are unlikely to be valid. As shown above, female demographics, which are highly correlated with male demographics, have a

[17]Caplan (1956) notes that a break in farm prices was a major component of the downturn. While private wages and salaries declined 5% in the recession, farm income dropped 35%.

strong relationship with female labor force participation. For completeness, I report a similar IV strategy in appendix table 12. The coefficients on casualty rates are positive, but not significantly different from zero. The first state regressions are reported in table 13. These results should also be interpreted with caution given that the exclusion restriction may be violated.

3.1.2 Individual-Level Results

Because the county-level aggregates are highly sensitive to controls, I employ a different research design that avoids having to select and accurately measure the right set of county-level controls. In particular, I test for differences between the labor force outcomes of women living in high mobilization areas in 1950 who also likely lived there during the war ("stayers") and women who moved into the area from other places ("movers") after the war. By construction, movers to high mobilization counties come from lower mobilization counties on average, and vice verse. Because both groups of women face the same labor demand, both are affected by *the same* county-level variation in factors that influence FLFP (at least through labor demand). Comparing the two groups should therefore account for any bias due to the correlation between mobilization and these factors.

Stayers and movers do not need to have the same average labor force outcomes or characteristics to identify the effect of mobilization, though it turns out their characteristics are similar (see Table 7; note also the relatively small sample size of movers). Instead, we require that mobilization is not also correlated with other factors that differentially affect the labor force outcomes of movers vs. stayers. Though this assumption is not trivially true, I view it as weaker than the assumption that mobilization is exogenous to labor force outcomes conditional on controls, which is required to identify effects in the county-level results presented above and similar specifications estimated in earlier work.

I use the 1950 IPUMS samples to implement this test. Because individual records cannot be identified at the county level in 1950, I aggregate enlistment and casualty data into State

Table 7: Characteristics of Movers vs. Stayers in 1950

	Movers		Stayers	
	mean	sd	mean	sd
Age	33.17	14.77	39.53	17.04
Highest Grade	13.64	3.22	12.58	3.33
Share married	0.72	0.45	0.65	0.48
LFP rate	0.29	0.45	0.29	0.46
Employment rate	0.26	0.44	0.28	0.45
Weeks Worked	12.26	18.12	13.02	20.48
Obs.	4401		139804	

See text for variable definitions. Source: IPUMS, author's analysis.

Economic Areas (SEAs), which form roughly 400 groupings of contiguous and economically intertwined counties within states. Since I also do not observe complete moving histories for all individuals, I define stayers as individuals living in their state of birth who did not move between SEAs in the year before the 1950 census (i.e., after March 1948). Movers are those who changed SEAs in the year before the 1950 census. This variable is measured with error, since the stayers category will include some women who moved within their state of birth between 1940 and 1948. Without more detailed information on individual locations between the 1940 census and 1950 census, this error is unavoidable.[18] There is no obvious reason why it might introduce systematic bias beyond attenuation, however. I include all women aged 15 to 65 and born in the U.S. unless otherwise noted.

The basic estimating equation takes the following form:

$$y_{b,s,i} = \xi_b \cdot \delta_{stay} + \beta_1' \cdot Z_s + \beta_2' \cdot X_i + \beta_3 \cdot cas_s + \beta_4 \cdot \delta_{stay} + \Gamma \cdot \delta_{stay} \cdot cas_s + \epsilon_{b,s,i} \qquad (4)$$

were $y_{b,s,i}$ is the labor force outcome (participation, employment or weeks worked) for woman i living in State Economic Area s and born in state b; $\xi_b \cdot \delta_{stay}$ are separate state-of-birth fixed effects for movers and stayers; Z_s is a vector of county-level aggregates such as the share of agricultural employment; X_i is a vector of individual characteristics such as

[18] In future work, I hope to match stayers in 1950 to the 1940 Census and include only women living in their 1950 county of residence at the outbreak of war.

a quartic in age, marital status, and race; cas_s is the same variable from before calculated over the SEA; and δ_{stay} is an indicator for if the individual is a stayer. The coefficient of interest is Γ, which measures the differential impact of mobilization on stayers vs. movers. This can be thought of as a "double-difference" – the first is the difference between labor force outcomes of movers and stayers; the second is the relationship between this difference and SEA mobilization rates.

Table 8: All Women: Impact of Mobilization on Movers vs. Stayers

	Labor Force Participation			Weeks Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
	β/se	β/se	β/se	β/se	β/se	β/se
Mob-1950-stay	-12.0	-6.6	-6.6	-514.8	-299.0	-258.9
	(5.3)*	(5.2)	(5.2)	(232.8)*	(217.8)	(223.1)
Bpl by stay FE	Yes	Yes	Yes	Yes	Yes	Yes
Age quartic	No	Yes	Yes	No	Yes	Yes
Married	No	Yes	Yes	No	Yes	Yes
Children	No	Yes	Yes	No	Yes	Yes
High grade	No	Yes	Yes	No	Yes	Yes
Race	No	Yes	Yes	No	Yes	Yes
County char.	No	No	Yes	No	No	Yes
County demos	No	No	Yes	No	No	Yes
R2	0.01	0.20	0.20	0.01	0.19	0.20
Obs.	129968	129968	129968	129968	129968	129968

Standard errors are clustered at the SEA level. Source: IPUMS, National Archives, author's analysis.

