Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury

Daron Acemoglu and David H. Autor

Massachusetts Institute of Technology and National Bureau of Economic Research

David Lyle

U.S. Military Academy

We exploit the military mobilization for World War II to investigate the effects of female labor supply on the wage structure. The mobilization drew many women into the workforce permanently. But the impact was not uniform across states. In states with greater mobilization of men, women worked more after the war and in 1950, though not in 1940. These induced shifts in female labor supply lowered female and male wages and increased earnings inequality between high school– and college-educated men. It appears that at midcentury, women were closer substitutes for high school men than for those with lower skills.

I. Introduction

In 1900, 82 percent of U.S. workers were men, and only 18 percent of women over the age of 13 participated in the labor force. As shown in figure 1, this picture changed radically over the course of a century. In 2001, 47 percent of U.S. workers were women, and 61 percent of women

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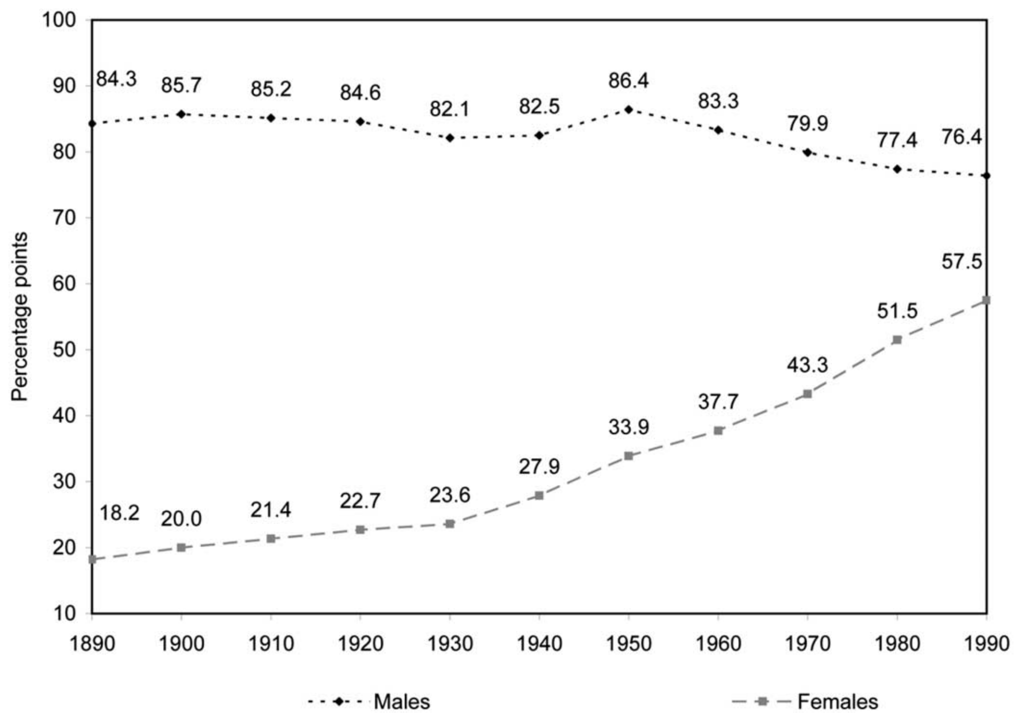

FIG. 1.—Labor force participation by gender of U.S. residents, 1890–1990. Source: Blau, Ferber, and Winkler (2002, table 4.1). Participation rates pertain to the total population prior to 1950 and the civilian population thereafter. Data include individuals 14+ years of age prior to 1950 and 16+ years thereafter.

over the age of 15 were in the labor force. Despite these epochal changes in women's labor force participation, economists currently know relatively little about how female labor force participation affects the structure of male and female wages.

The relative scarcity of convincing studies on this topic reflects the complexity of the phenomenon: increased labor participation of women is driven by both supply and demand factors. Women participate in the labor force more today than 100 years ago for a myriad of supply-side reasons including changes in tastes, gender roles, and technology of household production. But women also participate more because there is greater demand for their labor services. To advance our understanding of how rising female labor force participation affects male and female earnings levels, we require a source of "exogenous" variation in female labor supply.

In this paper, we study female labor force participation before and after World War II (WWII) as a source of plausibly exogenous variation in female labor supply. As evocatively captured by the image of Rosie the Riveter, the war drew many women into the labor force as 16 million men mobilized to serve in the Armed Forces, with over 73 percent deploying overseas. As depicted in figure 2, only 28 percent of U.S. women over the age of 15 participated in the labor force in 1940. By 1945 this figure exceeded 34 percent.[1] Although, as documented by Goldin (1991), more than half of the women drawn into the labor force by the war left again by the end of the decade, a substantial number also remained (see also Clark and Summers 1982). In fact, the decade of the 1940s saw the largest *proportional* rise in female labor force participation during the twentieth century.

Although this aggregate increase in female labor force participation is evident from figures 1 and 2, it is not particularly useful for empirical analysis; the end of the war and other aggregate factors make the early 1950s difficult to compare to other decades. But, central to our research strategy, the extent of mobilization for the war was not uniform across U.S. states. While in some states, for example, Massachusetts, Oregon, and Utah, almost 55 percent of men between the ages of 18 and 44 left the labor market to serve in the war, in other states, such as Georgia, the Dakotas, and the Carolinas, this number was between 40 and 45 percent. These differences in mobilization rates reflect a variety of factors, including exemptions for farmers; differences in age, ethnic, and occupational structures; as well as idiosyncratic differences in the behavior of local draft boards. We exploit differences in state WWII mobilization rates, as well as components of these mobilization rate dif-

[1] For convenience, we refer to census years as 1940, 1950, etc. In reality, census data provide labor supply information for the prior calendar year.

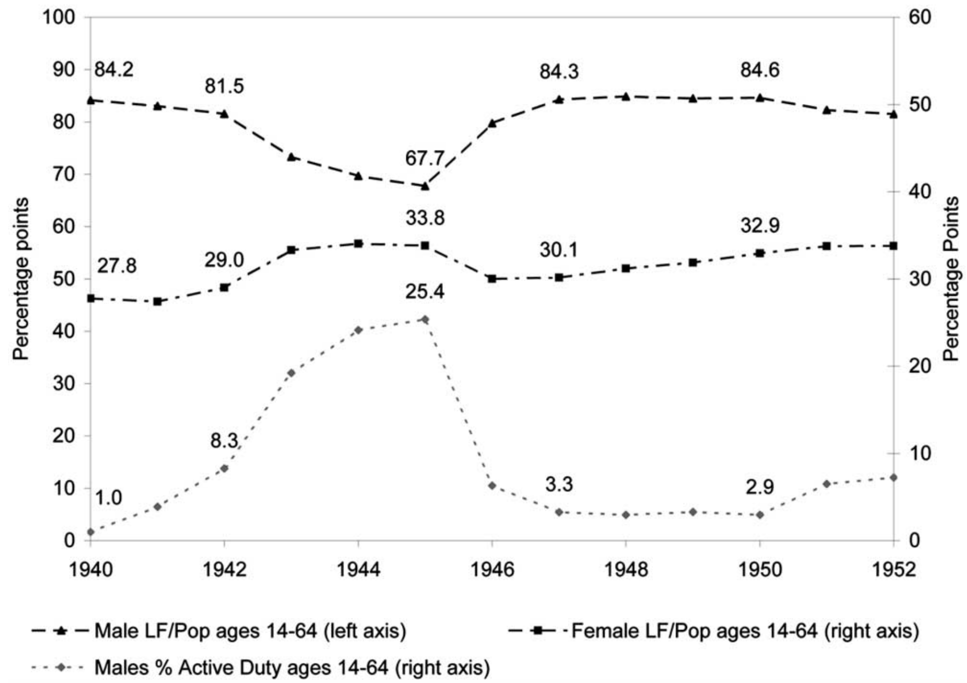

Fig. 2.—Male and female labor force participation and military active service personnel, 1940–52. Source for employment and active service data: *Statistical Abstract of the United States* (1944/45, 1951, 1954), based on census data for 1940–44 and Current Population Reports, ser. P-50 and P-57 for 1943–52. Population denominators for all years are interpolated by the authors using the 1940 and 1950 census IPUMS (Ruggles et al. 1997).

ferences that are plausibly exogenous to other labor market outcomes, to study women's labor supply.

Figures 3 and 4 show that women worked substantially more in 1950—but not in 1940—in states with greater mobilization of men during the war. The mobilization variable is the number of men 18–44 who served divided by the number registered in each state. Our baseline estimates suggest that women worked, on average, about 1.1 more weeks in a state that had a 10-percentage-point higher mobilization rate during WWII, corresponding to a nine-percentage-point increase in female labor supply. This difference is not accounted for by differences in age structure, racial structure, education, or the importance of farming across these states; nor is it explained by differences in occupational structure, regional trends in labor supply, or contrasts between southern and nonsouthern states. Our interpretation is that these cross-state changes in female employment were caused by the greater participation of women during the war years, some of whom stayed in the labor market after the war ended. Notably, we find in figure 5 that the sizable association between WWII mobilization rates and growth in female labor supply over the 1940s did *not* recur in the 1950s, lending support to the hypothesis that these shifts were caused by the war, and not by differential long-run trends in female employment.

Figure 6 shows an equally strong relationship between female wage growth over the 1940s and WWII mobilization rates: in states with greater mobilization for war, female wages grew much less. Figure 7 shows a negative relationship for male wages as well, but the slope of the relationship is considerably less steep.

We interpret the relationships shown in figures 6 and 7 as the causal effect of the WWII-induced increase in female labor supply on female and male wages. As figure 2 shows, the aggregate demand shock that drew many women into the labor force during the mobilization years had reversed itself by 1947. But women continued to work in greater numbers after 1947, presumably because employment during the war changed their preferences, opportunities, and information about available work.

Our interpretation of the relationship between mobilization, female labor supply, and wage growth faces two major challenges. First, high- and low-mobilization states may differ in other unobserved dimensions, and these factors may account for the differential cross-state growth in female labor supply during the 1940s. Second, mobilization of men for war may have had a direct effect on labor demand in the postwar years, distinct from its impact on female labor supply. For example, WWII veterans may have had difficulty reintegrating into the workforce or may have entered school instead because of the opportunities offered by the GI Bills (Bound and Turner 1999; Stanley 2003). In this case, the growth

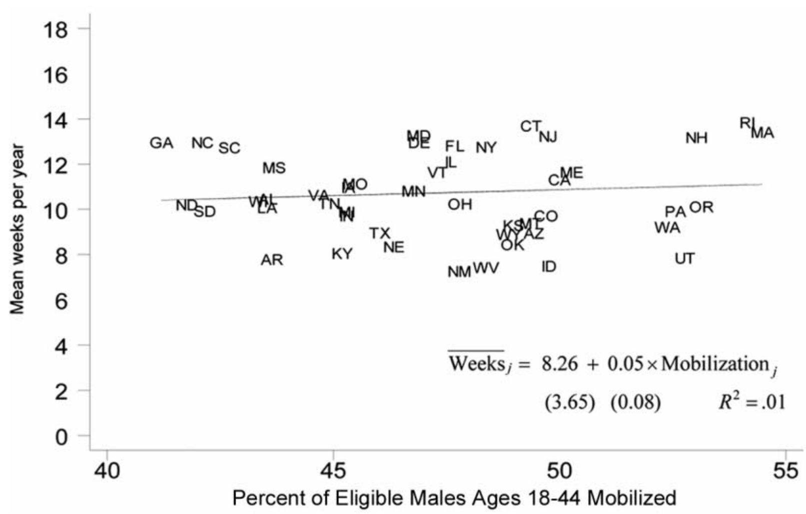

FIG. 3.—State WWII mobilization rates and mean female weeks worked per year, 1940

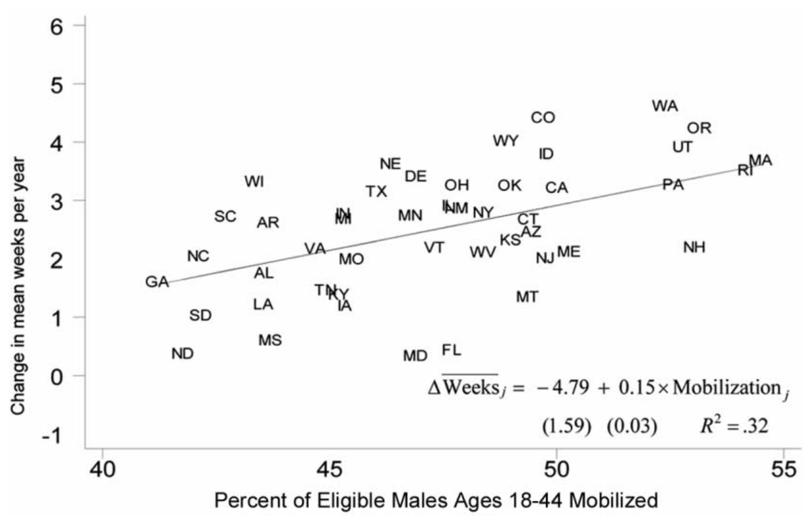

FIG. 4.—State WWII mobilization rates and change in mean female weeks worked per year, 1940–50.

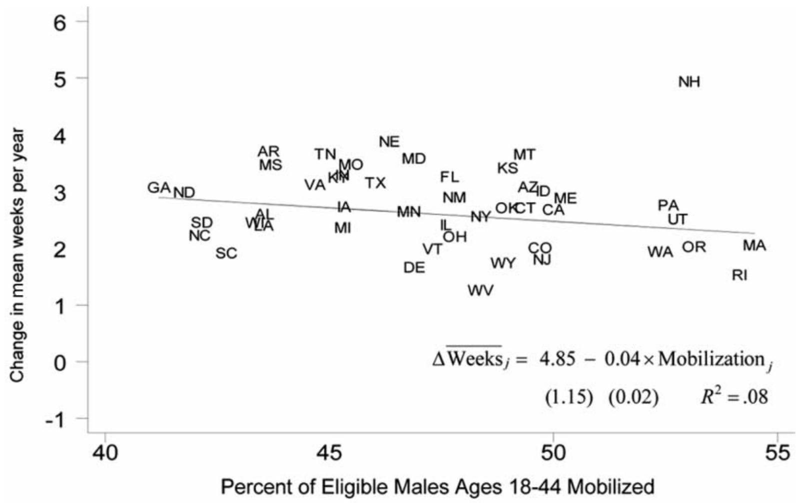

FIG. 5.—State WWII mobilization rates and change in mean female weeks worked per year, 1950–60.

of female labor force participation would reflect shifts in labor demand rather than shifts in female labor supply, which would substantially alter the interpretation of any wage consequences.

