Essays Using Military-Induced Variation to Study Social Interactions, Human Capital Development, and Labor Markets

by

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Abstract

This dissertation consists of four empirical studies, each using military-induced variation to examine various aspects of human capital production and the U.S. labor market. The first two chapters study the effects of social interactions on human capital development at the United States Military Academy where social groups are randomly assigned. Chapter 1 highlights the empirical difficulties associated with identifying social effects and contains evidence suggesting that occurrences common to a social group may account for a large part of social group correlations found in many studies. While models that address these identification concerns provide little evidence of social effects in academic performance, there is evidence that both peers and role models influence other dimensions of human capital that have important labor market consequences. Chapter 2 builds on the previous chapter by investigating whether peers are complements or substitutes in the production of human capital at West Point. Heterogeneity in peer group composition can provide evidence for the degree of substitutability between peers. Estimates reveal that more heterogeneous peer groups have positive effects on individual grades. This suggests that peers serve as substitutes, and therefore, mixing cadets by ability is optimal for the efficient production of education at West Point. Chapter 3 evaluates the impact of military-induced parental absences and household relocations on children's educational attainment. Estimates indicate that parental absences adversely affect children's test scores by a tenth of a standard deviation and frequent household relocations also have modest negative effects of similar magnitude. Chapter 4 investigates the effects of female labor supply on the U.S. wage structure at mid-century. As men mobilized for war in the 1940s, women were drawn into the workforce. In states with greater mobilization rates, women worked more after the War and in 1950, although not in 1940. Estimates indicate that increases in female labor supply lower female and male wages, and generally increase the college premium and male wage inequality. Finally, at mid-century, women were closer substitutes to high school graduate and relatively low-skill males, but not to those with the lowest skills.

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Contents

Introduction

This dissertation investigates the effects of social groups on human capital production, the effects of parental absences and household relocations on children's academic attainment, and the effects of female labor supply on wages. In each of these empirical studies, military-induced variation is used to identify the treatment effects.

Chapter 1 highlights many of the difficulties confronting the interpretation of social effects and presents an experiment that contends with the predominant identification problems. The random assignment of cadets to Companies at the United States Military Academy provides a rare opportunity to address potentially misleading estimates of social effects in human capital production and to estimate the effect of an important dimension of social relationships, role models. Combining individual-level pretreatment characteristics with measures of human capital from time spent at West Point and in the Army, I estimate the impact of peer and role model relationships on academic GPA, Math grades, the choice of academic major, and the decision to remain on active duty military status past an initial five-year obligation period. Estimates of contemporaneous social effects, which are subject to several potential biases, are positive and significant; however, evidence suggests that occurrences common to a social group may account for a large part of this correlation. While reduced form specifications that deal with the primary identification problems provide little evidence of social group effects in academic performance, they provide compelling evidence of social influences in the choice of academic major and the decision to remain in the Army.

Chapter 2 builds on the model in Chapter 1 by investigating whether peers are complements or substitutes in the production of human capital at West Point. The degree of substitutability between peers has important production efficiency implications for how schools organize classrooms. Benabou (1996b) presents a theoretical model where heterogeneity in peer group composition can provide evidence for the degree of peer substitutability. I test a version of this model by estimating the impact of peer group heterogeneity in Math SAT scores on freshmen-year Math grades and academic grade point averages for cadets at West Point. Estimates reveal that more heterogeneous peer groups have positive effects on a cadet's grades. A one standard deviation increase in the peer group 75-25 differential in peer Math SAT distributions increases the Company average Math grade by thirteen percent of a standard deviation; the 75th percentile, but not the 25th percentile, of the peer Math SAT distribution accounts for most of this effect. According to the theoretical model, this evidence suggests that peers serve as substitutes in this setting, and therefore, mixing cadets by ability is optimal for the efficient production of education at the United States Military Academy.

In Chapter 3, I exploit labor force requirements of the United States Army to estimate the impact of work-related parental absences and work-induced household relocations on children's educational attainment. Combining U.S. Army personnel data with children's standardized test scores from the State of Texas, I estimate the effects of current academic year parental absences, cumulative four-year parental absences, the number of household relocations, and the average time between relocations on children's test scores. Reduced form estimates indicate that parental absences during the current school year adversely affect children's test scores by a tenth of a standard deviation. Cumulative four-year absences also negatively influence children's academic attainment; officers' children experience as much as a fifth of a standard deviation decline in test scores. Furthermore, household relocations have modest negative effects on children's test scores for enlisted soldiers, but no significant effect on officer's children. Other evidence suggests that parental absences and household relocations cause additional detrimental effects to test scores of children with single parents, children with mothers in the Army, children with parents having lower AFQT scores, and younger children.

Chapter 4 contains a study that is co-authored with Daron Acemoglu (MIT) and David Autor (MIT). This study investigates the effects of female labor supply on the U.S. wage structure at mid-century. To identify variation in female labor supply, we exploit the military mobilization for World War II, which drew many women into the workforce as males exited civilian employment. The extent of mobilization was not uniform across states, however, with the fraction of eligible males serving ranging from 41 percent to 54 percent. We find that in states with greater mobilization of men, women worked substantially more after the War and in 1950, although not in 1940. We interpret these differentials as labor supply shifts induced by the War. We find that increases in female labor supply lower female wages, lower male wages, and generally increase the college premium and male wage inequality. Our findings indicate that at mid-century, women were closer substitutes to high school graduate and relatively low-skill males, but not to those with the lowest skills.

Chapter 1: Estimating and Interpreting Peer and Role Model Effects from Randomly Assigned Social Groups at West Point

1.1 Introduction

Substantial correlations in outcomes frequently exist between individuals and their associated social groups. A few examples include educational attainment within schools (Coleman, 1966; Sacerdote, 2001), pregnancy and dropout behavior among teenagers (Evans, Oats, and Schwab, 1992), and crime within neighborhoods and families (Case and Katz, 1991).[1] Economists have focused particular attention on social effects in education production for the obvious link to labor market outcomes. Studies that report positive correlations often interpret them as evidence of human capital externalities, or peer effects. However, there are at least two other potential interpretations.

Variation among social groups can also be a result of selection. Selection into a social group could be a decision by the individual, the peer group, or a third party who assigns individuals to a group based on some defining characteristic. For example, families may choose neighborhoods by the quality of surrounding schools, parents may request teachers with stronger reputations, students may choose peers with similar attributes, and schools may assign students to classrooms by measures of past ability. In any case, social groups are likely formed on the basis of characteristics that may also be correlated with the outcomes of the group. Most recent studies have attempted to account for the selection problem.

A second potential source of variation in social groups, which has received much less attention in the literature, is a common occurrence that influences the outcomes of everyone in the group. I refer to this as a common shock. Examples of common shocks in an educational setting may include teachers, the sequence of daily instruction, the location of the classroom, or even classroom seating configurations. Common shocks can also affect social groups over time. The positive educational impact that a first-grade teacher has on a group of students can result in a positive correlation for as many grade levels as the students remain together. In rural schools with low mobility rates, this effect could persist for many years.

Differentiating between the selection effect, the common shock effect, and the true peer effect is difficult because the selection criteria and the common shocks are typically unobserved.

[1] These are a few of the more commonly cited studies in the literature. See Section 1.8 for references to many other related studies in the social effects literature.

These main identification obstacles are further complicated by several additional modeling concerns. First, it is necessary to determine the influential constituents of the social group. They could be members of a student's homeroom class, fellow companions on an athletic team, neighborhood acquaintances, or any number of other possibilities. Second, one must choose the important characteristics that affect an individual's ability to learn from a virtually endless menu of previous and current behavior. Finally, assuming that the above issues are sufficiently addressed, a causal social effect interpretation must still account for potential endogeneity: an individual can impact his social group at the same time that his social group impacts him.

A common strategy for handling the first major issue, selection, is to employ an instrumental variable as an exogenous source of variation. For example, Evans, Oats, and Schwab (1992) use an instrumental variable to identify social effects for teen pregnancy and high school dropout behavior. They find statistically significant social effects with an Ordinary Least Squares (OLS) specification, yet they find no significant social effects with a Two Stage Least Squares (2SLS) specification.

Evans et al. (1992) is cited in many social effect studies to illustrate the importance of controlling for selection bias. However, Rivkin (2001) demonstrates that the 2SLS estimates in their study are sensitive to the chosen instruments. Rivkin's findings are not surprising because the underlying hypothesis of social relationships makes it exceedingly difficult to defend the validity of an instrumental variable. It must be correlated with an individual's social group behavior, yet uncorrelated with all other potential determinants of the individual's own behavior. The instruments used in the Evans et al. (1992) study are the unemployment rates, median family income, poverty rate, and the percentage of adults who completed college in the local metropolitan area. Arguing that these instruments are uncorrelated with the determinants of a teenager's peer group may be possible, but arguing that they are uncorrelated with potential common shocks is nearly impossible.[2]

Another method that has been used to address the selection problem is to locate an experiment where social groups are randomly assigned. For example, Sacerdote (2001) uses the random assignment of roommates at Dartmouth College to identify peer effects in academic attainment and in decisions to join fraternities. Unlike in Evans et al. (1992), the removal of

[2] Acemoglu and Angrist (2000) and Duflo and Saez (2002) use more convincing instrumental variables that are more likely to satisfy the exclusion restriction.

selection bias in Sacerdote (2001) still results in strong correlations between outcomes of individuals and their roommates. However, like Evans et al. (1992), common shocks are not sufficiently accounted for. Even though Sacerdote (2001) is one of the few studies in the literature to acknowledge the potential for common shocks, data restrictions preclude an adequate assessment of their importance.

The first part of this study investigates how common shocks may confound estimates of social effects by exploiting randomly assigned social groups at the United States Military Academy. The environment at West Point provides a unique opportunity to account for the selection problem, address many of the other modeling considerations discussed above, and measure the effect of several potential common shocks. The second part of this study builds on the existing literature by including different dimensions of social relationships (group level peers and role models) and by expanding the set of human capital related outcomes. The two performance outcomes used in this study are freshmen-year Academic Grade Point Averages (GPA) and Math grades; and, the two choice outcomes used in this study are the selection of academic major and the decision to remain on active duty military status past an initial five-year obligation period.

Each year West Point randomly assigns incoming cadets to one of thirty-six Companies. Companies have approximately thirty-five cadets in each year-group. Freshmen cadets are the focus of this study, so I will use West Point terminology and refer to them as "plebes." To minimize confusion, I will refer to all other upperclassmen using the standard convention (sophomores, juniors, and seniors). The term "peer effects" is used to describe how other plebes in a Company affect an individual plebe. The organizational structure at West Point also provides an opportunity to evaluate how role models impact human capital production. Thus, I use the term "role model effects" to describe how sophomores in a Company impact a plebe.

Estimates of contemporaneous peer effects reveal strong and positive correlations, however, common shocks appear to account for a large part of this effect. Consistent with the literature, I find little statistical evidence of social effects in academic performance outcomes using average pretreatment measures of academic ability. However, there is evidence for social group effects related to the choice of academic major and the decision to remain in the Army.

In the next section, I provide background information on the United States Military Academy. Section 1.3 describes the data and Section 1.4 explains the random assignment of cadets to

Companies. In Section 1.5, I present the empirical framework and formally discuss the identification assumptions and interpretations. Section 1.6 contains the main results and Section 1.7 concludes.

1.2 The United States Military Academy

The United States Military Academy is one of three service academies fully funded by the U.S. Government for the expressed purpose of "providing the Nation with leaders of character who serve the common defense."[3] Cadets offered admission to the Academy receive a fully funded four-year scholarship. Graduates obtain an accredited Bachelor of Science degree and must fulfill a five-year active duty service obligation as an officer in the U.S. Army.

The Corps of Cadets at West Point is organized into one Brigade consisting of thirty-six Companies, as seen in Figure 1.1. The Brigade is divided into four Regiments, each Regiment is divided into three Battalions, and each Battalion is further divided into three Companies. Every Company is directed by a Tactical Officer and a Non-Commissioned Officer (NCO) from the U.S. Army and has approximately 140 cadets, thirty-five from each of the four classes. West Point randomly assigns cadets to a Company conditional on several observable characteristics: gender, race, recruited athlete, and measures of prior performance and behavior. Cadets maintain the same initially assigned Company through the end of the sophomore-year, when they are reassigned to a different Company for the remaining two years.

Plebes arrive at West Point prior to the beginning of the academic-year to take part in six weeks of Cadet Basic Training with their assigned Company. Plebes eat, sleep, attend mandatory social activities, and conduct military training together as a Company. By design, there is little interaction with plebes outside of the Company. Upon completion of Cadet Basic Training, each Company of plebes joins the upperclassmen in their Company to begin the academic-year.

During the academic-year, Cadets from all four classes of each Company live together in a section of the barracks. The hierarchical structure of a Company at West Point is similar to a Company in an active duty Army unit and is designed to develop the leadership skills of the upperclassmen and to foster teamwork among the plebes. In general, seniors fill the role of

[3] Bugle Notes (1990-1994) page 4.

officers, juniors fill the role of NCOs, sophomores fill the role of small-unit leaders, and plebes fill the role of privates.

As small-unit leaders, sophomores supervise plebes in the performance of routine duties such as keeping the Company area in immaculate condition, delivering items (newspapers, mail, and laundry), and memorizing institutional knowledge. In an effort to encourage teamwork and promote achievement, sophomores frequently attribute failures and successes of one plebe to other plebes within the Company. This spills into the academic realm, as sophomores regularly organize plebes for study sessions prior to major exams.

All cadets take the same courses the first two years of study. During plebe-year, Calculus, English, History, Computer Science, Behavioral Psychology, and Chemistry contribute to a plebe's academic GPA.[4] At the end of the second year, cadets declare their major area of study from one of thirteen different Academic Departments ranging from History, Foreign Language, and Social Sciences to Engineering, Physics, and Chemistry.

An additional feature of the academic program at West Point, which is important to this study, is that plebes do not usually take academic classes with other plebes from their Company. However, all plebes receive the same program of instruction, complete the same homework assignments, and take the same exams. Since the Company is the dominant organization, nearly all homework assignments and exam preparations are conducted between plebes within the same Company.

1.3 Data Description

The data for this study is from the Office of Economic Manpower Analysis (OEMA), West Point, NY. I combine data from several sources for the graduating classes of 1992-1998: Admissions files, Survey of Incoming Freshmen, Cadet Personnel records, and Active Duty Officer Personnel records. The data is organized into three categories: academic performance and choice outcomes, pretreatment (prior to West Point) characteristics, and randomization controls. Table 1.1 contains Company-level summary statistics and is divided into panels by these three categories. In most cases, data is available for plebes in 252 Companies (36 Companies over 7 years).

[4] Statistics, Calculus II, Physics, Philosophy, Economics, Political Science, and Foreign Language count towards the sophomore-year GPA.

Panel A contains data used as outcome variables in this study. All grades are assigned along a scale ranging from 0 to 4.3 points: a 4.3 equates to an A+, a 4.0 equates to an A, and a 3.7 equates to an A-. The average plebe academic GPA is 2.66 points (C+) and the average Plebe Math grade is 2.69 (C+) points. The actual choice of academic major ranges from 9 percent in the Natural Sciences to approximately 41 percent in Engineering. Roughly 50 percent of all graduates remained in the U.S. Army at least one year past their initial obligation of five years. I determine this by verifying whether or not a graduate is still on active duty status six years after graduating from the Academy. This data is only available for plebes in 180 Companies because six years past graduation has only transpired for year-groups 1992-1996 at the time of this study. Finally, about 7 percent of each class drops out of the Academy during Cadet Basic Training and an additional 5 percent drop out during plebe-year.

In panel B, I present summary statistics for the pretreatment data. All cadets have an SAT score. Most take the SAT, but about 10 percent only take the ACT. The Admissions Office converts ACT scores into SAT scores with a standard conversion factor.[5] All SAT scores were taken prior to the 1995 renormalization, so they are comparable. The average Total SAT score is approximately 1200 points, and the average Math SAT score is about 640 points. The Leadership Potential Score (LPS) is a cumulative measure of leadership experience prior to entering West Point. For example, being the captain of a varsity high school basketball team may contribute 75 points to the LPS and being a member of a high school student council may result in 50 more points. The LPS ranges from 0-800 points and has a mean of 600 points. The remaining background data is from the Survey of Incoming Freshmen.[6] Plebes complete this survey during the first week of Cadet Basic Training. This data is available for plebes in only 216 Companies because the graduating class of 1993 did not participate in the survey. The proposed major of study ranges from 11 percent in the Natural Sciences to more than 44 percent in Engineering. Finally, 36 percent of incoming cadets plan to make the military a career.

Panel C contains summary statistics for the randomization controls. Almost 12 percent of the Corps of Cadets is made up of females. Blacks and Hispanics combine to account for about 10 percent of each class. A little more than 21 percent of incoming cadets are recruited for one

[5] Schneider D. and N.J. Doran (1999). This conversion factor only produces a Total SAT score and not Math and Verbal components. Thus, some observations have a Total SAT, but not a Math SAT score.
[6] The American Council on Education and the University of California at Los Angeles conducts this survey each year.

of the 20 NCAA Division-One athletic programs at the Academy. Also, 14 percent attended the United State Military Academy Prep School the year before entering West Point. The College Entrance Exam Rank (CEER) is a weighted average between the high school graduation ranking of the cadet and the SAT/ACT scores. The range of this ranking is from 0-800 points, with a mean of approximately 600 points. The Whole Candidate Score (WCS) is similar to the LPS in that it aggregates assigned values to various activities and performance outcomes from high school. The WCS ranges from 0-8000 points and has a mean of about 6000 points.

1.4 Social Groups and Random Assignment

There are few instances where it is possible to clearly indicate an individual's social group and there are even fewer cases where the composition of a social group is not also tainted with selection bias. However, the structure of the United States Military Academy provides an opportunity to address both concerns. Not only does "Uncle Sam" issue cadets a uniform and a 'tight haircut', he also issues them peers and role models.

The value of determining the appropriate peer group is demonstrated in Sacerdote (2001). There is modest evidence for peer effects with fraternity participation at the roommate level, yet there is stronger evidence for the same peer effects at the dorm level. The above discussion on the organization of West Point suggests that the Company is the appropriate group for the outcomes used in this study. This cannot be formally tested because data on further divisions within a Company (roommate assignments for example) are not available, but interviews with faculty and graduates also support this claim.

The critical identification assumption for this experiment is that the assignment of cadets to Companies at West Point is random, conditional on the eight individual-level controls listed in panel C of Table 1.1. The following description of the assignment process and some brief empirical analysis supports this assumption.

West Point uses a computer program to assign a random number to each incoming plebe and to each of the thirty-six Companies in a process known at the Academy as *scrambling*.[7] The goal of scrambling is to produce Companies with comparable means across these eight characteristics. Incoming plebes are initially assigned to a Company based on their random

[7] USMA publication 98-007, "Evaluation of Scrambling in the Corps of Cadets 1962-1998." Discussions with managers in charge of scrambling from the Institutional Research and Analysis, Office of Policy Planning & Analysis, West Point NY, also confirm this description of the process.

number. The computer program then shuffles plebes between Companies in an attempt to equalize the means of the eight characteristics. All subsequent rearrangements of plebes between Companies are a function of the eight characteristics and the random number.

Estimates in Table 1.2 support this description of the assignment process. I regress average social group pretreatment characteristics on corresponding individual level characteristics to determine if a cadet's background predicts the background of his social group. The peer average is the average pretreatment characteristics of the plebes in a Company minus the individual plebe. The role model average is the average pretreatment characteristics of the sophomores in a Company.

Peer assignments are tested in panel A. Estimates in column (1) are from a bivariate regression of average peer Total SAT score on individual Total SAT score. There is a small and negative correlation as would be expected given the equalizing intent of the scrambling process described above.[8] The specification in column (2) adds the eight individual-level scrambling controls. The point estimate is smaller in absolute value and no longer significant. I conduct a similar exercise for the other pretreatment measures used in this study as listed in the column headings. In general, estimates from specifications without the scrambling controls have a small and negative correlation and estimates from specifications with the scrambling controls have no significant correlation. Panel B contains estimates from identical regressions, except for role models instead of peers. In all cases, a cadet's background characteristic does not predict the background characteristics of his role models, regardless of whether or not the scrambling controls are included. Therefore, to account for the conditional randomization process, I include the individual-level scrambling controls in all specifications.

1.5 Empirical Framework

Manski (1993) identifies three primary sources of measured social group effects: exogenous effects, endogenous effects, and correlated effects. In the context of this study, exogenous effects refer to the pretreatment behavior of the social group, endogenous effects refer to the contemporaneous behavior of the social group, and correlated effects refer to the selection and

[8] Since the peer average Total SAT is constructed without the own Total SAT score, plebes with higher SAT scores will likely be assigned to Companies with lower average Total SAT scores.

common shock effects discussed in the introduction. To more formally investigate these issues, consider a model with the following structure:

$$Y_{ict} = \alpha + \theta_t + \lambda \cdot Z_{ict-1} + \gamma \cdot \overline{Z}_{gt-1} + \delta \cdot \overline{Y}_{gt} + \beta \cdot X_{ict-1} + \varepsilon_{ict} \quad (1.1)$$

The left-hand side variable, Y_{ict}, is the outcome of interest (academic GPA) for cadet *i*, in Company *c*, in year *t* (plebe-year). On the right-hand side, α is a constant and θ_t are year dummies for 1993-1998. λ represents the effect of own pretreatment (*t-1*) measures (SAT) and γ represents the effect of average pretreatment measures (average SAT) of the social group, *g* (*g*=*c*-*i*). δ is the effect of contemporaneous average behavior (average GPA) of the social group and β denotes the individual-level scrambling controls contained in X_{ict-1}. ε_{ict} corresponds to other potential determinants of individual-level outcomes, where $\varepsilon_{ict} = \sigma_{ict} + \omega_{ct} + \eta_{ict}$. Here σ_{ict} represents unobserved selection effects, ω_{ct} represents unobserved common shock effects, and η_{ict} represents a standard stochastic error term.

In most settings, estimates for coefficients of interest γ and δ would be subject to selection bias due to correlations between $\overline{Z}_{gt-1}$ and σ_{ict} and between $\overline{Y}_{gt}$ and σ_{ict}. However, the conditional random assignment of cadets to Companies makes it likely that $E\left[\overline{Z}_{gt-1}\sigma_{ict}\right] = E\left[\overline{Y}_{gt}\,\sigma_{ict}\right] = 0$. Likewise, common shocks may bias estimates of γ and δ due to correlations between $\overline{Z}_{gt-1}$ and ω_{ct} and between $\overline{Y}_{gt}$ and ω_{ct}. Randomly assigned social groups imply $E\left[\overline{Z}_{gt-1}\omega_{ct}\right] = 0$ because shocks to pretreatment characteristics are no longer common to members of the newly assigned social group. On the other hand, random assignment does not imply that $E\left[\overline{Y}_{gt}\,\omega_{ct}\right] = 0$. Therefore, common shocks potentially confound the interpretation of estimates using specifications like Equation (1.1).

If $E\left[\overline{Y}_{gt}\,\omega_{ct}\right] = 0$, then estimates of δ can indicate the presence of a social effect, but they do not have a causal interpretation due to the endogeneity between Y_{ict} and $\overline{Y}_{gt}$. Since cadet *i* and his peers earn their GPA concurrently, the OLS estimates of δ in Equation (1.1) do not reflect whether cadet *i* affects the other cadets in his social group, whether his social group affects him, or whether both affect each other. This can be viewed as the standard simultaneous equations model where Equation (1.1) and Equation (1.2) form a system of equations linked through Y_{ict} and $\overline{Y}_{gt}$.

$$\overline{Y}_{gt} = \widetilde{\alpha} + \widetilde{\theta}_t + \widetilde{\lambda} \cdot Z_{ict-1} + \widetilde{\gamma} \cdot \overline{Z}_{gt-1} + \widetilde{\delta} \cdot Y_{ict} + \widetilde{\beta} \cdot \overline{X}_{gt-1} + \widetilde{\varepsilon}_{gt} \qquad (1.2)$$

For both the common shock and the endogeneity issues, having contemporaneous outcomes in the specification is the source of the problem. In theory, an instrumental variable would overcome both concerns. Although as already discussed, the nature of social relationships makes it particularly difficult to find an appropriate instrument that is correlated with contemporaneous outcomes of members of an individual's social group and uncorrelated with other potential determinants of the individual's own outcomes.

Several other approaches have been used to address these identification concerns. Some studies have employed a lagged value of the endogenous variable ($\overline{Y}_{gt-1}$) and other studies have dropped the endogenous variable ($\overline{Y}_{gt}$) altogether. Both methods deal with the endogeneity problem by imposing a timing structure on the social effect of interest: Y_{ict} cannot influence either $\overline{Y}_{gt-1}$ or $\overline{Z}_{gt-1}$. However, neither method adequately addresses the common shock problem without an additional condition. Absent the reassignment of social groups between period *t-1* and period *t*, there is still likely to be serial correlation in common shocks associated with either $\overline{Y}_{gt-1}$ or $\overline{Z}_{gt-1}$.

The Zimmerman (2003) study comes closest to dealing with the main identification problems. His study exploits the random assignment of roommates at Williams College and estimates specifications that contain only pretreatment characteristics. This is equivalent to estimating the reduced form of the simultaneous system characterized by Equations (1.1) and (1.2).

$$Y_{ict} = \pi_{10} + \pi_{11} \cdot Z_{ict-1} + \pi_{12} \cdot \overline{Z}_{gt-1} + \pi_{13} \cdot X_{ict-1} + \mu_{ict} \qquad (1.3)$$

The coefficient of interest is π_{12}, where $\pi_{12} = \gamma / \left(1 - \delta \cdot \widetilde{\delta}\right)$. Estimates of π_{12} are free of selection, common shock, and endogeneity problems, and therefore, can provide interpretable evidence of peer effects. The reduced form estimate of π_{12} accounts for multiple channels through which a social group's average SAT score may impact an individual's GPA. For example, if Equations (1.1) and (1.2) represent the correct structural model, then

π_{12} contains a direct component of the social group's average SAT effect and an indirect component of the social group's average SAT effect that works through the average GPA. Even though untangling the two effects is not possible without additional restrictions, the reduced form specification in Equation (1.3) allows for causal estimates of the net effect of average social group SAT scores on individual GPA.

1.6 Peer and Role Model Effects

I begin by estimating a single-equation model as in Equation (1.1) to demonstrate how common shocks potentially confound interpretations of peer effects. Estimates of δ do not have a causal interpretation because this form of the model ignores endogeneity problems. However, given the design of this experiment, a non-zero estimate of δ suggests either peer effects or common shocks. Since the key right-hand side variables vary by social group, all standard errors are corrected for clustering at the Company times year level with Huber-White robust standard errors.