The results for FLFP rate and mean weeks worked are displayed in Table 8. Columns (1) and (4) show the basic regressions including state of birth fixed effects. The results in columns (2) and (5) control for individual level characteristics including a quartic in age, martial status, presence of children, and highest grade of education. The coefficient is roughly halved in both variables. The negative point estimates suggest a 0.5 p.p. and one half week decrease in response to a one standard deviation increase in casualty rates for FLFP and mean weeks worked respectively. In columns (3) and (6), I include the county-level characteristics that substantially influenced the difference-in-difference estimation above. As expected, they do not affect the results. Clustering at the SEA-level, however, the results

are not precisely estimated, and I cannot reject a modest positive effect.

Interestingly, the coefficients are much larger for the sub-population of white women aged 25-34 with more than 12 years of education, though they are still imprecisely estimated. This is the same sub-population that other authors have identified as most affected in 1950 by mobilization. As noted above, white married women increased their labor force participation substantially more than others during the war. Educated women may have been more likely to remain in the labor force. This does not appear to be the case, as shown in Table 9.

Table 9: White Married Women 25-34 with 12+ Years Education: Impact of Mobilization on Movers vs. Stayers

	Labor Force Participation			Weeks Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
	β/se	β/se	β/se	β/se	β/se	β/se
Mob-1950-stay	-20.0	-12.3	-13.5	-758.6	-344.3	-340.2
	(10.8)	(11.9)	(11.5)	(478.6)	(472.8)	(472.5)
Bpl by stay FE	Yes	Yes	Yes	Yes	Yes	Yes
Age quartic	No	Yes	Yes	No	Yes	Yes
Married	No	No	No	No	No	Yes
Children	No	Yes	Yes	No	Yes	Yes
High grade	No	Yes	Yes	No	Yes	Yes
County char.	No	No	Yes	No	No	Yes
County demos	No	No	Yes	No	No	Yes
R2	0.01	0.17	0.17	0.01	0.22	0.23
Obs.	13504	13504	13504	13504	13504	13504

Standard errors are clustered at the SEA level. Source: IPUMS, National Archives, author's analysis.

Constructing a continuous measure of "treatment" that measures the percentage difference between an individual's current SEA's and their previous SEA's mobilization rates yields similar results. In Table 10, I repeat the same regressions using the following variable instead of cas_i:

$$treat = \frac{cas_{sea,pre-1949}}{cas_{sea,1949}} - 1$$

Individuals who do not move have $treat = 0$. Individuals moving from high mobilization areas to low mobilization areas have $treat > 0$. And individuals moving from low mobilization areas to high ones have $treat < 0$. This specification more explicitly compares women living

Table 10: Continuous Treatment Measure: All Women: Movers vs. Stayers

	Labor Force Participation			Weeks Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
	β/se	β/se	β/se	β/se	β/se	β/se
Mob old SEA / new	-0.02	-0.01	-0.01	-0.73	-0.14	-1.04
	(0.04)	(0.04)	(0.03)	(1.48)	(1.33)	(1.37)
Bpl by stay FE	Yes	Yes	Yes	Yes	Yes	Yes
Age quartic	No	Yes	Yes	No	Yes	Yes
Married	No	Yes	Yes	No	Yes	Yes
Children	No	Yes	Yes	No	Yes	Yes
High grade	No	Yes	Yes	No	Yes	Yes
County char.	No	No	Yes	No	No	Yes
County demos	No	No	Yes	No	No	Yes
R2	0.01	0.20	0.20	0.01	0.19	0.20
Obs.	129833	129833	129833	129833	129833	129833

Standard errors are clustered at the SEA level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis

in high mobilization areas to women who moved there from low mobilization areas, and vice versa. The results are similar – negative but imprecisely estimated coefficients.

3.2 Reconciliation with Previous Estimates

Several previous analyses of World War II and female labor supply have argued that states that experienced higher mobilization rates – measured as the fraction of registrants who ultimately served from each state – had higher levels of female work in 1950. Acemoglu, Autor & Lyle (2004), for example, find that a 10 p.p. (well above the standard deviation of 3.5 p.p.) increase in mobilization rate was associated with 1.1 additional mean weeks worked by women in 1950. Fernandez, Fogli & Olivetti (2004) and Goldin & Olivetti (2013) use the same data and similar methodology to find similar results. I argue that several corrections to the data and specifications employed can account for these results.

The first is a measurement issue. In 1940, census enumerators asked respondents to report the number of *full-time equivalent* weeks worked in the reference year. A full time equivalent week was defined as the "number of hours locally regarded as a full-time week for the given

occupation," or 40 hours if the respondent was unsure. In 1950, however, enumerators counted a week in which *any work was done* as a whole week. This change mechanically inflates intensive labor supply measures for part-time workers. A woman working every Saturday and Saturday only in 1940, for example, would have reported roughly 10 weeks worked. This same women would have reported 52 weeks worked in 1950.