Although we cannot entirely dismiss these two interpretations, we provide evidence to suggest that they are not the primary source of our findings. Our results are typically robust to including a variety of aggregate characteristics of states, including the fraction of farmers before the war and racial, educational, and occupational structures. We also obtain similar results when we focus on the component of mobilization rate generated by cross-state differences in aggregate age and ethnic structure, which were important determinants of state mobilization rates and should have no direct effect on growth of the female labor supply once we condition on individual age and ethnicity. These findings weigh against an interpretation along the lines of the first objection above. Moreover, female labor force participation did not vary systematically between high- and low-mobilization states prior to the war, suggesting that these states were initially broadly comparable along this dimension. Finally, figure 5 documents that high-mobilization states did not experience faster growth in female employment between 1950 and 1960, and we show below that there are also no differential state employment trends correlated with WWII mobilization during the 1930s.

If, on the other hand, the second concern were important—that is, if returning veterans had trouble reintegrating into the labor market—there should be lower labor force participation among men in 1950 in

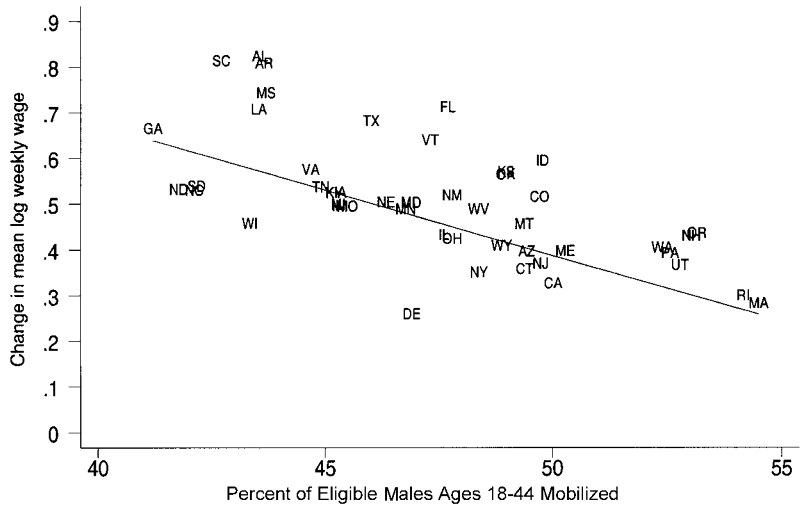

FIG. 6.—State WWII mobilization rates and change in mean log weekly real wages (1959 dollars) of full-time female workers, 1940–50.

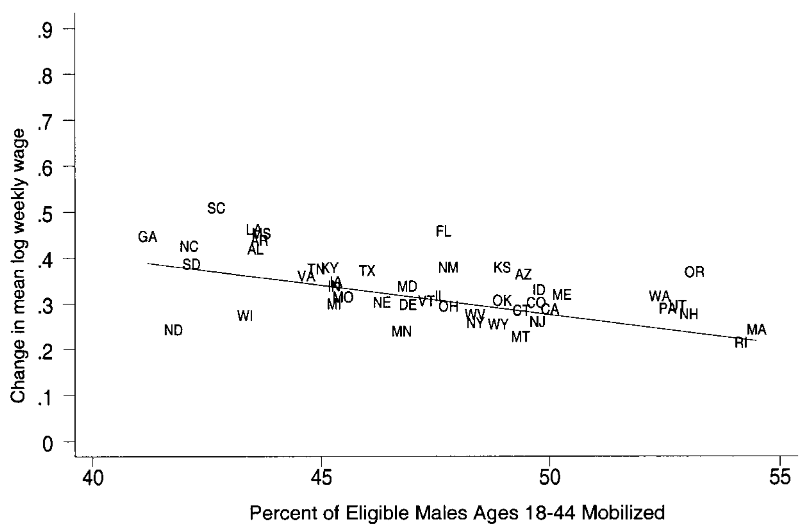

FIG. 7.—State WWII mobilization rates and change in mean log weekly real wages (1959 dollars) of full-time male workers, 1940–50.

high-mobilization states. We find that this is not the case. Furthermore, if greater female participation in 1950 were driven by demand rather than supply factors, we would expect relatively greater wage growth for both women and men in high-mobilization states. Instead, consistent with our interpretation, figures 6 and 7 show that both men and women earned relatively less in high-mobilization states in 1950 than in 1940. Nor are our results driven by cross-state wage convergence between agricultural and industrialized states during the 1940s (e.g., Wright 1986); in specifications that control for lagged state wage measures, we continue to find a significant impact of mobilization on the structure of male and female earnings. Finally, figures 8 and 9 show no relationship between state WWII mobilization rates and wage growth between 1950 and 1960. Hence, the cross-state correlations that we exploit between WWII mobilization and female labor supply or relative wage changes by gender appear unique to the WWII decade.

Exploiting the differential growth in female employment between 1940 and 1950 related to cross-state differences in WWII mobilization, we estimate the impact of female employment on earnings level by gender and education. Our main findings are as follows:

1. Greater female labor supply reduces female wages. A 10 percent increase in female labor supply relative to male labor supply lowers female wages by 7–8 percent, implying a labor demand elasticity of -1.2 to -1.5.
2. Greater female labor supply also reduces male wages. A 10 percent increase in relative female labor supply typically lowers male earnings by 3–5 percent.
3. The combination of these two findings indicates that male and female labor inputs are imperfect substitutes, with an elasticity of substitution of around three.
4. The impact of female labor supply on male earnings is not uniform throughout the male earnings distribution. Women drawn into the labor market by the war were closer substitutes for men at the middle of the skill distribution than for those with either the lowest or highest education.

These estimates conceptually correspond to short-run elasticities since we are looking at equilibria in state labor markets shortly after the war, that is, shortly after the changes in female labor supply. Migration, changes in interstate trade patterns, and changes in technologies could make the long-run relationship between labor market outcomes and female labor supply quite different from the short-run relationship.

The economics literature on the effect of WWII on female labor force participation and the effect of female labor supply on the structure of wages contains a small number of well-known contributions. The paper

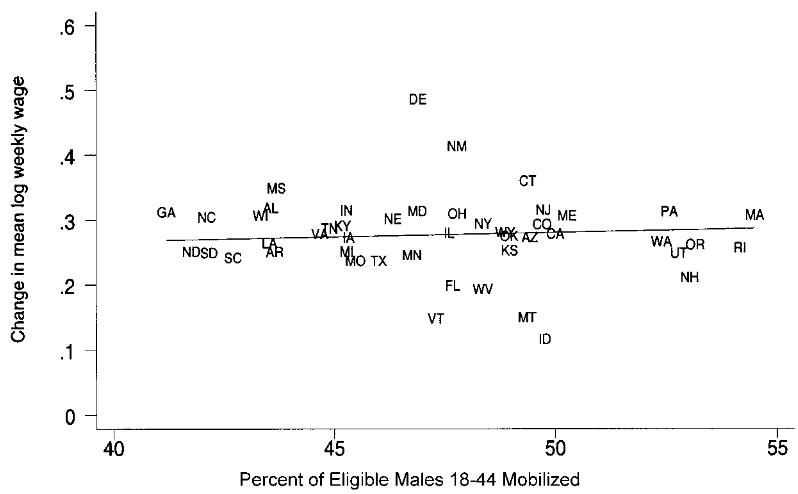

FIG. 8.—State WWII mobilization rates and change in mean log weekly real wages (1959 dollars) of full-time female workers, 1950–60.

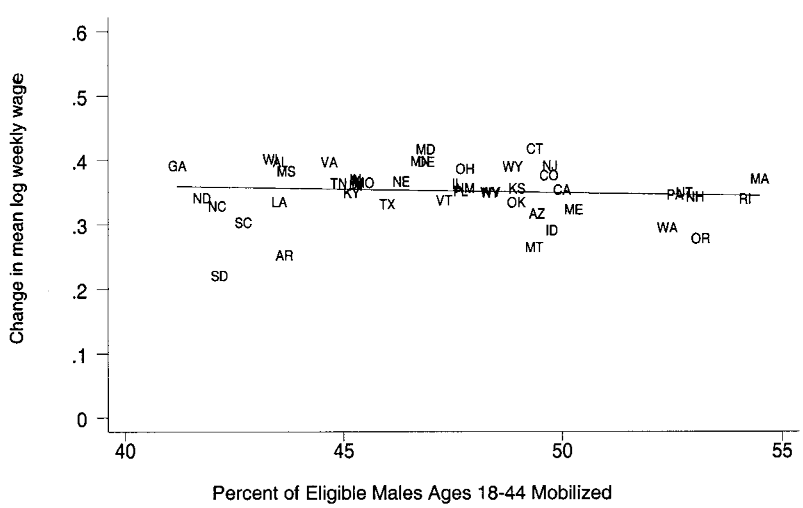

FIG. 9.—State WWII mobilization rates and change in mean log weekly real wages (1959 dollars) of full-time male workers, 1950–60.

by Goldin (1991) is most closely related to our work. She investigates the effects of WWII on women's labor force participation and finds that a little over half of the women who entered the labor market during the war years exited by 1950. Our labor supply estimates appear consistent with these findings, though differences in the sample frame make exact comparisons difficult. Mulligan (1998) investigates the causes of the increase in labor supply during the war and concludes that nonpecuniary factors rather than market incentives drove this growth. Neither Goldin nor Mulligan nor, to the best of our knowledge, any other author investigates the relationship between cross-state mobilization rates and female labor supply, or the causal effect of the induced change in female labor supply on labor market outcomes of men.[2]

In Section II, we briefly discuss the predictions of a simple competitive model regarding the effect of increased female labor force participation on male labor market outcomes. Section III describes our micro data and documents the correlation between female employment and a range of female and male labor market outcomes. In Section IV, we provide a brief overview of the draft and enlistment process for WWII and explain the causes of the substantial differences in mobilization rates across states. Section V documents the relationship between WWII mobilization rates and female labor supply in 1950 and argues that mobilization rates generate a plausible source of exogenous variation in female labor supply. Section VI contains our main results. It exploits cross-state differences in female labor supply induced by mobilization rates to estimate the impact of increased female labor supply on female wages, male wages, and the returns to education among men. Section VII presents conclusions.

II. Some Simple Theoretical Ideas

To frame the key questions of this investigation, it is useful to briefly discuss the theoretical implications of increased female labor force participation. Let us start with a competitive labor market consisting of three factors: male labor, M_t, female labor, F_t, and capital, K_t, which stands for all nonlabor inputs. Imagine that all these factors are imperfectly substitutable in the production of a single final good. In particular, consider the following nested constant elasticity of substitution

[2] Dresser (1994) studies the relationship between federal war contracts and labor market participation of women across metropolitan areas and finds that metropolitan statistical areas that had a relatively large number of war contracts during the war experienced differential increases in female labor force participation between 1940 and 1950. Goldin and Margo (1992) provide the seminal work on changes in the overall structure of earnings during the decade of the war. For excellent syntheses of the state of knowledge of the role of women in the labor force, see Goldin (1990, 1994), O'Neill and Polachek (1993), Blau and Kahn (1994, 1997, 2000), and Blau et al. (2002).

aggregate production function:

$$Y_t = A_t K_t^{\alpha}[(1-\lambda)(B_t^M M_t)^{\rho} + \lambda(B_t^F F_t)^{\rho}]^{(1-\alpha)/\rho}, \tag{1}$$

where $\rho \leq 1$, λ is the share parameter, A_t is a neutral productivity term, and the B_t's are factor-augmenting productivity terms. In particular, B_t^F is an index of female productivity, which may reflect observed or unobserved components of female human capital as well as technical change favoring women relative to men. This specification implies that the elasticity of substitution between the labor aggregate and nonlabor inputs is equal to one and the elasticity of substitution between female labor and male labor is $\sigma_{MF} \equiv 1/(1-\rho)$.

Since in competitive markets all factors will be paid their marginal product, we have that female and male wages are given by

$$w_t^F = (1-\alpha)\lambda B_t^F A_t K_t^{\alpha}(B_t^F F_t)^{-\alpha}\left[(1-\lambda)\left(\frac{B_t^M M_t}{B_t^F F_t}\right)^{\rho} + \lambda\right]^{(1-\alpha-\rho)/\rho} \tag{2}$$

and

$$w_t^M = (1-\alpha)(1-\lambda)B_t^M A_t K_t^{\alpha}(B_t^M M_t)^{-\alpha}\left[(1-\lambda) + \lambda\left(\frac{B_t^F F_t}{B_t^M M_t}\right)^{\rho}\right]^{(1-\alpha-\rho)/\rho}. \tag{3}$$

We are interested in the effects of an increase in female employment on female and male wages. These effects depend on how other factors adjust. With the empirical exercise we shall perform in mind, the most interesting elasticities are "short-run in general equilibrium elasticities," with the level of capital stock and male labor supply held constant. Differentiating (2) with respect to female employment (holding male labor and capital constant), we obtain the price elasticity of female labor demand as

$$\left.\frac{\partial \ln w_t^F}{\partial \ln F_t}\right|_{M_t, K_t} \equiv \frac{1}{\sigma_F} = -(1-s_t^m)\alpha - s_t^m\frac{1}{\sigma_{MF}}, \tag{4}$$

where $s_t^m \equiv w_t^M M_t/(w_t^M M_t + w_t^F F_t)$ is the share of male labor in total labor cost, and recall that σ_{MF} is the elasticity of substitution between male and female labor. Next, when we differentiate (3), the cross-elasticity of male labor demand is

$$\left.\frac{\partial \ln w_t^M}{\partial \ln F_t}\right|_{M_t, K_t} \equiv \frac{1}{\sigma_M} = -(1-s_t^m)\alpha + (1-s_t^m)\frac{1}{\sigma_{MF}}. \tag{5}$$

To see the intuition for these expressions, first consider the case in which male and female labor are perfect substitutes, so that $\sigma_{MF} \to \infty$. In this case, female labor supply reduces both male and female levels identically by lowering the capital-labor ratio in the economy (recall that capital supply is held fixed). The elasticity of wages with respect to overall labor supply is equal to $-\alpha$. Because the female share of overall labor supply is $1 - s_t^m$, a proportional increase in female employment reduces both male and female wages by the factor $(1 - s_t^m)\alpha$.

Next suppose that $\alpha \to 0$ or that capital is supplied perfectly elastically. Now, female employment has no impact on the capital-labor ratio. Consequently, since male and female labor are q-complements, additional female labor supply raises male wages. The extent of this increase depends on the share of female labor in total labor costs and the elasticity of substitution. With a lower elasticity of substitution, the effect on male wages is greater. For women, the opposite is the case: the less substitutable men and women are, the more an increase in female employment reduces female wages.

Therefore, the simple model shows that with capital fixed in the short run (or, more generally, less than perfectly elastic), female wages will fall as a result of an increase in female employment, whereas the effect on male wages is ambiguous. The model also relates both demand elasticities to the share of capital income in output, to the share of male labor in total labor costs, and to the elasticity of substitution between male and female labor.