Table 1.3 contains estimates using the plebe-year GPA as the outcome, and the Total SAT score as the pretreatment characteristic of interest. In column (1), I regress individual GPA on own SAT score, a constant, year dummies, and the scrambling controls. Own SAT score is a positive predictor of own plebe GPA: a 100 point increase in own SAT score implies a .04 point increase in academic GPA (4 percent of a letter grade).[9] In column (2), I add the average SAT score for the peer group. The own effect is identical to that measured in column (1), while the peer effect is insignificant. In column (3), I drop the average peer SAT score and include the average peer GPA. The own SAT effect is unchanged and the effect of the average peer GPA is large, positive, and well estimated.

Column (4) contains the full specification as in Equation (1.1). The own SAT effect remains stable, there is no significant average peer SAT effect, and a one standard deviation increase in average peer GPA translates to a .03 point increase in own GPA (3 percent of a letter grade). The magnitude of the correlation between own GPA and average peer GPA is striking, especially since there are no selection concerns. Sacerdote (2001) reports correlations of similar

[9] Since the CEER score is partially determined by SAT scores, this point estimate may be low given its positive correlation with CEER. An identical regression without the CEER control, reveals a point estimate of .10 with a standard error of .006 on the own SAT effect. This gives an idea of the actual magnitude of the own effect for comparison to the magnitude of the peer effects.

size in his study. Interpreting this as evidence for contemporaneous peer effects is not entirely unreasonable because common shocks would have to play a significant role to account for such a sizeable correlation. So, how important are common shocks?

Hanushek, Kain, Markman, and Rivkin (2001) provide some evidence suggesting that common shocks could be substantial. They use a matched panel data set for children in the Texas public school system and find sizeable differences in estimates of coefficients on $\overline{Y}_{gt-2}$ when fixed effects are included at varying levels of group organization. Sacerdote (2001) also attempts to deal with the common shock problem by including dorm level fixed effects. However, in his study, the correlations between roommate and own GPA remain positive and significant. I conduct a similar exercise in column (5) by including fixed effects for Battalions and Regiments (the next two levels above a Company) and also find little evidence of common shocks in the data. Nevertheless, if common shocks are room specific in Sacerdote's study or Company specific in this study, then including fixed effects at higher levels of organization will not account for them.

The environment at West Point provides an opportunity to investigate the common shock problem further. I am able to control for a possible common shock that would otherwise remain latent. The hierarchical structure of each Company implies that attitudes and behavior of upperclassmen are also likely to affect plebes. For example, the sophomore class is directly responsible for supervising all plebes in a Company, juniors and seniors establish the Company environment, and the Cadet Company Commander (a senior) is responsible for leading the Company and may have particular influence over policies that affect plebes. Therefore, I represent a potential common shock with a vector of academic, military, and physical attributes of the upperclassmen and the Cadet Company Commander in each Company.[10]

I do not have data on upperclassmen and Company Commanders for plebes in the earlier year-groups, so column (6) contains the same specification as column (4) for data from year-groups 1995 through 1998. There are only slight changes in the point estimates with the change in sample from column (4) to column (6). Column (7) contains the specification with the vector of common shocks included. Comparing column (6) with column (7) reveals that common shocks attributed to upperclassmen reduce the contemporaneous peer effect by almost half, while

[10] This vector of characteristics contains Company average academic GPA, military GPA, and physical GPA for sophomores, juniors, seniors, and the cadet Company Commander.

not affecting the point estimate of the average peer SAT or the own SAT effect. Undoubtedly, there are countless other unobservable common shocks that could further impact the estimate of average peer GPA. Consequently, common shocks could account for most or all of the measured correlation between own and average peer GPA.

The random assignment process and the contrast between the average SAT effect and the average GPA effect provide further suggestive evidence that common shocks may be substantial in this study. The reduced variation in average pretreatment measures of peer ability that results from the scrambling process implies that common shocks to GPA may be even more important here than in other settings. Given that own SAT is a positive predictor of own GPA, average peer SAT is likely to be a positive predictor of average peer GPA. A regression of average peer GPA on average peer SAT and the full set of controls reveals a positive correlation with a point estimate of .093 and a standard error of .037.[11] The random assignment process is apt to negate any common shocks between average peer SAT and average peer GPA. Thus, the correlation found between average peer SAT and average peer GPA is likely attributable to a measure of academic ability, which is arguably a component of both SAT and GPA. The lack of an average SAT effect suggests that the academic ability component of the average peer GPA is not responsible for the positive average peer GPA effect found in Table 1.3. Therefore, some other component of the average peer GPA is probably responsible for this sizeable correlation and common shocks are a leading suspect.[12]

On balance, the results from Table 1.3 suggest that common shocks confound estimates of contemporaneous peer effects at West Point. Given the design of this experiment and the potentially sizeable common shocks, the reduced form specification in Equation (1.3) provides the most credible method of estimating social effects. For the second part of this study, I use the reduced form specification to test for social effects in peer and role model relationships. The hierarchical structure of Companies discussed in Section 1.2 implies that upperclassmen can have a considerable impact on plebes. The importance of role models at West Point is also demonstrated, to a certain extent, by the common shock exercise. Since sophomores are

[11] This estimate and standard error equal 100 times the actual estimate for comparison to the estimates in Table 1.3. Regression also includes a constant, year dummies, and the individual-level scrambling controls.
[12] In a 2SLS context, if average SAT could instrument for average GPA, there is a strong first stage but no reduced form.

assigned the duty of mentoring and supervising plebes, I use characteristics of the sophomores in each Company to estimate role model effects.

I begin this part of the analysis by testing whether or not social group assignments affect the decision to drop out of West Point prematurely. Of the approximately 1200 cadets admitted each year to the Academy, a little more than 7 percent drop out during Cadet Basic Training, about 5 percent drop out during plebe-year, and nearly 18 percent of the initial class drops out before graduation. Cadets may choose to leave the Academy prior to the start of their junior-year without incurring any active duty Army obligation. However, dropouts typically occur from discipline infractions, honor code violations, or failing to meet academic, military, and physical standards. While the outcome of this exercise is interesting in and of itself, it also has implications for the composition of social group characteristics in subsequent analysis.

Table 1.4 contains estimates from a linear probability model of the form in Equation (1.3).[13] The left-hand side variable is binary, where a one denotes a dropout. The right-hand side variable of interest is one of the two pretreatment academic ability measures used in this study (Total SAT or Math SAT). Panel A contains peer estimates and panel B contains role model estimates. Columns (1) and (2) reveal no significant effects on Cadet Basic Training dropouts for either measure of SAT score. A similar result is found in columns (3) and (4) for plebe-year dropouts. Likewise, estimates in panel B indicate that plebe-year dropouts are not driven by the average academic ability of role models. Since dropouts are not driven by social group composition, I construct measures of average social group behavior using data from all cadets who were initially assigned to the social group.[14]

Specifications in Table 1.5 test for social effects in academic performance outcomes. Estimates for plebe-year academic GPA and plebe-year Math grade are found in panels A and B respectively. I use Total SAT score to predict GPA and Math SAT scores to predict Math grades. Math comparisons provide a more concentrated measure of a specific skill. There is also much less subjectivity in assessing quantitative ability than other types of ability. Support for this argument is found by comparing the estimates in column (1) of panel A with panel B. The own Math SAT score is a stronger predictor of Math performance than the own Total SAT score is for overall GPA.

[13] Nearly identical marginal effects from corresponding Probit specifications are in Appendix Table 1.1.
[14] The other social group measures used in this study (not reported) also have no effect on dropouts. Sophomores do not interact with the plebes during Cadet Basic Training, so no estimates are reported.

Estimates in columns (2) and (3) reveal no statistically significant peer effect. The specification in column (3) is identical to the specification in column (2), except the sample does not contain data for the year-group 1992. I include this specification to compare estimates with role model specifications because data is not available for year-group 1992 role models. In column (4), I replace the average peer measures with the average role model measures. In both panels there is no statistically significant role model effect. Column (5) shows little change in the estimates when both peer and role model background characteristics are included in the same regression.[15]

The results in Table 1.5 provide little evidence for average peer or role model effects in academic performance. It is possible that the scrambling process reduces the variation in average peer pretreatment ability measures to the point where no effect is identifiable. However, insignificant effects of pretreatment measures of peer ability are a consistent result across other similar studies. Sacerdote (2001) finds no significant pretreatment peer effects for roommates at Dartmouth College and Zimmerman (2003) finds small effects for only one of the three pretreatment measures that he tests for roommates at Williams College. This suggests that any social effects for academic performance are apt to be modest. It may also be the case that choices, and not performance, are more susceptible to social influences at these undergraduate institutions.

I focus on two choices at West Point that may have important labor market consequences: the choice of academic major of study and the decision to remain in the military past an initial five-year obligation period. Undergraduate academic majors of study provide skills in specific disciplines, which affect job market prospects, income, and even graduate school opportunities. Likewise, the decision to remain in the military past the initial obligation period influences the availability of future jobs, income, and the development of human capital, particularly in the form of leadership skills. The results from this analysis are found in Table 1.6.

Columns (1) through (4) contain estimates from a linear probability model for the choice of several academic majors. The left hand-side variable is dichotomous, where a one denotes the actual choice of major as listed in the column headings.[16] The pretreatment characteristics are the proposed academic major of study as indicated on the Survey of Incoming Freshmen. It is

[15] This further confirms that peer assignments are independent of role model assignments.
[16] Nearly identical marginal effects from corresponding Probit specifications are in Appendix Table 1.2.

conceivable that peers and role models influence the choice of academic major based on preexisting intentions. Estimates in column (1) of panel A reveal that cadets who intended to study Engineering prior to coming to West Point are 38 percentage points more likely to select Engineering than cadets who did not intend to be Engineer majors. While there is no significant peer effect, the role model effect is positive and significant. A 10 percentage point increase in the fraction of role models in each Company who intended to study Engineering leads to a 1.5 percentage point increase in the probability that a plebe will choose Engineering as a major. There are no statistically significant social effects for the other majors of study tested in columns (2) through (4).

A possible explanation for the presence of role model effects, yet no peer effects is that cadets choose their academic major at the end of the sophomore-year. Accordingly, a common topic of professional development sessions between sophomores and plebes is the choice of academic major. The effect found in Engineering, but not in the other majors is possibly due to West Point's strength in Engineering. Table 1.1 shows that 44 percent of all cadets proposed Engineering as their academic major. Cadets who chose to come to West Point specifically to study Engineering may have strong prior attitudes about this program, thereby exerting a greater influence on plebes.

The final two columns in Table 1.6 address the decision to remain in the Army one year past an initial obligation period of five years. Here, the left-hand side dichotomous variable equals one, if the individual is still in the Army six years after graduation. The first pretreatment measure of interest is the Leadership Potential Score (LPS). As described in the data section, the Admissions Office assigns the LPS based on participation in leadership related activities prior to entering West Point. Since the Army develops and promotes the leadership skills of officers, the LPS is likely correlated with an individual's decision to remain in the Army.

Estimates in column (5) show that a 100 point increase in own LPS results in a 9 percentage point higher chance of remaining in the military longer than six years. While there is no statistically significant peer effect, there is a marginally significant role model effect. A 100 point increase in the average LPS of role models results in a 15 percentage point higher chance of remaining on active duty six years past graduation.

Given the implicit leadership dimensions involved in a role model relationship and the nature of the decision to remain in the military, the magnitude of the role model effect seems reasonable. An officer who experienced better leadership from the sophomore class during his plebe-year may choose to spend more time in the Army for a couple of reasons. He may wish to improve his own leadership skills, if he valued the good leadership that he experienced during his plebe-year. He may also choose to remain in a profession where he has a comparative skill advantage, if his own leadership skills improved as a result of experiencing good leadership during his plebe-year.

The second pretreatment measure of interest is the expressed intent of a cadet to make the military a career as indicated on the Survey of Incoming Freshmen. Estimates in column (6) reveal that cadets who anticipated making the military a profession prior to entering West Point are 11 percentage points more likely to remain in the military one year past their initial obligation period than cadets who did not anticipate a military career. In this case, there is a significant peer effect, but no significant role model effect. A 10 percentage point increase in the fraction of peers that anticipated a military career results in a 2.5 percentage point higher chance of remaining in the military at least six years after graduation. Attitudes towards the challenging demands of military service are likely to play an important role in the decision to remain on active duty status. These results suggest that peer attitudes toward military service may be quite influential in shaping a cadet's own attitude toward military service, particularly during plebe-year.

A falsification exercise for the role model results is found in panel B. Here I reverse roles and test whether or not plebes affect decision that sophomores make. For all outcomes, the own effects are similar in magnitude to those in panel A. However, plebes do not appear to serve as role models for the sophomore class. In general, the estimates in Table 1.6 provide evidence of social effects for choice outcomes related to two important labor market decisions.

1.7 Conclusion

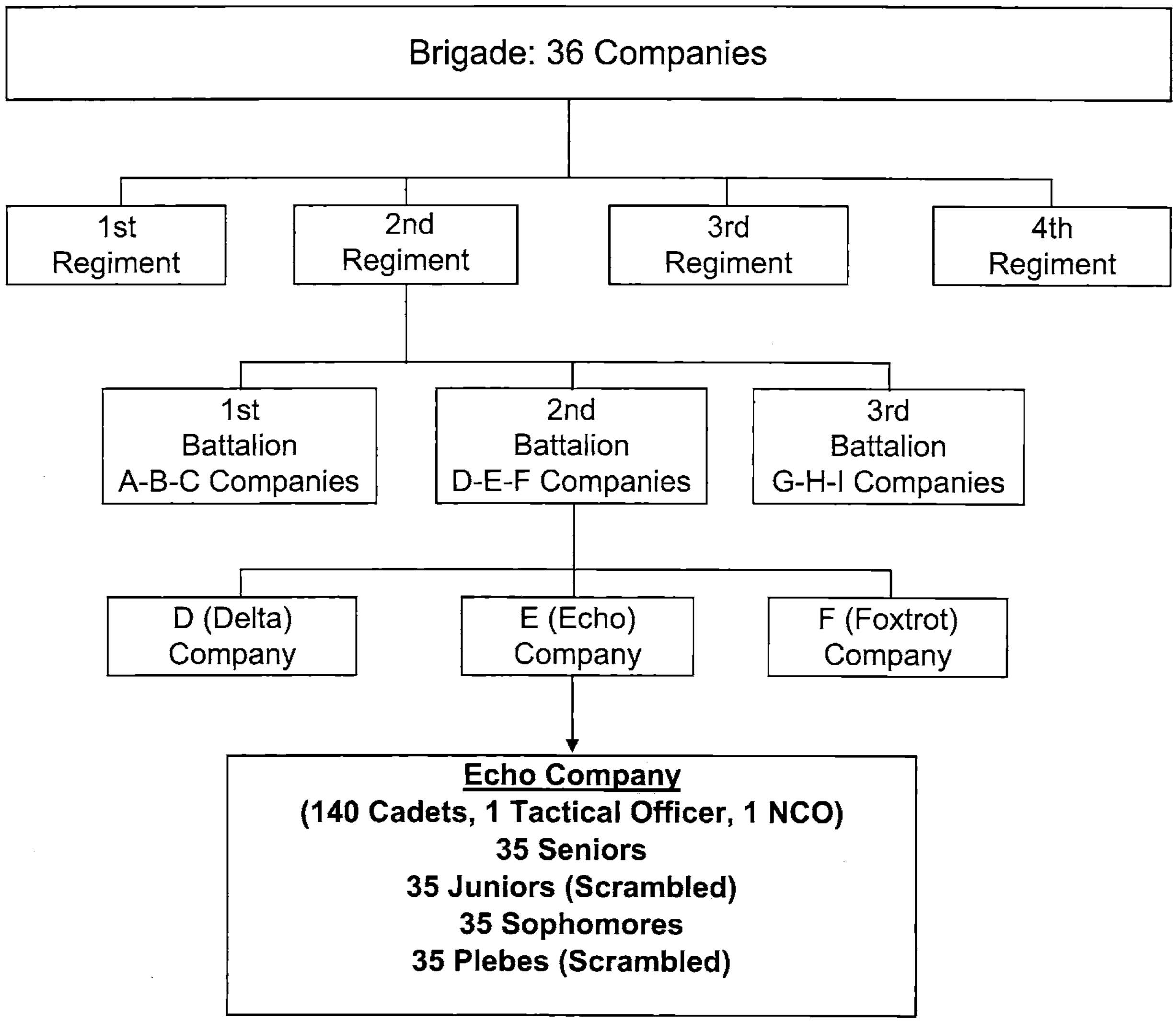
Identifying social effects is empirically challenging due to several difficult modeling problems. The current literature has focused primarily on the selection problem and has given less attention to the common shock problem. I present evidence that suggests common shocks may play a significant role in the correlations found in many studies. This study addresses the main identification concerns by exploiting the random assignment of peer and role model groups at the

United States Military Academy, relying on military institutions to clearly define social groups, and estimating reduced form specifications.

Consistent with other studies on college-level students, I find little evidence of average social group effects in academic performance. However, social groups at West Point appear to impact at least two choice outcomes that are likely to have labor market consequences. In particular, role models have a positive effect on a plebe's choice of Engineering as an academic major. Evidence also suggests that role models with higher Leadership Potential Scores and peers who anticipate making the military a career have positive effects on the decision to remain in the Army past an initial obligation period of five years.

This study highlights two important issues for subsequent attempts to identify social effects. First, future analysis of social effects in any area must consider the potential bias associated with common shocks. And second, research on social relationships other than peers and on measures of outcomes other than academic performance may provide valuable insights into other key components of the human capital production process.

THE CORPS OF CADETS



Term	Description
Company	**Made up of all four classes: 35 cadets in each class**
Cadet	**General term referring to an individual from any of the four classes**
Plebe	**Freshmen or 4th classmen**
Yearling	**Sophomore or 3rd classmen**
Cow	**Junior or 2nd classmen**
Firstie	**Senior or 1st classmen**

Figure 1.1: Organization of the United States Military Academy Corps of Cadets

Table 1.1: Company Level Summary Statistics

	Companies	Mean	Std. Dev.	Minimum	Maximum
	A. Outcome Variables				
	Companies	Mean	Std. Dev.	Minimum	Maximum
Academic GPA	252	2.66	0.11	2.31	2.93
Math Grade	252	2.69	0.23	2.10	3.18
Choose Engineer Major	252	0.407	0.101	0.120	0.643
Choose Natural Science Major	252	0.094	0.060	0.000	0.290
Choose Social Science Major	252	0.136	0.075	0.000	0.375
Choose All Other Majors	252	0.363	0.093	0.080	0.542
Continue in Army Past 6 Years	180	0.505	0.108	0.231	0.864
Left Academy During Cadet Basic Training	252	0.073	0.046	0.000	0.257
Left the Academy During Plebe Year	252	0.050	0.037	0.000	0.194
	B. Pretreatment Characteristics				
	Companies	Mean	Std. Dev.	Minimum	Maximum
Total SAT Score (Math + Verbal)	252	1189.2	16.8	1149.1	1237.3
Math SAT Score	252	636.7	10.6	599.0	661.8
Leadership Potential Score	252	603.8	7.6	578.4	621.2
Propose Engineer Major	216	0.444	0.098	0.100	0.667
Propose Natural Science Major	216	0.114	0.063	0.000	0.350
Propose Social Science Major	216	0.172	0.083	0.000	0.471
Propose All Other Majors	216	0.269	0.082	0.000	0.538
Anticipates an Army Career	216	0.360	0.096	0.115	0.630
	C. Random Scrambling Controls				
	Companies	Mean	Std. Dev.	Minimum	Maximum
Female	252	0.118	0.025	0.032	0.212
Black	252	0.065	0.030	0.000	0.167
Hispanic	252	0.043	0.026	0.000	0.143
Recruited Football Players	252	0.075	0.032	0.000	0.184
Other Recruited Athletes	252	0.141	0.043	0.000	0.314
Attended the West Point Prep School	252	0.140	0.030	0.054	0.219
College Entrance Exam Rank (CEER)	252	607.3	5.0	586.3	623.7
Whole Candidate Score (WCS)	252	6032.3	31.8	5952.2	6167.1

The data is from the Office Economic Manpower Analysis, West Point, NY. Data includes personnel, admissions, performance, and extracurricular cadet data for the graduating classes of 1992-1998. There are 36 companies across 7 years. Background information from the Survey of Incoming Freshmen is available for graduating classes of 1992, and 1994-1998. Active duty Army personnel data is only available for the classes of 1992-1996. The LPS is an aggregated score of pretreatment leadership activities. The CEER score is a weighted average of SAT, ACT, and high school rank. WCS is an aggregated score of pretreatment activities and performance. SAT scores are comparable across years because they were all taken prior to the 1995 renormalization.

Table 1.2: Randomly Assigned Peer and Role Model Groups
Outcome Variable: Social Group Mean Score (listed as column headings)

A. Peer Pretreatment Characteristic Correlations

	Total SAT		Math SAT		Proposed Engineer Major		Leadership Potential Score		Anticipates an Army Career	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total SAT	-0.011 (0.002)	0.003 (0.003)								
Math SAT			-0.010 (0.002)	0.004 (0.004)						
Proposed Engineer Major					0.000 (0.004)	0.001 (0.004)				
Leader Potential Score (LPS)							-0.015 (0.002)	0.000 (0.003)		
Anticipates an Army Career									-0.002 (0.003)	-0.001 (0.004)
R^2	0.07	0.08	0.06	0.06	0.01	0.01	0.21	0.22	0.11	0.11
Observations	8,508	8,508	7,733	7,733	5,791	5,791	8,555	8,555	6,049	6,049
Scrambling Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

B. Role Model Pretreatment Characteristic Correlations

	Total SAT		Math SAT		Proposed Engineer Major		Leadership Potential Score		Anticipates an Army Career	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total SAT	0.000 (0.001)	0.000 (0.003)								
Math SAT			0.000 (0.002)	0.001 (0.003)						
Proposed Engineer Major					0.003 (0.004)	0.002 (0.004)				
Leader Potential Score (LPS)							-0.001 (0.001)	0.001 (0.003)		
Anticipates an Army Career									-0.001 (0.004)	-0.001 (0.004)
R^2	0.08	0.08	0.05	0.05	0.00	0.00	0.10	0.10	0.06	0.06
Observations	7,220	7,220	6,584	6,584	3,524	3,524	7,265	7,265	3,638	3,638
Scrambling Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of peer means on individual-level characteristics. All specifications include year dummies and a constant. Random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS are included as indicated. Changes in sample size reflect available data for the given characteristic. See Table 1.1 notes for sample description.

Table 1.3: Interpreting Contemporaneous Peer Effects With Potential Common Shocks
Outcome Variable: Individual Level Plebe Academic GPA

	Plebe Academic GPA						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own Total SAT / 100	0.042 (0.006)	0.042 (0.006)	0.042 (0.006)	0.042 (0.006)	0.042 (0.006)	0.037 (0.008)	0.037 (0.009)
Average Peer Total SAT / 100		-0.002 (0.035)		-0.024 (0.027)	-0.018 (0.030)	-0.013 (0.033)	-0.011 (0.038)
Average Peer Academic GPA			0.234 (0.056)	0.241 (0.057)	0.206 (0.061)	0.256 (0.076)	0.140 (0.092)
CEER / 100	0.398 (0.021)	0.398 (0.021)	0.399 (0.021)	0.398 (0.021)	0.397 (0.021)	0.352 (0.030)	0.352 (0.030)
WCS / 1000	0.203 (0.028)	0.203 (0.028)	0.206 (0.028)	0.206 (0.028)	0.205 (0.028)	0.283 (0.039)	0.281 (0.039)
Female	-0.071 (0.016)	-0.071 (0.016)	-0.071 (0.016)	-0.071 (0.016)	-0.071 (0.016)	-0.064 (0.023)	-0.065 (0.023)
Black	-0.141 (0.019)	-0.141 (0.019)	-0.142 (0.019)	-0.141 (0.019)	-0.141 (0.019)	-0.115 (0.025)	-0.114 (0.026)
Hispanic	-0.046 (0.024)	-0.046 (0.024)	-0.047 (0.024)	-0.047 (0.024)	-0.047 (0.024)	-0.048 (0.033)	-0.048 (0.032)
Football	-0.031 (0.020)	-0.031 (0.020)	-0.032 (0.020)	-0.032 (0.020)	-0.033 (0.020)	-0.014 (0.030)	-0.013 (0.030)
Other Athletes	-0.009 (0.015)	-0.009 (0.015)	-0.010 (0.015)	-0.010 (0.015)	-0.010 (0.015)	0.013 (0.020)	0.015 (0.020)
Attended the West Point Prep School	-0.036 (0.014)	-0.036 (0.014)	-0.038 (0.014)	-0.038 (0.014)	-0.037 (0.014)	-0.037 (0.019)	-0.035 (0.019)
R^2	0.43	0.43	0.43	0.43	0.43	0.40	0.41
Observations	7,527	7,527	7,527	7,527	7,527	4,048	4,048
Battalion and Regiment Controls	No	No	No	No	Yes	No	No
Average Co. Cdr. & Upperclassmen Controls (Shocks)	No	No	No	No	No	No	Yes

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of individual level academic GPA on individual and peer average Total SAT and peer average GPA. All specifications include year dummies and a constant. Sample size changes in columns (6) and (7) are a result of unavailable data for the Company Commander and upperclassmen for year-groups 1992-1994. See Table 1.1 notes for sample description.

Table 1.4: Reduced Form Peer and Role Model Effects for Drop Outs
Outcome Variable: Left Academy = 1 & Remain at Academy = 0

	A. Peer Effects				B. Role Model Effects	
	Left Academy During Cadet Basic Training		Left Academy During Plebe Year		Left Academy During Plebe Year	
	(1)	(2)	(3)	(4)	(1)	(2)
Average Social Group Total SAT / 100	0.006 (0.017)		0.013 (0.014)		0.016 (0.014)	
Average Social Group Math SAT / 100		0.020 (0.023)		-0.035 (0.024)		0.012 (0.025)
CEER / 100	0.012 (0.010)	0.024 (0.010)	0.002 (0.010)	0.009 (0.010)	-0.002 (0.011)	-0.002 (0.011)
WCS / 1000	-0.039 (0.016)	-0.059 (0.016)	-0.050 (0.015)	-0.051 (0.016)	-0.042 (0.016)	-0.042 (0.016)
Female	0.017 (0.010)	0.016 (0.010)	-0.006 (0.008)	-0.005 (0.008)	-0.008 (0.008)	-0.008 (0.008)
Black	-0.020 (0.010)	-0.016 (0.011)	-0.006 (0.011)	-0.006 (0.011)	-0.007 (0.012)	-0.007 (0.012)
Hispanic	0.006 (0.015)	0.011 (0.015)	-0.020 (0.010)	-0.012 (0.011)	-0.023 (0.010)	-0.023 (0.010)
Football	0.011 (0.011)	-0.001 (0.011)	0.015 (0.011)	0.018 (0.011)	0.010 (0.012)	0.010 (0.012)
Other Athlete	0.002 (0.009)	0.005 (0.010)	0.011 (0.009)	0.014 (0.009)	0.008 (0.009)	0.008 (0.009)
Attended the West Point Prep School	-0.046 (0.007)	-0.041 (0.007)	-0.026 (0.007)	-0.021 (0.007)	-0.024 (0.007)	-0.024 (0.007)
R^2	0.01	0.01	0.01	0.01	0.01	0.01
Observations	8,691	7,870	8,020	7,314	6,879	6,879

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect linear probability regressions of individual choice to leave the academy (dropout = 1) on peer and role model average SAT scores. All specifications include year dummies, a constant, and the own effect. Sophomores do not interact with plebes during Cadet Basic Training, so no estimates are presented. Nearly identical marginal estimates using a Probit specification are found in Appendix Table 1.1. See Table 1.1 notes for sample description.