It appears that the prevalence of part-time work is also correlated with mobilization in 1940. The basic point is clear from Figure 10, which use the same registration data, sample and labor force definitions as in Acemoglu, Autor & Lyle (2004). More women were in the labor force during the reference week in 1939 in high mobilization states, but they did not work more weeks on average in the reference year. This is likely driven by the fact that mobilization is correlated with the share of non-farm workers, urban population, education and other factors correlated with the availability of part-time work. Moreover, much of the increase in female labor force participation throughout this period came through the rise of part-time work in white-collar occupations, which was also concentrated in urban areas. The share of female employment that was part-time increased from 18 to 19% overall from 1940 to 1950, for example, but added 11 p.p. in the sales industry (14 to 25%) (Goldin 1991). In appendix Table 14, I report replications of Acemoglu et. al's regressions using both weeks worked and LFP as outcome variables. These regressions show that the effect of mobilization appears only in weeks worked, the variable with measurement issues, and yields negative but imprecise point estimates on labor force participation. Even if the weeks worked variable was not measured with error, it would be surprising to find an effect exclusively along the intensive margin. This would suggest that World War II did not draw net additional women into the work force, but made existing workers increase their supply, which is hard to reconcile with the narrative record.

The second issue may be an incomplete accounting for the correlation between mobilization and other factors likely to affect female labor force participation. As was shown above, the basic relationships at the county-level are highly sensitive to controls. The strat-

egy in most previous attempts has been to control for 1940 characteristics of states that are likely correlated with both mobilization and female labor force participation, such as the share of non-farm employment. These results are robust, though not invariant, to the inclusion of a variety of state-level controls similar to those above. But state-level aggregates may mask important compositional heterogeneity exposed in analyses of smaller geographic units. Moreover, no analyses have offered compelling controls for any potential omitted variable bias due to the inclusion of volunteers in mobilization variables.

3.3 Impacts in 1970

I now explore the impact of mobilization on female labor force participation beyond 1950. To do so, I turn to the 1% population sample of the 1970 census from IPUMS.[19] Because county of residence is unavailable in the 1970 census, I group enlistment and casualty data into roughly 400 "county groups," the smallest geographic unit identifiable. These groups correspond to commuting zones with combined populations over 250,000.

In order to distinguish between direct effects on women who were old enough to work during World War II and effects on the subsequent generation, the baseline specification builds on Equation 4 by incorporating 5-year age groups in the following way:

$$y_{b,s,a,i} = \xi_{b,a} \cdot \delta_{stay} + \beta_1' \cdot Z_s + \beta_2' \cdot X_i + \sum_a \beta_{3,a} \cdot agegrp_a \cdot \delta_{stay} + \sum_a \beta_{4,a} \cdot agegrp_a \cdot cas_s + \sum_a \Gamma_a \cdot agegrp_a \cdot \delta_{stay} \cdot cas_s + \epsilon_{b,s,a,i} \quad (5)$$

where each variable is defined as before, except $\xi_{b,a} \cdot \delta_{stay}$ is now a state-of-birth by age group by stayer fixed effect, which removes differences in average labor force outcomes among movers and stayers of different ages and born in different states, and $agegrp_a$ are dummies for 5 year age bins starting at 15 (i.e., 15-19, 20-24, 24-25, etc.) up to 60 years old. The

[19] Unfortunately, the smallest geographic unit identifiable in the IPUMS 1960 census is the state, so it cannot be combined with my enlistment data at the county level.

coefficients of interest are the Γ_a terms, which shows the differential impact of mobilization on movers vs. stayers for each age group a.

Since I do not observe complete moving histories in the 1970 census, I define stayers as individuals who are living in their state of birth and have not moved outside their county group in the last five years. Movers are those who have moved from outside their county group in the last five years. This variable is likely measured with significant error, since women may have moved before 1964 but after the war into their current county of residence, and will thus be counted as stayers. Unfortunately, given data limitations, this is the only feasible way to identify movers vs. stayers. Although this strategy will likely attenuate the results, there is no obvious reason why it would introduce other systematic biases.

The results are shown in Figure 11, which plots the Γ_a coefficients and 95% confidence intervals for regressions using FLFP as the outcome variable. A positive coefficient indicates that in county groups which experienced high mobilization rates, stayers were more likely to work than movers, conditional on all controls. Since stayers have lived in that county group during the war or after, and movers by construction come from areas with lower mobilization rates, I interpret this as the impact of mobilization on labor force outcomes in 1970.

The coefficients are not significantly different from zero for all women over the age of 40, who would have been old enough to work during the World War II period. This is expected given that there was a limited or negative impact of mobilization on these same women's labor force outcomes in 1950. The younger generation, however, appears to be strongly influenced. The coefficient for women aged 30-34 implies that a one standard deviation increase in mobilization rates increased their likelihood of working by 2 percentage points. The coefficient for women aged 25-29 is similar. These women were too young (some were not yet born) to work during the war. Their mothers, however, were old enough to work.

This evidence does not give any insight into the mechanisms driving these results, and many are plausible. One natural interpretation, for example, is that the Rosies of the war passed on positive attitudes towards work to their daughters through a "role-model" effect.

Since high mobilization counties were home to more soldiers, however, it is also possible that World War II veterans differentially affected their daughters' attitudes towards work. Tackling this question is an interesting area for future research. I note here, however, that whatever mechanism is driving these results appears to be specific to women. The same pattern of labor force participation does not appear for men in 1969 in nearly the same magnitude, as shown in Figure 12, although this may be because men already participated in the labor force to a high degree in 1969, and thus do not have a significant room to increase.