Note finally that the response of relative female wages to relative female employment depends only on the elasticity of substitution

$$\frac{\partial \ln (w_t^F/w_t^M)}{\partial \ln (F_t/M_t)} = -\frac{1}{\sigma_{MF}}. \tag{6}$$

In the production function (1), male labor is taken to be homogeneous. To discuss the implications of an increase in female employment on men of different skill levels, consider an extension to production function (1) that distinguishes between high-skill and low-skill men:

$$Y_t = A_t K_t^{\alpha}\{(B_t^L L_t)^{\zeta} + [(B_t^F F_t)^{\mu} + (B_t^H H_t)^{\mu}]^{\zeta/\mu}\}^{(1-\alpha)/\zeta}. \tag{7}$$

Here, H_t denotes the employment of high-skill men and L_t is the employment of low-skill men, and we drop the share parameters to simplify notation.[3] In this specification, the elasticity of substitution between the labor aggregate and nonlabor inputs is equal to one as above, the elasticity of substitution between female labor and high-skill male labor is $1/(1 - \mu)$, and the elasticity of substitution between low-skill male labor and the aggregate between female and high-skill male labor is $1/(1 - \zeta)$. When $\zeta > \mu$, female labor competes more with low-skill male labor

[3] We do not distinguish between high- and low-skill women to reduce the number of factors and because, in the empirical work, we will have a source of exogenous variation only in the total number of women in the labor force.

than with high-skill male labor, whereas when $\zeta < \mu$, it competes more with high-skill male labor. This nested constant elasticity of substitution is similar to the one used by Krusell et al. (2000) with high-skill and low-skill labor and equipment capital.

The implications of this aggregate production function for female and male labor demand elasticities are similar to those from (1), but the production function (7) also enables an analysis of the effects of female labor supply on the male skill premium (i.e., the wage ratio of high-skill to low-skill men).

Again when we exploit the fact that wages are equal to marginal products, the male skill premium is

$$\omega_t \equiv \frac{w_t^h}{w_{lt}^l} = \frac{B_t^H}{B_t^L}\frac{(B_t^H H_t)^{\mu-1}[(B_t^F F_t)^{\mu} + (B_t^H H_t)^{\mu}]^{(\zeta-\mu)/\mu}}{(B_t^L L_t)^{\zeta-1}}.$$

It is then straightforward to show that

$$\text{sign}\left\langle\frac{\partial \ln \omega_t}{\partial \ln F_t}\right\rangle = \text{sign}\langle\zeta - \mu\rangle.$$

An increase in effective female labor supply increases male wage inequality when women compete more with low-skill men than with high-skill men, that is, when $\zeta > \mu$. If, as argued by Grant and Hamermesh (1981) and Topel (1994, 1997), female labor is a closer substitute for low-skill male labor than for high-skill male labor, increased female labor force participation should act as a force toward greater returns to skills among men.

Can we use this framework to interpret the relationship between female labor supply and wages at the state level in the aftermath of WWII? At least three caveats apply. First, this interpretation requires U.S. states to approximate separate labor markets. This may be problematic if migration makes the entire United States a single labor market. For example, in the extreme case in which migration is free and rapid, there would be no systematic relationship between relative employment and factor price differences across state labor markets. Many studies, however, find migration to be less than perfect in the short run (e.g., Blanchard and Katz 1992; Bound and Holzer 2000; Card and DiNardo 2000), whereas others document significant wage differences across state or city labor markets (e.g., Topel 1994; Moretti 2000; Acemoglu and Angrist 2001; Bernard, Jensen, and Schott 2001; Hanson and Slaughter 2002). Our results also show substantial differences in relative employment and wages across states related to WWII mobilization.

Second, the single-good setup is an important simplification. When there are multiple goods with different factor proportions, trade between different labor markets can also serve to equalize factor prices

(Samuelson 1948). It is reasonable to presume that changes in interstate trade patterns required to achieve factor price equalization do not take place in the short run.[4]

Third, short-run and long-run elasticities may also differ significantly, either because there are factors, such as capital or entrepreneurial skills, that adjust only slowly (cf. the Le Chatelier principle in Samuelson [1947]) or because technology or organization of production is endogenous and responds to the availability of factors (Acemoglu 1998, 2002).

These caveats motivate us to interpret the elasticity estimates in this paper as corresponding to short-run elasticities (and hence our expressions above with K_t fixed). Our empirical analysis exploits the differential increase in female labor supply at the end of the war on labor market outcomes shortly after the war. Migration, changes in interstate trade patterns, and changes in technologies are likely to make the long-run relationship between female labor supply and labor market outcomes quite different from the short-run relationship.

III. Data Sources and OLS Estimates

A. Data

Our main data source is the 1 percent Integrated Public Use Microdata Series (IPUMS) of the decennial censuses (Ruggles et al. 1997). Samples include men and women aged 14–64 in the year for which earnings are reported, who are not residing in institutional group quarters (such as prisons or barracks), and are not employed in farming. Throughout the paper, we exclude from the analysis Alaska, Hawaii, Washington, D.C., and Nevada. Alaska and Hawaii did not become states until the 1950s, and Nevada underwent substantial population changes during the critical period of our analysis.[5] Because educational attainment is not reported in the full 1950 census sample, our sample for this decade is further limited to "sample line" household members who completed the full questionnaire. Sampling weights are employed in all calculations, and in 1950, they correct for the underrepresentation of members of large households in the sample line subsample.

Earnings samples include workers in paid nonfarm employment excluding self-employed workers who earned the equivalent of $0.50–$250 an hour in 1990 dollars during the previous year (deflated by the consumer price index (CPI) All Urban Consumers series CUUR0000SA0).

[4] This is especially true at midcentury, since construction of the U.S. Interstate Highway System did not begin until 1956 with the authorization of the Federal Aid–Highway Act.

[5] Nevada had an extremely high mobilization rate yet, despite this, lies directly along the regression line for most of our analyses. Inclusion of Nevada affects none of our results.

Our wage measure is the logarithm of weekly earnings, computed as total wage and salary income earned in the previous year divided by weeks worked in the previous year.[6] Top-coded earnings values are imputed as 1.5 times the censored value. To minimize sample composition issues, we focus primarily on the earnings of white, full-time, full-year workers, defined as 40 plus weeks of work in the prior year and at least 35 hours in the survey reference week. In 1940, weeks worked are reported as full-time equivalents; hence we apply no further hours restrictions for the earnings sample. We also perform robustness checks using white and nonwhite workers combined and using all workers (i.e., part- and full-time) in paid hourly employment. In this case, the earnings measure is constructed as the logarithm of weekly earnings divided by hours worked in the sample reference week in 1950 or by 40 hours in 1940 (because the weeks measure corresponds to full-time weeks).

Tables 1 and 2 provide descriptive statistics for the 1940, 1950, and 1960 censuses, our main samples. Statistics are given for all 47 states in our sample and also separately for states with high, medium, and low mobilization rates, corresponding to below 45.4, between 45.4 and 49.0, and above 49.0 percent mobilization. This distinction will be useful below since differences in mobilization rates will be our instrument for female labor supply. Details on the construction of mobilization rates are given in Section IV.

As is visible in table 1, high-mobilization states have higher average education, higher wage levels, and slightly older populations than low-mobilization states in 1940. Farm employment and nonwhite population shares are also considerably lower in these states. Notably, however, female labor supply, measured by average weeks worked per woman, does not differ appreciably among high-, medium-, and low-mobilization states in 1940.

B. Female Employment and Earnings

Before turning to our instrumental variables analysis, we document the cross-state correlations between female labor supply and male and female earnings levels over 1940–90. Table 3 presents ordinary least squares (OLS) regressions of male and female log weekly full-time earnings on a measure of average weeks worked per female state resident aged 14–64, our initial measure of female labor supply. All regression models control for year main effects, state of residence and state or country of birth dummies, a full set of education dummies, a quartic

[6] We exclude self-employment income from the analysis since this income is not reported in 1940. Restricting the sample to those not in farm employment likely reduces the importance of self-employment income in our samples.

TABLE 1
CHARACTERISTICS OF U.S. STATE RESIDENTS IN LOW–, MEDIUM–, AND HIGH–MOBILIZATION RATE STATES, 1940, 1950, AND 1960

	1940				1950				1960			
	All	Low	Medium	High	All	Low	Medium	High	All	Low	Medium	High
	A. Nonfarm Females Aged 14–64											
Weeks worked	11.2	10.9	11.3	11.4	13.7	12.8	13.9	14.4	16.6	15.8	16.8	17.2
	(1.7)	(1.6)	(1.8)	(1.8)	(1.7)	(1.6)	(1.6)	(1.6)	(1.5)	(1.4)	(1.6)	(1.4)
Log weekly earnings	2.61	2.33	2.67	2.76	3.60	3.45	3.64	3.66	4.06	3.92	4.08	4.15
	(.27)	(.29)	(.20)	(.14)	(.16)	(.19)	(.10)	(.11)	(.16)	(.18)	(.12)	(.11)
Mean age	35.8	34.9	36.0	36.5	37.3	36.4	37.7	37.8	38.0	37.4	38.3	38.3
	(1.1)	(1.2)	(.9)	(.7)	(1.0)	(1.0)	(1.0)	(.5)	(.8)	(.6)	(.9)	(.6)
Mean years of schooling	9.0	8.5	9.1	9.4	9.7	9.2	9.8	10.1	10.4	10.0	10.4	10.7
	(.7)	(.9)	(.4)	(.6)	(.7)	(.8)	(.3)	(.5)	(.5)	(.6)	(.3)	(.4)
	B. Nonfarm Males Aged 14–64											
Weeks worked	34.3	34.2	34.6	34.1	38.7	38.3	39.1	38.5	40.1	38.8	40.3	40.8
	(1.7)	(1.4)	(1.6)	(2.0)	(1.6)	(2.0)	(1.7)	(1.1)	(1.6)	(1.7)	(1.5)	(1.2)
Log weekly earnings	3.23	3.07	3.27	3.32	4.07	3.96	4.09	4.13	4.60	4.49	4.62	4.67
	(.18)	(.24)	(.12)	(.08)	(.13)	(.18)	(.08)	(.08)	(.14)	(.19)	(.09)	(.08)
Mean age	35.8	34.7	36.2	36.4	37.4	36.4	37.7	37.8	37.7	36.8	38.1	38.1
	(1.2)	(1.4)	(1.0)	(.7)	(1.1)	(1.2)	(.9)	(.6)	(1.1)	(1.1)	(1.0)	(.8)
Mean years of schooling	9.1	8.6	9.2	9.4	9.7	9.1	9.8	10.1	10.4	9.8	10.4	10.8
	(.6)	(.8)	(.3)	(.5)	(.7)	(.8)	(.4)	(.5)	(.6)	(.6)	(.3)	(.4)

NOTE.—See the note to table 2.

TABLE 2

Demographic Characteristics in 1940 of Males Aged 13–44 in Low–, Medium–, and High–Mobilization Rate States

	Percentage Mobilized, 1940–47				Share Farmers, 1940				Share Nonwhite, 1940			
	All	Low	Medium	High	All	Low	Medium	High	All	Low	Medium	High
Percentage mobilization	47.8	44.0	47.6	51.5	13.4	23.9	11.4	6.9	8.6	16.8	6.9	3.6
	(3.2)	(1.4)	(1.0)	(1.9)	(10.8)	(10.2)	(8.8)	(6.4)	(10.1)	(15.2)	(5.8)	(2.1)

Note.—.Cross-state standard deviations are in parentheses. Data are from Selective Service System (1956) monographs and census IPUMS 1 percent samples for 1940, 1950 (sample line subsample), and 1960. State mobilization rate is the number of men serving in WWII divided by the number registered aged 18–44 during the draft years and is assigned by state of residence. The census IPUMS sample includes those aged 14–64 (in earning year) not living in institutional group quarters, not employed in farming, and residing in the continental United States, excluding District of Columbia and Nevada. There are 16 states in the low-mobilization category (mobilization rate less than 45 percent: Georgia, North Dakota, North Carolina, South Dakota, South Carolina, Wisconsin, Louisiana, Alabama, Arkansas, Mississippi, Virginia, Tennessee, Kentucky, Indiana, Michigan, and Iowa), 15 states in the medium category (mobilization rate between 45 percent and 49 percent: Missouri, Texas, Nebraska, Minnesota, Maryland, Delaware, Vermont, Illinois, Florida, New Mexico, Ohio, West Virginia, New York, Wyoming, and Oklahoma), and 16 states in the high category (mobilization rate greater than or equal to 49 percent: Kansas, Montana, Connecticut, Arizona, Colorado, New Jersey, Idaho, California, Maine, Washington, Pennsylvania, Utah, New Hampshire, Oregon, Rhode Island, and Massachusetts). Weeks worked for 1960 is calculated using the midpoint of the intervalled weeks worked. Earnings samples include workers in paid employment excluding self-employed who earned between \$0.50 and \$250 an hour in 1990 dollars during the previous year (deflated by the CPI All Urban Consumers series CUUR0000SA0) and worked at least 35 hours in the survey reference week and 40 weeks in the previous year. Top-coded values are imputed as 1.5 times the censored value. Average years of schooling is calculated using the highest grade completed. Share nonwhite, share farmers, and average education are the fraction of men in each state aged 13–44 in 1940 with these characteristics (including farm population). Census sample weights are used for all calculations.

TABLE 3
OLS Estimates of Impact of Female Labor Supply on Earnings: 1940–90 at Various Time Intervals
Dependent Variable: Log Weekly Earnings of Full-Time Workers

	1940–90 (1)	1970–90 (2)	1940–60 (3)	1940–50 (4)
	Sample: White Full-Time Workers: A. Female Weekly Earnings			
Weeks worked per female	.019	−.006	.015	−.002
	(.003)	(.004)	(.008)	(.011)
R^2	.87	.69	.71	.58
Observations	287,373	356,192	135,587	69,335
	B. Male Weekly Earnings			
Weeks worked per female	.000	−.015	.009	−.005
	(.003)	(.003)	(.006)	(.006)
R^2	.89	.67	.73	.55
Observations	490,112	622,591	381,871	198,385
	Sample: All Full-Time Workers: C. Female Weekly Earnings			
Weeks worked per female	.008	−.004	.016	−.006
	(.005)	(.004)	(.008)	(.011)
R^2	.88	.70	.74	.64
Observations	338,322	417,019	152,428	78,094
	D. Male Weekly Earnings			
Weeks worked per female	−.008	−.011	−.001	−.008
	(.003)	(.003)	(.006)	(.006)
R^2	.89	.67	.74	.58
Observations	545,483	694,219	413,793	213,966

Note.—Standard errors (in parentheses) account for clustering on state and year of observation. Each coefficient is from a pooled micro data regression of female or male earnings from the two relevant decades regressed on average female weeks worked by state, a year main effect, a quartic in potential experience, and dummies for years of completed schooling, nonwhite (where relevant), marital status, state/country of birth, and state of residence. All individual variables, aside from state of birth, are also interacted with a year dummy. Data are drawn from census IPUMS 1 percent samples (1950 sample line subsample) for the years 1940–70 and 1990. Data for 1980 are drawn from the census 5 percent sample using a randomly drawn 20 percent subsample. All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

in (potential) experience, and dummies for marital status; panels C and D also include a dummy for nonwhites. In these and all other wage models reported in the paper, each covariate other than the state dummies is interacted with a time dummy to allow the returns to education, experience, and demographics to differ by decade.[7] To account for the fact that the female labor supply variable is an aggregate rather than an individual measure, we apply Huber-White robust standard errors throughout the analysis, clustered at the state-year level.