Table 1.5: Reduced Form Peer and Role Model Effects for Academic Outcomes
Outcome Variable: Individual Level Score (listed as panel headings)

	A. Plebe Academic GPA				
	(1)	(2)	(3)	(4)	(5)
Own Total SAT / 100	0.042 (0.006)	0.042 (0.006)	0.036 (0.007)	0.036 (0.007)	0.036 (0.007)
Average Peer Total SAT / 100		-0.002 (0.035)	-0.002 (0.041)		-0.002 (0.041)
Average Role Model Total SAT / 100				-0.023 (0.036)	-0.023 (0.036)
R^2	0.43	0.43	0.41	0.41	0.41
Observations	7,527	7,527	6,417	6,417	6,417
	B. Plebe Math Grade				
	(1)	(2)	(3)	(4)	(5)
Own Math SAT / 100	0.191 (0.019)	0.191 (0.019)	0.167 (0.020)	0.167 (0.020)	0.167 (0.020)
Average Peer Math SAT / 100		-0.029 (0.088)	-0.019 (0.098)		-0.019 (0.098)
Average Role Model Math SAT / 100				-0.071 (0.081)	-0.070 (0.082)
R^2	0.29	0.29	0.28	0.28	0.28
Observations	6,309	6,309	5,447	5,447	5,447

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of individual level academic outcomes as indicated in panel headings on individual and social group average SAT scores. All specifications include year dummies, a constant, and random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. Sample restricted to 1993-1998 in columns (3) - (5) because role model measures are not available for year-group 1992. See Table 1.1 notes for sample description.

Table 1.6: Reduced Form Peer and Role Model Effects for Academic Major and Military Service Choices
Outcome Variable: Individual Level Choice (listed as column headings)

	A. Sophomores as Role Models for Plebes and Plebe Peer Effects					
	Engineer Major	Natural Sciences	Social Sciences	All Other Majors	In Army after 6 years	In Army after 6 years
	(1)	(2)	(3)	(4)	(5)	(6)
Own Effect	0.382 (0.016)	0.279 (0.026)	0.213 (0.020)	0.173 (0.023)	0.091 (0.032)	0.113 (0.028)
Average Peer Effect	-0.064 (0.080)	0.048 (0.078)	-0.012 (0.065)	0.062 (0.098)	0.034 (0.122)	0.246 (0.136)
Average Role Model Effect	0.148 (0.073)	0.048 (0.079)	0.060 (0.058)	-0.085 (0.094)	0.156 (0.098)	0.021 (0.135)
R^2	0.17	0.12	0.07	0.08	0.02	0.04
Observations	3,068	3,068	3,068	3,068	3,912	1,286
Pretreatment Characteristic	Proposed Major	Proposed Major	Proposed Major	Proposed Major	Leader Potential Score / 100	Anticipates an Army Career

	B. Falsification: Plebes as Role Models for Sophomores and Sophomore Peer Effects					
	Engineer Major	Natural Sciences	Social Sciences	All Other Majors	In Army after 6 years	In Army after 6 years
	(1)	(2)	(3)	(4)	(5)	(6)
Own Effect	0.403 (0.015)	0.317 (0.025)	0.211 (0.020)	0.186 (0.021)	0.110 (0.029)	0.091 (0.021)
Average Peer Effect	-0.080 (0.084)	0.015 (0.079)	-0.058 (0.060)	-0.142 (0.092)	0.045 (0.083)	0.113 (0.114)
Average Role Model Effect	-0.027 (0.068)	-0.094 (0.070)	-0.078 (0.062)	-0.024 (0.086)	-0.091 (0.107)	-0.113 (0.104)
R^2	0.18	0.15	0.07	0.09	0.02	0.04
Observations	3,161	3,161	3,161	3,161	4,845	2,252
Pretreatment Characteristic	Proposed Major	Proposed Major	Proposed Major	Proposed Major	Leader Potential Score / 100	Anticipates an Army Career

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect linear probability regressions of individual choice as indicated in column heading (choice=1) on corresponding peer and role model average characteristics (listed at the bottom of each column). All specifications include year dummies, a constant, and random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. Sample restricted to year-groups 1995-1998 for all specifications because role model measures are not available for year-groups 1992 and 1994 and proposed major data is not available for year-group 1993 for specifications in columns (1) - (4). Samples for specifications in column (5) are restricted to year-groups 1992-1996 because 6 years past graduation has not transpired for subsequent year-groups at the time of this study. Samples for specifications in column (6) are restricted to year-groups 1995 and 1996 for a combination of aforementioned reasons. In panel B, specifications are identical as in panel A, except plebes and role models are reversed. Nearly identical marginal estimates using a Probit specification are found in Appendix Table 1.2. See Table 1.1 notes for sample description.

Appendix Table 1.1: Reduced Form Probit Peer and Role Model Effects for Drop Outs
Outcome Variable: Left Academy = 1 & Remain at Academy = 0

	A. Peer Effects				B. Role Model Effects	
	Left During Cadet Basic Training		Left During Plebe Year		Left During Plebe Year	
	(1)	(2)	(3)	(4)	(1)	(2)
Social Group Total SAT / 100	0.006 (0.017)		0.013 (0.014)		0.015 (0.013)	
Social Group Math SAT / 100		0.023 (0.023)		-0.033 (0.023)		0.014 (0.023)
Observations	8,691	7,870	8,020	7,314	6,879	6,879

Standard errors in parenthesis account for clustering at the Company and year level. Specifications are identical to Table 1.4, except using a Probit specification instead of a linear probability model. All estimates reported are for the marginal changes and are comparable to Table 1.4. See Table 1.4 notes for specification description and Table 1.1 notes for sample description.

Appendix Table 1.2: Reduced Form Probit Peer and Role Model Effects for Academic Major and Military Service Choices
Outcome Variable: Individual Level Choice (listed as column headings)

	A. Sophomores as Role Models for Plebes and Plebe Peer Effects					
	Engineer Major	Natural Sciences	Social Sciences	All Other Majors	In Army after 6 years	In Army after 6 years
	(1)	(2)	(3)	(4)	(5)	(6)
Own Effect	0.386 (0.016)	0.275 (0.026)	0.215 (0.020)	0.176 (0.024)	0.092 (0.033)	0.115 (0.029)
Average Peer Effect	-0.070 (0.094)	0.034 (0.069)	-0.016 (0.068)	0.066 (0.103)	0.034 (0.124)	0.254 (0.141)
Average Role Model Effect	0.165 (0.084)	0.053 (0.072)	0.061 (0.061)	-0.098 (0.099)	0.156 (0.100)	0.023 (0.139)
Observations	3,068	3,068	3,068	3,068	3,912	1,286
Pretreatment Characteristic	Proposed Major	Proposed Major	Proposed Major	Proposed Major	Leader Potential Score / 100	Anticipates an Army Career
	B. Falsification: Plebes as Role Models for Sophomores and Sophomore Peer Effects					
	Engineer Major	Natural Sciences	Social Sciences	All Other Majors	In Army after 6 years	In Army after 6 years
	(1)	(2)	(3)	(4)	(5)	(6)
Own Effect	0.407 (0.015)	0.308 (0.025)	0.210 (0.019)	0.189 (0.022)	0.112 (0.029)	0.093 (0.021)
Average Peer Effect	-0.091 (0.098)	0.001 (0.073)	-0.068 (0.062)	-0.149 (0.098)	0.046 (0.085)	0.117 (0.118)
Average Role Model Effect	-0.035 (0.081)	-0.085 (0.067)	-0.076 (0.063)	-0.024 (0.094)	-0.091 (0.108)	-0.116 (0.107)
Observations	3,161	3,161	3,161	3,161	4,845	2,252
Pretreatment Characteristic	Proposed Major	Proposed Major	Proposed Major	Proposed Major	Leader Potential Score / 100	Anticipates an Army Career

Standard errors in parenthesis account for clustering at the company and year level. Specifications are identical to Table 6, except using a Probit specification instead of a linear probabiliy model. All estimates reported are for the partial changes and are comparable to Table 1.6. See Table 1.6 notes for specification description and Table 1.1 notes for sample description.

Chapter 2: Are Peers Complements or Substitutes in the Production of Human Capital at the United States Military Academy?

"An important part of a child's school environment consists not of the physical facilities of the school, the curriculum, and the teachers, but of his fellow-students. A child's fellow-students provide challenges to achievement and distractions from achievement; they provide the opportunities to learn outside the classroom through association and casual discussion." - James Coleman, Equality of Educational Opportunity, 1966.

2.1 Introduction

Human capital acquisition typically occurs in group-settings like classrooms, social organizations, athletic teams, or gangs. Understanding how peers contribute or detract from learning has drawn significant attention from educators, parents, sociologists, and economists alike. One unresolved issue that has first-order economic implications for the organization of educational institutions is whether peers are complements or substitutes in the production of human capital.

Becker (1973) shows that if inputs from two parties are complements, then positive assortive matching is the optimal allocation.[17] When heterogeneity is introduced, the gain that a low ability student receives from high ability students is more than offset by the loss that the high ability students experience due to the low ability student. Therefore, it is more efficient to segregate the students by ability. Hoxby (1996) and Benabou (1996b) refer to this as the "one bad apple" scenario. On the other hand, if inputs from two parties are substitutes, then negative assortive matching is the optimal allocation. In this case, the loss that the "one good apple" experiences from the other students is less than the gain that the other students receive from the one good student. Here, it is more efficient to mix students by ability.

To date, there is little empirical evidence indicating whether peers are complements or substitutes in education production. The fact that we observe students of higher ability attending Ivy League Colleges and students of lower ability attending Community Colleges is consistent with peers serving as complements. However, there are at least two other reasons why we may observe segregation in this setting. First, credit market imperfections may prevent lower ability

[17] Becker (1973a), in his model of marriage, contains formal proofs in Appendix section 1.

students, who tend to come from disadvantaged socio-economic backgrounds, from obtaining a more expensive Ivy League education. Second, colleges may impose screening criteria to promote prestige, develop networks, or improve the quality of the labor market signal for their graduates.

The lack of empirical evidence on this subject is mainly a result of several challenging econometric problems that confront the identification of peer effects. These issues are discussed in Evans, Oats, Schwab (1992), Manski (1993), Hoxby (1996), Acemoglu and Angrist (2000), and Sacerdote (2001). I summarize the main problems as selection, endogeneity, and common shocks.

The selection problem arises because individuals typically choose their peer group. This makes it difficult to determine the appropriate members of the peer group and also to separate the true peer effect from the selection effect. Endogeneity problems are present in contemporaneous peer effects because an individual and the members of his peer group impact each other concurrently. Common shocks are common occurrences that affect the outcomes of all members of the social group, like a teacher in the classroom. In practice, there are few instances where it is possible to adequately deal with all of the identification concerns and there are even fewer instances where it is possible to address the question of peers as complements and substitutes in education production.

However, the random assignment of cadets to Companies at the United States Military Academy provides such a rare opportunity. Clearly definable peer groups, random assignment, and reliable measures of pretreatment ability combine to mitigate the selection, endogeneity, and common shock concerns. The Academy assigns incoming cadets to one of thirty-six Companies. Companies serve as the clearly definable peer group and are composed of seniors, juniors, sophomores, and freshmen. The focus of this study is on freshmen. I use West Point terminology and refer to freshmen as *plebes*. Thus, the term *peer* refers to the approximately 35 other plebes assigned to the same Company.

Plebes are randomly assigned to Companies, conditional on several observable characteristics: gender, race, recruited athlete, and measures of prior performance and behavior. West Point attempts to equalize Company means across these characteristics, which reduces the variation in peer group means. However, other moments of the peer group distribution, measures of dispersion for example, vary considerably. Benabou (1996b) presents a general education production framework, which demonstrates that measures of dispersion in peer group distributions can provide evidence for the degree of substitutability between peers.

Estimates of several measures of dispersion in the distributions of Math SAT scores reveal that more heterogeneous peer groups have positive effects on individual grades. For example, a one-standard deviation increase in the peer group 75-25 differential in Math SAT scores increases the Company average Math Grade by 13 percent of a standard deviation. This effect is relatively uniform across the distribution of academic outcomes. Further evidence suggests that the 75th percentile, but not the 25th percentile, in peer Math SAT distributions accounts for most of the effect. A positive effect of greater heterogeneity suggests that peers are substitutes, and therefore, mixing cadets by ability is the optimal allocation for the efficient production of human capital at West Point.

In the next section, I discuss the education production model and its implications for the degree of substitutability between peers. Section 2.3 provides background information on the United States Military Academy. Section 2.4 describes the data and Section 2.5 explains the random assignment of cadets to Companies. In Section 2.6, I present the empirical model and formally discuss the identification assumptions and interpretations. Section 2.7 contains the main results and Section 2.8 concludes.

2.2 Peers as Complements or Substitutes in Education Production

Benabou (1996b) discusses an educational production model where peer influences are represented by a distribution of peer ability.[18] The degree of substitutability between peers determines how heterogeneity in peer group composition affects individual outcomes. This provides a tractable way of linking measures of dispersion in peer group distributions to the degree of substitutability between peers.

To more formally investigate these issues, I present a specific case of this model for educational attainment at the United States Military Academy. Consider the following education production function for plebe-year Math grades and plebe-year academic grade point averages (GPA):

$$g_i = q_i^{\alpha} \cdot \overline{q}_i^{\omega} \cdot \left[\frac{1}{N} \sum_{j=1}^{N} q_j^{\frac{\sigma - 1}{\sigma}} \right]^{\frac{\sigma \cdot \beta}{\sigma - 1}} \qquad (2.1)$$

[18] See Equations (11) - (14) in Benabou (1996b), page 589.

Plebe (i) is assigned to a Company with N other plebes (j). The N other plebes form the corresponding peer group (p). Plebes in a Company study together and receive a Math grade and a GPA at the end of the plebe-year, denoted by g_i. Each plebe has academic ability (q_i) obtained prior to entering West Point. Own ability (q_i), average ability of an individual's peer group ($\overline{q}_i$), and individual-level peer ability (q_j) are inputs in the production of grades with corresponding weights α, ω, and β. α is greater than zero, so that grades are increasing in own academic ability and σ is a measure of the degree of substitutability between peers.

The sign on the cross partial derivative of the third term (in brackets) in Equation (2.1) determines whether peers are complements or substitutes in this model.[19] If the cross partial derivative is positive, then peers are complements and if the cross partial derivative is negative, then peers are substitutes. In this case, the sign on $\frac{1}{\sigma}$ establishes the sign on the cross partial derivative. Therefore, peers are complements when $\frac{1}{\sigma} > 0$ and peers are substitutes when $\frac{1}{\sigma} < 0$. Benabou (1996b) defines $\frac{1}{\sigma}$ as the "costs of heterogeneity."[20]

The natural logarithm form of the model in Equation (2.1) is similar to what is found in the literature on peer effects.

$$Ln(g_i) = \alpha \cdot Ln(q_i) + \omega \cdot Ln(\overline{q}_i) + \frac{\sigma \cdot \beta}{\sigma - 1} \cdot Ln\left[\frac{1}{N}\sum_{j=1}^{N} q_j^{\frac{\sigma-1}{\sigma}}\right] \quad (2.2)$$

Most peer effect specifications simply regress an individual academic outcome, $Ln(g_i)$, on a measure of individual ability, $Ln(q_i)$, and a measure of average peer ability, $Ln(\overline{q}_i)$. In addition to this standard approach, the final term in Equation (2.2) also accounts for the full distribution of peer ability. Estimating models of the form in Equation (2.1) or Equation (2.2) can provide evidence for the sign on the measure of substitutability $\frac{1}{\sigma}$.[21] Expanding each q_j in Equation (2.2) around the mean of plebe i's peer group $\overline{q}_i$, a second-order Taylor expansion results in the following linear model.

$$Ln(g_i) = \alpha \cdot Ln(q_i) + (\beta + \omega) \cdot Ln(\overline{q}_i) - \frac{1}{2} \cdot \beta \cdot \frac{1}{\sigma} \cdot \left[\frac{Var(q_p)}{\overline{q}_i^{\,2}}\right] + R \quad (2.3)$$

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[19] See Acemoglu (2002).

[20] See Benabou (1996b) page 590.

[21] With additional assumptions, non-linear estimation techniques could provide consistent estimates of $\frac{1}{\sigma}$ in Equations (2.1) or (2.2).

The linear model in Equation (2.3) captures the effect of own ability, average peer ability, and peer group heterogeneity as measured by the square of the coefficient of variation (variance of the peer group ability metric divided by the square of the mean).[22] The sign of both the peer group heterogeneity effect and β determine the sign on the measure of substitutability or the cost of heterogeneity ($\frac{1}{\sigma}$). β is one of two components of the average peer effect ($\beta+\omega$). Any effect of $\beta+\omega$ is likely positive because higher average ability peers are apt to be more beneficial than lower average ability peers. Assuming a positive β, a negative sign on the heterogeneity effect implies $\frac{1}{\sigma}$ is positive and a positive sign on the heterogeneity effect implies $\frac{1}{\sigma}$ is negative.

In practice, estimating linear models in the form of Equation (2.3) requires an educational environment where individuals have a known distribution of peers and where the selection, endogeneity, and common shock identification problems can be addressed. The production of education at the United States Military Academy provides one such setting that satisfies these conditions.

2.3 The United States Military Academy

The United States Military Academy is one of three service academies fully funded by the U.S. Government for the expressed purpose of "providing the Nation with leaders of character who serve the common defense."[23] Cadets offered admission to the Academy receive a fully funded four-year scholarship. Graduates earn an accredited Bachelor of Science degree and must fulfill a five-year active duty service obligation as an officer in the U.S. Army.

The Corps of Cadets at West Point is organized into one Brigade consisting of thirty-six Companies, as seen in Figure 2.1. The Brigade is divided into four Regiments, each Regiment is divided into three Battalions, and each Battalion is further divided into three Companies. Every Company is directed by a Tactical Officer and a Non-Commissioned Officer (NCO) from the U.S. Army and has approximately 140 cadets, 35 from each of the four classes. West Point assigns cadets to a Company during the summer prior to the start of plebe-year. Cadets maintain

[22] See Appendix A for Taylor expansion calculations. *R* represents higher order terms of the approximation and is assumed to be inconsequential.
[23] Bugle Notes (1990-1994) page 4.

the same initially assigned Company through the end of the sophomore-year, when they are reassigned to a different Company for the remaining two years.

Companies serve as the dominant social organization. Cadets within the same Company eat together, study together, attend mandatory social activities together, perform their military duties together, and live in a section of the barracks together. The hierarchical structure of a Company at West Point is similar to a Company in an active duty Army Unit and is designed to provide leadership training to upperclassmen and to promote teamwork among plebes. In general, seniors fill the role of officers, juniors fill the role of NCOs, sophomores fill the role of small unit supervisors, and plebes fill the role of privates. Plebes perform many routine duties under close scrutiny of the upperclassmen such as delivering items (newspapers, mail, and laundry), notifying all upperclassmen of formations and important appointments, keeping the Company area in immaculate condition, serving meals to upperclassmen, and memorizing institutional knowledge. The nature of the duties assigned to plebes and the organization of a Company forces plebes to cooperate in order to accomplish their many requirements.

All cadets take the same courses throughout the first two years at West Point. Calculus, English, History, Computer Science, Behavioral Psychology, and Chemistry comprise the academic Grade Point Average (GPA) during plebe-year. Plebes do not necessarily take classes with other plebes from their Company, however all plebes receive the same program of instruction, complete the same homework assignments, and take the same exams. Since the Company is the dominant organization, nearly all homework assignments and exam preparations are conducted between plebes within the same Company.

2.4 Data Description

The data for this study is from the Office of Economic Manpower Analysis (OEMA), West Point, NY. It combines Admissions files and Personnel records for cadets in the graduating classes of 1992-1998 and includes approximately 7000 plebes across 252 Companies (36 Companies each year). I organize the data into three categories: academic attainment at West Point, pretreatment measures of academic ability, and randomization controls. Table 2.1 contains summary statistics and is divided into panels by these three categories.

In panel A, I present Company-level and individual-level summary statistics for the two outcome variables used in this study, plebe-year Math Grade and academic GPA. All grades are assigned along a scale ranging from 0 to 4.3 points: a 4.3 equates to an A+, a 4.0 equates to an A, and a 3.7 equates to an A-. Plebe Math grades have a mean of 2.69 points (C+) and a standard deviation of .23 points across Companies. The plebe-year academic GPA is has a similar mean of 2.66 points (C+), but a smaller standard deviation of .11 points across Companies.

Panel B contains summary statistics for measures of the Company-level Math SAT distribution. The Math SAT scores represents a measure of quantitative ability obtained prior to entering West Point and corresponds naturally to Math grades and skills measured by the overall plebe-year GPA. The average Math SAT score is approximately 640 points with a standard deviation of about 10 points. I also provide summary statistics for three measures of dispersion in the Company Math SAT distributions: the square of the coefficient of variation, the variance, and the 75-25 differential. Despite comparatively equal Company-level average SAT scores, some Companies have a high concentration of cadets in the tails and other Companies have a high concentration of cadets in the middle of the Math SAT distributions. The sizeable variation in measures of dispersion relative to the minimal variation in means across Companies is an important feature of this data. I discuss this issue further in subsequent sections.

Company-level statistics for the randomization controls used for assigning cadets to Companies are in panel C. Approximately 12 percent of the Corps of Cadets is made up of females. Blacks and Hispanics combine to account for about 10 percent of each class. A little more than 21 percent of incoming cadets are recruited for one of the 20 NCAA Division-One athletic programs at the Academy. Also, 14 percent attended the United State Military Academy Prep School the year before entering West Point. The College Entrance Exam Rank (CEER) is a weighted average between the high school graduation ranking of the cadet and the SAT/ACT scores. The range of this ranking is from 0-800 points, with a mean of approximately 600 points. The Whole Candidate Score (WCS) aggregates assigned values to various high school activities and performance outcomes. For example, playing varsity high school basketball may contribute 250 points to the WCS and being a member of a high school student council may result in 500 more points. The WCS ranges from 0-8000 points with a mean of about 6000 points.

2.5 Social Groups and Random Assignment

The critical identification assumption for this experiment is that the assignment of cadets to Companies at West Point is random, conditional on the individual level controls listed in panel C of Table 2.1. The following description of the assignment process and some brief empirical analysis supports this assumption.

West Point uses a computer program to assign a random number to each incoming plebe and to each of the thirty-six Companies in a process known at the Academy as *scrambling*.[24] The goal of scrambling is to produce Companies with comparable average characteristics. Incoming plebes are initially assigned to one of the Companies based on their random number. The computer program then shuffles plebes between Companies in an attempt to equalize the means of the eight characteristics. All subsequent rearrangements of plebes are a function of the scrambling controls and the random number.

Estimates in Table 2.2 support this description of the assignment process. I regress peer average Math SAT scores on corresponding individual level Math SAT scores to determine if a plebe's background predicts the background of his peer group. The peer average Math SAT score is the average Math SAT score of the plebes in a Company minus the individual plebe. Estimates in column (1) are from a bivariate regression of average peer Math SAT score on individual Math SAT score. There is a small negative correlation as would be expected, given the equalizing intent of the scrambling process.[25] When I include the scrambling controls in column (2), the point estimate is smaller in absolute value and no longer statistically significant. Therefore, individual-level controls for the eight scrambling variables are included in all specifications to account for the conditional randomization process.

2.6 Empirical Framework

The structural model in Equation (2.3) indicates that individual academic outcomes are a function of own ability, average peer ability, and the square of the coefficient of variation in peer ability. Data that represents these variables are available for plebes at West Point. Assuming a positive mean effect, estimates for the sign on the cost of heterogeneity measure ($\frac{1}{\sigma}$) can indicate whether peers are substitutes or complements in education production. To determine the sign of

[24] USMA publication 98-007, "Evaluation of Scrambling in the Corps of Cadets 1962-1998." Discussions with managers in charge of scrambling from the Institutional Research and Analysis, Office of Policy Planning & Analysis, West Point NY, also confirm this description of the process.

[25] Since the peer average Math SAT is constructed without the own Math SAT score, plebes with higher SAT scores will likely be assigned to Companies with lower average peer Math SAT scores.

$\frac{1}{\sigma}$ in this setting, I estimate variations of the model in Equation (2.3) that have the following structure:

$$Y_{ict} = \kappa + \theta_t + \lambda \cdot Z_{ict-1} + \delta \cdot \overline{Z}_{pt-1} + \gamma \cdot \widehat{Z}_{pt-1} + \tau \cdot X_{ict-1} + \varepsilon_{ict} \quad (2.4)$$

The left-hand side variable Y_{ict} is the academic outcome of interest (Math grade or GPA) for plebe *i*, in Company *c*, in year *t* (plebe-year). On the right-hand side of the equation, κ is a constant, θ_t are year dummies for 1993-1998, and λ denotes the effect of own pretreatment (*t*-*1*) measures of academic ability (Math SAT). δ represents the effect of average Math SAT score for the peer group (*p*), where $\overline{Z}_{pt-1}$ contains the average peer Math SAT score. γ represents the effect of varying measures of the peer group's distribution of Math SAT scores, where $\widehat{Z}_{pt-1}$ contains either the square of the coefficient of variation, the variance, the 75-25 differential, or the 75th and 25th percentiles of the peer group distribution.[26] τ denotes the individual level scrambling controls contained in X_{ict-1} and ε_{ict} denotes other potential determinants of individual-level academic attainment.