4 Conclusion

I have used a new dataset on Army and Army Air Forces enlistments and casualties to study the impact of manpower mobilization for World War II on FLFP in 1950 and 1970. Since women went to work when and where men went to war, the data provide some insight into whether the large increases in FLFP experienced when Rosie the Riveter and others dove into the war-time economy persisted into peacetime. The evidence suggests, however, that by 1950 women were not working significantly more in counties that experienced higher mobilization rates.

The non-random draft process that supplied America's soldiers during the war and the large share of troops who volunteered complicate inference, since the incidence of mobilization was likely correlated with many factors that also influence female labor force outcomes. Consequently, analyses of aggregated female labor force outcomes are highly sensitive to controls and cannot reject a zero effect in most specifications. An alternative design comparing the outcomes of women living in high mobilization counties in 1950 to women who moved to the same place from lower mobilization counties after the war yields similar results along both the intensive and extensive margin. The point estimates, in fact, often suggest a negative impact, especially among the groups thought to be most influenced by the war:

white, married and educated women. This design relies on a weaker identifying assumption and is not sensitive to the same controls employed in aggregate analysis.

It is important to recognize that these results do not prove that World War II had no immediate impact on female labor force outcomes. Instead, the claim is that it is difficult to detect any impact on labor force participation in 1950 as a result of manpower mobilization, though war-time workers may have been affected nonetheless. It is possible that many Rosies would have kept working if not for demand-side conditions, such as the 1948-1949 recession, and cultural institutions, such as marriage bars, that prevented them from doing so. Transitioning from work in war-time industries, which absorbed most of the increases in female workers from 1940 to 1945, to work in civilian sectors may have been difficult. Manpower mobilization may also not capture other important impacts of the war, such the local effects of government purchases, that also changed FLFP.

The labor force behaviors of the next generation support the view that World War II's effects were more subtle than results for 1950 suggest. Using the same movers-stayers comparisons, I show that though women who were old enough to work during the war are not more likely to be working some 25 years later, the next generation is. A one standard deviation increase in a county-group's exposure to the war, measured using war casualties as a fraction of the military-aged male population, is associated with a 2 percentage point increase in the likelihood of a 30-35 year-old working in 1970. The effect appears to be specific to daughters. And since it is identified using comparisons of women living in the same place, the effect appears to operate primarily through labor supply.

With the data at hand, it is not possible to identify the mechanisms driving this effect. We cannot separate explanations focused on "economic" factors such as skills and networks passed across generations from the role of "culture" broadly defined, both of which have been shown to play a part in other contexts. Ultimately, persistent effects due to any mechanism are surprising in light of the relatively short duration of the the war, the exceptional nature of the work women did during it, and lack of effects by 1950. Investigating how the war

experience may have affected both cultural attitudes and the economic determinants of female labor supply at a longer horizon is an interesting area for further study.