The results in table 3 show no consistent relationship between female employment and female or male earnings. For example, column 1 of

[7] Results without such interactions are similar and are available on request.

panels A and B, using data from 1940–90, indicates that growth in female employment is associated with growth in female wages but is unrelated to male wage levels. Estimates that also include nonwhites (panels C and D of table 3) show similarly modest relationships.

If the results in table 3 corresponded to the causal effect of female employment on female and male wages, we would conclude that demand for female labor was highly elastic (effectively flat) and that male and female workers were not particularly close substitutes. These conclusions would be premature, however, since variations in female employment reflect both supply and demand forces. To the extent that female labor supply responds elastically to labor demand, the OLS estimate of the effect of female employment on female wages will be biased upward by simultaneity; that is, female labor supply will be positively correlated with the level of labor demand and hence positively correlated with wages. Similarly, to the extent that demands for male and female labor move together, OLS estimates of the effect of female employment on male wages will also be biased upward.

To obtain unbiased estimates of the effect of female employment on earnings levels, we require a source of variation in female labor supply that is uncorrelated with demand for female labor. In the next section, we explore whether variation in state mobilization rates for WWII may serve as such a source of variation.

IV. Mobilization for World War II

Following the outbreak of the war, the Selective Service Act, also known as the Burke-Wadsworth Bill, initiated a mandatory national draft registration in October 1940 for all men aged 21–35. By the time the draft was discontinued in 1947, there had been a total of six separate registrations, with the age range expanded to include 18–64-year-olds. Only 18–44-year-olds were liable for military service, however, and many of them either enlisted or were drafted for the war. Following each of the registrations, a series of lotteries determined the order in which registrants were called to active duty. Local draft boards classified registrants into qualification categories as they were called up for active duty.

An important component of the variation stems from cross-state differences in the frequency of draft deferrals. The Selective Service's guidance for deferred exemption was based on marital status, fatherhood, essential skills for civilian war production, and temporary medical disabilities, but it left considerable discretion to the local boards. Because of the need to maintain an adequate food supply to support the war effort, one of the main considerations for deferment was farm status. We show below that states with a higher percentage of farmers had substantially lower mobilization rates, and this explains a considerable share of the variation in state mobilization rates. Also, most military units were still segregated in the 1940s, and there were relatively few black units. This resulted in proportionally fewer blacks serving in the military than whites, and hence states with higher percentages of blacks also had lower shares of draftees. In addition, individuals of German, Italian, and Asian origin may have been less likely to be drafted because of concerns about sending them to battle against their countries of origin.

Our measure of the mobilization rate is the fraction of registered men between the ages of 18 and 44 who were drafted or enlisted for war. It is calculated from the published tables of the Selective Service System (1956). Since essentially all men in the relevant age range were registered, our mobilization rate variable is effectively the fraction of men in this age range who have served. We use this variable as a proxy for the decline in the domestic supply of male labor induced by the war. Volunteers were not accepted into the military after 1942. Hence, the great majority of those who served, 67 percent, were drafted.[8] Consequently, the main source of variation in mobilization rates is cross-state differences in draft rates.

Table 4 shows the cross-state relationship between the mobilization rate and a variety of potential determinants. These right-hand-side variables are all calculated from the census and measure the percentage of men aged 13–44 in 1940 in each state who were farmers, nonwhite, married, fathers, German-born, or born in other Axis nations (Italy or Japan) or fell in the age brackets of 13–24 and 24–34.[9] We also calculate average years of completed schooling among men in this age bracket since, as table 1 shows, this variable differs significantly among high- and low-mobilization states. We focus on the age bracket 13–44 because those aged 13 in 1940 would be 18 in 1945 and, thus, part of the at-risk group for mobilization.

Column 1 of table 4, which includes all of these variables in a regression model, shows that the farm, schooling, and ethnicity (German-born and Italian- or Japanese-born) variables are significant, whereas the other variables are not. The significant negative coefficient on the farm employment variable implies that a state with 10-percentage-point higher farm employment in the prewar year of 1940 is predicted to have

[8] According to data from the Selective Service System (1956), 4,987,144 men were enlisted and 10,022,367 men were drafted during the war years. In 1940, prior to declaration of hostilities, 458,297 men were already serving in the military. Since it is probably misleading to count these peacetime enlistees as wartime volunteers, a more precise estimate of the share of draftees is 70 percent.

[9] The "fathers" variable measures the fraction of women aged 13–44 who had children. Though ideally we would have this fraction for men, this information is not directly available from census data. The percentage of women with children is presumably highly correlated with the desired variable.

TABLE 4

1940 State-Level Determinants of WWII Mobilization Rates (N=47 States)

Dependent Variable: Mobilization Rate

	Mean	Regression (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Share farmers	.15	−.15	−.16	−.17	−.17	−.23	−.26	−.22	−.17	−.16	−.17
	[.11]	(.05)	(.04)	(.03)	(.04)	(.06)	(.04)	(.04)	(.05)	(.04)	(.05)
Share nonwhite	.10	−.01	−.07		−.03	−.38	.04	−.03	−.03	.02	−.03
	[.11]	(.05)	(.04)		(.06)	(.27)	(.05)	(.05)	(.06)	(.06)	(.06)
Average education	8.89	.02		.01	.01	.01	.03	.01	.01	.01	.01
	[.71]	(.01)		(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Share aged 13–24	.42	.25					.73				
	[.03]	(.34)					(.24)				
Share aged 25–34	.31	.15					.38				
	[.01]	(.48)					(.48)				
Share German	.007	−3.19						−1.88			
	[.006]	(.89)						(.55)			
Share Italian or Japanese	.010	1.70							.00		
	[.012]	(.52)							(.42)		
Share married	.50	−.10								−.22	
	[.03]	(.17)								(.13)	
Share fathers	.47	.08									.00
	[.03]	(.13)									(.12)
R^2		.78	.57	.58	.58	.39	.68	.67	.58	.61	.58
Southern states		yes	yes	yes	yes	no	yes	yes	yes	yes	yes

Note.—Standard errors are in parentheses and standard deviations are in brackets. Each column is a separate regression of WWII state mobilization rates on 1940 state-level male characteristics. Regressions are weighted by male state population aged 13–44 in 1940. Share German, Italian, and Japanese are the fractions of male state residents aged 13–44 born in those countries. Share fathers is the fraction of women aged 14–44 with any children in 1940 (a proxy for paternity). Southern states excluded in col. 5 are Delaware, Virginia, Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Kentucky, Maryland, Oklahoma, Tennessee, and West Virginia.

a 1.5-percentage-point lower mobilization rate. The coefficient on the German-born variable implies that a one-percentage-point higher fraction of the population born in Germany translates into over three-percentage-point lower mobilization (though the point estimate is significantly smaller in later columns). This is a very large effect, though not entirely implausible if our measure of foreign-born Germans also captures the presence of larger ethnic German enclaves. Interestingly, the share Italian or Japanese variable has the wrong sign in this regression, but the reason seems to be that it is correlated with the share German-born, and when entered individually, it is insignificant. Column 2 displays a specification that includes only the farm and nonwhite variables, and column 3 shows a specification with only the farm and education variables. Column 4 combines the farm, nonwhite, and schooling variables. Because of collinearity, neither the nonwhite nor the schooling variable is individually significant.

To explore robustness, column 5 drops the 16 southern states from the analysis. Their omission has little impact on the farm or schooling variables, though it does cause the coefficient and standard error of the nonwhite population share measure to rise substantially. The subsequent columns add the age structure, ethnic mix, married, and father variables one by one to the model in column 4. The only variables that have additional explanatory power are age structure and German ethnicity. In net, the farm, schooling, race, German-born, and age variables explain a substantial share of the cross-state variation in the mobilization rate (with R^2 values ranging from .58 to .68). We think of the farm, nonwhite, and schooling variables as capturing potentially "economic" determinants of mobilization rates and the age composition and the German-born variables as capturing systematic "noneconomic" components. Finally, the remaining 30–40 percent corresponds to idiosyncratic or nonsystematic variation. Below we present estimates of the effect of mobilization on growth in female labor supply that exploit various combinations of these sources of variation.

V. WWII Mobilization and Female Labor Supply

A. *Cross-State Relationships*

As depicted in figure 2, the rise in women's labor force participation between 1940 and 1945 closely tracks the mobilization of men. During these five years, male labor force participation declined by 16.5 percentage points, whereas female labor force participation rose by 6.0 percentage points. Hence, the rapid increase in female employment during 1940–45 appears to be a response to the labor demand shock caused by WWII mobilization.[10]

By 1949, the size of the military was at peacetime levels, male labor force participation slightly exceeded prewar levels, and the wartime labor supply shock had arguably subsided. Despite the resumption of peacetime conditions, however, female labor force participation was 5.1 percentage points higher in 1950 than in 1940 (though 0.9 percentage point lower than at the war's peak).[11] The sharp decline in female employment at the war's end visible in figure 2 was transitory, induced by a range of factors including the termination of wartime contracts, a widespread expectation that prewar recessionary conditions would return, and efforts by employers to "give back" jobs to returning veterans (Milkman 1987, chap. 7). With the postwar economic surge, women's employment quickly rebounded, and by 1947, the labor force participation rate of married women was 90 percent of its 1944 level and 140 percent of its 1940 level (U.S. Bureau of the Census 1975, ser. D60).

If female employment was higher in 1950 than it would have been without WWII mobilization, this can be thought of as the result of a change in female labor supply behavior induced by the war. Women who worked during wartime may have potentially increased their earnings capacity or their information about available jobs, thereby inducing additional labor supply. Alternatively, the preferences of women who worked—or even those who did not—may have been altered by widespread female labor force participation during the war. Our empirical strategy is to exploit these changes in female labor supply.

As discussed in the Introduction, mobilization for WWII was not uniform across states. The fraction of men aged 18–44 mobilized by state ranged from 43 to 53 percent, with a ninetieth-tenth percentile difference of 9.2 percentage points. As seen in figures 3–5, although female employment did not systematically vary between high- and low-mobilization states in the prewar period of 1940, women worked significantly more in high- than in low-mobilization states by 1950. Notably, this positive relationship is unique to the decade of the war. As shown in figure 5, there was no additional relative growth in female labor supply during 1950–60 in high-mobilization states (in fact, there is a slight reversion to the mean).

Our hypothesis is that this striking cross-state pattern of female em-

[10] Women may also have sought to replace earnings of spouses serving in the war. Annual military pay in 1944 averaged $1,811 vs. $2,109 for all full-time civilian workers in the same year (U.S. Bureau of the Census 1975, ser. D924, D722).

[11] Our female labor force participation numbers in fig. 2, which use detailed annual labor force series for 1939–52 from the Current Population Reports, differ slightly from the series provided by Blau et al. (2002) displayed in fig. 1. The Blau et al. data place the rise of female labor force participation at 6.0 percentage points as compared to 5.1 percentage points in fig. 2.

ployment growth between 1940 and 1950 reflects the effects of WWII mobilization on female labor supply. To investigate this hypothesis more formally, table 5 reports results from regressions of female labor supply, measured in weeks worked, on state mobilization rates. These models, which pool data from 1940 and 1950, have the following structure:

$$y_{ist} = \delta_s + \gamma \cdot d_{1950} + X'_{ist}\beta_t + \varphi d_{1950} m_s + \epsilon_{ist}. \tag{8}$$

Here the left-hand-side variable, y_{ist}, is weeks worked by woman i residing in state s in year t (1940 or 1950); δ_s denotes a full set of state of residence dummies; d_{1950} is a dummy for 1950; and X_{ist} denotes other covariates including state of birth or country of birth, age, race, share of farmers and nonwhites, and average schooling in the state in 1940, interacted with the 1950 dummy. The coefficient of interest is φ, which corresponds to the interaction term between a 1950 dummy and the mobilization rate, m_s. To save on terminology, we refer to this interaction term simply as the "mobilization rate." This variable measures whether states with higher rates of mobilization for WWII experienced a greater increase in female employment from 1940 to 1950.

Column 1 of panel A is our most parsimonious specification, including only state dummies, year main effects, a nonwhite dummy in panel B, and the mobilization rate measure. This model indicates that there was a large and highly significant increase in female employment between 1940 and 1950 in high-mobilization states. The point estimate of 11.2 (standard error 1.9) implies that 10-percentage-point higher mobilization translated into a 1.1-week increase in female employment between the start and end of the decade. While suggestive, this specification is not entirely appropriate since it does not control for any individual or state characteristics that might explain the rise in female labor supply in high-mobilization states. Column 2 adds a full set of age and marital status dummies interacted with year dummies. In addition, we include state of birth dummies (and country of birth dummies for immigrants) to control for cross-state migration. These controls reduce the mobilization rate coefficient only slightly to 9.85 (standard error 2.05).[12] We next test the robustness of this initial result.

[12] The working paper version of this paper (Acemoglu, Autor, and Lyle 2002) provides a number of additional checks on these basic results. Appendix table 1 displays a set of specifications comparable to panels A and B of table 5 in which WWII mobilization rates are assigned to women by their state of birth rather than current state of residence as in our main models. The point estimates and standard errors are very similar to the models in table 5. Panel B of app. table 1 of the working paper also reports results from specifications that use (log) total labor supply by gender as the dependent variable. Consistent with our findings here, total female labor supply—but not male labor supply—grew relatively more in high-mobilization states.