Estimates of δ correspond to $\beta + \omega$ and estimates of γ correspond to $-\frac{1}{2} \cdot \beta \cdot \frac{1}{\sigma}$ from Equation (2.3). Specifying the mean effect (δ) with two components ($\beta + \omega$) provides additional flexibility to this model. Without ω, a mean effect of zero would imply that the heterogeneity effect is also zero. However in practice, it is possible that there is no effect of the mean, yet there is an effect of peer group heterogeneity. This is particularly important in this experiment because the random assignment process attempts to equalize peer group means. The benefit of relatively constant peer group means is that it allows for a clear comparison of peer group heterogeneity. The downside of this feature of the data is that it limits the ability of this experiment to identify average peer group effects. Therefore, I make the assumption that β is the positive component of the mean effect and ω offsets β to the true mean effect. With this assumption, I cannot estimate $\frac{1}{\sigma}$ precisely, but I can determine the sign of $\frac{1}{\sigma}$.[27]

In most settings, estimates for coefficients of interest δ and γ would be biased by selection, common shocks, and endogeneity due to correlations between $\overline{Z}_{pt-1}$ and ε_{ict} and

[26] Note *p* equals all plebes in Company (*c*) minus plebe (*i*).

[27] This assumption primarily affects the interpretation, and allows for the overall mean effect to have either sign.

between $\widehat{Z}_{pt-1}$ and ε_{ict}. However, the random assignment of cadets to Companies and the use of only pretreatment characteristics for all right-hand side variables imply that $E\left[\overline{Z}_{pt-1}\varepsilon_{ict}\right] = E\left[\widehat{Z}_{pt-1}\varepsilon_{ict}\right] = 0$. Random assignment is likely to negate the selection component of ε_{ict} and it is unlikely that any shocks to pretreatment characteristics are common to members of the newly assigned social group. Finally, using only pretreatment measures of academic ability also deals with the endogeneity problem by exploiting the timing structure on the peer effect of interest: Y_{ict} cannot influence either $\overline{Z}_{pt-1}$ or $\widehat{Z}_{pt-1}$.

In general, the design of this experiment addresses the main obstacles confronting the identification of peer effects. Therefore, Ordinary Least Squares (OLS) estimates of δ and γ from models of the form in Equation (2.4) are likely to have a causal interpretation. Since the key right-hand side variables, $\overline{Z}_{pt-1}$ and $\widehat{Z}_{pt-1}$, vary by peer group, all standard errors are clustered at the Company times year level using Huber-White robust standard errors.

2.7 Heterogeneity in Peer Groups and Distributional Effects

2.7.1 Heterogeneity in Peer Groups

I begin by estimating the empirical model in Equation (2.4) using the structural form of Equation (2.3). In this case, Y_{ict} is the natural logarithm of the Math Grade or GPA, Z_{it-1} is the natural logarithm of the individual Math SAT score, $\overline{Z}_{pt-1}$ is the natural logarithm of the mean peer Math SAT score, and $\widehat{Z}_{pt-1}$ is the square of the coefficient of variation in peer group Math SAT distributions. These estimates are found in Table 2.3.

Structural estimates of γ have a positive and significant effect of 3.97 and 1.29 log points for the Math grade and the GPA, respectively. This implies that greater heterogeneity, as measured by the square of the coefficient of variation, improves academic scores. An increase of one standard deviation in the square of the coefficient of variation in peer Math SAT scores positively affects the Company average natural log of Math grades by 10.8 percent and the Company average natural log of GPA by 8.2 percent of a standard deviation.[28] Assuming $\beta > 0$,

[28] The Company-level standard deviation is .11 for the natural log of Math grades and .047 for the natural log of GPA.

the cost of heterogeneity measure ($\frac{1}{\sigma}$) is negative for both Math grades and GPA. A negative sign on the cost of heterogeneity measure implies that peers are substitutes in the production of human capital. According to Becker's assignment model, this implies that mixing is the optimal allocation of individuals for the efficient production of education at West Point.

One concern with a structural interpretation of these estimates is that they are subject to bias if the functional form of the model is incorrectly specified. Thus, for the remainder of this analysis, I estimate a somewhat more flexible version of the empirical model in Equation (2.4) and provide a reduced form interpretation of these estimates. I define Y_{ict} as the Math Grade or GPA, Z_{it-1} as a cubic in own Math SAT score, $\overline{Z}_{pt-1}$ as the mean peer Math SAT score, and $\widehat{Z}_{pt-1}$ as varying measures of dispersion in the peer group Math SAT distribution. In the context of the theoretical model, a reduced form interpretation of these estimates can still indicate the sign of $\frac{1}{\sigma}$.

It is useful to begin this portion of the analysis by considering estimates of δ separately. Table 2.4 contains estimates from a specification of Equation (2.4), where $\widehat{Z}_{pt-1}$ is omitted. Omitting $\widehat{Z}_{pt-1}$ should not bias estimates of δ because the mechanics of the random scrambling process imply that they are uncorrelated.[29] Panel A shows that average peer group Math SAT scores have no statistically significant effect on plebe Math grades or plebe GPA. In panel B, I interact the average peer group Math SAT score with dummy variables that indicate whether a plebe belongs to the top 25 percent, middle 50 percent, or bottom 25 percent of the Math SAT distribution in each Company. These estimates reveal a relatively uniform and insignificant effect of the mean across the distribution of Math SAT scores.

The zero mean effect can be interpreted several ways. It could simply be that the average Math SAT score for a peer group has no effect. It could also be that average peer Math SAT score is not a good predictor of academic performance. In this particular experiment, it could also be that peer group means do not have enough variation to identify an effect due to the equalizing intent of the random scrambling process. In any case, the small and insignificant

[29] Comparisons of average Math SAT estimates from specifications without measures of dispersion in Table 2.4 to estimates from specifications with measures of dispersion in Table 2.5 confirm this claim.

mean effect is consistent with findings in other similar studies on peer effects at the undergraduate level.

Sacerdote (2001) finds no statistically significant effects for roommates at Dartmouth College and Zimmerman (2003) finds small, positive, and significant effects in only one of three measures of academic ability that he tests for roommates at Williams College. To date, the existing empirical evidence suggests that any average peer effects attributed to prior academic ability at the college level are apt to be quite modest. Nevertheless, the one finding in the Zimmerman (2003) study supports the assumption that any average peer effects are likely to be positive.

Using the full specification in Equation (2.4), the first measure of dispersion that I test in $\widehat{Z}_{pt-1}$ is the variance of peer group distributions. Estimates of γ_{var} are found in panels A and B of Table 2.5 for Math grades and GPA respectively. In both panels, column (1) contains the main specification. There is a positive and significant effect of the variance, but no significant effect of the mean. A one standard deviation increase in the variance of peer group Math SAT scores improves Company average Math grades by 7.8 percent of a standard deviation and Company average GPA by 8.3 percent of a standard deviation. These estimates are comparable to the structural estimates in Table 2.3.

To test whether these estimates are sensitive to the inclusion of other aggregate peer group characteristics that may also be correlated with academic outcomes, I include average peer group measures of the eight scrambling controls in column (2).[30] This has little effect on any of the estimates. As a further comparison to the structural estimates in Table 2.3, I include an additional specification check where $\widehat{Z}_{pt-1}$ contains the square of the coefficient of variation in column (2). The difference between this specification and the structural form is that there is a cubic in own SAT and both the own effect and mean peer effect are in levels instead of logs. A one standard deviation increase in the square of the coefficient of variation in peer group Math SAT scores positively affects Company average plebe-year Math grades by 8.2 percent and GPA by 9.1 percent of a standard deviation. These effects are comparable to the estimates in column (1) and to the structural estimates in Table 2.3.

[30] All specifications already control for individual-level random scrambling controls.

One concern with using the variance as a measure of dispersion is that it is sensitive to observations at the extremes. An alternative measure of dispersion that is less susceptible to outliers is the 75-25 differential in the peer group Math SAT distribution (75^{th} Math SAT percentile minus the 25^{th} Math SAT percentile). As a further robustness check, I include estimates for γ_{75-25} in panels A and B of Table 2.6.

Column (1) contains the preferred specification and column (2) adds the average peer group measures of the eight scrambling controls. The sign on the estimates of γ_{75-25} are consistent with those of γ_{var} and the point estimates are not sensitive to the inclusion of additional aggregate controls. Estimates of γ_{75-25} imply that a one standard deviation increase in the peer group 75-25 differential in Math SAT scores positively affects Company average plebe-year Math grades by 13 percent of a standard deviation and Company average GPA by 15.6 percent of a standard deviation. The effects of the 75-25 differential are slightly larger than the variance effects found in Table 2.5.

On balance, reduced form estimates indicate that plebes with greater peer group heterogeneity in Math SAT scores perform better in Math grades and GPA than plebes with less peer group heterogeneity in Math SAT scores. This result is robust to different measures of dispersion and is similar to the structural estimates in Table 2.3. Moreover, it is not affected by other aggregate peer group characteristics. Assuming any mean effect is positive, the theoretical model suggests that peers serve as substitutes in education production at West Point. This implies that if West Point continues to maintain relatively equal means in their assignment process, they can improve educational production efficiency by also maximizing the dispersion in peer ability.

2.7.2 Distributional Effects

The finding that greater heterogeneity in peer group composition has positive effects on academic attainment raises the question of whether having a higher top or lower bottom of the distribution is responsible for this result. For instance, the positive effect of γ_{75-25} may be due to plebes having a higher 75^{th} or a lower 25^{th} Math SAT percentile of their peer group. To determine which tail of the distribution is responsible for the 75-25 differential result, I include the 75th percentile and the 25th percentile of the peer Math SAT distribution in $\hat{Z}_{pt-1}$.

Panels C and D of Table 2.6 contain these specifications and are organized similarly to panels A and B. For both Math grades and GPA, having a higher 75th percentile of the peer Math SAT distribution has positive and significant effects on individual academic attainment, while the 25th percentile has no significant effect. A one standard deviation increase in the 75th percentile of the peer Math SAT distribution increases Company average Math grades by 18 percent of a standard deviation and Company average GPA by 26 percent of a standard deviation.[31] This finding further supports the "good apple" scenario and is consistent with the idea that having better high ability peers enhances average academic performance.

It is also interesting to know what the effects of increased heterogeneity are across the distribution of academic outcomes. To address this question, I estimate specifications using Quantile Regression instead of OLS. Panels A and B of Table 2.7 contain quantile regression estimates of the peer group 75-25 peer Math SAT differential and panels C and D of Table 2.7 contain quantile regression estimates of the 75th and 25th peer Math SAT percentiles. Quantiles of the Math grade distribution are found in panels A and C and quantiles of the GPA are found in panels B and D. In all panels, column (1) contains estimates at the 75th quantile, column (2) contains estimates at the 50th quantile, and column (3) contains estimates at the 25th quantile of the Math grade or GPA distribution.

The magnitude of the estimates in panels A and B of Table 2.7 are comparable to the magnitude of the estimates in panels A and B of Table 2.6. They reveal relatively uniform, positive, and significant effects of heterogeneity across the distribution of academic outcomes. The point estimates for Math grades in panel A indicate a stronger effect at the 25th quantile than at the 75th quantile, although these are not statistically different from each other. In general, the estimates in panels A and B suggest that all parts of the academic outcome distribution may benefit to some degree from greater heterogeneity.

Likewise, estimates in panels C and D of Table 2.7 are comparable to estimates in panels C and D of Table 2.6. For both Math grades and GPA, the 25th Math SAT percentile has no significant effect across any part of the distribution of academic outcomes. However, in panel C,

[31] Slight changes in the point estimates of δ are due to correlations with the percentiles. However, estimates of δ in panels C and D are not statistically different from those in panels A and B.

the 75th Math SAT percentile has positive and significant effects at the 25th and 50th quantile and insignificant effects at the 75th quantile, although they are not statistically different. The point estimates suggest that having a higher 75th percentile of the Math SAT distribution may be more helpful at the lower tail than at the upper tail of the Math grade distribution. Estimates in panel D show a positive and relatively uniform effect of the 75th Math SAT percentile across the GPA distribution.

2.8 Conclusion

Whether peers serve as complements or substitutes in the production of education has important implications for production efficiency. The Benabou (1996b) model indicates that the degree of substitutability between peers determines how peer group heterogeneity impacts average academic outcomes. The unique environment at the United States Military Academy provides an opportunity to estimate the sign on the measure of substitutability. The random assignment of plebes to peer groups and the exclusive use of pretreatment measures of ability as the peer effects of interest overcomes the well-documented empirical problems associated with identifying peer effects.

OLS estimates of measures of dispersion in peer group Math SAT distributions reveal that more heterogeneous peer groups have positive effects on academic outcomes. This finding is robust across several measures of dispersion. A one standard deviation increase in the peer group 75-25 differential in Math SAT scores increase Company average Math grades by 13 percent of a standard deviation and Company average GPA by 15.6 percent of a standard deviation. This effect is relatively uniform across the distribution of academic outcomes. Assuming average peer effects are small and positive, the theoretical framework indicates that peers are substitutes in the production of education at West Point. This implies that if West Point continues to maintain relatively equal peer group means in their assignment process, they can improve educational production efficiency by also maximizing heterogeneity in peer group ability.

For the estimates of the 75-25 differential in peer group Math SAT scores, the 75th percentile, but not the 25th percentile, accounts for most of this effect. A one standard deviation increase in the 75th percentile of the peer group Math SAT distribution increases Company average Math grades by 18 percent of a standard deviation and Company average GPA by 26 percent of a standard deviation. This supports the idea that having better high ability peers enhances average academic performance. While this result is probably consistent with the view of most educators, this experiment affords a rare opportunity to provide empirical evidence for this commonly held belief.

The findings in this study raise several important questions for future research. To begin with, peer group heterogeneity may impact academic attainment differently in settings less unique than the United States Military Academy. In this experiment, peer means are relatively uniform across individuals. Other settings where means vary to a greater extent may produce different results. Moreover, plebes at West Point are a highly selected and relatively homogeneous group compared to other undergraduate academic institutions. There may also be other factors like signaling and networks that create larger economic effects by segregating students. These gains may offset the gains found by mixing individuals in this study. Future research, which incorporates some of these issues, may provide valuable insights into this important question.

Appendix A: Taylor Expansion Derivation of Equation (2.3)

The initial Equation is: $$g_i = q_i^{\alpha} \cdot \overline{q}_i^{\omega} \cdot \left[\frac{1}{N} \sum_{j=1}^{N} q_j^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma \cdot \beta}{\sigma-1}}$$

Taking Logs: $$Ln(g_i) = \alpha \cdot Ln(q_i) + \omega \cdot Ln(\overline{q}_i) + \frac{\sigma \cdot \beta}{\sigma-1} \cdot Ln\left[\frac{1}{N} \sum_{j=1}^{N} q_j^{\frac{\sigma-1}{\sigma}} \right]$$

Expanding each q_j around $\overline{q}_i$, where $\overline{q}_i = \frac{1}{N}\sum_{j=1}^{N} q_j$:

$$Ln(g_i) = \underbrace{f(\overline{q}_i)}_{(1)} + \underbrace{\sum_{j=1}^{N} \frac{\partial f(\overline{q}_i)}{\partial q_j} \cdot (q_j - \overline{q}_i)}_{(2)} + \underbrace{\frac{1}{2} \sum_{j=1}^{N} \sum_{k=1}^{N} \frac{\partial^2 f(\overline{q}_i)}{\partial q_j \partial q_k} \cdot (q_j - \overline{q}_i) \cdot (q_k - \overline{q}_i)}_{(3)} + R$$

Components of the Taylor Expansion:

(1) $f(\overline{q}_i) = \alpha \cdot Ln(q_i) + (\beta + \omega) \cdot Ln(\overline{q}_i)$

(2) This term goes to zero.

$$\frac{\partial f(\overline{q}_i)}{\partial q_j} = \beta \cdot \frac{1}{\overline{q}_i} \cdot \frac{1}{N} \quad \Rightarrow \quad \sum_{j=1}^{N} \frac{\partial f(\overline{q}_i)}{\partial q_j} \cdot (q_j - \overline{q}_i) = \beta \cdot \frac{1}{\overline{q}_i} \cdot \frac{1}{N} \sum_{j=1}^{N} q_j - \beta \cdot \frac{1}{\overline{q}_i} \cdot \overline{q}_i = 0$$

(3) $$\left. \frac{\partial^2 f(\overline{q}_i)}{\partial q_j \partial q_k} \right|_{j=k} = \left(\frac{-\beta}{N \cdot \sigma \cdot \overline{q}_i^{\,2}} \right) + \left(\frac{-\beta \cdot (\sigma - 1)}{N^2 \cdot \sigma \cdot \overline{q}_i^{\,2}} \right) \qquad \left. \frac{\partial^2 f(\overline{q}_i)}{\partial q_j \partial q_k} \right|_{j \neq k} = \left(\frac{-\beta \cdot (\sigma - 1)}{N^2 \cdot \sigma \cdot \overline{q}_i^{\,2}} \right)$$

Rewriting and evaluating (3), where q_p represents the distribution of peers *j*:

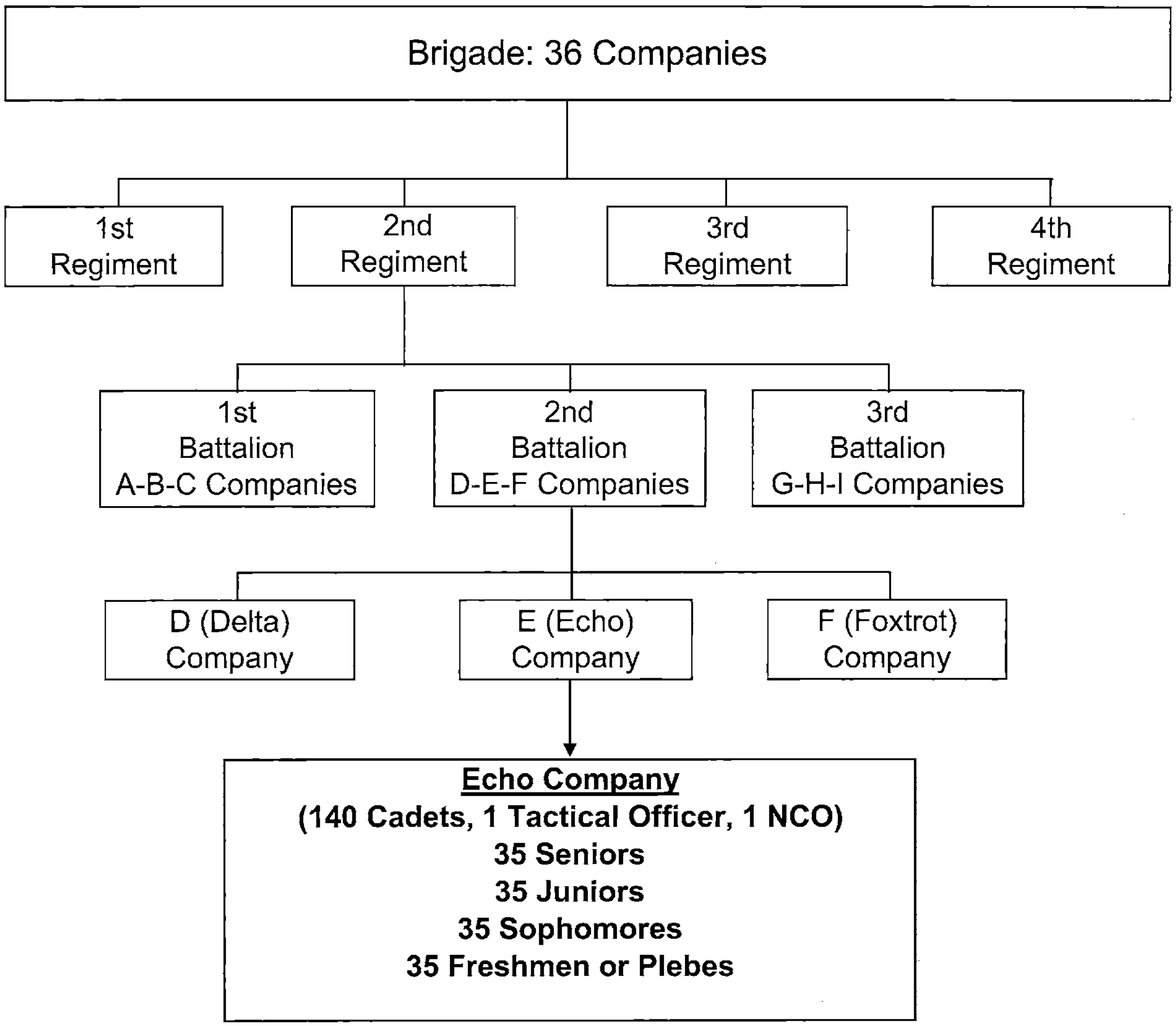
$$\Rightarrow \frac{1}{2} \sum_{j=1}^{N} \left(\frac{-\beta}{N \cdot \sigma \cdot \overline{q}_i^{\,2}} \right) (q_j - \overline{q}_i)(q_j - \overline{q}_i) + \frac{1}{2} \sum_{j=1}^{N} \sum_{k=1}^{N} \left(\frac{-\beta \cdot (\sigma - 1)}{N^2 \cdot \sigma \cdot \overline{q}_i^{\,2}} \right) (q_j - \overline{q}_i)(q_k - \overline{q}_i)$$

$$\Rightarrow \frac{1}{2} \left(\frac{-\beta}{\sigma \cdot \overline{q}_i^{\,2}} \right) Var(q_p) + 0$$ Second term goes to zero as in (2).

Final form of Equation (2.3), where *R* denotes higher order terms:

$$Ln(g_i) = \alpha \cdot Ln(q_i) + (\beta + \omega) \cdot Ln(\overline{q}_i) - \frac{1}{2} \cdot \beta \cdot \frac{1}{\sigma} \cdot \left[\frac{Var(q_p)}{\overline{q}_i^{\,2}} \right] + R$$

THE CORPS OF CADETS



Term	Description
Company	Made up of all four classes: 35 cadets in each class
Plebe	Freshmen cadet
Cadet	General term referring to an individual from any of the four classes

Figure 2.1: Organization of the United States Military Academy Corps of Cadets

Table 2.1: Company Level Summary Statistics

	A. Plebe-Year Academic Attainment				
	Observations	Mean	Std. Dev.	Minimum	Maximum
Math Grade Plebe Year (Company)	252	2.69	0.23	2.10	3.18
Math Grade Plebe Year (Individual)	6,309	2.69	0.80	0.50	4.30
Academic GPA Plebe Year (Company)	252	2.66	0.11	2.31	2.93
Academic GPA Plebe Year (Individual)	6,870	2.66	0.54	0.19	4.10
	B. Pretreatment Characteristics				
	Companies	Mean	Std. Dev.	Minimum	Maximum
Math SAT Score	252	636.7	10.6	599.0	661.8
Coefficient of Variation^2 of Math SAT Score	252	0.011	0.003	0.005	0.023
Variance of Math SAT Score	252	4450.0	1122.2	1989.5	8725.2
75-25 Math SAT Score Differential	252	93.9	21.2	40.0	180.0
75th Percentile of the Math SAT Score	252	684.9	15.4	640.0	730.0
25th Percentile of the Math SAT Score	252	591.0	15.8	550.0	620.0
	C. Random Scrambling Controls				
	Companies	Mean	Std. Dev.	Minimum	Maximum
Female	252	0.118	0.025	0.032	0.212
Black	252	0.065	0.030	0.000	0.167
Hispanic	252	0.043	0.026	0.000	0.143
Recruited Football Players	252	0.075	0.032	0.000	0.184
Other Recruited Athletes	252	0.141	0.043	0.000	0.314
Attended the West Point Prep School	252	0.140	0.030	0.054	0.219
College Entrance Exam Rank (CEER)	252	607.3	5.0	586.3	623.7
Whole Candidate Score (WCS)	252	6032.3	31.8	5952.2	6167.1

The data is from the Office Economic Manpower Analysis, West Point NY. Data includes personnel, admissions, and performance files for the graduating classes of 1992-1998. Differences in individual level Math grade and individual level GPA sample size is due to missing Math grades from the West Point database (a result of changing data management systems). There are 36 companies across 7 years. The coefficient of variation is the variance divided by the square of the mean. The CEER score is a weighted average of SAT, ACT, and high school rank. WCS is an aggregated score from pretreatment activities and performance. SAT scores are comparable across years because they were all taken prior to the 1995 renormalization.

Table 2.2: Randomly Assigned Peers at West Point
Outcome Variable: Peer Group Average Math SAT

	(1)	(2)
Math SAT	-0.011 (0.002)	0.004 (0.004)
R^2	0.06	0.07
Observations	6,870	6,870
Scrambling Controls	No	Yes

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of average peer Math SAT on individual Math SAT. All specifications include year dummies and a constant. Individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS are included as indicated. See Table 2.1 notes for sample description.

Table 2.3: Structural Estimates for the Determinants of the Cost of Heterogeneity
Outcome Variable: Individual Plebe Math Grade or Plebe GPA

	Math Grades (1)	GPA (2)
Peer Group Coefficient of Variation^2 in Math SAT	3.971	1.287
	(1.382)	(0.767)
Log of Peer Group Mean Math SAT	0.087	0.244
	(0.270)	(0.141)
Log of Individual Math SAT	0.440	0.091
	(0.058)	(0.030)
R^2	0.25	0.40
Observations	6,309	6,870

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of the natural log of individual academic outcomes (as indicated in the column headings) on the natural log of own Math SAT, the natural log of average peer Math SAT, and the square of the coefficient of variation in peer Math SAT scores. All specifications also include year dummies, a constant, and individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. See Table 2.1 notes for sample description.

Table 2.4: Reduced Form Estimates of Peer Group Means
Outcome Variable: Individual Plebe Math Grade or Plebe GPA

	A. Mean	
	Math Grades (1)	GPA (2)
Peer Group Mean Math SAT / 100	-0.029 (0.088)	0.070 (0.052)
R^2	0.29	0.43
Observations	6,309	6,870

	B. Mean Interactions	
	Math Grades (1)	GPA (2)
Peer Group Mean Math SAT x Top 25% / 100	-0.031 (0.096)	0.073 (0.055)
Peer Group Mean Math SAT x Middle 50% / 100	-0.031 (0.094)	0.073 (0.054)
Peer Group Mean Math SAT x Bottom 25% / 100	-0.030 (0.093)	0.072 (0.054)
R^2	0.29	0.43
Observations	6,309	6,870

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates in panel A reflect regressions of individual academic outcomes as listed in the column headings on the peer mean Math SAT score. Panel B interacts the peer mean Math SAT with dummies for if a cadet falls in the bottom 25%, middle 50%, or top 25% of the Math SAT distribution for each Company. All specifications also include year dummies, a constant, and individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. See Table 2.1 notes for sample description.