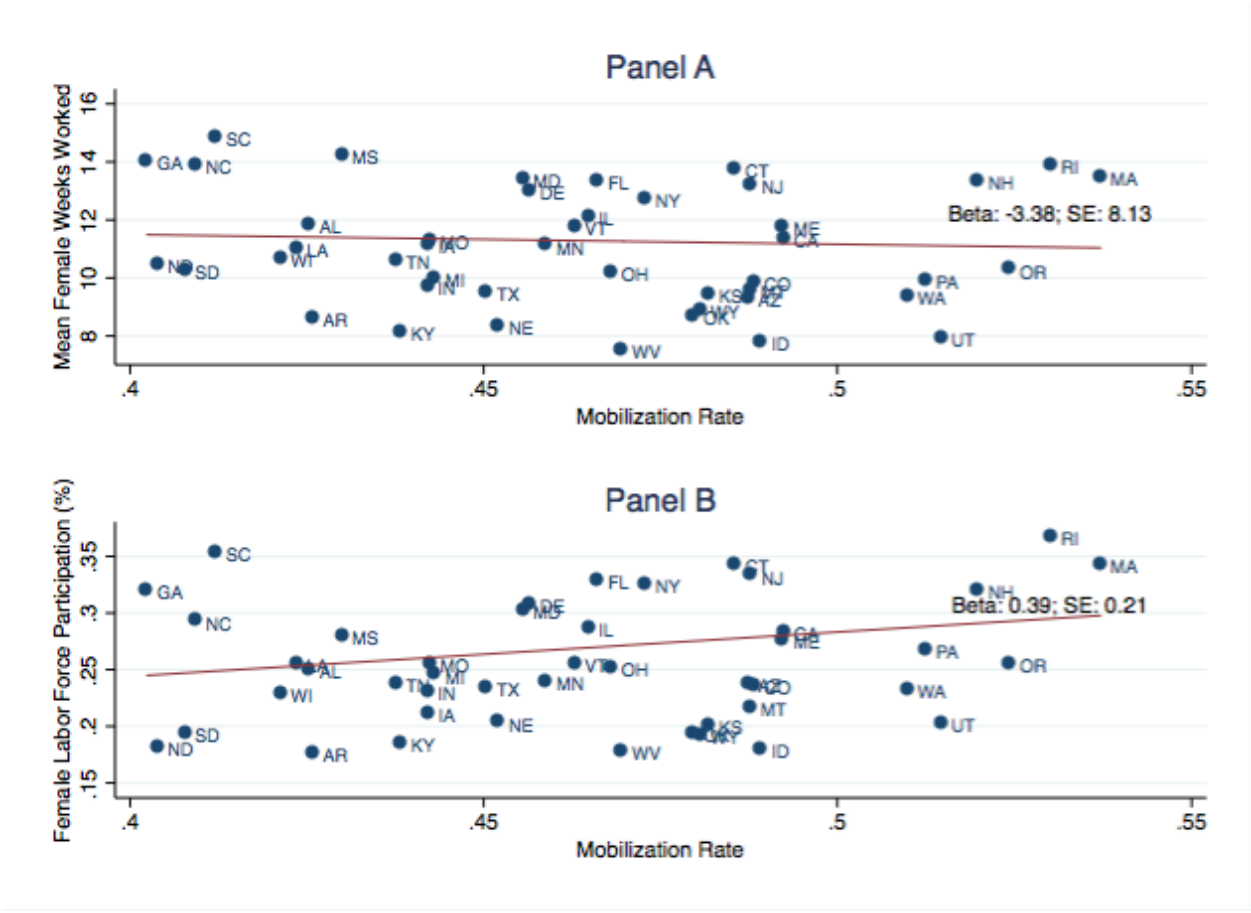

Figure 10: Mobilization and Part-Time Employment in 1940

Panel A displays state-level relationship between mean weeks worked by women against mobilization rates. Weeks worked includes both employed and unemployed women aged 14 through 64 and not living in DC or Nevada. Panel B displays average female labor force participation during the 1939 Census reference week. Respondents in the labor force entered 1. Respondents not in the labor force entered 0. Mobilization is defined as in Acemoglu, Autor & Lyle (2004) as total enlistments and inductions in each state / total registrants in that state from 1940 to 1945 and is taken from Selective Service Administration manuscripts. Labor force statistics are from the 1940 1% IPUMS sample. Regression lines are fitted values from regressions weighted by the total number of women aged 14 through 64 in each state. Source: Selective Service Administration, U.S. Census 1940 and 1950, author's analysis.

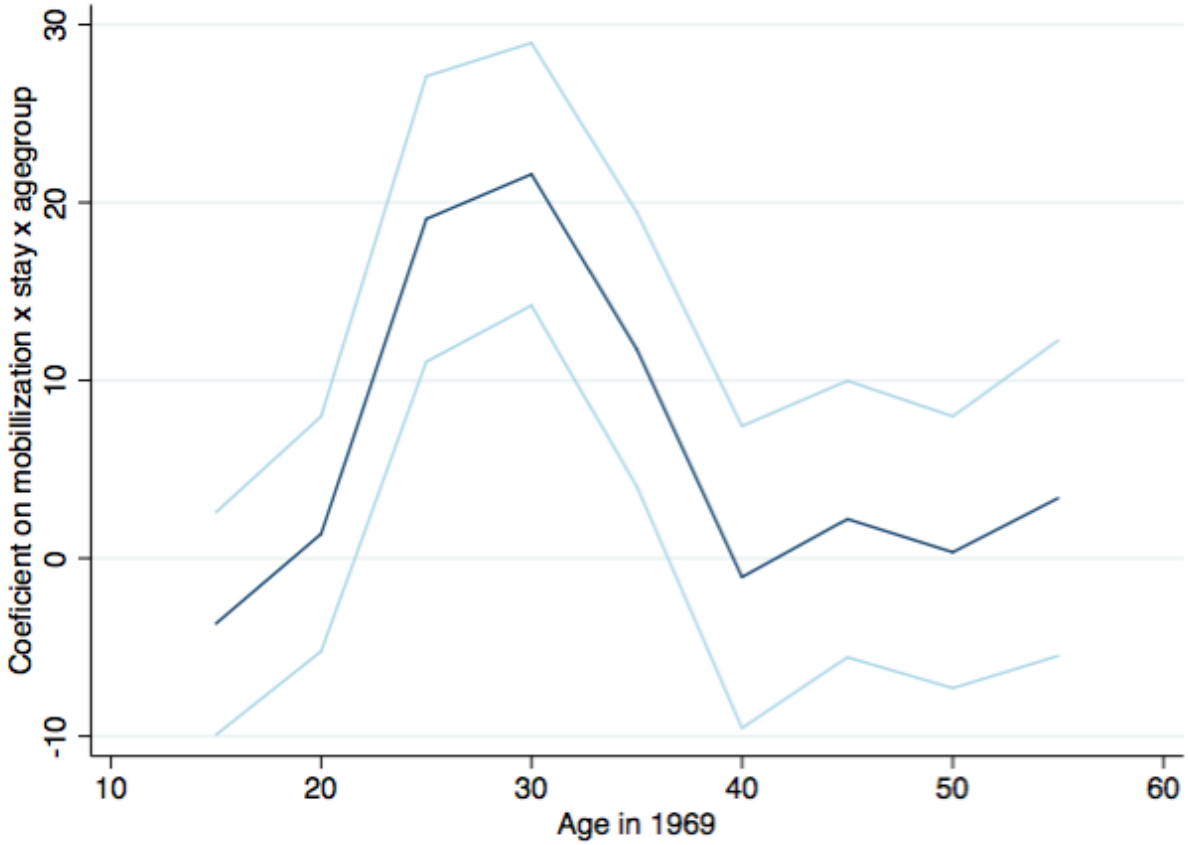

Figure 11: Impact of Mobilization on Women in 1970

Figure displays the coefficients Γ_a from equation 5 and 95% confidence intervals. The coefficient can be interpreted as the differential impact of mobilization in 1969 on women of different ages who moved into high mobilization counties after the war vs. those who may have lived there during the war. A positive coefficient implies that mobilization had a larger positive impact on labor force outcomes of those who may have lived in that area during the war only. Estimation includes women aged 15 to 60 born in the U.S. and not working in farm industries. Source: IPUMS, National Archives, U.S. Census 1970, author's analysis.