TABLE 5
Impact of WWII Mobilization Rates on Labor Supply, 1940–50
Dependent Variable: Annual Weeks Worked

	Regression			
	(1)	(2)	(3)	(4)
	A. White Females (N=530,026)			
Mobilization rate × 1950	11.17	9.85	10.64	8.51
	(1.89)	(2.05)	(2.65)	(2.37)
1940 male share farmers × 1950			1.74	1.04
			(1.08)	(1.05)
1940 male share nonwhite × 1950			−1.96	−.72
			(1.15)	(1.37)
1940 male share average education × 1950				.52
				(.16)
R^2	.01	.18	.18	.18
Includes marital status, age, state of birth	no	yes	yes	yes
	B. All Females (N=585,745)			
Mobilization rate × 1950	13.89	9.06	10.22	8.28
	(1.78)	(2.35)	(2.61)	(2.39)
1940 male share farmers × 1950			2.04	1.45
			(1.13)	(1.13)
1940 male share nonwhite × 1950			−2.04	.70
			(1.24)	(1.86)
1940 male share average education × 1950				.51
				(.18)
R^2	.01	.17	.17	.17
Includes marital status, age, state of birth	no	yes	yes	yes
	C. White Males (N=441,343)			
Mobilization rate × 1950	.46	3.60	−6.56	−6.56
	(5.98)	(5.43)	(7.12)	(7.59)
1940 male share farmers × 1950			1.97	1.97
			(1.27)	(1.41)
1940 male share nonwhite × 1950			−9.11	−9.11
			(1.21)	(1.46)
1940 male share average education × 1950				.00
				(.44)
R^2	.02	.36	.36	.36
Includes marital status, age, state of birth	no	yes	yes	yes
	D. Log(Female/Male) (Whites) (N=971,369)			
Mobilization rate × 1950	1.41	1.63	1.24	1.22
	(.31)	(.19)	(.28)	(.27)
1940 male share farmers × 1950			−.21	−.21
			(.10)	(.10)
1940 male share nonwhite × 1950			.06	.06
			(.10)	(.10)
1940 male share average education × 1950				−.02
				(.03)
R^2	.98	.99	.99	.99
Includes marital status, age, state of birth	no	yes	yes	yes

Note.—Standard errors (in parentheses) account for clustering on state of residence and year of observation. Estimates in panels A, B, and C are from separate pooled 1940 and 1950 micro data regressions of female or male weeks worked on WWII state mobilization rate interacted with a 1950 dummy, a year main effect, a nonwhite dummy (where relevant), and state of residence dummies. All individual covariates except for state of birth/residence are interacted with time dummies. Estimates in panel D are from pooled 1940–50 micro data regressions of the log ratio of female to male labor supply, regressed on the same covariates as previous panels, each interacted with a female dummy. As indicated, models also control for individual age dummies and marital status interacted with a 1950 dummy and state/country of birth. All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

B. Correlation or Mobilization?

The correlations shown in table 4 between state mobilization rates and measures of agricultural employment, nonwhite population, and educational attainment raise a concern as to whether we are capturing the causal effect of the mobilization on women's labor supply, or instead differential trends in female employment in nonagricultural, better-educated, and low-minority states. To state this concern concretely, we can think of the variation in cross-state mobilization rates as arising from three components:

$$m_s = m_s^e + m_s^{ne} + e_s. \tag{9}$$

The first of these, m_s^e, is the component of state mobilization rates that is correlated with observable economic factors such as agricultural, educational, and nonwhite distributions. The second component, m_s^{ne}, is correlated with noneconomic factors that we can potentially measure, such as age and ethnicity (e.g., German heritage). Finally, e_s is a source of other idiosyncratic variation that we cannot proxy with our existing data. Our estimates so far exploit all three sources of variation in m_s. Among them, m_s^e is the most problematic since economic factors that cause differences in mobilization rates could also potentially affect female labor supply and earnings growth directly between 1940 and 1950.

Our first strategy to purge the mobilization measure of potentially problematic variation is to control directly for several measures of m_s^e in estimating (8), thus exploiting only the variation in mobilization rates coming from m_s^{ne} and e_s. To implement this approach, columns 3 and 4 of table 5 add an interaction between the 1950 dummy and the fractions of men who were farmers and were nonwhite in 1940 and the average schooling of men in 1940.[13] The nonwhite and farm interaction terms are typically only marginally significant, whereas the schooling variable is positively related to growth in female labor force participation. Notably, these variables have little impact on the mobilization rate coefficient, which remains highly significant. As an alternative check on the influence of racial composition on female employment growth, panel B expands the sample to include both white and nonwhite women. The results in this expanded sample are quite comparable to those in panel A.

A closely related concern is that the mobilization measure may be correlated with other cross-state differences in the distribution of occupations or industries that have greater demand for women, and these

[13] Although the component of mobilization rate correlated with fraction nonwhite may be thought to be "noneconomic," we are more comfortable classifying this as an "economic" component given the rapid changes in the economic status of blacks during this time period.

differences may explain the differential growth in female employment between 1940 and 1950. Table 6 allows for female labor supply growth to differ by states' initial occupational and industrial structure. In particular, we control (in separate regressions and in one pooled regression) for the interaction between the 1950 dummy and the fraction of men in 1940 in each of 10 one-digit occupations as well as the fraction of men in defense-related industries.[14] By and large, inclusion of these controls does not appreciably affect the magnitude or significance of the coefficient on the state mobilization rate.[15]

To supplement the aggregate labor supply patterns depicted in the previous tables, Appendix table A1 presents evidence on the impact of the mobilization rate on female weeks worked by age, education, and birth cohort. We generally find that WWII mobilization had the greatest impact on the labor supply of women who were high school graduates, women between the ages of 14–24 and 35–44, and the cohorts that were 14–24 or 35–44 in 1940. Point estimates for the impact of mobilization on the labor supply of cohorts that were 25–34 or 45–54 in 1940 are sensitive to the inclusion of the aggregate state variables.

C. *Male Labor Supply*

To explore whether the differential cross-state growth in female employment between 1940 and 1950 may be interpreted as stemming from female labor supply shifts, we estimate analogous models for male labor supply. If, in contrast to our hypothesis, the correlation between mobilization and the growth in female labor primarily reflected unmeasured demand shifts, we should expect to see similar (positive) labor supply shifts among men in high-mobilization states. Alternatively, a negative relationship between the mobilization rate and male labor supply might indicate that veterans had not yet reintegrated into the labor force by 1950, thereby indirectly increasing demand for female employment.[16] Either of these findings could conflict with our interpre-

[14] Defense industries correspond to IPUMS 1950 industry codes 326–88: primary and fabricated metals; industrial and electronic machinery and equipment; motor vehicles, aircraft, and shipbuilding; railroads and other transportation; and instruments and related products.

[15] Only in the specification in which all occupation/industry measures are simultaneously included (panel B, col. 10) does the relationship between the WWII mobilization and female labor supply become insignificant. Given the limited cross-state variation available to identify this model, this is not surprising.

[16] One reason veterans might be out of the labor force is that they were obtaining education under the WWII GI Bill. As noted by Goldin and Margo (1992, n. 24), however, college attendance under the WWII GI Bill peaked in 1947 and declined sharply after 1949. See Bound and Turner (1999) and Stanley (2003) for further information on the labor force impacts of the GI Bill.

TABLE 6

Impact of WWII Mobilization Rates on Female Labor Supply, 1940–50, Controlling for the Fraction of Males in Occupations and Industries in 1940 ($N = 530{,}026$)

Dependent Variable: Female Annual Weeks Worked by State

	Professional/ Technical (1)	Managers (2)	Clerks (3)	Sales (4)	Crafts (5)	Operatives (6)	Services/ Private (7)	Services (8)	Laborers (9)	Defense Industries (10)	All Previous (11)
	A. Baseline Specification (Whites)										
Mobilization rate × 1950	9.08	8.73	11.40	10.05	9.28	11.54	8.88	11.09	9.93	9.63	11.21
	(2.19)	(1.89)	(2.17)	(2.27)	(2.40)	(2.42)	(2.18)	(2.17)	(2.01)	(2.06)	(3.31)
1940 male occupation share × 1950	3.72	8.56	−4.99	−.87	1.25	−2.68	−44.90	−9.06	−5.00	.56	
	(6.19)	(6.19)	(3.51)	(5.59)	(3.49)	(2.06)	(57.70)	(5.23)	(3.56)	(1.07)	
R^2	.18	.18	.18	.18	.18	.18	.18	.18	.18	.18	.18
	B. Controlling for 1940 Male Share Farmers, Share Nonwhite, and Average Education (Whites)										
Mobilization rate × 1950	8.33	8.28	6.14	8.12	8.70	8.58	8.50	7.75	8.70	10.83	7.72
	(2.44)	(2.55)	(2.42)	(2.19)	(2.69)	(2.26)	(2.34)	(2.38)	(2.57)	(2.55)	(4.71)
1940 male occupation share × 1950	−2.73	−3.29	−10.02	−12.53	1.00	2.84	1.30	−8.93	−.73	2.18	
	(8.11)	(8.25)	(5.30)	(6.45)	(7.59)	(2.65)	(75.76)	(6.77)	(3.35)	(1.02)	
R^2	.18	.18	.18	.18	.18	.18	.18	.18	.18	.18	.18

Note.—Standard errors (in parentheses) account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 and 1950 micro data regression of female weeks worked on WWII state mobilization rate interacted with a 1950 dummy, the fraction of males in the listed occupational (industry) category in 1940 interacted with a 1950 dummy, a year main effect, a constant, and dummies for age, marital status, state of residence, and state/country of birth. All individual variables, aside from state of residence/birth, are also interacted with a 1950 dummy. Occupation and industry codes correspond to major (one-digit) occupational and industry categories. Defense industries correspond to IPUMS 1950 industry codes 326–88. All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

tation of the table 5 estimates as reflecting shifts in female labor supply rather than shifts in labor demand.

Estimates of equation (8) for the labor supply of white men are found in panel C of table 5. Reassuringly, these models yield comparatively small and statistically insignificant estimates. We have also estimated comparable models that are further disaggregated by veteran and nonveteran status. In no case do these models detect a significant relationship between the state mobilization rate and the labor supply of either group of men (though the estimates are more negative for veterans).[17] In net, we find little evidence that male labor supply was systemically affected by cross-state patterns of mobilization.

In later models, we shall examine the effect of female relative to male employment on male and female wages. Hence, it is useful to study the effect of wartime mobilization on the log ratio of total female to male weeks worked, which corresponds to $\ln(F_{st}/M_{st})$ in our model from Section II. Panel D of table 5 presents estimates of equation (8) using this dependent variable. These estimates confirm the expected relationship between the mobilization rate and the growth in female labor supply. The coefficient of 1.41 (standard error of 0.31) in column 1 indicates that a 10 percent higher mobilization rate translated into a roughly 14-percentage-point rise in the ratio of female to male labor supply during the 1940s. This point estimate has a sensible magnitude. In 1940, aggregate female labor supply was about 35 percent of male labor supply, and so a 14 percent increase in this ratio would correspond to a roughly five-percentage-point rise in female relative to male labor supply. In subsequent columns of panel D, we add the complete set of control variables used above. In all cases, the relationship between mobilization and the growth in relative female labor supply is robust and economically large.[18]

D. *Instrumental Variables Estimates*

In terms of the notation of equation (9), the estimates in tables 5 and 6 exploit two sources of variation in state mobilization rates: the "noneconomic" component, m_s^{ne}, and the "idiosyncratic" component, e_s. An alternative strategy to explore whether these results may be interpreted as the causal effect of WWII mobilization on female labor supply growth is to attempt to isolate the noneconomic component of the mobilization rate, m_s^{ne}. To implement this approach, we focus on the variation in

[17] A table containing these results is available on request.

[18] The very high R^2 values of these estimates reflect the fact that the outcome variable is a state-year mean and so is mostly explained by state and time dummies. To account for the grouped error structure, we continue to cluster the standard errors at the state-year level.

TABLE 7
Instrumental Variables Estimates of the Impact of WWII Mobilization Rates
Dependent Variable: Annual Weeks Worked

	White Females (N=530,026)			White Males (N=441,343)		
	(1)	(2)	(3)	(4)	(5)	(6)
Mobilization rate × 1950	15.78 (5.38)	13.19 (5.49)	11.42 (3.97)	10.93 (12.23)	−17.00 (13.98)	−.04 (11.94)
1940 male share farmers × 1950	2.65 (1.30)	2.19 (1.11)	1.87 (1.01)	2.66 (1.75)	−2.45 (2.45)	.65 (1.65)
1940 male average education × 1950	.36 (.14)	.39 (.15)	.41 (.13)	.65 (.37)	.96 (.36)	.77 (.36)
	First-Stage Coefficients					
1940 male share ages 13–24 × 1950	.56 (.14)		.27 (.15)	.73 (.14)		.44 (.15)
1940 male share ages 25–34 × 1950	.04 (.22)		−.22 (.25)	−.01 (.21)		−.20 (.21)
1940 male share German × 1950		−1.83 (.39)	−1.33 (.46)		−2.03 (.38)	−1.30 (.41)
p-value (first-stage)	.00	.00	.00	.00	.00	.00

Note.—Standard errors (in parentheses) account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 and 1950 micro data 2SLS regression of weeks worked by female or male state of residence on instrumented WWII state mobilization rate interacted with a 1950 dummy, year main effects, and dummies for age, marital status, state of residence, and state/country of birth. All individual variables, aside from state of residence/birth, are also interacted with a 1950 dummy. Instruments for the mobilization rate are the fraction of males aged 13–44 in 1940 who are German-born or who are in the listed age categories (each interacted with a 1950 dummy). All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

mobilization rates accounted for by differences in the age structure and German heritage of the population of men at risk for mobilization by state (recall that the fraction of those who were Italian and Japanese did not have a significant effect on mobilization rates in table 4). When we condition on individual characteristics, in particular, age and ethnicity (country of birth), it is plausible that these variables should have no direct effect on female labor supply growth.

Motivated by this reasoning, in table 7 we report results from two-stage least-squares (2SLS) estimation of equation (8), using the 1940 age or ethnic structure (or both) as instruments for the mobilization rate. In these models we also control for share of farmers and male average education in 1940. Though not as precisely estimated, the results of these 2SLS models are similar to the previous estimates using all components of the variation in mobilization rates and to those that control for the economic component of the mobilization rate, m_s^e, directly. Therefore, it appears that all sources of variation in mobilization rates exert a similar effect on female labor supply during the decade of the war. It is also encouraging to note that the 2SLS models for male labor supply in table 7 find insignificant and inconsistently signed effects of mobilization during this decade.[19]

E. Alternative Labor Supply Measures and Time Periods

In table 8, we provide a final set of robustness checks on our labor supply estimates. Here, we present results for a second outcome measure, positive weeks worked. We also explore the importance of regional variation for the main findings and compare the 1940–50 results to estimates for the 1950s and 1930s, when there was no mobilization for war.

Column 1 of table 8 presents results for weeks worked per female state resident in 1940–50, analogous to the table 5 estimates. Rows A and B replicate specifications 2 and 4 from table 5, which include all state "economic" controls (i.e., share farmers, share nonwhite, and average years of completed schooling, each interacted with a 1950 dummy). Rows C and D demonstrate that our results are not primarily driven by regional trends in female labor supply. Dropping southern states reduces the size of the coefficient, but the relationship remains economically and statistically significant. Adding four census region dummies interacted with the 1950 dummy increases the estimated relationship between the mobilization rate and female employment growth but does not change the overall pattern.

Column 2 of table 8 presents identical models in which the dependent variable is an indicator variable equal to one if a woman worked positive weeks in the previous year (and zero otherwise). In all but the first specification, these models indicate a sizable impact of the mobilization rate on the share of women participating in the labor force. A 10-percentage-point higher mobilization rate is associated with one to three percentage points of additional growth in female labor force participation over this decade.