Table 2.5: Reduced Form Estimates of Peer Group Variance
Outcome Variable: Individual Plebe Math Grade or Plebe GPA

	A. Math Grades			B. GPA		
	(1)	(2)	(3)	(1)	(2)	(3)
Peer Group Variance in Math SAT / 10,000	0.159 (0.075)	0.168 (0.082)		0.081 (0.047)	0.088 (0.048)	
Peer Group Coefficient of Variation^2 in Math SAT			6.309 (3.016)			3.334 (1.874)
Peer Group Mean Math SAT / 100	-0.002 (0.087)	-0.003 (0.096)	0.018 (0.088)	0.083 (0.053)	0.097 (0.056)	0.094 (0.054)
R^2	0.29	0.29	0.29	0.43	0.43	0.43
Observations		6,309			6,870	
Average Peer Group Scrambling Controls	No	Yes	No	No	Yes	No

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates reflect regressions of individual academic outcomes on the designated measure of dispersion in peer Math SAT scores. All specifications include year dummies, a constant, a cubic in own Math SAT, average peer Math SAT, and individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. Average peer group scrambling controls added as indicated. See Table 2.1 notes for sample description.

Table 2.6: Reduced Form Estimates of the 75-25 Differential and the 75th & 25th Percentiles
Outcome Variable: Individual Plebe Math Grade or Plebe GPA

75-25 Differential of the Peer Math SAT Distribution

	A. Math Grades (Differential)		B. GPA (Differential)	
	(1)	(2)	(1)	(2)
Peer Group 75-25 Math SAT Differential / 100	0.142	0.142	0.081	0.080
	(0.040)	(0.042)	(0.024)	(0.024)
Peer Group Mean Math SAT / 100	-0.031	-0.031	0.068	0.083
	(0.087)	(0.095)	(0.052)	(0.055)
R^2	0.29	0.29	0.43	0.43
Observations	6,309		6,870	
Average Peer Group Scrambling Controls	No	Yes	No	Yes

75th & 25th Percentiles of the Peer Math SAT Distribution

	C. Math Grades (Percentiles)		D. GPA (Percentiles)	
	(1)	(2)	(1)	(2)
Peer Group 75th Math SAT Percentile / 100	0.270	0.272	0.184	0.184
	(0.088)	(0.089)	(0.055)	(0.055)
Peer Group 25th Math SAT Percentile / 100	-0.030	-0.025	0.008	0.008
	(0.086)	(0.093)	(0.051)	(0.051)
Peer Group Mean Math SAT / 100	-0.261	-0.277	-0.116	-0.116
	(0.181)	(0.192)	(0.112)	(0.112)
R^2	0.29	0.29	0.43	0.43
Observations	6,309		6,870	
Average Peer Group Scrambling Controls	No	Yes	No	Yes

Standard errors in parenthesis account for clustering at the Company and year level. OLS estimates in panels A and B reflect regressions of individual academic outcomes on the 75-25 differential of the peer Math SAT distribution. Estimates in panels C and D reflect regressions of individual academic outcomes on the 25th and 75th percentile of the peer Math SAT distribution. All specifications include year dummies, a constant, a cubic in own Math SAT, average peer Math SAT, and individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. Average peer group scrambling controls added as indicated. See Table 2.1 notes for sample description.

Table 2.7: Quantile Regression for the 75-25 Differential and the 75th & 25th Percentiles
Outcome Variable: Individual Plebe Math Grade or Plebe GPA

	75-25 Differential of the Peer Math SAT Distribution					
	A . Math Grades (Differential)			B. GPA (Differential)		
	75th Math Quantile (1)	50th Math Quantile (2)	25th Math Quantile (3)	75th Math Quantile (1)	50th Math Quantile (2)	25th Math Quantile (3)
Peer Group 75-25 Math SAT Differential / 100	0.063 (0.044)	0.116 (0.041)	0.158 (0.049)	0.077 (0.030)	0.099 (0.031)	0.096 (0.033)
Peer Group Mean Math SAT / 100	-0.018 (0.089)	-0.078 (0.083)	0.050 (0.100)	0.073 (0.062)	0.033 (0.062)	0.058 (0.065)
Observations		6,309			6,870	
	75th & 25th Percentiles of the Peer Math SAT Distribution					
	C. Math Grades (Percentiles)			D. GPA (Percentiles)		
	75th Math Quantile (1)	50th Math Quantile (2)	25th Math Quantile (3)	75th Math Quantile (1)	50th Math Quantile (2)	25th Math Quantile (3)
Peer Group 75th Math SAT Percentile / 100	0.076 (0.107)	0.196 (0.096)	0.277 (0.105)	0.169 (0.067)	0.190 (0.073)	0.191 (0.083)
Peer Group 25th Math SAT Percentile / 100	-0.055 (0.096)	-0.048 (0.086)	-0.054 (0.094)	-0.003 (0.060)	-0.024 (0.066)	-0.011 (0.076)
Peer Group Mean Math SAT / 100	-0.033 (0.197)	-0.210 (0.176)	-0.192 (0.194)	-0.074 (0.123)	-0.131 (0.135)	-0.108 (0.153)
Observations		6,309			6,870	

Standard errors are in parenthesis. Quantile regression estimates for either the 75th, 50th, or 25th academic grade quantiles are listed as column headings. These estimates reflect quantile regressions of individual academic outcomes on measures of dispersion as listed in the panel headings. All specifications include year dummies, a constant, a cubic in own Math SAT, average peer Math SAT, and individual-level random scrambling controls: gender, race, recruited athlete, prep school, CEER, and WCS. See Table 2.1 notes for sample description.

Chapter 3: Effects of Parental Absences and Household Relocations on the Educational Attainment of Military Children

3.1 Introduction

One aspect of the human capital production process that has received particular attention from economists is investments in children's human capital. Haveman and Wolfe (1993, 1995) organize this vast literature into three main categories: public investments in children, parental investments in children, and children's investments in themselves. The public sector forms the human capital production infrastructure by initiating educational opportunities. Parents invest time and resources in the human capital of their children, given the educational opportunities available. Children respond to the public and parental investments with choices that impact their own human capital development. This study focuses on the second category, parental investments in children.

I analyze two parental choices that may affect children's human capital development considerably, parental absences and household relocations. The psychology literature suggests several ways that parental absences and household relocations can affect children: disrupting social groups, increasing responsibilities and expectations, creating a sense of uncertainty about the future, and altering levels of adult supervision (Hillenbrand, 1976; Hochschild, 1989; Kelley, 1994; Kelley et al., 1994; Landy, 1994; and Yeatman, 1981). These, and other related factors, can have both favorable and unfavorable educational consequences. On one hand, children could improve in the classroom, if a newfound sense of responsibility accompanies the parental absence. On the other hand, children could fall behind, if parental absences result in less supervision of classroom performance. Similar competing arguments are made for household relocations. The 1994 General Accounting Office study argues that the disruptions associated with moves are likely to result in lower test scores, while others like Piaget have argued that exposing children to different environments facilitates their understanding of the world and improves performance in the classroom. Ultimately, determining the net effect of parental absences and household relocations on children's educational attainment requires an empirical analysis.

Recent trends in American family structure underscore the importance of understanding how family disruptions affect children. In 1970, 12 percent of all children lived with a single parent, and by 1996, that number increased to 28 percent.[32] The recent rise in female labor force participation has also dramatically affected the number of families with both parents in the work force. In 1970, only 29 percent of children under the age of six and 39 percent of children under the age of eighteen had both parents in the labor force, but by 2000, this number grew to more than 61 percent and 68 percent respectively.[33] Another important source of parental absences over the last few decades is the work place. In 1970, there were roughly 93 million business trips in the U.S., or approximately 1.4 business trips per household. By 1997, there were more than 213 million business trips in the U.S., equating to around 2.1 business trips per household.[34] Finally, 2000 Census data shows that nearly seventeen percent of American households relocate each year.

Ascertaining a child's academic response to parental absences and household relocations is complicated because there are a number of factors that affect children's educational attainment as well as parental absences and household relocations. For example, lower income is correlated with single parenting, household relocations, and children's academic performance. In 1992, only 5.6 percent of married households with children qualified for welfare, while 36.7 percent of single parent households qualified for welfare (Bauman, 2000). In 2000, 21 percent of households with annual income less than 25,000 dollars moved and only 15 percent of households with annual income more than 25,000 dollars moved (Schachter, 2001). As well, several studies document a strong correlation between income and a child's academic performance (Heinlein and Shinn, 2000; McLanahan and Sandefur, 1994; and Taubman, 1989).

It is also possible that parental absences and household relocations cause income levels to change. Moreover, parents may choose to be absent more or less often, or relocate a household, in response to a child's classroom performance. Given these and other potentially confounding issues, a source of variation in parental absences and household relocations that is unrelated to other determinants of children's educational attainment is required to identify causal treatment effects. In this paper, I exploit work-related parental absences and work-induced household relocations as a plausible source of exogenous variation.

The work-related parental absences and work-induced household relocations for this study are a product of labor force requirements in the U.S. Army. Military deployments and

[32] U.S. Census Bureau (1996-CPS).
[33] Haveman and Wolfe (1993), page 156 and Fields and Casper (2001).
[34] Statistics are from Census data from the Statistical Abstracts of the U.S.

other temporary duty assignments generate parental absences, while the military's expressed intention to move soldiers every two to four years produces frequent household relocations. Using the demands of military service is empirically appealing because it seems reasonable that the assignment of military absences and relocations is unrelated to children's educational attainment. Media coverage of a recent deployment quoted one soldier who explained that the challenges of military service for families are compounded by, "somebody else other than us deciding where we live, what missions we will be on, and how long we will be separated."[35] I explore the military's mechanism for assigning parental absences and household relocations as well as employ an instrumental variable to test this claim of exogenous assignment. The evidence suggests that the assignment of parental absences and household relocations is uncorrelated with other potential determinants of children's educational attainment.

Estimates indicate that parental absences during the current school year adversely affect children's test scores by a tenth of a standard deviation. Cumulative four-year absences also negatively influence children's test scores and officers' children experience as much as a fifth of a standard deviation decline. Furthermore, frequent household relocations have modest negative effects on children's test scores for enlisted soldiers, but no significant effects on officer's children. Other evidence suggests that parental absences and household relocations cause additional detrimental effects to test scores of children with single parents, children with mothers in the Army, children with parents having lower AFQT scores, and younger children.

In the next section, I discuss this experiment in the context of the existing literature. In Section 3.3, I provide background information on standardized tests in Texas and military assignment mechanisms. Section 3.4 describes the Army data and the Texas Education Agency (TEA) data. In Section 3.5, I present the empirical framework and discuss the identification assumptions. Sections 3.6 and 3.7 contain the main results for parental absences and household relocations, respectively, and Section 3.8 concludes.

3.2 This Study in the Context of the Existing Literature

The growing parental absence literature primarily focuses on absences attributed to one of four events: divorce, separation, out of wedlock birth, or death. Haveman and Wolfe (1995) provide

[35] Nearin and Segal (2002), National Public Radio broadcast, 11 Nov. 2002, "Military Families Prepare for Possible War with Iraq."

a detailed review of the research on parental absences and find predominantly negative effects, but they interpret these results with caution.[36] Many of the adverse effects assigned to parental absences may instead be attributable to lower household income or lower parental education that is associated to a greater degree with parental absences due to divorce, separation, out of wedlock birth, or death.

In the household relocation literature, there is little consensus for both the magnitude and direction of the treatment effect. Norford and Medway (2002) review the behavioral psychology literature noting nine studies that find no effects and three studies that find small negative effects of relocations on various metrics of social adjustment, behavior, and peer relations for children. Heinlein and Shinn (2000) focus specifically on the effect of household relocations on children's educational attainment. In their review of the literature, they report twenty-six studies that find no effects, nineteen studies that find negative effects, and eight studies that find positive effects. Both sets of authors suggest that much of the ambiguity in the magnitude and the direction of the effect is a result of varying degrees of quality in research methods.

The difficulty interpreting much of the parental absence literature and the inconclusive household relocation literature serves as the primary motivation for this study. While using the military as a source of variation may address the main identification problems in the literature, military children may not be representative of children in the civilian population. At the time of this study, the military is manned with an all-volunteer force and only accepts applicants who meet a baseline minimum mental, physical, and medical standard. Temporary episodes of parental absences and frequent relocations in the military may also be different on other dimensions than work-related parental absences and work-induced relocations in the civilian sector. A mother said the following about her son during a recent interview of families preparing for a deployment, "it affected his grades last year when he knew his father was in Afghanistan - he worries more about daddy dieing than just going away and coming back."[37]

Although the unique nature of military service requires a more careful interpretation of estimates from such an experiment, it is nevertheless useful to conduct this research for two purposes. First, it is important to understand how military labor force requirements affect the sizeable military population. The Department of Defense is the second largest employer in the

[36] Haveman and Wolf (1995) page 1839.
[37] Nearin and Segal (2002), National Public Radio, "Military Families Prepare for Possible War with Iraq."

United States and approximately 60 percent of the 2.4 million active duty and reserve soldiers have children.[38] Second, there is some quantitative as well as suggestive evidence implying that these results may generalize, at least in part, to the civilian population.

Hiew (1992) directly compares military parental absences with civilian parental absences by estimating the effects of deployments in the Canadian Military as well as employment-induced separation of fathers in Japan on children's behavioral and academic outcomes. Both military and civilian parental absences result in comparable increased stress levels, behavioral problems, and correspondingly poor academic achievement for elementary children.

There is also a small history of previous attempts to use the military to identify these effects. Pisano (1992) estimates the effect of Gulf War deployments on children's educational outcomes using the California Assessment Test. He finds that sixth grade girls perform slightly worse in reading when a parent deploys. Marchant and Medway (1987) use the military as a source of variation in household relocations, and find no significant effect on children's academic outcomes.

While both Pisano (1992) and Marchant and Medway (1987) exploit the military as a source of variation, several empirical shortcomings distinguish their studies from this study. Pisano (1992) uses a difference-in-differences identification strategy where members of the control group are not necessarily comparable to members of the treatment group.[39] Furthermore, there are only 158 observations in his study. Drawing firm conclusions from Marchant and Medway (1987) is also difficult because their results are based on only 40 observations. This study improves upon these military specific studies by using more than 13,000 observations and a richer set of descriptive variables. This study also contributes to the existing literature on parental investments in children by conducting an experiment that addresses many of the documented biases.

The design of this experiment is most similar to Angrist and Johnson (2000). They estimate the effect of Gulf War Deployments on divorce rates, spousal employment, and children's disability rates and find that male deployments do not affect divorce rates, but decrease spousal employment, while female deployments increase divorce rates, but do not impact spousal employment. They also find no effect of military deployments on children's disability rates. In

[38] Estimates based on 2002 data from the Directorate for Information Operations and Reports, OSD.
[39] It is possible that some members of the control group do not have military parents and there are sizeable differences in the pretreatment characteristics of the control and treatment group.

part, this study builds on their study by estimating the effect of military deployments on children's educational attainment.

3.3 Testing Children in Texas and Military Assignment Mechanisms

Texas has conducted state-level testing since 1980, and has one of the leading standardized testing programs in the U.S. In 1990, the Texas Education Agency (TEA) implemented the Texas Assessment of Academic Skills (TAAS) program. The TAAS program serves as a screening mechanism, certifying the student's ability to graduate to the next grade. Schools administer the tests in April and May of each year to students in grades 3-8 and 10. The TAAS evaluates performance in Math, Reading, Science, Writing, Foreign Languages, and Social Studies at different points throughout a child's progression within the public and charter school systems. However, only Math scores are used in this study because they are available for all years and all grade levels. Scores in each subject receive a Texas Learning Index (TLI) value from 0-100. Students must achieve a TLI score of 70 to advance to the next grade. In addition, TLI scores are normalized to allow comparisons across years for the same student. For example, a student receiving a Math score of 75 in the 4th grade and a Math score of 80 in the 5th grade has demonstrated individual improvement across grades.

The TEA ultimately requires all students in both public and charter schools to pass the exams with a TLI score of 70 in the 10th grade in order to receive a high school diploma. Many private schools also voluntarily administer the TAAS exams, but passing them is not a requirement for graduation. The TEA's policy for testing applies to children of military service members as well. Since schools located on military installations also belong to local public school districts, almost all military children living in Texas take the TAAS exams.

This study focuses on children of Army soldiers who took the TAAS exam in the spring of 1997 and 1998. There were approximately 60,000 active duty soldiers stationed in Texas during this time period. A majority of these soldiers belonged to two of the ten active duty Army Divisions (1st Cavalry Division or 4th Infantry Division).[40] Divisions are made up of subunits called Brigades, Brigades are made up of Battalions, and Battalions are made up of Companies. Units at each level consist of combat and support forces.

[40] Army population in Texas is from the Public Affairs Office, Fort Hood and Fort Bliss Texas. Also, there were no Special Forces or Ranger Units stationed in Texas during this time period.

The Army Personnel Command (PERSCOM) is responsible for assigning soldiers to Division-level units based on the "needs of the Army." This Army-specific term captures the essence of all assignments, in that, the Army sends soldiers where they are needed to accomplish missions. The Army also attempts to relocate soldiers to different Divisions throughout the world every two to four years to improve the combat effectiveness of both individuals and units. While soldiers may submit a preference for their next duty location, the timing of the move and the assignment of a soldier to one of the subordinate Army units is largely independent of a soldier's preferences.

A standard example of how relocations transpire is as follows. Assume that the 1st Cavalry Division, stationed at Fort Hood Texas, reports a shortage of fifty tank mechanics to PERSCOM. PERSCOM will order tank mechanics that have recently joined the Army, along with tank mechanics that have been stationed the longest at other Divisions, to report to the 1st Cavalry Division to fill the shortages. While it is possible that PERSCOM gives consideration to soldiers who request a future assignment at Fort Hood, all subsequent subordinate unit assignments are determined by the unit-level command. The Division assigns the soldiers to one of several Brigades; the Brigade assigns them to one of several Battalions; and the Battalion assigns them to one of several Companies.

Once assigned to a Company level unit, the soldier accomplishes the missions and requirements of that unit.[41] The missions that a soldier's unit receives largely determine the amount of time that a soldier spends away from his/her family. Extended soldier absences are a result of two main missions: deployments and temporary duty assignments. Deployments can be at the individual or unit level and are characterized by the soldier leaving the base to perform some component of a larger military operation. During the timeframe of this study, unit deployments from Texas occurred below the Brigade-level to support peacekeeping operations in the Balkans, training and show of force maneuvers in the Middle East, and humanitarian aid missions in Third World Countries. In contrast, temporary duty assignments usually occur at the individual level and involve personal training for the soldier or the performance of his/her skill specialty for a temporary period of time at a different location within the U.S.

The assignment of absences is similar to the assignment of a soldier to a subordinate unit. They are based on the "needs of the Army" and largely independent of a soldier's preferences.

[41] Most Companies have approximately 100 to 150 soldiers.

For instance, assume the Army receives a mission requiring a Company to demolish a bridge in El Salvador. The Army assigns the mission to one of the Divisions; the Division assigns the mission to one of the Brigades; the Brigade assigns the mission to one of the Battalions; and the Battalion assigns one of the Companies to demolish the bridge.

In general, the "needs of the Army" determine the assignment of soldiers to units and the assignment of individual and unit-level absences. The Army's assignment mechanisms regard soldiers of the same rank and occupation as equals, and units of the same type as equals. If one Company needs enlisted mechanics and another Company needs Engineer officers, Battalions assign incoming enlisted mechanics to the one Company and incoming Engineer officers to the other.[42] Likewise, a Brigade will deploy a Battalion based primarily on the type of mission and the availability of units to complete the mission. A Brigade will send an Engineer Battalion, and not a Maintenance Battalion, to demolish a bridge. For the most part, changing world events drive the missions that determine the "needs of the Army" and the associated assignments.

The above description of absence and relocation assignments applies to enlisted personnel and officers alike. Aside from job definitions, a notable difference between officers and enlisted soldiers is the method of initial entry into the military. Enlisted soldiers commonly join the military shortly after high school by passing a medical exam and the Armed Services Vocational Aptitude Battery (ASVAB) test. A component of the ASVAB exam is the Armed Forces Qualification Test (AFQT). In part, the AFQT score determines which Military Occupational Specialties (MOS) are available for candidates to select as their job in the Army. Alternatively, officers are commissioned into specialty fields through one of several college scholarship programs. By law, all officers must have a college degree or a contract specifying a completion deadline, and therefore, officers have more education than enlisted soldiers.

3.4 U.S. Army and Texas Education Agency Data

The military data used in this study is from active duty Army enlisted and officer records, dependant records, and pay records for fiscal year 1996, 1997, and 1998. Samples include children ages six to nineteen, who have parents serving in the Active Duty Army, and who are stationed in Texas from 1996 to 1998. There are two separate pay supplements above and

[42] Prior performance is not a formal factor in assignments. This is based on discussions with assignment officers (CPT Sam Cubberly, USMA) and on the authors experience as an officer in the U.S. Army.

beyond the base salary that accurately identify soldier absences: hostile fire pay and family separation allowance.[43] A soldier on a deployment to the Balkans would receive hostile fire pay, while a soldier on a flood relief mission in Florida or attending a school for training would receive family separation allowance. The inherent incentives associated with pay data suggest a relatively accurate assignment of absences; the soldier will ensure that he receives compensation for his/her absence and the Army will ensure that he/she does not receive too much.

Using the pay structure outlined above, I generate two absence variables, "deployed" and "ever gone." The "deployed" variable denotes parental absences attributed solely to military deployments. The "ever gone" variable represents parental absences for any military work-related reason like deployments, off-site training, or other temporary duty assignments. I calculate the number of months a soldier is "deployed" by dividing the sum of all hostile fire pay received over a designated period of time by the standard monthly hostile fire pay allotment. In a similar fashion, I determine the number of months a soldier is "ever gone" by adding the number of months that a soldier received hostile fire pay plus the number of months that he/she received family separation allowance over a given period of time. If a soldier receives both hostile fire pay and family separation allowance in the same month, he/she only gets credit for one month "ever gone."[44]

Next, I construct two relocation variables: the number of moves a child experiences while his/her parents are in the military, and the average time a child spends at each military location. I define the number of moves that a child encounters as the number of Army induced moves. To establish the average time at a given location, I divide the number of years the child has spent with a parent in the military by the number of Army induced moves for the child.

The Army data contains an observation for each child and associated child, parent, parental absence, and child relocation characteristics. Subsequently, the Texas Education Agency (TEA) matched children's test scores and the grade level of the child to the Army data. The Army data met a minimum number of observations for each combination of included

[43] The military data does not otherwise contain reliable deployment or temporary duty status information. The Army assigns regions around the world where deployed soldiers receive hostile fire pay after 7 days. Soldiers spending more than 30 days away from home receive family separation allowance pay.
[44] At times Army pay is late, so I look two months past the end of the period of interest for lump sum payments exceeding the normal monthly amount and also count these as time absent.

variables in order to comply with the confidentiality requirements of the TEA.[45] The final data set contains observations for the matched Army and TEA data at the individual child level.

Table 3.1A displays descriptive statistics for the *current school year parental absence* data. Standard errors are in parenthesis and standard deviations are in brackets. The current school year is defined as 1 September of the previous year through 31 March of the current year, since the TAAS exams are given in April and May. I use this data to determine the presence of short-term effects of parental absences. Panel A compares children with enlisted parents to children with officer parents. Officers' children score five to six points higher and have a smaller standard deviation in TLI test scores than enlisted soldiers' children. Also, all officers have at least some college and officers do not have an AFQT score. Therefore, I will present the results for children of enlisted soldiers and officers separately in this study.

In panel B, I further stratify the sample by whether or not a child's parent was "deployed" more than three months during the current academic school year. Similarly, I divide panel C based on a child's parent being "ever gone" for more than three months during the current academic school year. Approximately six percent of both enlisted soldier's and officer's children have a parent "deployed" for greater than three months during the current academic year, and roughly nine percent have a parent "ever gone" greater than three months during the current academic year. In Table 3.1B, I present descriptive statistics organized identically to Table 3.1A, but for *cumulative parental absences* over a four-year period. I use this data to ascertain the long-term effects of parental absences.

In both Tables 3.1A and 3.1B, there is an evident decline in TLI Math scores for children with absent parents. There are also three descriptive variables that are notably different across deployment categories: marital status, gender of the military parent, and education level. Soldiers with these characteristics are absent more or less frequently than others. To explore the extent of these correlations, I present estimates from regressions of the parental absence variable on the full set of descriptive variables in panels A and B of Appendix Table 3.1 for current school year absences and Appendix Table 3.2 for cumulative four-year absences.

Although some of the correlations are significant, they are all quite small. Also, despite including fifteen variables in the regressions, they explain only two to three percent of the

[45] Data matched by child's social security number. Less than 1,000 observations were dropped due to small cell sizes prior to the match and there was a 40 percent match rate between the Army and Texas data: this was expected since 6-19 year olds were in the Army data (in theory, fourteen grades) and the exam is only given in seven grades.

variation in the absence variable. While the estimates in Appendix Tables 3.1 and 3.2 do not explicitly test the exogenous absence assignment assumption, they do have two implications. First, the extent to which these characteristics are also correlated with children's educational achievement could result in biased estimates. Second, it is important to consider possible explanations for these correlations because they may suggest a degree of selection by either the Army or the individual soldier for absences.

The two most concerning correlations are children with single parent soldiers and mothers in the Army, both of whom experience fewer absences than their counterparts. One possible explanation is that single parent soldiers and female soldiers may be sent home from a mission prematurely to handle family problems.[46] This would shorten their absence, possibly enough to have them classified as absent less than three months in the data. Assuming the child's reactions to these problems spill over to classroom performance, failing to control for these characteristics would likely impose a positive bias on the treatment effect.

Also, lower educated soldiers are absent more frequently than higher educated soldiers. All deployed units leave a small detachment of soldiers at the home station to care for families and process administrative actions. Commanders usually select injured soldiers or those on orders to relocate as part of this contingent. However, unexpected deployments often cause soldiers to loose tuition money for college courses that they take during off-duty hours. These soldiers would be the next likely candidates for stay-back personnel. Therefore, without education control variables, parental absence estimates would likely posses a negative bias, if parent's education level is positively correlated with children's classroom achievement.

In each of these cases, the occurrences are nominal, but possibly enough to account for the modest correlations found in the data. I will examine this issue further in the context of the empirical model provided in the next section and conduct additional specification checks for the main results.

3.5 Empirical Framework

Military absence and relocation assignments are likely to provide a source of exogenous variation that allows for a causal interpretation of the effect of work-related parental absences

[46]Angrist and Johnson (2000) find that female deployments result in higher incidents of marital dissolution. Also, intuitions are from discussions with Army Officers and from the author's nine years of service in the U.S. Army.

and work-induced household relocations on children's educational outcomes. To investigate this hypothesis more formally, I propose a simple model using pooled data from 1997 and 1998 with the following structure:

$$E_{it} = \alpha + \theta_{1998} + \delta \cdot D_{it} + \beta \cdot X_{it} + \varepsilon_{it} \qquad (3.1)$$

Here the left-hand side variable, E_{it}, is the TLI education score in Math, α is a constant, and θ_{1998} is a year dummy for 1998. The coefficient, δ, on the dichotomous variable of interest, D_{it}, represents the effect of parental absences or household relocations on the TLI Math score of child i in time period t (1997 or 1998). X_{it} denotes other covariates including child gender, child race, child grade level, marital status of the parents, gender of the military parent, civilian education level of the military parent, and AFQT score of the military parent where applicable. Since some children appear in the data in both years, all standard errors are corrected for clustering on the full set of variables contained in X_{it} using Huber-White robust standard errors.