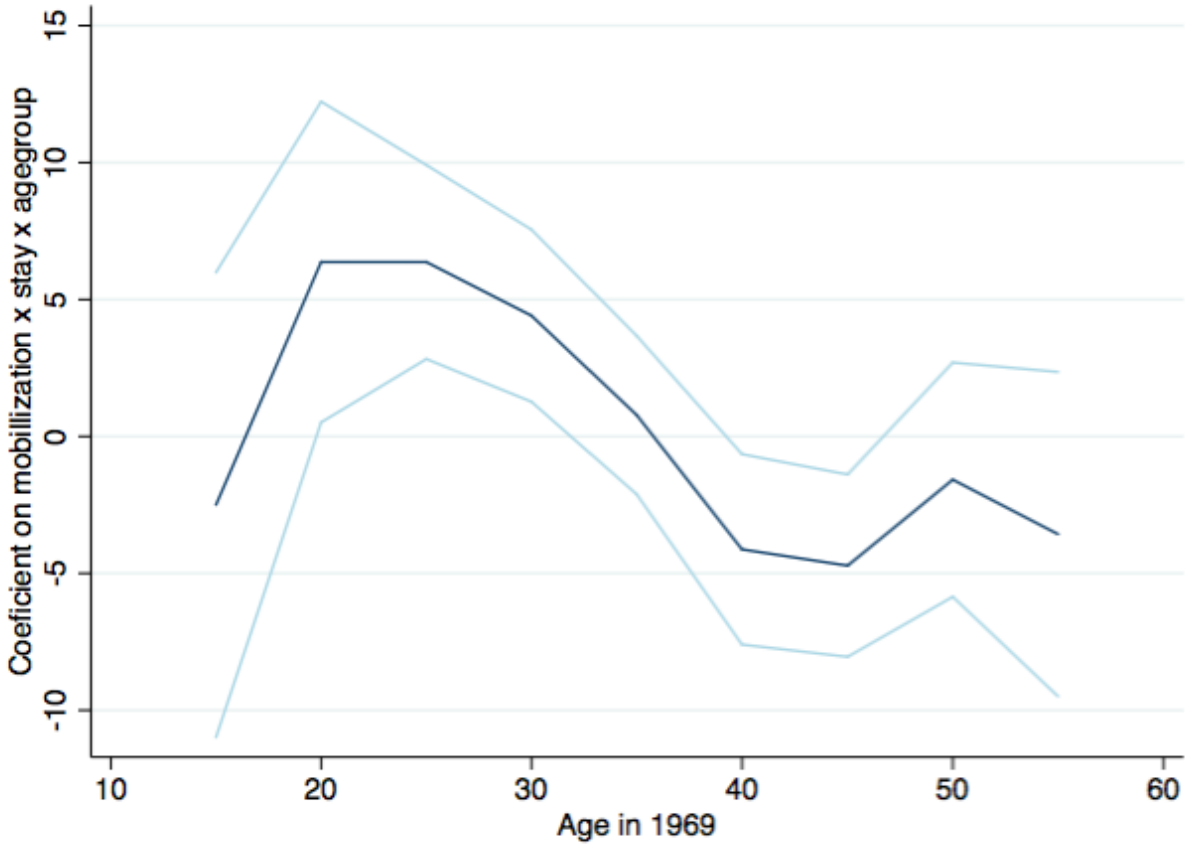

Figure 12: Impact of Mobilization on Men in 1970

Figure displays the coefficients Γ_a from equation 5 and 95% confidence intervals estimated on men aged 15 to 60 in 1969. The interpretation is the same as in figure 11. Source: IPUMS, National Archives, U.S. Census 1970, author's analysis.

A Appendix Figures and Tables

A.1 Jaworski 2014 Replication

Since the 1960 census educational variables for women have not been aggregated at the county level, I use 1950 census data on the share of high-school dropouts among women 25 and older.[20] Since only women at least 20 years old in 1945 will be included in this group – meaning women graduating from high school in 1944 and 1945 are excluded – this is a crude measure for dropout rates for women in high-school during the war. Still, to the extent that the measure under-reports the war's impact on eduction, the check will be biased down only.

As expected, the results shown in figure 13 suggest that after conditioning on state fixed effects, population size, urban-share of population, race and female demographics, casualties are associated with lower female educational achievement. OLS coefficients suggest a one standard deviation increase in casualty rates are associated with a 0.5% increase in the fraction of women 25 or older without high school education. Instrumenting for enlistment rates using casualty rates and using the same set of controls suggests an increase of 0.8%.