Columns 3 and 4 of table 8 present comparable estimates for the years 1950–60, in this case interacting the mobilization rate with a 1960 dummy. These results provide a useful specification test since a large increase in female employment in high-mobilization states between 1950 and 1960 would indicate that our mobilization rate variable is likely capturing other secular cross-state trends in female employment. In no

[19] We have also performed a "falsification" exercise for this instrumental variables approach in which we regress the change in female (or male) labor supply during 1950–60 on lagged state age and ethnic variables from the 1950 decade interacted with a 1960 dummy. *F*-tests of these "false instruments" are never significant in models that use the state ethnic structure as the instrument. In models that include the age structure alone or the age and ethnic structures together, *p*-values range from .01 to .03, though age variables have the opposite sign to those in table 7.

TABLE 8
Impact of WWII Mobilization Rates on Female Labor Supply, 1940–50, 1950–60, and 1930–40
Dependent Variable: Female Labor Supply (Whites)

	1940–50		1950–60		1930–40
	Weeks Worked (1)	Any Weeks Worked (2)	Weeks Worked (3)	Any Weeks Worked (4)	Gainful Employment (5)
A. Baseline specification	9.85 (2.05)	.063 (.069)	−7.25 (1.81)	−.057 (.049)	−.026 (.120)
R^2	.18	.18	.15	.14	.88
Observations	530,026		615,590		94
B. Includes share farmers, share nonwhite, average education	8.51 (2.37)	.174 (.071)	2.15 (1.95)	−.006 (.067)	−.281 (.115)
R^2	.18	.18	.15	.14	.96
Observations	530,026		615,590		94
C. Excludes southern states	6.34 (2.40)	.130 (.081)	3.56 (1.86)	.035 (.069)	−.194 (.101)
R^2	.20	.20	.17	.15	.97
Observations	393,820		449,275		62
D. Includes region × postyear	11.28 (2.97)	.253 (.080)	.39 (2.23)	.002 (.064)	−.111 (.146)
R^2	.18	.18	.15	.14	.96
Observations	530,026		615,590		94

Note.—Standard errors (in parentheses) account for clustering on state of residence and year of observation. Estimates in cols. 1–4 are from separate pooled 1940–50 or 1950–60 micro data regressions of individual weeks worked on WWII state mobilization rate interacted with a 1950 or 1960 dummy, year main effects, and dummies for marital status, individual years of age, state of residence, and state/country of birth. All individual variables, aside from state of residence/birth, are also interacted with a 1950 or 1960 dummy. Specifications in rows B, C, and D also control for the 1940 share farmers, share nonwhite, and average education of males. Any weeks worked is defined as weeks worked greater than zero. All models in cols. 1–4 are weighted by census sampling weights. Estimates in col. 5 are from pooled 1930–40 regressions of mean state gainful employment in 1930 (as defined in the 1930 census) and state-level aggregated labor force participation (as defined in the 1940 census) on WWII state mobilization rate interacted with a 1940 dummy, year main effects, and state of residence dummies. The 1930–40 gainful employment measure includes farm employment. Models in col. 5 are weighted by 1940 state female population aged 20–64. See n. 20 for additional details on the col. 5 estimates. Southern states are Delaware, Virginia, Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Kentucky, Maryland, Oklahoma, Tennessee, and West Virginia. Region dummies correspond to the four census geographic regions. See the note to table 2 for additional sample details.

case do we find a significant positive relationship between the mobilization variable and the growth of female labor supply over the 1950–60 decade. The cross-state growth in women's labor force participation was therefore significantly correlated with WWII mobilization rates only during the decade of the war. In column 5, we perform similar estimates for the 1930–40 decade. These estimates also show no significant positive relationship between the growth in female labor force participation during the 1930s and the subsequent mobilization; in fact, the relationship is always negative. The reliability of this inference, however, is curtailed by data comparability issues between the 1930 and 1940 censuses, and hence we do not regard this test as definitive.[20]

Finally, it would be useful to complement these results with evidence on whether women worked relatively more in high-mobilization states during the war years (as well as afterward). Unfortunately, we are not aware of a data source with information on state labor force participation rates by gender during the intracensus years. Nevertheless, we can partially complete the picture given by the census data by investigating whether women worked more in the immediate aftermath of the war (between 1947 and 1950) in high-mobilization states. To do so, we use the Current Population Survey (CPS) Social Security Earnings Records Exact Match file, which reports information from Social Security earnings records on quarters worked in covered employment (i.e., private sector, non-self-employed) for adults interviewed for the CPS in March 1978. These data are available only for those who survived to 1978 and report valid social security numbers (SSNs). Because the quarterly employment data do not start until 1947 and contain only the sum of quarters worked for the first three years of the sample (1947–50), we cannot investigate whether women worked more in high-mobilization states during the war.[21] These data nonetheless provide a rare glimpse at women's employment in the immediate postwar years.

[20] The labor supply measure available from the 1930 census monograph, "gainful employment," is not fully comparable to the modern measure of labor force participation first implemented in 1940. To increase comparability, we apply adjustment factors to the 1930 census from Durand (1948, table A-2). In addition, because micro data for the 1930 census are not available, we employ aggregate state data from 1930 census monographs and constructed similar aggregates from the IPUMS micro data for 1940. Because the 1930 census monographs do not separately tabulate gainful employment for farm vs. nonfarm workers, we included farm employment in both the 1930 and 1940 aggregates, in contrast to our estimates above. A final difficulty lies with the "in labor force" measure in the 1940 census. Tabulations by decade show that this measure appears to have been greatly affected by wartime activity in 1940. In particular, more than 60 percent of women who reported working one to five weeks in 1939 participated in the labor force in March 1940. This is more than twice as high as in the subsequent decade and at least 50 percent higher than in the decades of the 1980s and 1990s (there are no comparable measures in the 1960 and 1970 censuses). Consequently, we imputed the "in labor force" variable for 1940 using the observed relationship between weeks worked and "in labor force" in the 1950 census. Note that our primary labor supply estimates in table 5 use the census weeks worked variable, which refers to labor force activity during the 1939 calendar year and does not appear affected by wartime mobilization.

[21] Because we do not have information on respondents' state of birth, we use state of residence as an imperfect proxy. Social security numbers are essentially available only for women with a positive work history, and hence we treat missing SSNs as indicating no work history (except in cases in which respondents refused to provide an SSN or the SSN failed to match Social Security data). To attempt to isolate farm workers (who are typically not in covered employment), we variously dropped women in farming occupations, women with farm income, and women residing on farms (and all three). These exclusions had little impact on the results. Although the CPS Exact Match file reports annual quarters worked for 1937–46, these data are imputed from aggregate income data for these years and consequently are not useful for our analysis.

Figure 10 depicts the (standardized) relationship between state mobilization rates and female employment during 1947 and 1950 and separately in each of the years from 1951 to 1977. For women who were aged 16–55 in 1945, we run a regression of total quarters of work in a given period divided by mean quarters of work by women in that period on individual characteristics (age, education, marital status, and a dummy for nonwhite) and the state mobilization rate. The figure plots the coefficients on the mobilization rate measure and robust 90 percent confidence intervals for each estimate. The results confirm the patterns detected in the census data: there is a strong relationship between mobilization rates and female labor employment in 1951 and a weaker but still substantial relationship in 1959 and 1960. There appears to have been an even more positive relationship between the mobilization rate and female labor supply in the years immediately following the war (1947–50). Consistent with Goldin's (1991) findings, the impact of the war on female labor supply fades substantially with time, but greater female labor supply in high-mobilization states persists for at least 15 years after the war's end.[22]

VI. The Impact of Female Labor Supply on Earnings

The previous section developed the argument that cross-state differences in WWII mobilization rates are a plausible source of variation in female labor supply in 1950. This section exploits this source of variation in female employment to estimate the effect of female supply on a range of labor market outcomes.

A. *Initial Evidence*

Figures 6 and 7 in the Introduction depict the negative relationship between state WWII mobilization rates and the change in average weekly (log) female and male wages during 1940 and 1950 at the state level. We now investigate these relationships formally.

In table 9, we start with instrumental variables estimates in which individual log weekly earnings by gender are regressed on a standard set of wage determinants augmented with our measure of weeks worked per woman and instrumented by the state mobilization rate. Figures 6 and 7 correspond to the reduced form for these instrumental variables estimates (without covariates). More formally, the estimating equation

[22] By 1976, the final year depicted in fig. 10, the cohort of women in this sample ranged from age 47 to 86. Hence, the cross-state convergence in labor supply visible in the figure likely reflects the fact that many members of this cohort have reached retirement age.

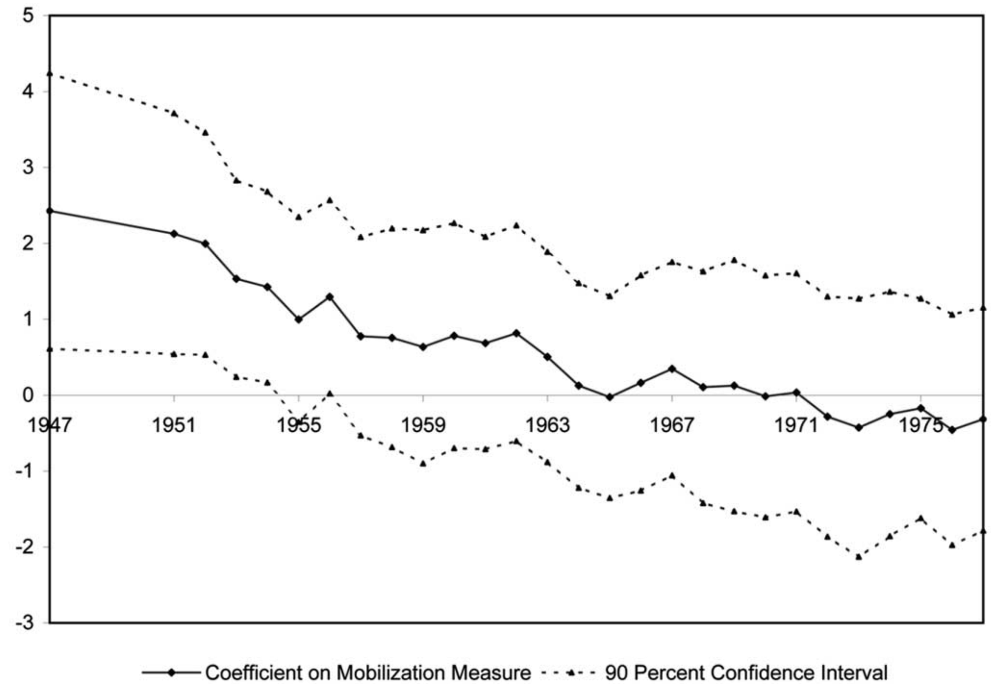

Fig. 10.—Estimated impact of state WWII mobilization rate on standardized quarters worked annually by females who were aged 16–55 in 1945: 1947–77.

is

$$\ln w_{ist} = \delta_s + \gamma_{1950} + X'_{ist}\beta_t + \phi Y_{st} + u_{ist}. \tag{10}$$

The left-hand-side variable is log weekly earnings, $\ln w_{ist}$, and the endogenous regressor is average weeks worked by women in the state of residence of individual i, Y_{st}. In all specifications, we include state of residence dummies, a dummy for 1950, a complete set of education dummies, marital status dummy, veteran status dummy, and a quartic in potential experience. All individual-level covariates are interacted with the 1950 dummy. The coefficient of interest, ϕ, measures the effect of female labor supply on earnings. Standard errors are again clustered to account for the fact that the labor supply measure operates at the state by year level. The estimates of ϕ in equation (10) do not have a direct structural interpretation in terms of our model in Section II. We therefore view these results as descriptive and adopt a more structural approach in the following section.

Our first-stage equation for female labor supply is analogous to equation (8) above except that the endogenous variable in this case is not individual weeks worked, but rather average weeks worked per woman in each state. The first-stage coefficient is tabulated below the second-stage estimates in each column. The excluded instrument is the interaction between the 1950 dummy and the mobilization rate. The exclusion restriction implied by this instrumental variables strategy is that differential mobilization rates affect women's wages across states only through their impact on female labor supply.

In column 1 of table 9, we report a parsimonious specification that includes state and time dummies and our standard set of human capital controls. This estimate finds that a one-week increase in female labor supply is associated with a 12.4 percent decline in female weekly earnings. In column 2, we augment the model with state and country of birth dummies and a set of aggregate state age structure controls that account for the fact that, as shown in table 4, mobilization rates depended in part on the age structure of male residents (which is presumably highly correlated with the age structure of female residents).[23] These covariates reduce the estimated wage impact of a one-week increase in female labor supply slightly to −10.8 percent, which remains highly significant.[24] Column 3 adds the interaction between the 1950 dummy and the 1940 aggregate state measures. These interactions reduce the magnitude of the estimate by one-third and increase the stan-

[23] Female age structure variables measure the share of female state residents aged 14–64 in each of the following age categories (with one omitted): 14–17, 18–24, 25–34, 35–44, 45–54, and 55–64.

[24] The farm employment variable is primarily responsible for the reduction in the coefficient.

TABLE 9

Instrumental Variables Specifications: Impact of Female Labor Supply on Female and Male Earnings, 1940–50

Dependent Variable: Log Weekly Earnings

	Variations of the Baseline Specification			Lagged Dependent Variable	Excludes the South	Region × 1950	Ages 25–34	Hourly Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. White Females							
IV: Weeks worked per female	−.124	−.108	−.072	−.097	−.135	.001	−.101	−.070
	(.029)	(.025)	(.038)	(.034)	(.071)	(.022)	(.024)	(.018)
1940 state mean log wage × 1950				−.360				
				(.113)				
	First-Stage Coefficients							
Mobilization rate × 1950	10.22	11.68	11.20	11.10	8.04	15.05	11.35	11.58
	(1.81)	(2.14)	(2.72)	(2.60)	(3.08)	(4.03)	(2.10)	(2.15)
Observations	69,335	69,335	69,335	69,335	55,847	69,335	20,449	102,518
	B. White Males							
IV: Weeks worked per female	−.080	−.071	−.021	−.029	−.031	.014	−.096	−.017
	(.018)	(.015)	(.017)	(.017)	(.022)	(.013)	(.022)	(.011)
1940 state mean log wage × 1950				−.093				
				(.077)				
	First-Stage Coefficients							
Mobilization rate × 1950	10.23	11.65	11.44	12.88	8.28	14.46	11.46	11.70
	(1.90)	(2.16)	(2.73)	(3.22)	(2.86)	(4.08)	(2.16)	(2.14)
Observations	198,385	198,385	198,385	198,385	155,743	198,385	60,143	252,276
Includes female age structure and state of birth	no	yes	yes	yes	yes	yes	yes	yes
Includes share farmers, share nonwhite, and average education	no	no	yes	yes	yes	yes	no	no

NOTE.—Standard errors (in parentheses) account for clustering on state and year of observation. Each column is from a separate pooled 1940–50 micro data 2SLS regression of log weekly earnings on weeks worked per female or male instrumented by the state mobilization rate interacted with a 1950 dummy. All regressions control for year main effects, state of residence, years of completed schooling, a quartic in potential experience, marital status, and WWII veteran (for males). As indicated, models control for state-level female age structure and state/country of birth. All individual variables, aside from state of residence/birth, are interacted with a 1950 dummy. State-level female age structure is the share of female state residents in age categories 14–17, 18–24, 25–34, 35–44, 45–54, and 55–64. The lagged dependent variable in col. 4 is equal to the 1940 state mean log weekly full-time wage for the relevant gender/race group interacted with a 1950 dummy. Hourly earnings sample includes all workers in paid employment excluding self-employed who earned between $0.50 and $250 an hour in 1990 dollars during the previous year (deflated by the CPI All Urban Consumer series CUUR0000SA0). Southern states are Delaware, Virginia, Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Kentucky, Maryland, Oklahoma, Tennessee, and West Virginia. Region dummies correspond to the four census geographic regions. All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

dard error. The negative estimated impact of female labor supply on mean female earnings is in all cases significant (in the final specifications, at the 10 percent level) and, as suggested by theory, indicates that the demand curve for female labor is downward sloping (at least in the short run).[25] Comparing these estimates to the OLS models in table 3 indicates that the OLS estimates are likely biased toward zero by simultaneity, presumably because female employment increased relatively more in states with greater demand for female labor.