In order to interpret δ as the reduced form causal effect of parental absences and household relocations, D_{it} must be free of measurement error and orthogonal to other potential determinants of children's educational outcomes, represented by ε_{it}. Measurement error concerns are minimal because both the data management systems of the Army and the soldiers review and update individual records monthly. The discussion of military induced absences and relocations in Section 3.3 suggests that soldiers have little control over deployments or the timing of moves. Instead, the "needs of the Army" determine absences and relocations. If these Army-induced assignments are orthogonal to other potential determinants of children's educational attainment, then δ has a causal interpretation. While it seems unlikely that the Army makes assignment decisions with children's academic attainment in mind, it is possible that the Army does not deploy some soldiers, or relocates them less frequently, for reasons that are correlated with a child's academic attainment.

One way to address this issue is to include observable characteristics in X_{it} that the Army could use from its database to assign absences and relocations, which are also potentially correlated with children's educational attainment. If this effort is successful and the assignment arguments in Section 3.3 are correct, then the estimated treatment effect is even more likely to have a causal interpretation. Ultimately, aside from clear evidence of true randomization, as in a lottery, the most compelling way to test the exogenous assignment hypothesis is through the use of a valid instrumental variable. For parental absences, a valid instrumental variable is correlated with individual parental absences and uncorrelated with other potential determinants of children's educational attainment.

I construct an instrumental variable for parental absences using the soldier's unit of assignment at the Battalion level. Since deployments can occur at individual and varying degrees of unit level, I create the instrumental variable by assigning a one to everyone in a Battalion where more than one-third of the Battalion deployed. The instrumental variable is binary to comply with confidentiality requirements for the matching process. One third of a unit is a logical choice, since Battalions commonly consist of three Companies. Most unit-level deployments occur at the Task Force level. Task Forces are generally a heterogeneous mix of Companies from different Battalions. The selection of which Companies form a Task Force for a deployment seems likely uncorrelated with individual-level children's educational attainment.

For reasons already discussed, I test the extent of correlations between the instrument and the characteristic variables contained in X_{it}. These estimates are found in panel C of Appendix Table 3.1. As in panels A and B, the marital status of the parent, the gender of the military parent, and the education level of the parent have statistically significant correlations, but they are small and explain very little of the variation in the instrument. This suggests that the higher-level units like Divisions and Brigades, and not lower-level units like Battalions and Companies, are responsible for the small correlations found in panels A and B of Appendix Table 3.1. It is doubtful that higher-level echelons of command can make unit-level deployment decisions based on individual soldier characteristics that are correlated with the academic performance of their children. Given that only six percent of the sample deployed, these small correlations are likely an unsystematic occurrence.

Finally, I anticipate a positive correlation between the deployment of a Battalion and an individual soldier. A soldier has a higher probability of deploying if his/her unit receives a mission to deploy than if his/her unit does not receive a mission to deploy. Thus, this instrumental variable is apt to be correlated with individual absence assignments and arguably uncorrelated with other potential determinants of children's educational attainment. I provide empirical evidence to support these assertions in the next section.

3.6 Empirical Results for Work-Related Parental Absences

In Table 3.2, I present Ordinary Least Squares (OLS) and Two Stage Least Squares (2SLS) estimates for the impact of *current academic year* parental absences on children's Math scores using the model in Equation (3.1). Panels A and B contain estimates associated with a "deployment", while panels C and D contain estimates associated with "ever gone" for three or more months during the current academic school year. Panels A and C include estimates for children of enlisted soldiers and panels B and D include estimates for officers' children. Columns (1) and (2) are the most parsimonious specifications, containing only a constant, a dummy variable for the year 1998, dummy variables for grades 4-8, and 10, and the absence variable. However, as mentioned above, a causal interpretation is more likely after conditioning on observable characteristics that are also possibly correlated with academic outcomes. Therefore, specifications in columns (3) and (4) contain child characteristics and the preferred specifications in columns (5) and (6) contain both child and parent characteristics. Including child controls alters δ only slightly and adding parental controls changes δ even less.[47]

Before discussing estimates of δ, it is informative to briefly highlight the coefficients on the characteristic variables contained in X_{it} (found in columns (5) and (6) of panels A and B). Most of the estimates are statistically significant, have reasonable magnitudes, and a logical sign. These estimates suggest that, boys score slightly lower than girls (although not for officer's children), whites score higher than non-whites, children with a male parent in the Army score higher than children with a female parent in the Army, children with less educated parents score lower than children with more educated parents, and children with high ability parents score higher than children with low ability parents (as measured by the AFQT) in children's Math scores.

OLS estimates for the variable of interest, δ, in panel A indicate approximately a 1-point decline, corresponding to 10 percent of a standard deviation, in test scores associated with a current school year parental absence of three or more months for children of enlisted soldiers. Comparing estimates in panel A with estimates in panel C reveals that the size of the effect is not overly sensitive to whether the parent was on a "deployment" or "ever gone." A similar effect in direction, but with a slightly larger magnitude, is measured for officer's children in panels B and

[47] Altonji, Elder, Taber (2000) show that this is a reasonable informal test to indicate the degree to which small correlations between the variable of interest and other unobservable characteristics plausibly bias the estimates.

D. Due to the smaller sample size, these estimates are not measured precisely enough to be statistically significant.

The first stage results for the 2SLS specifications are found at the bottom of columns (2), (4), and (6). They are well estimated and of the anticipated sign. Moreover, the instrument explains from one-third to two-thirds of the variation in individual absences. The 2SLS estimates are used to test the hypothesis that military induced parental absences are exogenous, or rather that the orthogonality assumption for OLS holds for the specification in Equation (3.1). Comparisons of OLS and 2SLS point estimates reveal only small differences. Moreover, a Hausman specification test fails to reject the null hypothesis that the orthogonality condition holds for all OLS specifications in Table 3.2.[48]

In Table 3.3, I provide an additional robustness test to determine if the two concerning correlations discussed above, children with single parent soldiers and children with mothers in the Army, affect the OLS estimates. Column (1) in each panel contains the estimates from column (5) in the corresponding panels of Table 3.2. Samples in column (2) do not include children with single parent soldiers or children with mothers in the Army. Comparisons between estimates in column (1) and column (2) reveal only modest changes in the point estimates.[49]

On balance, the claim that military-induced parental absences are uncorrelated with other potential determinants of children's academic attainment is supported by the description of parental absence assignments in the military and validated by the specification tests presented above. Consequently, I proceed with the remainder of the analysis using OLS to estimate the model in Equation (3.1).

Table 3.4 is structured similar to Table 3.2, except the variable of interest is *cumulative four-year* parental absences. This approach tests for the presence of aggregate, or long-term, effects of parental absences. I stratify parental absences by three categories: zero months, one to six months, and seven or more months absent over the previous four years. Using an OLS specification of Equation (3.1), I include dummy variables for children with parents absent one to six months and children with parents absent seven or more months in D_{it} (children with parents not absent over the past four years is the omitted category).

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[48] See Table 3.2 notes for the Hausman test p-values; all are insignificant at the 95 percent confidence level.
[49] Restricting samples to children with male/married parents in the Army does not alter the Hausman test results.

As in Table 3.2, the estimates in all panels are only modestly sensitive to the inclusion of additional control variables. Column (3) is the preferred specification because it contains the full set of observable characteristics. The sample in column (4) does not include children with single parent soldiers and mothers in the Army as an additional robustness check. Again, the estimates change only trivially.

Estimates in column (3) of panel A reveal a marginally significant .63-point decline in Math scores for children with parents "deployed" between one to six months over the last four years relative to children with no parental "deployments" over the past four years. There is also a significant decline of 1.5-points for children with parents "deployed" seven or more months during the previous four years relative to children with no parental "deployments" over the past four years. The estimates in column (3) of panel C for absences attributed to "ever gone" are smaller, although not statistically different, than absences for "deployments."

The point estimates for children of enlisted soldiers provide some evidence for claims in the psychology literature that absences attributed to "deployments" place additional strains above and beyond work-related parental absences that result from reasons other than "deployments." Recall that the "ever gone" sample in panel C includes soldiers that were absent due to "deployments" along with soldiers absent for reasons other than "deployments." Therefore, using the estimates for parental absences due to "deployments" from panel A, I can recover the effect of parental absences that result from reasons other than "deployments." I find a .60-point decline in Math scores (with a standard error of .30) for children of enlisted parents absent seven or more months for reasons other than "deployments." This is less than half of the effect (1.51-point decline) attributed solely to "deployments." Given the plausible anxiety associated with military "deployments," it seems reasonable that civilian work-related parental absence effects are closer to the negative .60-point estimate. However, since the absence variables are categorical, this exercise should be interpreted with caution. It is possible that absences attributed to "deployments" greater than seven months are longer or shorter than absences for reasons other than "deployments."

Panels B and D contain the estimates for officers' children. Like the enlisted estimates, the effect on children with parents absent one to six months over the past four years remains small and insignificant, but children with parents absent seven or more months experience a 2-

point (20 percent of a standard deviation) decline in test scores. For officer's children, the effect is similar regardless of whether or not the absence is attributable to a "deployment."[50]

In general, the estimates in Table 3.2 and Table 3.4 indicate both a short-term and long-term impact of parental absences on children's educational attainment. Since education is a building process, children exposed to frequent parental absences may find it increasingly difficult to catch up to their peers. There may also be other characteristics of children and their parents that further exacerbate these effects.

To investigate how parental absence effects vary with the characteristics of children and their parents, I include interaction specifications for children of enlisted soldiers in Table 3.5.[51] Panel A compares absence estimates for children with fathers in the Army relative to children with mothers in the Army. For all absence metrics used in Tables 3.2 and 3.4, the absence of a mother has at least as strong of an adverse effect as the absence of a father, and in most cases, has a much larger adverse effect. Since only thirteen percent of the children in this sample have mothers in the Army, the standard errors on the female parent estimates are relatively large. Only estimates of mothers "deployed" or "ever gone" greater than seven months over the last four years are statistically significant. The estimates in column (4) indicate that children with fathers in the Army who are "deployed" for seven or more months during the past four years experience a 1.36-point decline relative to children with no parental absences over the past four years. In contrast, children with mothers in the Army who are "deployed" seven or more months experience a 5.07-point decline in test scores relative to children with no parental absences over the past four years. Also, the difference between the point estimates for absences attributed to "deployments" and absences attributed to "ever gone" are larger for children with mothers in the Army than for children with fathers in the Army.

Panel B contains similar specifications for comparing children with married parents and children with single parents. Single parents make up only ten percent of the sample, so again the standard errors are large. Comparisons of the point estimates reveal a similar trend with the estimates in panel A. Given the high correlation between female parents in the Army and single parents, the similarity in the estimates is not surprising.[52] Children with an absent single parent

[50] A similar calculation for officer "non-deployment" absences results in a 1.72-point decline (s.e.=.56).
[51] There are no officer results presented here or in Table 3.9 because officers do not have AFQT scores and the sample is too small to produce informative estimates. However, interpretations based on point estimates are similar.
[52] Approximately 51 percent of female soldiers and only 5 percent of male soldiers are single parents.

score worse than children with an absent married parent and the difference between point estimates of absences attributed to "deployments" and absences attributed to "ever gone" is larger for children with single parents than for children with married parents.

Comparisons for children with parents in the top 40 percent and bottom 60 percent of the AFQT distribution are found in panel C. In all cases, there is no statistically significant decline in academic achievement for children with parents in the top 40 percent of the AFQT distribution who experience parental absences. Yet, there is a well-estimated negative effect for children with parents in the bottom 60 percent of the AFQT distribution. This finding supports the Army's use of the AFQT score as a measure of potential success in the Armed Forces. Soldiers with higher AFQT scores appear to be better suited for a military vocation and the associated parental absences.

Finally, panel D contains evidence that parental absences have larger adverse effects on younger children than older children. Jensen, Martin, Watanabe (1996) find similar results for behavioral responses of young children with parents deployed during the Gulf War. Weisenberg, Schwarzwald, Waysman, Solomom, and Klingman (1993) also observe a larger adverse effect among younger Israeli children during the Gulf War.

In summary, estimates in Table 3.2 and Table 3.4 reveal modest short-term and long-term detrimental effects of parental absences on children's academic attainment. Interaction specifications in Table 3.5 show that children with single parents, children with mothers in the Army, children with parents in the bottom of the AFQT distribution, and younger children are particularly vulnerable to parental absences.

3.7 Empirical Results for Work-Induced Relocations

This section evaluates the impact of work-induced relocations and follows a similar approach as the previous section. Unfortunately, I was unable to find a suitable instrumental variable to formally test the exogenous relocation assignment assumption. Therefore, I will refer the reader to Section 3.3 for discussion on the Army reassignment procedures, where I suggest that the "needs of the Army" is the driving force for military induced household relocations, and I briefly highlight a few issues here.

One possible threat to this assumption is that soldiers may submit a preference request for a duty location. Since Army posts are located throughout the world, there is little doubt that varying degrees of education quality exist across the spectrum of Army installations. It is possible that soldiers request assignments to place their children in better schools. However in this study, it is the number of relocations that serves as the source of variation. As long as the frequency of moves is uncorrelated with duty location preferences and the previous assignment locations, the identifying assumption still holds.

I draw additional support for the exogenous relocation assignment claim from Angrist and Johnson (2000). They assert, "the nature of duty assignments varies considerably, and families have little control over the timing of moves or the location of the next job."[53] This commonly held belief by both soldiers in the Army and credible researchers, coupled with the "needs of the Army" as the impetus for military-induced relocations, further substantiates the exogenous assignment claim.

In Table 3.6, I include summary statistics for the household relocation data. I present them in panel A by the rank of the parent. In Panel B, I further stratify rank by the *number of moves* the child has made since the parent has been in the military. Panel C, is stratified by the *average time on station*, or the average time a child spends at each location in years. Similar to the parental absence section, comparisons across the relocation categories reveal small correlations with a few descriptive variables.

Appendix Table 3.3 contains OLS regression estimates to assess the level of correlation between relocation categories and other observable characteristics. For most of the characteristic variables, the correlations are small and insignificant. Nevertheless, marital status of the parents, gender of the military parent, and grade level of the child has several notable correlations with the relocation categories. It is difficult to explain potential causes of the single parent and female parent in the Army correlations because they vary in sign and magnitude across the different relocation categories. However, the correlations with the grade level of the child are undoubtedly a result of child age. It is unlikely that a third grader will have moved more than five times. As in the parental absence section, I will conduct tests to ensure that these correlations are not driving the relocation results.

The main estimates for relocation effects are found in Table 3.7. Panels A and B contain OLS estimates for the impact of the number of household moves and panels C and D contain

[53] Angrist and Johnson (2000), pg 43. Also, interviews with Army soldiers support the author's own experiences with the random nature of assignments in the Army.

OLS estimates for the impact of the average time on station on children's test scores. Results for children of enlisted soldiers are found in panels A and C, while results for officers' children are found in panels B and D. For specifications in panels A and B, dummy variables for children having three to four moves and children having five or more moves comprise D_{it} (the omitted category is for children experiencing fewer than three moves). For specifications in panels C and D, a dummy variable for children averaging three or more years at each location comprises D_{it} from Equation (3.1). The estimates of δ are only modestly sensitive to the inclusion of additional control variables. Dropping all children who have a single parent or a mother in the Army has only minor effects on the point estimates in column (4).

The preferred specification for children of enlisted soldiers, in column (3) of panel A, indicates that children who move three to four times score .7-points lower than children who move less than three times. Furthermore, children who move more than five times score 1.5-points lower than children who move fewer than three times. These estimates reflect an increasingly negative relationship with each additional move, suggesting cumulative adverse relocation effects. Parallel estimates for officers are found in panel B. For all specifications, the point estimates are close to zero and insignificant

In panels C and D, I investigate how the length of time between relocations impacts children's academic attainment. Depending on when the soldier joined the military or during what part of the relocation cycle that a child was born, it is possible that children with the same number of moves may have averaged different amounts of time at each location. As seen in column (3) of panel C, children that spend an average of three or more years at each location score .74-points higher than children that spend less than three years on station. A longer average time on station may provide more stability for children, and improve their overall human capital development. These results could also suggest that children may be able to regain foregone human capital lost from the disruption of a move, if given enough time. Similar to panel B, there is no significant effect found for officers' children in panel D.

There is, however, a potentially confounding issue with regard to measuring relocation effects on standardized test scores. A common criticism of standardized tests is that students become more proficient at taking the exams over time. By default, the construction of the relocation variables allow for the possibility that children who have moved less or spent more average time in one location could also have spent more time in Texas. Consequently, these results may be picking up a *time in Texas* effect rather than a true relocation effect. For about sixty percent of the sample, I am able to establish the number of concurrent years that a child has taken the TAAS exams. Therefore, in Table 3.8, I include control variables for time in Texas in the standard relocation specifications to determine if they affect the main estimates for the number of moves and average time on station found in Table 3.7.

Table 3.8 is organized identically to Table 3.7. Column (1) contains the specification in column (3) from the corresponding panel in Table 3.7. In column (2), the sample contains only children with whom I am able to ascertain the number of TAAS exams taken. I test for a time in Texas effect in column (3) and I include estimates from specifications containing both the relocation variable and the time in Texas controls in column (4).

In panel A, comparisons of columns (1) and (2) reveal that the point estimate for three to four moves is about half the size of the estimate in column (1), while the point estimate for the five or more move category is only slightly changed by the reduced sample. Estimates in column (3) indicate a strong time in Texas effect. The second time children take the TAAS exam, they score 3.12-points higher than the first time they took the TAAS exam. By the third time children take the TAAS exam, they score 5.17-points higher than the first time they took the TAAS exam. Comparing estimates in column (4) to those in columns (2) and (3) reveals very little evidence of any significant correlations between the number of moves and the time in Texas. A similar pattern is found in panel B, even though there is no significant relocation effect for officers.

The estimates for average time on station in panels C and D are a bit more sensitive to the inclusion of time in Texas controls. This is not surprising since the construction of the average time on station variable also includes the time a child has spent in Texas. Therefore, the mechanical correlation of the average time on station variable with the time in Texas variable casts doubt on a causal interpretation for the average time on station estimates found in panels C and D of Table 3.7.

Although the results for time in Texas are empirically striking, they are difficult to interpret. One possibility may be that the time in Texas estimates are picking up an effect of children becoming more familiar with the TAAS exams over time. An alternative explanation could be that prior locations may have had lower quality educational programs relative to Texas schools, and therefore, these estimates reflect a pure increase in human capital development, or a catching up effect. Another possibility is that children may perform better on the test due to increased stability from remaining in one location longer. Unfortunately, untangling this issue is not possible with the data used in this study, but these results do raise important questions for future research.

Finally, Table 3.9 contains estimates from interacted specifications (as in Table 3.5) to determine how household relocation effects vary across different characteristics of children and their parents. I also include the average years on station as a robustness check on the estimates for the number of moves a child experiences. The interpretations of these findings are consistent with those discussed in the parental absence section. In panel A, children with fathers in the Army who move five or more times score 1.5-points lower than children who move less than three times. However, children with mothers in the Army who move five or more times score 2.6-points lower than children who move less than three times. Likewise, estimates in panel B reveal a .83-point difference in test scores between children of single parents and children of married parents experiencing five or more moves. In panel C, children with parents in the top 40 percent on the AFQT experience no significant effect with increased moves. Meanwhile children with parents in the bottom 60 percent on the AFQT score nearly 2-points lower when they move five or more times relative to children who move less than three times. Also, panel D shows that younger children perform worse than older children with more moves.

In general, this section provides evidence of modest adverse relocation effects. Children who move more frequently score increasingly worse on standardized test scores. Estimates of average time on station are consistent with these findings, however they are difficult to interpret because of the time in Texas effect. Also, children with single parents, children with mothers in the Army, children with parents in the bottom of the AFQT distribution, and younger children are especially susceptible to household relocations. [54]

3.8 Conclusion

Disruptions in the family take many forms. This paper investigates how parental absences and household relocations affect children's academic attainment. The recent trends in family

[54] In Appendix Table 3.3, I provide estimates from specifications that include both parental absence and household relocation variables in the same regression to test for possible confounding correlations. Estimates in columns (1) - (7) reject any concerning correlations. I also test for correlations between parental absence and time in Texas in columns (8) - (10) and find no evidence of a substantive correlation.

dynamics raise concern over parental investments in children's human capital development. An empirical investigation of this issue requires an exogenous source of variation. I provide informal and formal evidence that suggests military induced parental absences and household relocations are likely uncorrelated with other potential determinants of children's educational attainment.

Proceeding with OLS, I estimate a 1-point decline (one-tenth of a standard deviation for enlisted children) associated with current academic year parental absences and a larger effect (one-fifth of a standard deviation for officer's children) for cumulative four-year parental absences of more than seven months. Estimates for the effects of household relocations indicate that enlisted children who move more than five times score approximately 1.5-points (about one-tenth of a standard deviation) lower than enlisted children who move less than three times. In contrast, relocations do not affect officer's children. Other evidence suggests that parental absences and household relocations cause additional detrimental effects to children with single parents, children with mothers in the Army, children with parents in the bottom of the AFQT distribution, and younger children.

Even though the nature of military service may render these findings imperfectly applicable to the civilian population, they still motivate the need for future research in this area for several reasons. First, the quality of the future U.S. labor supply depends, in part, on understanding the extent to which these and other family stressors impact children's educational production. Second, even though the economic magnitude of the estimates presented here is small, the presence of cumulative effects for both parental absences and household relocations suggest the potential for considerable long-term effects. Finally, discerning the channels through which these effects occur is useful for establishing more effective policy solutions.

Table 3.1A: Summary Statistics for Current School-Year Parental Absences

	A. Parent's Rank		B. Parent "Deployed" ≥ 3 Months				C. Parent "Ever Gone" ≥ 3 Months			
	Enlisted Parent	Officer Parent	Enlisted Parent		Officer Parent		Enlisted Parent		Officer Parent	
			No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
TLI Math Score	77.14	82.47	77.21	75.91	82.56	80.91	77.24	76.28	82.57	81.35
	(0.11)	(0.18)	(0.11)	(0.50)	(0.18)	(0.83)	(0.12)	(0.39)	(0.18)	(0.69)
	{12.01}	{9.57}	{11.97}	{12.64}	{9.51}	{10.47}	{11.96}	{12.47}	{9.49}	{10.19}
Male Child	0.51	0.51	0.51	0.51	0.51	0.49	0.51	0.51	0.51	0.53
	(0.00)	(0.01)	(0.00)	(0.02)	(0.01)	(0.04)	(0.00)	(0.02)	(0.01)	(0.03)
	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}
White Child	0.44	0.79	0.44	0.47	0.79	0.75	0.44	0.45	0.79	0.76
	(0.00)	(0.01)	(0.00)	(0.02)	(0.01)	(0.03)	(0.00)	(0.02)	(0.01)	(0.03)
	{0.50}	{0.41}	{0.50}	{0.50}	{0.41}	{0.44}	{0.50}	{0.50}	{0.41}	{0.43}
Parents Married	0.90	0.93	0.90	0.98	0.93	0.97	0.90	0.96	0.93	0.98
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
	{0.30}	{0.26}	{0.30}	{0.15}	{0.26}	{0.16}	{0.30}	{0.19}	{0.26}	{0.13}
Male Parent in Army	0.87	0.91	0.87	0.98	0.90	1.00	0.87	0.97	0.90	1.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
	{0.33}	{0.29}	{0.34}	{0.12}	{0.30}	{0.00}	{0.34}	{0.17}	{0.30}	{0.00}
Army Parent a High School Graduate	0.45		0.44	0.57			0.44	0.52		
	(0.00)		(0.00)	(0.02)			(0.00)	(0.02)		
	{0.50}		{0.50}	{0.50}			{0.50}	{0.50}		
Army Parent has Some College	0.55	0.14	0.56	0.43	0.13	0.28	0.56	0.48	0.12	0.33
	(0.00)	(0.01)	(0.00)	(0.02)	(0.01)	(0.04)	(0.00)	(0.02)	(0.01)	(0.03)
	{0.50}	{0.34}	{0.50}	{0.50}	{0.33}	{0.45}	{0.50}	{0.50}	{0.33}	{0.47}
Army Parent AFQT (Top 40%)	0.27		0.28	0.25			0.28	0.26		
	(0.00)		(0.00)	(0.02)			(0.00)	(0.01)		
	{0.45}		{0.45}	{0.43}			{0.45}	{0.44}		
Elementary Age Child (Grades 3 - 6)	0.64	0.60	0.64	0.68	0.61	0.59	0.63	0.68	0.60	0.62
	(0.00)	(0.01)	(0.00)	(0.02)	(0.01)	(0.04)	(0.00)	(0.01)	(0.01)	(0.03)
	{0.48}	{0.49}	{0.48}	{0.47}	{0.49}	{0.49}	{0.48}	{0.47}	{0.49}	{0.49}
Observations	11,548	2,900	10,904	644	2,742	158	10,457	1,047	2,676	219

Standard errors are in parenthesis and standard deviations are in brackets. Observations in Panel A correspond with data in Panel B. Panel C has slightly fewer observations due to matching requirements. There are only 11,311 observations for AFQT in panels A and B and 11,284 observations for AFQT in panel C. The Army data is from the Office of Economic Manpower Analysis (West Point, NY). The data contains children ages 6-19 with a social security number and parents in the Active Duty Army stationed in Texas from 1996 to 1998. Dual military families are dropped. Absence variables are constructed using Army pay data over the current academic school year defined as 1 September of the previous year through 31 March of the current year. "Deployments" correspond to a soldier receiving hostile fire pay. A soldier is designated "ever gone" if the soldier received hostile fire pay or family separation pay during this time period. The number of months absent is determined by dividing the sum of hostile fire pay or family separation pay over the period by the monthly hostile fire pay or family separation pay allowance. Children's Math scores (range 0-100) are from the Texas Education Agency (TEA) testing years 1997 and 1998 in grades 3-8, and 10. Under advisement of the TEA, scores below 35 are dropped to account for children who did not take the exam seriously or quit in the middle of it (this accounts for .5% of the sample and does not affect the results). Army officers must have at least some college and they do not take the AFQT. This study separates AFQT quintiles into two groups: top 40% and bottom 60%.