In addition, I use the IPUMS micro samples from 1940 and 1950 to estimate a model of the following form:

$$y_{b,s,i,t} = \delta_b + year_t + X'_{s,i,t} \cdot \beta + \gamma \cdot year_t \cdot cas_s + \epsilon_{s,i,t} \tag{6}$$

where $y_{s,i,t}$ is a measure of highest grade of schooling achieved by time t for individual i born in state b living in State Economic Area (SEA) s and time t, δ_b are state of birth fixed effects, $X'_{s,i,t}$ is a vector of time-varying covariates including race and marital status, $year_t \times cas_s$ is the measure of mobilization aggregated to the SEA level and interacted with a dummy for 1950, and $\epsilon_{s,i,t}$ is a disturbance term. Appendix figure 14 plots the coefficients of repeated regressions of the same form as equation 6 for women born in groups of four

[20] This is the educational variable reported in census documents as county aggregates. Ideally, one would like to use finer age gradations, as well as women under 25.

years from 1900 to 1932. As in Jaworski's findings at the state level, women in counties that experienced high levels of mobilization had less schooling. For example, coefficient estimates suggest that women born between 1916 and 1920, who were in their early 20's during the war, accumulated roughly 1 year less of schooling. This coefficient is in line with Jaworksi's findings, although his effects are focused on women in 1923 to 1931 birth year cohorts.

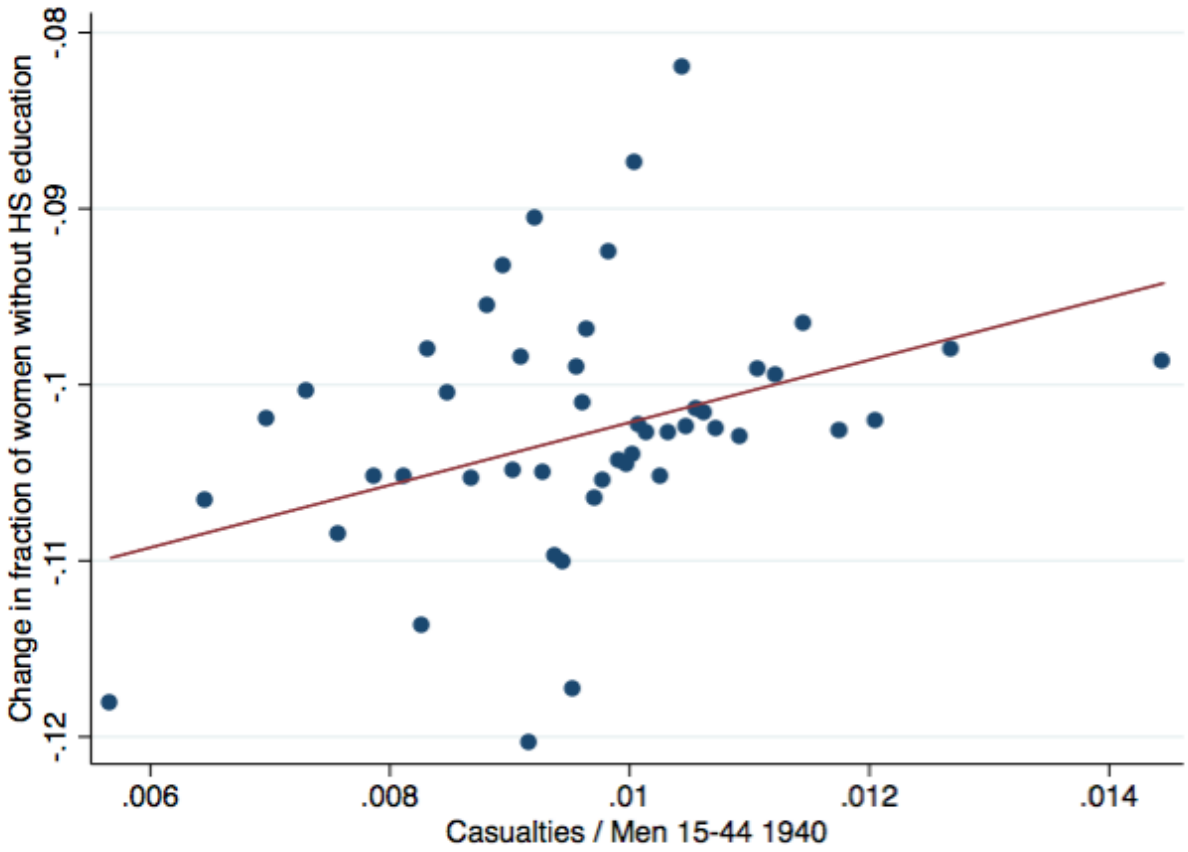

Figure 13: Impact of Mobilization on Share of Women without High School Exposure
Figure displays a binned partial association scatter plot of the change in the fraction of women over 25 with 8 years of education or less from 1940 to 1950. The outcome is the residual after controlling for state fixed effects, log total population 1950, change in log population 1940 to 1950, change in urban share of the population 1940 to 1950, change in white share of population 1940 to 1950 and change in log female population aged 15 to 34 from 1940 to 1950. The resulting regression line (including all controls) is plotted as well. The coefficient on mobilization is 1.734 with a standard error of 0.266. Both the scatter plot and regression are weighted by the size of the female population 25 or older in each county in 1950. Source: Army enlistment data, National Archives, U.S. Census 1940 and 1950, author's analysis.

Figure 14: Impact of Mobilization on Highest Grade Completed by Birth Year
Figure plots the coefficients on the interaction of the 1950 dummy with mobilization rates in repeated regressions of the form of 6 on white women born in groups of four years from 1900 to 1950 (rolling). The dotted lines are plus or minus one standard error. The dependent variable is the highest grade of schooling achieved. Controls include fixed effects for state of birth and year and a dummy for married women. Racial controls are not included, since the model is estimated using white women only. Source: Army enlistment data, National Archives, IPUMS, author's analysis.