Panel B of table 9 presents corresponding estimates for male earnings. Contrary to the case of female earnings, theory does not make strong predictions for male earnings: they should decline if male and female labor inputs are close substitutes and nonlabor inputs are supplied inelastically to state labor markets in the short run. In the data, we detect modest evidence of negative effects of female labor supply on male earnings. The point estimates in the primary specifications are highly significant, but this is not the case in the final specifications, where we control for the interaction between the 1950 dummy and several state aggregate measures. Interestingly, the estimated effect of female labor supply on male earnings is consistently 30–40 percent smaller in absolute magnitude than the corresponding estimates for female earnings. This result suggests that female labor supply is an imperfect substitute for male labor supply, a point that we explore in greater detail below.[26]

B. *Robustness*

Subsequent columns of table 9 present several checks on the interpretation of the foregoing analysis. A first concern is that the findings could be driven by the rapid convergence of wages between agricultural and southern U.S. states and the rest of the nation during the 1940s (Wright 1986).

We provide three checks on this hypothesis. In column 4 of table 9, we control for the prewar (1940) mean wage level by state for the relevant gender group, interacted with a 1950 dummy. As expected, the lagged wage variable is negative and, in the case of women, is also

[25] We have also experimented with adding an interaction between the 1940 state defense industry employment share (see table 6) and the 1950 dummy. This covariate does not change the qualitative findings but does further attenuate the wage estimates.

[26] Panel B of App. table A2 (in the electronic version of this article) presents instrumental variables models of the impact of female labor supply on earnings separately for WWII veterans and nonveterans. The point estimates for female labor supply are negative and, in the base specification, highly significant for both groups. The magnitude of the effect is typically somewhat larger for veterans than for nonveterans, a pattern consistent with Freeman's (1977) "active labor market" hypothesis: veterans, as recent labor market (re)entrants, may have borne a greater brunt of the wage effects of rising female labor supply.

significant. States with initially lower wage levels experienced greater wage gains during the decade. Nevertheless, the inclusion of the lagged wage measure has only a minimal effect on the coefficient for female labor supply.

As a second check on the importance of the South/non-South contrast, column 5 drops the 16 southern states from the wage equation. Their omission increases both the point estimates and the standard errors on the coefficient for female labor supply but does not change the main result. As a final test, column 6 adds four region dummy variables, each interacted with the 1950 dummy. This model therefore estimates the effect of female labor supply on wage levels using entirely within-region variation. As is visible, the estimates do not survive this stringent test. Clearly, some cross-region variation is required to identify the wage impacts. A possible interpretation is that state labor (or product) markets within a given region were economically integrated during the 1950s.[27]

A second set of concerns revolves around composition bias. We have so far limited the sample to full-time, full-year workers to mitigate sample composition issues. If women drawn into the full-time labor force by the mobilization possessed lower unobserved human capital than incumbents, this would reduce average female earnings, leading to spuriously negative estimates of the effect of female labor supply on wages (cf. Smith and Ward 1984). While in theory this bias could be quite important, there are several reasons to think that it is not a first-order concern. First, as shown in Appendix table A1, marginal female labor market participants were relatively highly educated, prime-age women, and so there is no compelling reason to expect that they would be adversely selected on unobserved skills. Second, the wage effects we estimate are too large to be plausibly explained by compositional changes. For example, our main specification shows a 10 percent decline in female wages in response to a 10 percent rise in female employment. For this effect to be caused solely by compositional shifts, marginal participants would have to earn zero (or even negative) wages!

To provide a further check on this concern, we estimate the effect of

[27] The results are also unlikely to be driven by institutional changes taking place in the U.S. labor market during this time period. The two major institutional changes of this era are increases in unionization and the imposition, and then removal, of the National War Labor Board (NWLB), which was responsible for approving, and limiting, wage increases. The NWLB, which was established in January 1942, was dissolved in December 1945, and effectively all wartime price controls were lifted in November 1946 (see Rockoff 1984), three years before our postwar observations. We have also estimated the key labor supply and wage models in tables 5 and 9 while controlling for differential trends in unionization across states during these years (using data from Troy and Sheflin [1985]). Controlling for unionization has little impact on the findings, and a supplemental table with these estimates is available on request.

female labor supply on the earnings of female workers aged 25–34. This group is selected because, as shown in Appendix table A1, labor supply in this age range did not increase differentially in high mobilization states, so compositional concerns are mitigated. Column 7 of table 9 finds that a one-week increase in female labor supply reduced wages of women aged 25–34 by 10 percent, which is quite comparable to the estimates for the full sample of women (col. 1). We conclude that the estimates for female wages are unlikely to be driven by changes in the composition of female employment.

So far we have not addressed whether our results generalize beyond full-time, full-year workers. To explore this question, column 8 presents wage models by gender for the hourly earnings of all nonfarm workers in paid wage and salary employment (further sample definitions are found above in Sec. III). The results for female hourly wages are quite consistent with the full-time results. In particular, we obtain a significant coefficient of -0.07 on the female labor supply measure, which is about two-thirds of the magnitude of the estimate for weekly earnings of full-time, full-year female workers. The corresponding estimate for the hourly wages of men is also negative but is smaller in magnitude than the full-time estimate and is only marginally significant. This again underscores that the own-wage effects for women are larger and more robust than the cross-effects on men.

C. Using Mobilization Rates to Estimate Elasticities of Demand and Substitution

The estimates in table 9 do not correspond to the equations implied by the theoretical model in Section II. To recover the elasticities of demand and substitution motivated by the theory, we now estimate a set of models that simultaneously account for the supply of female and male labor:

$$\ln w_{ist} = \delta_s + \gamma_{1950} + f_i + X'_{ist}\beta^g_t + \chi \ln\left(\frac{F_{st}}{M_{st}}\right) + \eta f_i \ln\left(\frac{F_{st}}{M_{st}}\right) + u_{ist}, \quad (11)$$

where the sample now includes all individuals (male and female), f_i is a dummy for female, and, as above, $\ln(F_{st}/M_{st})$ is the log ratio of female to male labor supply (in weeks) in the state of residence. As indicated by the superscripts and subscripts on β^g_t, each of the individual and aggregate state controls included in X_{ist} is permitted to affect male and female earnings differentially by gender and decade. The labor supply measure $\ln(F_{st}/M_{st})$ and its interaction with the female dummy variable are instrumented by the state mobilization rate and its interaction with the female dummy variable.

There are two coefficients of interest in this equation, χ and η. The coefficient χ measures the impact of an increase in (relative) female labor supply on both male and female earnings, and η measures the differential effect of female labor supply on female wages. Hence, η is an estimate of the inverse elasticity of substitution between male and female labor, $1/\sigma_{MF}$, and the quantity $\chi + \eta$ is an estimate of the inverse elasticity of demand for female labor, $1/\sigma_F$.[28]

Columns 1–3 of table 10 present estimates of equation (11), using the same control variables as in table 9.[29] These models confirm the pattern in figures 6 and 7 and table 9 that increases in female labor supply reduce female earnings. The point estimates in the first two rows of table 10, corresponding to χ and η in equation (11), are in all cases negative and in all but one case highly significant. Summing χ and η to obtain an estimate of the inverse elasticity of demand for female labor, $1/\sigma_F$, we find that a 10 percent increase in relative female labor supply reduces female wages by 7–8 percent. These wage effects correspond to an own-labor demand elasticity of between -1.2 and -1.5. They are somewhat larger than the consensus estimates of the elasticity of male labor demand, which are generally between zero and one (Hamermesh 1993, table 3.2).

The impact of female labor supply on wages is not uniform across genders, however. As suggested by the estimates in table 9, the wage effects of increases in female labor supply are uniformly more negative for women than for men. Specifically, a 10 percent increase in female labor supply lowers female wages relative to male wages by 3–4 percent. Female and male labor inputs are therefore highly, but not perfectly, substitutable. The point estimates for η imply a substitution elasticity, σ_{MF}, in the range of 3.2–4.2, with a somewhat smaller elasticity in the model that includes aggregate state controls (col. 3).

Is this substitution elasticity plausible? We know of no other estimates of the male-female substitution elasticity that can be used for comparison. Nevertheless, we can test whether the values of $\hat{\sigma}_F$ and $\hat{\sigma}_{MF}$ recovered from equation (11) are consistent with the restriction on these param-

[28] In the theory section (Sec. II), we demonstrate that

$$\left.\frac{\partial \ln w_t^F}{\partial \ln F_t}\right|_{M_t,K_t} = \frac{1}{\sigma_F} = -(1 - s_t^m)\alpha - s_t^m \frac{1}{\sigma_{MF}}.$$

Since this expression holds M_t constant, we also have

$$\left.\frac{\partial \ln w_t^F}{\partial \ln (F_t/M_t)}\right|_{M_t,K_t} = \frac{1}{\sigma_F} = -(1 - s_t^m)\alpha - s_t^m \frac{1}{\sigma_{MF}}.$$

[29] First-stage estimates tabulated at the bottom of each column indicate large and highly significant impacts of WWII mobilization on relative gender labor supplies, consistent with the estimates in panel D of table 5.

TABLE 10

Impact of Female Labor Supply on Female/Male Earnings Differential, 1940–50

Dependent Variable: Log Weekly Earnings (Whites)

	Variations of the Baseline Specification			Lagged Dependent Variable	Excludes the South	Region × 1950	Ages 25–34	Hourly Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(LS_{female}/LS_{male})$	−.51	−.47	−.25	−.24	−.43	.45	−.23	−.10
	(.11)	(.09)	(.20)	(.24)	(.20)	(.29)	(.23)	(.18)
$\ln(LS_{female}/LS_{male}) \times$ female	−.31	−.25	−.42	−.35	−.61	−.27	−.20	−.50
	(.13)	(.12)	(.19)	(.15)	(.31)	(.32)	(.17)	(.22)
Implied $\sigma_{M/F}$	3.21	4.26	2.25	2.82	1.47	3.98	6.08	1.86
Estimated $\sigma_{M/F}$	3.18	3.94	2.37	2.86	1.64	3.75	5.08	2.01
Estimated σ_F	−1.21	−1.38	−1.48	−1.69	−.96	5.56	−2.34	−1.68
p-value (implied $\sigma_{M/F}$=estimated $\sigma_{M/F}$)	.58	.51	.63	.90	.37	.84	.43	.45
	First-Stage Coefficients							
Mobilization rate × 1950	1.56	1.75	1.14	1.46	1.15	.79	1.32	1.29
	(.19)	(.22)	(.30)	(.30)	(.38)	(.32)	(.27)	(.28)
Observations			267,720		211,590	267,720	80,592	354,794
Includes share farmers, share nonwhite, and average education	no	no	yes	yes	yes	yes	yes	yes

Note.—Standard errors (in parentheses) account for clustering on state and year of observation. Each estimate is from a separate pooled 1940–50 micro data 2SLS regression of log weekly pooled male and female earnings on the log ratio of female to male labor supply instrumented by the WWII state mobilization rate interacted with a 1950 dummy. All models control for year main effects, veteran status (for males), marital status, years of completed education, a quartic in potential experience, and state of residence. Specifications except for col. 1 also control for female age structure and state/country of birth. All individual variables, aside from state of residence/birth, are interacted with a 1950 dummy. Variables are also interacted with a female dummy. Models are weighted by census sampling weights. See the notes to tables 2 and 9 for additional sample details.

eters implied by the theoretical model. Recall from equation (4) in Section II that the wage elasticity of female labor demand is related to the male/female substitution elasticity by the equation $1/\sigma_F = -(1 - s_t^m)\alpha - (s_t^m/\sigma_{MF})$. Hence, given an estimate of $\hat{\sigma}_F = 1/(\hat{\chi} + \hat{\eta})$, we can calculate the implied value of the elasticity of substitution, $\tilde{\sigma}_{MF}$, and test whether this value is consistent with $\hat{\sigma}_{MF} = 1/\hat{\chi}$. This amounts to testing whether $\hat{\chi}$ and $\hat{\eta}$ are consistent with one another.

To implement this test, we calculate that the male wage bill share s_t^m was 80.5 percent over 1940–50, and we assume that the share of capital in national income α was 33 percent. Solving for $\tilde{\sigma}_{MF}$, we obtain substitution elasticities ranging from 2.4 to 3.2, which in each case are remarkably comparable to the corresponding values of $\hat{\sigma}_{MF}$. The lower rows of table 10 provide estimates of $\hat{\sigma}_{MF}$ and $\tilde{\sigma}_{MF}$ and give the p-value for a test of their equivalence. In all cases, this test accepts the hypothesis of equality with $p > .50$. We view this as bolstering the case that our methodology recovers the relevant structural parameters.[30]

In columns 4–8 of table 10, we present further specification checks similar to those in table 9, controlling for lagged state mean wages, excluding the South, adding region times 1950 interactions, limiting the sample to those aged 25–34, and expanding the sample to also include part-time workers. The results are similar, except, as earlier, adding region times 1950 interactions significantly weakens the wage estimates. In addition, the results are now somewhat weaker in the age 25–34 sample.

In panels A and B of table 11, we present analogous estimates of the labor demand and substitution elasticities, performed separately by gender. In contrast to gender relative earnings models in table 10, we require two separate labor supply variables to perform this estimation. In particular, we must control for aggregate state labor supply for each gender, $\ln F_{st}$ and $\ln M_{st}$, rather than the ratio of the two as above. Since we have only one instrument and two endogenous variables, this presents a difficulty for the estimation. We take two approaches. In the first pair of specifications in panels A and B of table 11, we exclude the male labor supply measure from the estimating equation, which is strictly valid only if this measure is orthogonal to the mobilization rate. In the second set of specifications, we include male labor supply and treat it as exogenous.