Table 3.1B: Summary Statistics for Cumulative Four-Year Parental Absences

	A. Parent's Rank		B. Months "Deployed" of the Parent (past 4 years)						C. Months "Ever Gone" of the Parent (past 4 years)					
	Enlisted Parent	Officer Parent	Enlisted Parent			Officer Parent			Enlisted Parent			Officer Parent		
			0	1 - 6	7+	0	1 - 6	7+	0	1 - 6	7+	0	1 - 6	7+
	(1)	(2)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
TLI Math Score	77.28	82.53	77.41	77.17	76.05	82.73	82.38	80.59	77.47	77.55	76.60	83.17	82.28	80.89
	(0.11)	(0.18)	(0.13)	(0.32)	(0.43)	(0.20)	(0.51)	(0.71)	(0.17)	(0.20)	(0.22)	(0.23)	(0.35)	(0.45)
	{11.94}	{9.54}	{11.87}	{12.03}	{12.41}	{9.40}	{9.81}	{10.30}	{11.84}	{11.80}	{12.23}	{9.06}	{9.69}	{10.49}
Male Child	0.51	0.51	0.52	0.52	0.48	0.52	0.48	0.46	0.51	0.52	0.50	0.51	0.54	0.49
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}
White Child	0.44	0.79	0.43	0.50	0.40	0.78	0.81	0.76	0.44	0.46	0.40	0.81	0.76	0.75
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	{0.50}	{0.41}	{0.50}	{0.50}	{0.49}	{0.41}	{0.39}	{0.43}	{0.50}	{0.50}	{0.49}	{0.39}	{0.43}	{0.44}
Parents Married	0.91	0.93	0.89	0.96	0.96	0.91	0.99	0.99	0.88	0.93	0.92	0.91	0.96	0.96
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
	{0.29}	{0.26}	{0.31}	{0.19}	{0.21}	{0.28}	{0.12}	{0.12}	{0.32}	{0.26}	{0.28}	{0.29}	{0.19}	{0.19}
Male Parent in Army	0.89	0.91	0.87	0.98	0.96	0.88	0.99	1.00	0.86	0.91	0.92	0.86	0.96	0.98
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
	{0.31}	{0.29}	{0.33}	{0.15}	{0.19}	{0.32}	{0.07}	{0.00}	{0.34}	{0.28}	{0.28}	{0.34}	{0.19}	{0.15}
Army Parent a High School Graduate	0.44		0.41	0.56	0.51				0.38	0.48	0.48			
	(0.00)		(0.01)	(0.01)	(0.02)				(0.01)	(0.01)	(0.01)			
	{0.50}		{0.49}	{0.50}	{0.50}				{0.49}	{0.50}	{0.50}			
Army Parent has Some College	0.56	0.14	0.58	0.44	0.49	0.13	0.16	0.19	0.61	0.52	0.52	0.05	0.19	0.30
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
	{0.50}	{0.34}	{0.49}	{0.50}	{0.50}	{0.34}	{0.36}	{0.40}	{0.49}	{0.50}	{0.50}	{0.23}	{0.39}	{0.46}
Army Parent AFQT (Top 40%)	0.27		0.28	0.26	0.21				0.30	0.26	0.23			
	(0.00)		(0.00)	(0.01)	(0.01)				(0.01)	(0.01)	(0.01)			
	{0.44}		{0.45}	{0.44}	{0.41}				{0.46}	{0.44}	{0.42}			
Elementary Age Child (Grades 3 - 6)	0.63	0.61	0.63	0.66	0.66	0.62	0.56	0.58	0.61	0.65	0.64	0.60	0.62	0.61
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	{0.48}	{0.49}	{0.48}	{0.47}	{0.48}	{0.49}	{0.50}	{0.49}	{0.49}	{0.48}	{0.48}	{0.49}	{0.49}	{0.49}
Observations	11,032	2,873	8,841	1,373	818	2,294	366	213	4,619	3,399	3,027	1,567	753	550

Standard errors are in parenthesis and standard deviations are in brackets. Observations in Panel A correspond with data in Panel B. Panel C has slightly fewer observations due to matching requirements. There are only 10,891 observations for AFQT in panels A and B and 10,905 observations for AFQT in panel C. Absence variables are constructed using Army pay data starting from 31 March of the current academic school year and extending back 51 months. See notes in Table 3.1A for additional sample description.

Table 3.2: OLS and 2SLS Estimates for Current School-Year Parental Absences
Dependent Variable: Texas Learning Index Score for Mathematics

	A. Enlisted Parent "Deployed"						B. Officer Parent "Deployed"					
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Absent $\geq$ 3 Months	-0.914	-0.839	-1.123	-1.117	-1.133	-1.196	-1.573	-2.192	-1.437	-1.764	-1.231	-1.497
	(0.511)	(0.650)	(0.507)	(0.645)	(0.506)	(0.643)	(0.823)	(1.082)	(0.828)	(1.095)	(0.820)	(1.081)
Male Child			-0.784	-0.784	-0.766	-0.766			-0.202	-0.203	-0.228	-0.229
			(0.284)	(0.284)	(0.286)	(0.286)			(0.466)	(0.466)	(0.459)	(0.459)
White Child			3.470	3.470	2.687	2.687			3.658	3.654	3.437	3.434
			(0.283)	(0.283)	(0.303)	(0.303)			(0.597)	(0.597)	(0.597)	(0.597)
Parents Married			1.238	1.237	0.254	0.255			0.495	0.507	-0.012	-0.011
			(0.416)	(0.417)	(0.475)	(0.475)			(0.765)	(0.766)	(0.758)	(0.758)
Male Parent in Army					2.299	2.302					1.255	1.268
					(0.480)	(0.481)					(0.747)	(0.748)
Army Parent a High School Graduate					-0.952	-0.950						
					(0.294)	(0.294)						
Army Parent has Some College											-2.005	-1.989
											(0.639)	(0.636)
Army Parent AFQT (Top 40%)					2.213	2.213						
					(0.321)	(0.321)						
1st Stage		0.748		0.748		0.746		0.866		0.866		0.863
		(0.016)		(0.016)		(0.016)		(0.029)		(0.029)		(0.029)
Partial 1st Stage R^2		0.60		0.59		0.59		0.61		0.61		0.59
Observations	11,548	11,548	11,548	11,548	11,311	11,311	2,900	2,900	2,900	2,900	2,900	2,900
	C. Enlisted Parent "Ever Gone"						**D. Officer Parent "Ever Gone"**					
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Absent $\geq$ 3 Months	-0.787	-0.838	-0.914	-1.130	-0.936	-1.196	-1.254	-2.576	-1.185	-2.143	-0.868	-1.881
	(0.401)	(0.661)	(0.398)	(0.657)	(0.396)	(0.657)	(0.721)	(1.110)	(0.727)	(1.125)	(0.718)	(1.117)
1st Stage		0.735		0.733		0.729		0.856		0.857		0.843
		(0.016)		(0.016)		(0.016)		(0.028)		(0.028)		(0.028)
Partial 1st Stage R^2		0.37		0.36		0.36		0.45		0.44		0.42
Observations	11,504	11,504	11,504	11,504	11,284	11,284	2,895	2,895	2,895	2,895	2,895	2,895
Child: Gender, Race, and Married Parents	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Parent: Education and AFQT (Enlisted)	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998 and grades 4-8 and 10. Other controls added as indicated. Differences in sample sizes within a panel are a result of some enlisted soldiers missing AFQT scores. The instrument used in the 2SLS estimates is constructed from the unit of assignment. The unit is considered "deployed" when at least 1/3 of the unit is deployed during the academic school year. Hausman test statistics have the following p-values: Panel A: (1),(2)=.84, (3),(4)=.99, (5),(6)=.86. Panel B: (1),(2)=.32, (3),(4)=.60, (5),(6)=.67. Panel C: (1),(2)=.92, (3),(4)=.66, (5),(6)=.60. Panel D: (1),(2)=.08, (3),(4)=.19, (5),(6)=.18. See notes in Table 3.1A for additional sample description.

Table 3.3: Robustness Test: OLS Estimates for Current-School Year Parental Absences
Dependent Variable: Texas Learning Index Score for Mathematics

	A. Enlisted Parent "Deployed"		B. Officer Parent "Deployed"		C. Enlisted Parent "Ever Gone"		D. Officer Parent "Ever Gone"	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Absent $\geq$ 3 Months	-1.133 (0.506)	-1.082 (0.512)	-1.231 (0.834)	-1.173 (0.854)	-0.936 (0.396)	-0.887 (0.402)	-0.868 (0.730)	-0.789 (0.742)
Male Child	-0.766 (0.286)	-0.620 (0.312)	-0.228 (0.459)	-0.169 (0.495)	-0.770 (0.286)	-0.616 (0.312)	-0.184 (0.461)	-0.144 (0.496)
White Child	2.687 (0.303)	2.551 (0.329)	3.437 (0.597)	3.440 (0.667)	2.693 (0.304)	2.539 (0.329)	3.346 (0.597)	3.329 (0.666)
Parents Married	0.254 (0.475)		-0.012 (0.758)		0.244 (0.479)		0.100 (0.764)	
Male Parent in Army	2.299 (0.480)		1.255 (0.747)		2.302 (0.483)		1.247 (0.749)	
Army Parent a High School Graduate	-0.952 (0.294)	-0.949 (0.319)			-0.971 (0.294)	-0.968 (0.319)		
Army Parent has Some College			-2.005 (0.639)	-2.093 (0.647)			-1.960 (0.639)	-2.028 (0.650)
Army Parent AFQT (Top 40%)	2.213 (0.321)	2.342 (0.352)			2.223 (0.322)	2.349 (0.352)		
R^2 Observations	0.05 11,311	0.05 9,488	0.05 2,900	0.05 2,552	0.05 11,284	0.05 9,496	0.05 2,895	0.05 2,551
Sample contains only Children with Married/ Male Parents in Army	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998, and grades 4-8 and 10. Other controls added as indicated. Differences in sample sizes within a panel are a result of dropping children with single parents and mothers in the Army. See notes in Table 3.1A for additional sample description.

Table 3.4: OLS Estimates for Cumulative Four-Year Parental Absences
Dependent Variable: Texas Learning Index Score for Mathematics

	A. Enlisted Parent "Deployed"				B. Officer Parent "Deployed"			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Absent 1 - 6 Months	-0.401 (0.381)	-0.714 (0.377)	-0.633 (0.377)	-0.558 (0.389)	-0.289 (0.585)	-0.434 (0.580)	-0.482 (0.580)	-0.424 (0.590)
Absent $\geq$ 7 Months	-1.602 (0.515)	-1.593 (0.506)	-1.514 (0.504)	-1.368 (0.518)	-2.149 (0.819)	-2.126 (0.816)	-2.127 (0.819)	-2.095 (0.831)
Male Child		-0.785 (0.289)	-0.748 (0.289)	-0.653 (0.314)		-0.154 (0.468)	-0.181 (0.462)	-0.121 (0.498)
White Child		3.505 (0.287)	2.798 (0.307)	2.586 (0.331)		3.559 (0.593)	3.351 (0.596)	3.358 (0.669)
Parents Married		1.197 (0.427)	0.566 (0.496)			0.639 (0.770)	0.057 (0.770)	
Male Parent in Army			1.625 (0.513)				1.249 (0.750)	
Army Parent a High School Graduate			-0.778 (0.299)	-0.854 (0.323)				
Army Parent has Some College							-1.740 (0.641)	-1.797 (0.664)
Army Parent AFQT (Top 40%)			2.103 (0.325)	2.329 (0.355)				
R^2	0.02	0.05	0.05	0.05	0.02	0.05	0.05	0.05
Observations	11,032	11,032	10,891	9,339	2,873	2,873	2,873	2,527
	C. Enlisted Parent "Ever Gone"				D. Officer Parent "Ever Gone"			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Absent 1 - 6 Months	-0.126 (0.286)	-0.239 (0.283)	-0.089 (0.284)	0.057 (0.305)	-0.988 (0.459)	-0.829 (0.454)	-0.701 (0.462)	-0.671 (0.486)
Absent $\geq$ 7 Months	-1.074 (0.309)	-0.984 (0.305)	-0.845 (0.308)	-0.613 (0.330)	-2.368 (0.557)	-2.164 (0.550)	-1.881 (0.560)	-1.744 (0.577)
R^2	0.02	0.05	0.05	0.05	0.03	0.05	0.06	0.05
Observations	11,045	11,045	10,905	9,339	2,870	2,870	2,870	2,541
Child: Gender, Race, and Married Parents	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent: Education and AFQT (Enlisted)	No	No	Yes	Yes	No	No	Yes	Yes
Sample contains only Children with Married/ Male Parents in Army	No	No	No	Yes	No	No	No	Yes

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998 and grades 4-8 and 10. Other controls added as indicated. Differences in sample sizes within a panel are a result of some enlisted soldiers missing AFQT scores and dropping children with single parents and mothers in the Army where indicated. Parental months absent are the number of months absent out of the past 51 months. See notes in Table 3.1B for additional sample description.

Table 3.5: OLS Interacted Specifications for Current School-Year and Cumulative Parental Absences
Dependent Variable: Texas Learning Index Score for Mathematics

	"Deployed" ≥ 3 Months (1)	"Ever Gone" ≥ 3 Months (2)	"Deployed" 1 - 6 Months (3)	"Deployed" ≥ 7 Months (4)	"Ever Gone" 1 - 6 Months (5)	"Ever Gone" ≥ 7 Months (6)
A. Male Parent in the Army versus Female Parent in the Army						
Male Parent is in the Army	-1.111	-0.938	-0.594	-1.357	-0.022	-0.651
	(0.507)	(0.398)	(0.384)	(0.512)	(0.300)	(0.322)
Female Parent is in the Army	-2.478	-0.888	-1.570	-5.073	-0.456	-2.607
	(5.167)	(2.525)	(1.805)	(2.593)	(0.906)	(1.053)
B. Married Parent versus Single Parent						
Parent is Married	-1.026	-0.898	-0.573	-1.489	-0.028	-0.841
	(0.511)	(0.402)	(0.385)	(0.517)	(0.298)	(0.323)
Parent is Single	-5.513	-1.847	-2.075	-1.957	-0.772	-0.811
	(3.237)	(2.017)	(1.715)	(2.185)	(0.961)	(0.987)
C. Army Parent has High AFQT versus Army Parent has Low AFQT						
Parent in Top 40% of AFQT	-0.016	-0.235	-0.286	-0.925	0.006	-0.795
	(0.959)	(0.715)	(0.698)	(0.938)	(0.504)	(0.569)
Parent in Bottom 60% of AFQT	-1.503	-1.186	-0.754	-1.679	-0.126	-0.865
	(0.589)	(0.467)	(0.444)	(0.587)	(0.341)	(0.363)
D. Elementary Level versus Secondary Level Children						
Elementary Age Child (Grades 3-6)	-1.444	-1.166	-0.654	-1.858	0.032	-0.781
	(0.621)	(0.479)	(0.453)	(0.639)	(0.344)	(0.381)
Secondary Age Child (Grades 7, 8, and 10)	-0.478	-0.472	-0.599	-0.866	-0.301	-0.948
	(0.862)	(0.682)	(0.621)	(0.770)	(0.474)	(0.489)
Observations	11,311	11,284	10,891	10,891	10,905	10,905

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998, grades 4-8 and 10, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile. There are no officer results presented because officers do not have AFQT scores and the sample is too small to produce informative estimates. See notes in Tables 3.1A and 3.1B for additional sample description.

Table 3.6: Summary Statistics for Household Relocations

	A. Parent's Rank		B. Number of Moves						C. Avg. Time on Station (Years)			
	Enlisted Parent	Officer Parent	Enlisted Parent			Officer Parent			Enlisted Parent		Officer Parent	
			0 - 2	3 - 4	≥ 5	0 - 2	3 - 4	≥ 5	0 - 2	≥ 3	0 - 2	≥ 3
	(1)	(2)	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)
TLI Math Score	77.20	82.56	78.19	77.45	76.20	83.25	82.60	82.25	77.05	77.57	82.76	82.52
	(0.12)	(0.19)	(0.26)	(0.17)	(0.22)	(0.47)	(0.30)	(0.29)	(0.16)	(0.19)	(0.24)	(0.36)
	{11.95}	{9.60}	{11.36}	{11.87}	{12.35}	{9.33}	{9.86}	{9.41}	{11.94}	{11.94}	{9.39}	{9.88}
Male Child	0.52	0.52	0.48	0.52	0.52	0.51	0.50	0.54	0.52	0.51	0.53	0.50
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}	{0.50}
White Child	0.43	0.82	0.45	0.44	0.41	0.87	0.80	0.81	0.43	0.43	0.82	0.87
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	{0.49}	{0.39}	{0.50}	{0.50}	{0.49}	{0.34}	{0.40}	{0.39}	{0.49}	{0.49}	{0.38}	{0.33}
Parents Married	0.93	0.97	0.92	0.93	0.94	0.93	0.96	0.98	0.94	0.95	0.98	0.98
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	{0.25}	{0.18}	{0.28}	{0.25}	{0.23}	{0.26}	{0.19}	{0.12}	{0.24}	{0.21}	{0.15}	{0.13}
Male Parent in Army	0.92	0.95	0.84	0.92	0.95	0.85	0.96	0.98	0.92	0.95	0.98	0.96
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
	{0.28}	{0.21}	{0.37}	{0.27}	{0.22}	{0.36}	{0.19}	{0.13}	{0.27}	{0.22}	{0.15}	{0.19}
Army Parent a High School Graduate	0.45		0.56	0.44	0.40				0.46	0.43		
	(0.00)		(0.01)	(0.01)	(0.01)				(0.01)	(0.01)		
	{0.50}		{0.50}	{0.50}	{0.49}				{0.50}	{0.50}		
Army Parent has Some College	0.55	0.12	0.44	0.56	0.60	0.05	0.12	0.13	0.54	0.57	0.09	0.08
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	{0.50}	{0.32}	{0.50}	{0.50}	{0.49}	{0.22}	{0.33}	{0.34}	{0.50}	{0.50}	{0.29}	{0.27}
Army Parent AFQT (Top 40%)	0.25		0.26	0.27	0.22				0.24	0.24		
	(0.00)		(0.01)	(0.01)	(0.01)				(0.01)	(0.01)		
	{0.43}		{0.44}	{0.44}	{0.41}				{0.43}	{0.43}		
Elementary Age Child (Grades 3 - 6)	0.64	0.60	0.87	0.67	0.45	0.75	0.67	0.48	0.69	0.58	0.66	0.48
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
	{0.48}	{0.49}	{0.33}	{0.47}	{0.50}	{0.43}	{0.47}	{0.50}	{0.46}	{0.49}	{0.47}	{0.50}
Observations	10,206	2,502	1,965	5,023	3,218	389	1,093	1,020	5,593	3,989	1,492	734

Standard errors are in parenthesis and standard deviations are in brackets. Observations in Panel A correspond with data in Panel B. Panel C has slightly fewer observations due to matching requirements. There are only 10,121 observations for AFQT in panels A and B and 9,511 observations for AFQT in panel C. The Army data is from the Office of Economic Manpower Analysis (West Point, NY). The data contains children ages 6-19 with a social security number and parents in the Active Duty Army stationed in Texas from 1996 to 1998. Dual military families are dropped. The number of moves equals the number of moves that a child makes since the parent has been in the Army. The average time on station is constructed by dividing the number of years in the military by the number of military-induced moves that the child has made. Children's Math scores (range 0-100) are from the Texas Education Agency (TEA) testing years 1997 and 1998 in grades 3-8, and 10. Under advisement of the TEA, scores below 35 are dropped to account for children who did not take the exam seriously or quit in the middle of it (this accounts for .5% of the sample and does not affect the results). Army officers must have at least some college and they do not take the AFQT. This study separates AFQT quintiles into two groups: top 40% and bottom 60%.

Table 3.7: OLS Estimates for the Number of Household Relocations and Average Time on Station
Dependent Variable: Texas Learning Index Scores for Mathematics

	A. Enlisted Parent: Number of Moves				B. Officer Parent: Number of Moves			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
3 - 4 Moves	-0.376	-0.364	-0.703	-0.977	-0.344	-0.118	-0.194	-0.395
	(0.354)	(0.351)	(0.354)	(0.382)	(0.613)	(0.608)	(0.617)	(0.651)
$\geq$ 5 Moves	-1.237	-1.131	-1.487	-1.576	-0.210	-0.028	-0.104	-0.195
	(0.416)	(0.410)	(0.416)	(0.442)	(0.638)	(0.635)	(0.654)	(0.670)
Male Child		-0.739	-0.730	-0.647		-0.480	-0.491	-0.447
		(0.300)	(0.300)	(0.318)		(0.508)	(0.500)	(0.518)
White Child		3.348	2.533	2.515		3.775	3.648	3.635
		(0.299)	(0.323)	(0.334)		(0.700)	(0.699)	(0.720)
Parents Married		1.955	0.810			1.078	-0.064	
		(0.525)	(0.604)			(1.262)	(1.259)	
Male Parent in Army			2.410				2.210	
			(0.604)				(1.065)	
Army Parent a High School Graduate			-1.011	-0.978				
			(0.308)	(0.325)				
Army Parent has Some College							-2.007	-1.969
							(0.718)	(0.718)
Army Parent AFQT (Top 40%)			2.201	2.356				
			(0.346)	(0.360)				
R^2	0.02	0.05	0.05	0.05	0.02	0.04	0.05	0.05
Observations	10,206	10,206	10,121	9,038	2,502	2,502	2,502	2,355
	C. Enlisted Parent: Avg. Time on Station				D. Officer Parent: Avg. Time on Station			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average of $\geq$ 3 Years on Station	0.895	0.863	0.735	0.627	0.337	0.106	0.105	0.017
	(0.271)	(0.268)	(0.267)	(0.278)	(0.476)	(0.467)	(0.471)	(0.474)
R^2	0.02	0.04	0.05	0.05	0.02	0.04	0.05	0.05
Observations	9,582	9,582	9,511	8,722	2,226	2,226	2,226	2,152
Child: Male, White, Married Parents	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent: Male, Has Some College	No	No	Yes	Yes	No	No	Yes	Yes
Sample contains only Children with Married/ Male Parents in Army	No	No	No	Yes	No	No	No	Yes

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998 and grades 4-8 and 10. Other controls added as indicated. Differences in sample sizes within a panel are a result of some enlisted soldiers missing AFQT scores and dropping children with single parents and mothers in the Army where indicated. See notes in Table 3.6 for additional sample description.

Table 3.8: OLS Estimates for Household Relocations and Time in Texas
Dependent Variable: Texas Learning Index Scores for Mathematics

	A. Enlisted Parent: Number of Moves				B. Officer Parent: Number of Moves			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
3 - 4 Moves	-0.703 (0.354)	-0.481 (0.431)		-0.495 (0.428)	-0.194 (0.617)	0.039 (0.730)		0.115 (0.729)
$\geq$ 5 Moves	-1.487 (0.416)	-1.268 (0.522)		-1.036 (0.521)	-0.104 (0.654)	-0.150 (0.790)		-0.068 (0.786)
Child is Taking TAAS Exam for the 2nd Time			3.116 (0.411)	3.064 (0.412)			1.715 (0.563)	1.715 (0.565)
Child is Taking TAAS Exam for the 3rd Time			5.171 (0.562)	5.121 (0.563)			0.599 (1.049)	0.569 (1.043)
R^2	0.05	0.05	0.07	0.07	0.05	0.05	0.06	0.06
Observations	10,121	6,462	6,462	6,462	2,502	1,675	1,675	1,675
	C. Enlisted Parent: Avg. Time on Station				D. Officer Parent: Avg. Time on Station			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Average of $\geq$ 3 Years on Station	0.735 (0.267)	0.661 (0.336)		0.441 (0.335)	0.105 (0.471)	0.246 (0.595)		0.185 (0.592)
Child is Taking TAAS Exam for the 2nd Time			2.895 (0.421)	2.857 (0.422)			1.582 (0.576)	1.569 (0.572)
Child is Taking TAAS Exam for the 3rd Time			4.904 (0.576)	4.857 (0.577)			0.405 (1.106)	0.372 (1.105)
R^2	0.05	0.05	0.07	0.07	0.05	0.04	0.05	0.05
Observations	9,511	6,096	6,096	6,096	2,226	1,516	1,516	1,516

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998, grades 4-8 and 10, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile (enlisted only). The data used in Table 3.7 was merged by cells with a panel data set created specifically to test this. Cells consist of year, grades, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile (enlisted only), relocation status, and Math score. Identical cells were dropped (630) from the premerged test data. Since only data from 1997 and 1998 are used, a child can take the TAAS exam, at most, three times. Differences in sample size between column (1) and columns (2)-(4), is a result of matching with the duration data. All observations with a score prior to 1996 or have an initial score in 10th grade are dropped, since the length of time in Texas cannot otherwise be determined. See notes in Table 3.6 for additional sample description.

Table 3.9: OLS Interacted Specifications for Household Relocations
Dependent Variable: Texas Learning Index Score for Mathematics

	Moved 3 - 4 Times (1)	Moved ≥ 5 Times (2)	Average ≥ 3 Years on Station (3)
A. Male Parent in the Army versus Female Parent in the Army			
Male Parent is in the Army	-0.854	-1.476	0.632
	(0.374)	(0.432)	(0.275)
Female Parent is in the Army	0.675	-2.600	2.382
	(1.029)	(1.439)	(1.096)
B. Married Parent versus Single Parent			
Parent is Married	-0.770	-1.450	0.726
	(0.369)	(0.427)	(0.275)
Parent is Single	0.130	-2.282	0.896
	(1.174)	(1.434)	(1.169)
C. Army Parent has High AFQT versus Army Parent has Low AFQT			
Parent in Top 40% of AFQT	-0.698	-0.121	0.584
	(0.606)	(0.698)	(0.482)
Parent in Bottom 60% of AFQT	-0.698	-1.891	0.782
	(0.422)	(0.481)	(0.316)
D. Elementary Level versus Secondary Level Children			
Elementary Age Child (Grades 3-6)	-0.636	-1.979	1.192
	(0.379)	(0.486)	(0.327)
Secondary Age Child (Grades 7, 8, and 10)	-0.530	-0.694	-0.040
	(0.920)	(0.927)	(0.433)
Observations	10,121	10,121	9,511

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998, grades 4-8 and 10, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile. There are no officer results presented because officers do not have AFQT scores and the sample is too small to produce informative estimates. See notes in Table 3.6 for additional sample description.