A.2 Additional Figures and Tables

Table 11: OLS: Enlistments in 90% States and 1940 to 1950 Change in Female LFP

	(1)	(2)	(3)	(4)	(5)	(6)
	β/se	β/se	β/se	β/se	β/se	β/se
Enlistments / men 15-44	0.01	0.06	0.10	0.11	0.08	0.10
	(0.06)	(0.06)	(0.08)	(0.07)	(0.08)	(0.08)
Log pop. growth	-0.15			-0.02		
	(0.03)***			(0.02)		
Change % urban	0.02			0.03		
	(0.01)			(0.02)		
Change % white	-0.29			-0.23		
	(0.08)**			(0.09)*		
Log emp. growth	0.13				0.01	
	(0.03)**				(0.02)	
Change % emp. clerical	0.67				0.01	
	(0.15)***				(0.13)	
Change % women 4 years HS	0.01					-0.03
	(0.07)					(0.06)
Change % women 15-34	0.22					0.37
	(0.18)					(0.20)
Change % women 35-59	0.13					0.65
	(0.14)					(0.30)
State fixed effects	Yes	No	Yes	Yes	Yes	Yes
R2	0.52	0.01	0.35	0.39	0.35	0.39
Obs.	629	629	629	629	629	629

The outcome variable is the percentage point change in female labor force participation from 1940 to 1950. Covariates are all 1940 and 1950 census data. Each regression is weighted by total 1950 population and includes a state-specific constant where noted. Standard errors are clustered at the state level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

Table 12: IV: Determinants of 1940 to 1950 Change in Female LFP

	Age, German	Age only	German only
	(1)	(2)	(3)
	β/se	β/se	β/se
Casualties / men 15-44	2.56	2.68	1.01
	(3.24)	(4.05)	(3.33)
Log emp. growth	0.14	0.14	0.15
	(0.03)***	(0.04)***	(0.03)***
Log pop. Growth	-0.14	-0.14	-0.15
	(0.02)***	(0.03)***	(0.03)***
Change % urban	-0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)
Change % white	-0.11	-0.11	-0.09
	(0.05)*	(0.05)*	(0.05)
Change % women 4 years HS	-0.00	-0.00	0.01
	(0.05)	(0.05)	(0.04)
Change % emp. clerical	0.50	0.50	0.49
	(0.11)***	(0.12)***	(0.09)***
Change % women 15-34	0.21	0.21	0.25
	(0.06)***	(0.06)***	(0.09)**
Change % women 35-59	0.01	0.01	-0.01
	(0.08)	(0.08)	(0.07)
Obs.	2918	2918	2918

The outcome variable is the percentage point change in female labor force participation from 1940 to 1950. Covariates are all 1940 and 1950 census data. Each regression is weighted by total 1950 population and includes a state-specific constant. Standard errors are clustered at the state level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

Table 13: IV: First Stage Regressions for Mobilization Rate

	(1)	(2)	(3)
	β/se	β/se	β/se
Share men 15-19	0.01	0.01	
	(0.02)	(0.03)	
Share men 20-24	-0.02	-0.02	
	(0.01)	(0.01)	
Share men 25-29	0.02	0.03	
	(0.02)	(0.02)	
Share men 30-34	-0.04	-0.05	
	(0.03)	(0.03)	
Share pop born in Germany	-0.03		-0.05
	(0.01)***		(0.01)***
R2	0.31	0.30	0.29
Obs.	2918	2918	2918

The outcome variable is the percentage point change in female labor force participation from 1940 to 1950. Covariates are all 1940 and 1950 census data. Each regression is weighted by total 1950 population and includes a state-specific constant. Standard errors are clustered at the state level. Source: U.S. Census, National Archives, Selective Service Administration, author's analysis.

Table 14: Impact of Mobilization on Weeks Worked as in Acemoglu et. al. vs. LFP

	Weeks Worked			LFP		
	(1)	(2)	(3)	(4)	(5)	(6)
	β/se	β/se	β/se	β/se	β/se	β/se
Mobilization rate · 1950	11.20	9.90	10.42	-0.15	-0.17	-0.17
	(2.69)***	(2.87)**	(2.82)***	(0.06)*	(0.07)*	(0.07)*
State FE	Yes	Yes	No	Yes	Yes	No
Age dummies	No	Yes	No	No	Yes	No
Married dummy	No	Yes	No	No	Yes	No
Birthplace FE	No	Yes	Yes	No	Yes	Yes
Mobilization · 1940	No	No	Yes	No	No	Yes
R2	0.01	0.19	0.01	0.01	0.23	0.01
Obs.	525799	525799	525799	525799	525799	525799

Outcome variables are either annual weeks worked or labor force participation during census reference week for sample as defined in Acemoglu, Autor & Lyle (2004): Women aged 14 to 64 not living in institutional quarters, not employed in farming and living in the continental U.S. excluding DC and NV. To parallel Table 5 Panel A in Acemoglu, Autor & Lyle (2004), only white women are included. All covariates except state of residents and birth are interacted with a 1950 variable in addition to main effects. Standard errors are clustered at the state of residence levels. Census weights (including sample-line weights for 1950) are used. Source: Selective Service Administration, IPUMS.

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