The four specifications for female earnings in panel A produce de-

[30] We also estimate in Acemoglu et al. (2002, table 10) models that replace the aggregate weeks of labor supply measure above with a measure of labor supply calculated in efficiency units following the approach of Welch (1969). We find that the elasticities of demand and substitution calculated from these efficiency unit–based estimates are quite comparable to those estimated using the weeks worked measure, and hence they are not reported here.

TABLE 11
Impact of Female Labor Supply on Female and Male Full-Time Weekly Earnings, 1940–50
Dependent Variable: Log Weekly Earnings (Whites)

	(1)	(2)	(3)	(4)
	A. Log of Female Earnings (N=69,335)			
$\ln(LS_{female})$	−.59	−.52	−.43	−.65
	(.27)	(.24)	(.11)	(.20)
$\ln(LS_{male})$			.66	.73
			(.10)	(.22)
Implied $\sigma_{M/F}$	1.54	1.76	2.20	1.38
Estimated σ_F	−1.70	−1.91	−2.32	−1.54
	First-Stage Coefficients			
Mobilization rate × 1950	1.48	1.69	1.80	1.43
	(.41)	(.46)	(.21)	(.25)
Includes share farmers, share nonwhite, and average education	no	yes	no	yes
	B. Log of Male Earnings (N=198,385)			
$\ln(LS_{female})$	−.39	−.15	−.32	−.20
	(.16)	(.11)	(.07)	(.11)
$\ln(LS_{male})$			.46	.25
			(.07)	(.12)
Implied $\sigma_{M/F}$	.60	2.24	.75	1.40
	First-Stage Coefficients			
Mobilization rate × 1950	1.60	1.77	1.82	1.44
	(.41)	(.45)	(.21)	(.25)
Includes share farmers, share nonwhite, and average education	no	yes	no	yes

Note.—Standard errors (in parentheses) account for clustering on state and year of observation. Each estimate is from a separate pooled 1940–50 micro data 2SLS regression of log weekly male or female earnings on the log ratio of female to male labor supply instrumented by the WWII state mobilization rate interacted with a 1950 dummy. All models control for year main effects, veteran status (for males), marital status, years of completed education, a quartic in potential experience, and state of residence. Specifications except for col. 1 also control for female age structure and state/country of birth. All individual variables, aside from state of residence/birth, are interacted with a 1950 dummy. Models are weighted by census sampling weights. See the notes to tables 2 and 9 for additional sample details.

mand elasticity estimates that are comparable to those in table 10. The implied substitution elasticities recovered from this exercise are somewhat smaller than those in table 10 (in the range of 1.4–2.2) and are more sensitive to the inclusion of the male labor supply measure. Our inability to instrument both male and female labor supply simultaneously limits the reliability of these estimates.

In panel B of table 11, we find uniformly negative but mostly insignificant effects of female labor supply on male earnings. These estimates are also sensitive to the inclusion of male labor supply. The elasticities of substitution implied by these estimates are relatively low (in the range of 0.6–2.2). Given the imprecision of these estimates and the endogeneity of the male labor supply measure, we place greater confidence in the models in table 10.[31]

D. Does Female Labor Supply Raise Male Earnings Inequality?

The results above suggest that female labor supply lowers average male earnings. But this impact need not be uniform throughout the male wage distribution. Indeed, several authors have argued that rising female labor supply over recent decades is in part responsible for growing male earnings inequality in the U.S. labor market. Greater female labor supply will generally raise male earnings inequality if women are closer substitutes for low-earning men than for high-earning men. We explore this relationship here by using the mobilization to estimate how increases in female labor supply affect the relative earnings of men at different points of the education distribution.[32]

Consider a variant of equation (11) in which the dependent variable is log weekly earnings of men of two education groups, college and high school graduates (we later compare high school and eighth grade graduates):

$$\ln w_{ist}^{m} = \delta_s + \gamma_{1950} + c_i + X_{ist}'\beta_t^{e} + \pi \ln\left(\frac{F_{st}}{M_{st}}\right) + \theta c_i \ln\left(\frac{F_{st}}{M_{st}}\right) + \nu \ln\left(\frac{C_{st}^{m}}{H_{st}^{m}}\right) + u_{ist}. \tag{12}$$

In this equation, c_i is a dummy for whether individual i is a college graduate (the omitted group is high school graduates), F_{st}/M_{st} is relative female labor supply measured in aggregate weeks worked as above, and C_{st}^{m}/H_{st}^{m} is the relative supply of college versus high school male labor

[31] As noted above, the estimates reported here are likely to correspond to short-run elasticities. Acemoglu et al. (2002, app. table 6) provide estimates of longer-run elasticities of female labor demand by exploiting the WWII mobilization to isolate plausibly exogenous state-level shifts in female relative labor supply over the two decades from 1940 to 1960. These estimates provide some limited evidence that the long-run relative demand curve for women's labor is considerably more elastic than the short-run relative demand curve.

[32] Topel (1994, 1997) finds that the rising supply of college-educated women during the 1970s and 1980s substantially depressed the wages of high school dropout men. The papers by Blau and Kahn (1997) and Juhn and Kim (1999) conclude that female labor supply has had little impact on male earnings inequality during recent decades. The short papers by Fortin and Lemieux (2000) and Welch (2000) both posit a causal link between rising female wages and rising male inequality during the 1980s and 1990s. But they differ on whether the genesis of this link is supply or demand factors. Katz and Autor (1999) synthesize the literature on U.S. earnings levels and earnings inequality from the 1940s through the 1990s.

input.[33] All covariates are allowed to have different effects on earnings of college and noncollege men and to differ by decade. Since relative supplies of male college versus high school graduates, C^m_{st}/H^m_{st}, should also directly affect the male college/high school premium, we must either control for this measure or assume that the instrumented female labor supply measure is uncorrelated with it. We implement both approaches below.

The coefficients of interest in this equation are π and θ. The coefficient π measures the impact of female labor supply on the earnings of high school graduates, and θ measures the effect of female labor supply on the relative wages of college versus high school graduates. Therefore, keeping the employment levels of college and high school graduate men constant, we can think of $\sigma_{fh} = 1/\pi$ as the cross-elasticity of demand between female labor and high school graduates and $\sigma_{fc} = 1/(\pi + \theta)$ as the cross-elasticity of demand between female labor and college graduates. The ratio of cross-elasticities of women for high school versus college graduates, $\sigma^f_{hc} \equiv \sigma_{fh}/\sigma_{fc}$, is therefore equal to $(\pi + \theta)/\pi$. If $\sigma^f_{hc} < 1$, this implies that female labor has a larger wage impact on high school graduates; so women are closer substitutes for high school than for college men, and vice versa if $\sigma^f_{hc} > 1$.

Estimates of equation (12) for college and high school graduates, shown in panel A of table 12, reveal that growth in female labor supply exerts a small positive effect on male college/high school earnings inequality. A 10 percent increase in female labor supply is predicted to lower male high school wages by 2.5–4 percent, while reducing college wages by only 1–2.5 percent. These estimates imply a ratio of cross-elasticities of 0.4–0.6, but this is imprecisely estimated. This evidence suggests that women drawn into the labor force by WWII mobilization were more substitutable for high school– than for college-educated men, which appears consistent with the characteristics of female labor force entrants documented in panel B of Appendix table A1. But we cannot reject the hypothesis that women's labor supply reduced college and high school wages by equivalent amounts.

While the college/high school wage differential is of contemporary interest, 85 percent of men in 1950 had a high school or less education,

[33] In models that use the male college/high school relative supply, college labor supply is the sum of total weeks worked supplied by college graduates (or higher) plus half of those supplied by those with some college; high school labor supply is the sum of weeks worked supplied by high school graduates or less plus half of those supplied by those with some college. In models that use the male high school/eighth grade relative supply, high school labor input is the sum of weeks worked supplied by those with high school or more plus half of that supplied by those with more than eighth grade and less than high school education; eighth grade is the sum of weeks worked supplied by those with eighth grade or less, plus half of that supplied by those with more than eighth grade and less than high school.

TABLE 12

IMPACT OF FEMALE LABOR SUPPLY ON MALE EDUCATIONAL EARNINGS DIFFERENTIAL, 1940–50

Dependent Variable: Log Weekly Earnings

	Regression				
	(1)	(2)	(3)	(4)	(5)
	A. Male College/High School Graduate Log Weekly Earnings Differentials (Whites) (N=58,885)				
$\ln(LS_{female}/LS_{male})$	−.471	−.466	−.269	−.428	−.270
	(.126)	(.118)	(.228)	(.141)	(.228)
$\ln(LS_{female/male})\times$ college	.206	.261	.233	.283	.231
	(.126)	(.117)	(.210)	(.131)	(.210)
$\ln(LS_{college\text{-}male}/LS_{HS\text{-}male})$ $\times$ college				−.042	−.004
				(.087)	(.080)
	First-Stage Coefficients				
Mobilization rate × 1950	1.62	1.79	1.18	1.64	1.19
	(.20)	(.23)	(.30)	(.24)	(.30)
Implied relative cross-elasticity $\sigma_{HS/CLG}$	.56	.44	.14	.34	.15
	B. Male High School Graduate/Eighth Grade Log Weekly Earnings Differential (Whites) (N=93,244)				
$\ln(LS_{female}/LS_{male})$	−.192	−.220	−.115	−.173	−.129
	(.109)	(.107)	(.205)	(.149)	(.203)
$\ln(LS_{female/male})\times$ high school	−.279	−.246	−.154	−.153	−.140
	(.097)	(.095)	(.178)	(.117)	(.173)
$\ln(LS_{high\ school\text{–}male}/LS_{8th\text{-}male})$ $\times$ high school				−.070	−.145
				(.057)	(.072)
	First-Stage Coefficients				
Mobilization rate × 1950	1.64	1.80	1.20	1.56	1.20
	(.20)	(.24)	(.30)	(.26)	(.30)
Implied relative cross-elasticity $\sigma_{8th\ grade/HS}$	2.45	2.12	2.34	1.89	2.09
Includes share farmers, share nonwhite, and average education	no	no	yes	no	yes

NOTE.—Standard errors (in parentheses) account for clustering on state and year of observation. Samples and specifications are identical to those of table 10 except that the sample in panel A is restricted to males with exactly a college degree or a high school diploma, and the sample in panel B is restricted to males with exactly a high school diploma or eighth grade completion. In addition to time interactions on all individual-level controls, all variables are interacted with a college graduate dummy in panel A and a high school graduate dummy in panel B. The specifications in cols. 2–5 control for female age structure and state/country of birth. Log(female/male) labor supply and its interaction with the college or high school dummy are instrumented by the state mobilization rate and its interaction with the college or high school dummy. All models are weighted by census sampling weights. See the notes to tables 2 and 9 for additional sample details.

with the two modes of the distribution found at exactly high school (20.3 percent) and exactly eighth grade (18.2 percent). Therefore, it is of interest to ask whether female labor supply raised or lowered earnings inequality between these groups of men. We perform analogous estimates for the high school/eighth grade differential in panel B of table 12. These estimates indicate that a 10 percent increase in female labor supply reduces male high school relative to eighth grade earnings by 1.5–2.5 percentage points. This relative wage impact is highly significant in specifications that do not control for state aggregate measures but insignificant in columns that incorporate these measures. We also cannot reject the hypothesis that female labor supply had no impact on the wages of eighth grade men.

In net, the primary impact of increased female labor supply on male educational inequality during the 1940s was to lower the wages of male high school graduates relative to more and less educated men. Given that low-educated men in 1950 were predominantly employed in manual occupations, it is plausible that women would be closer substitutes for high school graduates. This result stands in some contrast to Topel's (1994) finding that high-skill women are strong substitutes for low-skill men. Of course, our findings pertain to another era, and these substitution parameters need not be fixed over long intervals.

VII. Conclusion

The epochal rise in female labor force participation is one of the most profound labor market transformations of the past century. And yet, the economics profession knows relatively little about the consequences of increased female labor force participation for the structure of wages. An empirical investigation of this issue requires a source of variation in female employment that is orthogonal to demand for female (and also male) labor.

In this paper, we developed the argument that the differential extent of mobilization for WWII across U.S. states provides a useful source of variation to identify the effects of women's labor force participation on a range of labor market outcomes. We documented that in 1950 women participated more in states in which a larger fraction of working-age men served in the military during the mid-1940s. This differential female labor supply behavior does not seem to be accounted for by other cross-state differences or possible demand factors and is not present in the pre-1940 or post-1950 data. We interpret this as a shift in female labor supply induced by the mobilization for the war.

Using this source of variation, we estimate the effect of greater female participation on female and male wages, returns to education, and wage inequality among men. Contrary to the results implied by OLS models, our estimates suggest that the female labor demand curve slopes measurably downward with an elasticity of -1.0 to -1.5, and further that men and women are close but far from perfect substitutes. We attribute the contrast between conventional OLS models and our instrumental variables to simultaneity bias in OLS estimates. Contrary to a common hypothesis in the literature, we also find that women at midcentury were not the closest substitutes for the lowest-education men, but for high school graduate men.

Appendix

TABLE A1
IMPACT OF WWII MOBILIZATION RATES ON FEMALE LABOR SUPPLY BY AGE, EDUCATION, AND COHORT, 1940–50
Dependent Variable: Annual Weeks Worked (Whites)

	Baseline Specification	Includes Share Farm, Share Nonwhite, and Average Education	Observations
	A. Weeks Worked by Age Group		
Ages 14–64	9.85	8.73	530,026
	(2.05)	(2.39)	
Ages 14–17	1.41	.88	
	(.32)	(.39)	
Ages 18–24	2.24	5.06	
	(.99)	(1.32)	
Ages 25–34	.57	2.00	
	(.88)	(1.58)	
Ages 35–44	2.41	2.31	
	(.73)	(.93)	
Ages 45–54	2.23	−1.44	
	(1.05)	(1.55)	
Ages 55–64	1.00	−.08	
	(.96)	(1.05)	
	B. Weeks Worked by Education Group		
8th grade and below	−1.05	−.82	530,026
	(1.47)	(1.76)	
9th–11th grade	−2.75	1.75	
	(1.51)	(1.49)	
12th grade and above	13.65	7.81	
	(1.85)	(2.02)	
	C. Weeks Worked by Age Cohort		
14–24 in 1940	4.16	18.14	138,870
	(5.25)	(5.71)	
25–34 in 1940	−2.33	16.21	122,083
	(3.55)	(6.53)	
35–44 in 1940	23.42	17.76	103,918
	(5.40)	(7.23)	
45–54 in 1940	13.91	−6.45	92,550
	(5.46)	(4.75)	

NOTE.—Standard errors (in parentheses) account for clustering on state of residence and year of observation. Each entry is from a separate pooled micro data regression of female weeks worked for the relevant demographic subgroup on state mobilization rate interacted with a 1950 dummy. All specifications include controls for year main effects, age, marital status, state of residence, and state/country of birth. All individual variables, aside from state of residence/birth, are interacted with a 1950 dummy. All models are weighted by census sampling weights. See the note to table 2 for additional sample details.

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