Appendix Table 3.1: Covariate Correlations With Current School-Year Parental Absences
Dependant Variable: Dummy Variable for Absent During Period

	Enlisted Mean {st.dev.}	Officer Mean {st.dev.}	A. "Deployed" ≥ 3 Months Enlisted (1)	A. "Deployed" ≥ 3 Months Officer (2)	B. "Ever Gone" ≥ 3 Months Enlisted (1)	B. "Ever Gone" ≥ 3 Months Officer (2)	C. One-Third of Unit Absent ≥ 3 Months Enlisted (1)	C. One-Third of Unit Absent ≥ 3 Months Officer (2)
Male Child	0.515	0.513	0.000	-0.003	-0.001	0.005	-0.002	-0.004
	{0.500}	{0.500}	(0.004)	(0.008)	(0.005)	(0.010)	(0.004)	(0.007)
White Child	0.439	0.788	0.004	-0.012	-0.003	-0.006	0.002	-0.025
	{0.496}	{0.409}	(0.005)	(0.010)	(0.006)	(0.011)	(0.005)	(0.010)
Parents Married	0.902	0.928	0.021	0.003	0.024	0.016	0.020	-0.004
	{0.297}	{0.258}	(0.005)	(0.013)	(0.007)	(0.014)	(0.005)	(0.013)
Male Parent in Army	0.872	0.908	0.043	0.051	0.066	0.060	0.050	0.045
	{0.334}	{0.290}	(0.004)	(0.008)	(0.005)	(0.008)	(0.004)	(0.008)
Army Parent High School Grad	0.448		0.022		0.019		0.024	
	{0.497}		(0.004)		(0.006)		(0.005)	
Army Parent has Some College	0.549	0.137		0.061		0.119		0.056
	{0.498}	{0.344}		(0.016)		(0.019)		(0.015)
Army Parent AFQT (Top 40%)	0.275		-0.004		0.001		0.001	
	{0.446}		(0.005)		(0.006)		(0.005)	
Child in 4th Grade	0.169	0.156	0.000	-0.001	-0.004	-0.022	-0.009	0.001
	{0.375}	{0.363}	(0.008)	(0.015)	(0.010)	(0.017)	(0.008)	(0.014)
Child in 5th Grade	0.160	0.150	-0.007	0.000	-0.010	0.004	-0.010	-0.005
	{0.366}	{0.357}	(0.008)	(0.015)	(0.010)	(0.018)	(0.008)	(0.013)
Child in 6th Grade	0.152	0.152	-0.012	0.011	-0.023	0.006	-0.015	0.005
	{0.359}	{0.359}	(0.008)	(0.016)	(0.010)	(0.019)	(0.008)	(0.014)
Child in 7th Grade	0.141	0.138	-0.012	0.002	-0.018	-0.009	-0.015	0.005
	{0.348}	{0.345}	(0.008)	(0.016)	(0.010)	(0.018)	(0.008)	(0.014)
Child in 8th Grade	0.125	0.136	-0.016	0.009	-0.028	-0.012	-0.018	0.007
	{0.331}	{0.342}	(0.008)	(0.016)	(0.010)	(0.018)	(0.009)	(0.015)
Child in 10th Grade	0.096	0.122	-0.014	0.000	-0.022	-0.010	-0.022	0.004
	{0.294}	{0.327}	(0.009)	(0.016)	(0.011)	(0.018)	(0.009)	(0.015)
Intercept			0.029	0.010	0.046	-0.002	0.040	0.029
			(0.007)	(0.015)	(0.011)	(0.017)	(0.008)	(0.014)
R^2			0.03	0.02	0.02	0.03	0.04	0.02
Observations	11,311	2,900	11,311	2,900	11,284	2,895	11,311	2,900

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). Standard deviations are in brackets for characteristic means. All regressions contain a year dummy for the year 1998. Means and standard deviations are calculated using data from panel A and C. See Table 3.1A for means and standard deviations for panel B estimates. See notes in Table 3.1A for sample description.

Appendix Table 3.2: Covariate Correlations With Cumulative Four-Year Parental Absences
Dependant Variable: Dummy Variable for Absent During Period

	Enlisted Mean {st.dev.}	Officer Mean {st.dev.}	A. "Deployed" 1 - 6 Months Enlisted (1)	A. "Deployed" 1 - 6 Months Officer (2)	B. "Deployed" ≥ 7 Months Enlisted (1)	B. "Deployed" ≥ 7 Months Officer (2)	C. "Ever Gone" 1 - 6 Months Enlisted (1)	C. "Ever Gone" 1 - 6 Months Officer (2)	D. "Ever Gone" ≥ 7 Months Enlisted (1)	D. "Ever Gone" ≥ 7 Months Officer (2)
Male Child	0.514	0.513	0.005	-0.018	-0.010	-0.017	0.014	0.030	-0.014	-0.015
	{0.500}	{0.500}	(0.007)	(0.014)	(0.006)	(0.011)	(0.010)	(0.018)	(0.010)	(0.017)
White Child	0.435	0.785	0.024	0.012	-0.012	-0.013	0.030	-0.043	-0.033	-0.034
	{0.496}	{0.411}	(0.008)	(0.017)	(0.006)	(0.014)	(0.011)	(0.023)	(0.011)	(0.021)
Parents Married	0.907	0.927	0.031	0.043	0.019	0.022	0.065	0.038	-0.008	-0.007
	{0.291}	{0.260}	(0.010)	(0.017)	(0.007)	(0.012)	(0.018)	(0.036)	(0.018)	(0.027)
Male Parent in Army	0.893	0.906	0.082	0.110	0.042	0.071	0.020	0.139	0.068	0.134
	{0.310}	{0.292}	(0.008)	(0.013)	(0.006)	(0.008)	(0.017)	(0.030)	(0.017)	(0.019)
Army Parent High School Grad	0.437		0.053		0.018		0.036		0.041	
	{0.496}		(0.008)		(0.006)		(0.010)		(0.010)	
Army Parent has Some College	0.559	0.137		0.009		0.026		0.098		0.243
	{0.497}	{0.344}		(0.020)		(0.019)		(0.030)		(0.030)
Army Parent AFQT (Top 40%)	0.271		-0.008		-0.014		-0.018		-0.033	
	{0.444}		(0.009)		(0.007)		(0.012)		(0.012)	
Child in 4th Grade	0.167	0.156	-0.008	0.033	0.015	-0.010	0.000	0.019	0.009	-0.033
	{0.373}	{0.363}	(0.010)	(0.019)	(0.008)	(0.016)	(0.014)	(0.027)	(0.013)	(0.023)
Child in 5th Grade	0.162	0.149	-0.014	0.033	0.011	-0.012	0.010	0.014	-0.012	-0.023
	{0.368}	{0.356}	(0.012)	(0.022)	(0.009)	(0.018)	(0.016)	(0.030)	(0.015)	(0.027)
Child in 6th Grade	0.152	0.155	-0.027	0.023	0.003	-0.014	-0.035	-0.005	-0.020	-0.030
	{0.359}	{0.362}	(0.012)	(0.022)	(0.009)	(0.018)	(0.016)	(0.029)	(0.015)	(0.026)
Child in 7th Grade	0.142	0.138	-0.016	0.065	0.000	0.007	-0.020	0.023	-0.012	-0.030
	{0.349}	{0.345}	(0.012)	(0.024)	(0.009)	(0.020)	(0.016)	(0.031)	(0.016)	(0.028)
Child in 8th Grade	0.127	0.134	-0.031	0.038	-0.005	-0.005	-0.043	-0.005	-0.027	-0.025
	{0.333}	{0.341}	(0.012)	(0.023)	(0.009)	(0.019)	(0.017)	(0.031)	(0.016)	(0.028)
Child in 10th Grade	0.097	0.121	-0.015	0.032	0.010	-0.012	-0.038	-0.047	0.002	-0.027
	{0.296}	{0.327}	(0.013)	(0.023)	(0.010)	(0.019)	(0.018)	(0.030)	(0.018)	(0.028)
Intercept			-0.003	-0.051	0.008	0.008	0.215	0.008	0.222	0.087
			(0.012)	(0.020)	(0.010)	(0.019)	(0.021)	(0.019)	(0.021)	(0.034)
R^2			0.02	0.02	0.01	0.01	0.01	0.02	0.01	0.06
Observations	10,891	2,873	10,891	2,873	10,891	2,873	10,905	2,870	10,905	2,870

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). Standard deviations are in brackets for characteristic means. All regressions contain a year dummy for the year 1998. Means and standard deviations are calculated using data from panels A and B. See Table 3.1B for means and standard deviations for data in panels C and D. See notes in Table 3.1B for sample description.

Appendix Table 3.3: Covariate Correlations With Household Relocations
Dependant Variable: Dummy Variable for Household Relocation Effect

	Enlisted Mean {st.dev.}	Officer Mean {st.dev.}	A. Moved 3 - 4 times Enlisted (1)	A. Moved 3 - 4 times Officer (2)	B. Moved ≥ 5 times Enlisted (1)	B. Moved ≥ 5 times Officer (2)	C. Avg. ≥ 3 Years Enlisted (1)	C. Avg. ≥ 3 Years Officer (2)
Male Child	0.516 {0.500}	0.517 {0.500}	0.009 (0.012)	-0.024 (0.023)	0.017 (0.010)	0.030 (0.022)	-0.002 (0.012)	-0.023 (0.022)
White Child	0.428 {0.495}	0.818 {0.386}	0.003 (0.013)	-0.040 (0.030)	-0.016 (0.011)	-0.006 (0.028)	-0.007 (0.013)	0.082 (0.028)
Parents Married	0.932 {0.251}	0.966 {0.182}	-0.023 (0.026)	-0.173 (0.065)	-0.043 (0.022)	0.083 (0.051)	0.026 (0.028)	0.243 (0.086)
Male Parent in Army	0.918 {0.274}	0.953 {0.211}	0.044 (0.024)	0.181 (0.060)	0.159 (0.019)	0.211 (0.044)	0.072 (0.026)	-0.285 (0.087)
Army Parent a High School Grad	0.446 {0.497}		-0.012 (0.012)		-0.042 (0.010)		-0.020 (0.012)	
Army Parent has Some College	0.554 {0.497}	0.116 {0.320}		0.031 (0.035)		0.048 (0.034)		-0.043 (0.036)
Army Parent AFQT (Top 40%)	0.249 {0.432}		0.042 (0.014)		-0.051 (0.013)		0.001 (0.015)	
Child in 4th Grade	0.169 {0.375}	0.159 {0.366}	0.063 (0.015)	0.020 (0.032)	0.086 (0.011)	0.066 (0.028)	0.012 (0.016)	0.041 (0.029)
Child in 5th Grade	0.162 {0.369}	0.150 {0.357}	0.089 (0.017)	-0.044 (0.037)	0.173 (0.013)	0.177 (0.033)	0.012 (0.018)	0.024 (0.034)
Child in 6th Grade	0.153 {0.360}	0.153 {0.360}	0.062 (0.018)	-0.048 (0.036)	0.210 (0.014)	0.238 (0.034)	0.035 (0.018)	0.010 (0.034)
Child in 7th Grade	0.141 {0.348}	0.135 {0.342}	0.023 (0.018)	-0.135 (0.039)	0.313 (0.015)	0.341 (0.035)	0.107 (0.019)	0.123 (0.037)
Child in 8th Grade	0.127 {0.333}	0.139 {0.347}	-0.037 (0.019)	-0.135 (0.037)	0.404 (0.016)	0.320 (0.035)	0.067 (0.019)	0.145 (0.036)
Child in 10th Grade	0.095 {0.293}	0.122 {0.327}	-0.030 (0.020)	-0.146 (0.039)	0.422 (0.018)	0.340 (0.036)	0.239 (0.021)	0.330 (0.039)
Intercept			0.445 (0.027)	0.562 (0.070)	0.029 (0.022)	-0.113 (0.055)	0.280 (0.029)	0.245 (0.084)
R^2			0.01	0.03	0.11	0.08	0.02	0.06
Observations	10,121	2,502	10,121	2,502	10,121	2,502	9,511	2,226

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). Standard deviations are in brackets for characteristic means. All regressions contain a year dummy for the year 1998. Means and standard deviations are calculated using data from panels A and B. See Table 3.6 for means and standard deviations of data in panel C. See notes in Table 3.6 for sample description.

Appendix Table 3.4: Specification Test for Parental Absence and Household Relocation Correlations
Dependent Variable: Texas Learning Index Score for Mathematics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	A. Enlisted Parent "Deployed"									
Absent ≥ 3 Months	-1.133 (0.507)	-0.592 (0.605)		-0.642 (0.605)	-0.260 (0.662)		-0.265 (0.664)	0.113 (0.681)		0.017 (0.673)
3 - 4 Moves			-0.561 (0.352)	-0.559 (0.352)						
≥ 5 Moves			-1.204 (0.412)	-1.212 (0.412)						
Average of ≥ 3 Years on Station						0.716 (0.264)	0.716 (0.264)			
Child is Taking TAAS Exam for the 2nd Time									3.073 (0.389)	3.073 (0.389)
Child is Taking TAAS Exam for the 3rd Time									4.776 (0.535)	4.775 (0.535)
R^2	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.07	0.07
Observations	11,311	10,306	10,306	10,306	9,686	9,686	9,686	7,178	7,178	7,178
	B. Enlisted Parent "Ever Gone"									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Absent ≥ 3 Months	-0.936 (0.396)	-0.850 (0.490)		-0.846 (0.489)	-0.518 (0.553)		-0.459 (0.554)	-0.319 (0.521)		-0.356 (0.515)
3 - 4 Moves			-0.548 (0.356)	-0.542 (0.355)						
≥ 5 Moves			-1.290 (0.419)	-1.287 (0.418)						
Average of ≥ 3 Years on Station						0.654 (0.268)	0.645 (0.268)			
Child is Taking TAAS Exam for the 2nd Time									3.106 (0.390)	3.105 (0.390)
Child is Taking TAAS Exam for the 3rd Time									4.775 (0.535)	4.780 (0.535)
R^2	0.05	0.05	0.05	0.06	0.05	0.06	0.06	0.06	0.07	0.07
Observations	11,284	10,089	10,089	10,089	9,415	9,415	9,415	7,156	7,156	7,156

Standard errors in parenthesis account for clustering at the cell level (the combination of characteristic variables, some children appear in both years). All regressions contain a constant and dummies for the year 1998, grades 4-8 and 10, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile (enlisted only). The data used in Table 3.7 was merged by cells with a panel data set created specifically for this test. Cells consist of year, grades, child's gender, child's race, parent's marital status, military parent's gender, military parent's education level, and military parent's AFQT quartile (enlisted only), parental absence status, and Math score. Identical cells were dropped (630) from the premerged test data. Since only data from 1997 and 1998 are used, a child can take the TAAS exam, at most, three times. Differences in sample sizes is a result of matching with the duration data. All observations with a score prior to 1996 or have an initial score in 10th grade are dropped, since the length of time in Texas cannot otherwise be determined. See notes in Tables 3.1A and 3.6 for additional sample description.

Chapter 4: Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Mid-Century

[55]Co-authored with Daron Acemoglu (MIT) and David Autor (MIT)

"In May, 1947, 31.5 percent of all women 14 years of age and over were workers as compared to 27.6 percent in 1940. This proportion has been increasing for many decades. However, both the number and the proportion of women working in 1947 are believed to be greater than would have been expected in this year had there not been a war." Constance Williams, Chief of the Research Division of the Women's Bureau of the United States Department of Labor, 1949.

4.1 Introduction

In 1900, 82 percent of U.S. workers were male, and only 18 percent of women over the age of 15 participated in the labor force. As is visible in Figure 4.1, this picture changed radically over the course of the past century. In 2001, 47 percent of U.S. workers were women, and 61 percent of women 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 labor market.

- Does it increase or decrease male wages?
- Does it adversely affect female wages?
- Does it impact male wage inequality?

The relative scarcity of convincing studies on this topic reflects the complexity of the phenomenon: increased labor participation of women is driven both by 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

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impacts the labor market, 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 leaving for overseas. As is depicted in Figure 4.2, 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.[56] 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 20th century.

Although this aggregate increase in female labor force participation is evident from Figures 4.1 and 4.2, it is not particularly useful for our purposes; 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 males 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 differences that are plausibly exogenous to other labor market outcomes, to study women's labor supply.

Panels A and B of Figure 4.3 show that women worked substantially more in 1950—*but* not in 1940—in states with greater mobilization of men during the War (see below for the exact definition of the mobilization variable). Our baseline estimates suggest that women worked on average about 1 week more in a state that had a 10 percentage point higher mobilization rate during WWII, corresponding to a 9 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

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

in occupational structure, regional trends in labor supply, or contrasts between Southern and non-Southern states. We interpret these cross-state changes in female employment as 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 Panel C of Figure 4.3 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.

Panel A of Figure 4.4 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. Panel B 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 Figure 4.4 as the causal effect of the WWII-induced increase in female labor supply on female and male wages. As Figure 4.2 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:

1. High- and low-mobilization states may be different in other unobserved dimensions. In that case, Figure 4.3 and 4.4 may be capturing the effect of these unobserved characteristics on labor market outcomes, and our identification strategy may be assigning the effects of these unobserved state characteristics on labor market outcomes to female labor supply.

2. Mobilization of men for war may have had a direct effect on the labor market, distinct from its impact through female labor supply. For example, men who served in the War may have had difficulty reintegrating into the workforce, or may have entered school instead due to the opportunities offered by the GI Bills (Bound and Turner, 1999; Stanley, 1999). If this were the case, our first-stage finding of a relationship between mobilization and female labor supply would remain valid, but our two-stage least square (2SLS) estimates would be biased: greater female labor force participation

in high-mobilization states could reflect greater demand for female labor input rather than shifts in female labor supply.

Although we cannot dismiss these two interpretations entirely, 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 fraction of farmers before the War, racial, education, 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, but should plausibly have no direct effect on female labor supply growth 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, as Panel C of Figure 4.3 documents, high-mobilization states did not experience faster growth in female employment between 1950 and 1960. Hence, there do not appear to be differential state employment trends correlated with WWII mobilization rates.

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 high-mobilization states. We find that this is generally not the case. Men who were *not* mobilized appear to participate slightly more after the War in high-mobilization states. And post-war labor supply of WWII veterans in 1950 is, for the most part, comparable across high and low-mobilization states, though some specifications do show negative but insignificant effects. 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, Figure 4.4 shows 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, Figure 4.5 shows 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 a range of labor market outcomes. Our main findings are:

1. Greater female labor supply reduces female wages. A 10 percent increase in relative female labor supply (that is, relative to males) lowers female wages by 6 to 7 percent, implying a labor demand elasticity of -1.4 to -1.7.

2. Greater female labor supply also reduces male wages. A 10 percent increase in the (log) ratio of female to male labor supply typically lowers male earnings by 3 to 5 percent.

3. The finding that female labor supply lowers women's wages by more than men's indicates that male and female labor inputs are imperfect substitutes. We estimate that a 10 percent increase in relative supply reduces relative female/male earnings by about 3 percentage points. This implies a substitution elasticity of approximately 3, which is large but far from perfect substitutability.

4. The impact of female labor supply on male earnings is not uniform throughout the male earnings distribution. A 10 percent increase in female relative labor supply raises earnings inequality between college and high school graduate males by about 1.5 percentage points and *lowers* earnings inequality between high school graduate and eight-grade males by about 2 percentage points. These findings indicate that the women drawn into the labor market by the War were closer substitutes to males at the middle of the skill distribution than those with either the lowest or highest education.

5. Although female labor supply has countervailing effects on educational differentials, its net impact on overall and residual earnings inequality among males is positive. A 10 percent increase in female labor supply is estimated to increase the male 90-10 weekly earnings differential by 5.5 log points, which is a very sizable effect.

It is important to note that 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. Results exploiting changes between 1940 and 1960 suggest that long-run elasticities are indeed larger than short-run elasticities.

The economics literatures on the effect of WWII on female participation and the effect of female labor supply on the structure of wages contains a small number of well-known contributions. 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 it difficult to make exact comparisons. Mulligan (1998) investigates the causes of the increase in labor supply during the War, and concludes that non-pecuniary 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, nor the causal effect of the induced change in female labor supply on labor market outcomes of men.[57]

Blau and Kahn (1999), Juhn and Kim (1999), Topel (1994 and 1997) and the short papers by Fortin and Lemieux (2000) and Welch (2000) also investigate the effect of female employment growth on male wage inequality.[58] Using Current Population Survey data from 1968-1990, Topel (1994) finds a strong positive correlation between regional changes in female labor supply and growth in male earnings inequality. By contrast, Juhn and Kim (1999) do not find a sizable effect of female labor supply on male wage inequality in a cross-state Census panel. Fortin and Lemieux hypothesize that the increase in male wage inequality during the past several decades may reflect the process of women substituting for males in the earnings distribution, and provide time-series evidence on the correlations between percentiles of the male and female wage distribution that are consistent with this hypothesis. Finally, Welch (2000) links both the decline in the wages of low-skill men and

[57] An unpublished dissertation by Dresser (1994) studies the relationship between federal war contracts and labor market participation of women across metropolitan areas and finds that MSAs that had a relatively large number of war contracts during the war experienced differential increases in female labor force participation between 1940 and 1950.

[58] 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 and 1994), Blau, Ferber and Winkler (2002), Blau and Kahn (1994, 1997 and 2000), and O'Neill and Polachek (1993).

the narrowing of the male-female wage gap over the past three decades to the overall increase in the demand for skills. While each of these analyses reaches provocative, albeit divergent, conclusions, the identification strategies used do not provide a means to separate supply- and demand-induced changes in female employment. What distinguishes our analysis is the use of plausibly exogenous variation in female labor market participation induced by World War II mobilization.

In the next section, 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 4.3 describes our microdata and documents the correlation between female employment and a range of female and male labor market outcomes. In Section 4.4, we provide a brief overview of the draft and enlistment process for World War II, and explain the causes of the substantial differences in mobilization rates across states. Section 4.5 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. Sections 4.6 and 4.7 contain our main results. They exploit cross-state differences in female labor supply induced by mobilization rates to estimate the impact of increased female labor supply on female wages and male wages, educational inequality, and overall earnings inequality among males. Section 4.8 concludes.

4.2 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. Consider a competitive labor market consisting of four factors: high-skill males, H_t, low-skill males, L_t, females, F_t, and capital, K_t, which stands for all nonlabor inputs.[59] Imagine that all these factors are imperfectly substitutable in the production of a single final good. In particular, to fix ideas, consider the following nested CES (constant elasticity of substitution) aggregate production function, where, to simplify notation, we ignore the share parameters:

$$Y_t = A_t K_t^{1-\alpha}\left[\left(B_t^L L_t\right)^{\eta} + \left(\left(B_t^F F_t\right)^{\rho} + \left(B_t^H H_t\right)^{\rho}\right)^{\eta/\rho}\right]^{\alpha/\eta},$$

where A_t is a neutral productivity term, and the B_t's are factor-augmenting productivity terms, which are for now taken as exogenous. In particular, B_t^F is an index of female

[59]We do not distinguish between high- and low-skill females both to reduce the number of factors and because, in the empirical work, we will only have a source of exogneous variation in the total number (or total efficiency units) of females in the labor force.

productivity, which may reflect observed or unobserved components of female human capital as well as technical change favoring women relative to men. This specification assumes that the elasticity of substitution between the labor aggregate and nonlabor inputs is equal to 1, the elasticity of substitution between female labor and high-skill male labor is $1/(1-\rho)$, and the elasticity of substitution between low-skill male labor and the aggregate between female and high-skill male labor is $1/(1-\eta)$. When $\eta > \rho$, female labor competes more with low-skill male labor than high-skill male labor, whereas when $\eta < \rho$, it competes more with high-skill male labor. This nested CES is similar to the one used by Krusell et al. (2000) with high-skill and low-skill labor, and equipment capital.

In this model, the wage ratio of high-skill to low-skill male wages, corresponding to the skill or education premium, is a natural index of male wage inequality. Since in competitive labor markets, wages are equal to marginal products, this ratio will be

$$\omega_t \equiv \frac{w_t^h}{w_{lt}^l} = \frac{B_t^H}{B_t^L} \frac{\left(B_t^H H_t\right)^{\rho-1} \left(\left(B_t^F F_t\right)^{\rho} + \left(B_t^H H_t\right)^{\rho}\right)^{(\eta-\rho)/\rho}}{\left(B_t^L L_t\right)^{\eta-1}}$$

It is then straightforward to show that

$$\text{Sign}\left\langle \frac{\partial \omega_t}{\partial B_t^F F_t} \right\rangle = \text{Sign}\left\langle \eta - \rho \right\rangle,$$

that is, an increase in effective female labor supply increases male wage inequality when women compete more with low-skill males than with high-skill males, i.e., when $\eta > \rho$. If female labor has traditionally been a closer substitute to low-skill male labor than high-skill male labor as argued by Grant and Hamermesh (1981) and Topel (1994 and 1997) among others, we may expect increased female labor force participation to act as a force towards greater wage inequality among men. The empirical magnitude of this effect is unclear, however.

The effect of increased female labor force participation on average wages of men, or on the level of high-skill and low-skill wages, depends upon the elasticity of supply of nonlabor inputs. It is straightforward to verify that if these nonlabor inputs are supplied elastically (or if $\alpha = 1$), increased female labor supply will always raise average male wages. If, on the other hand, the supply of nonlabor inputs to the economy is upward sloping, the effect is ambiguous, and depends on the elasticity of substitution between male and female labor and on the response of the rental price of these nonlabor inputs to the increase in female labor supply.

More explicitly, low-skill male wages are

$$w_t^l = \alpha A_t K_t^{1-\alpha} \left[\left(B_t^L L_t\right)^\eta + \left(\left(B_t^F F_t\right)^\rho + \left(B_t^H H_t\right)^\rho \right)^{\eta/\rho} \right]^{(\alpha-\eta)/\eta} \left(B_t^L L_t\right)^{\eta-1}.$$

Holding the supply of nonlabor inputs fixed, we have that

$$\text{Sign} \left\langle \frac{\partial w_t^l}{\partial B_t^F F_t} \right\rangle = \text{Sign} \left\langle \alpha - \eta \right\rangle. \tag{4.1}$$

In other words, when the elasticity of substitution parameter, η, is sufficiently high relative to the share of labor in production, α, an increase in female labor supply will reduce the (conditional) demand for and the earnings of low-skill males. In what follows, we will loosely refer to female and (a particular type of) male labor as "close substitutes" when greater female employment reduces the wages of that type of male labor, since a greater level of η (i.e., a greater elasticity of substitution) makes such negative wage effects more likely.

The same trade-offs determine whether average male wages increase or decline in response to increased female labor force participation. Substitution of female labor for male labor, by reducing the ratio of nonlabor inputs to effective labor, acts to depress earnings, while the complementarity between female and male labor raises the earnings of men. The overall effect will be determined by which of these two forces is stronger.

Can we use this framework to interpret the relationship between female labor supply and wages at the state level in the aftermath of WWII? There are at least three caveats that apply:

1. This interpretation requires U.S. states to approximate separate labor markets. This may be problematic if either migration makes the entire U.S. a single labor market, or if local labor markets are at the city or MSA level. Both problems would cause attenuation, which does not bias instrumental-variables estimates, but makes them less precise. For example, in the extreme case where migration is free and rapid, there would be no systematic 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), while others document significant wage differences across state or city labor markets (e.g., Topel, 1994, Acemoglu and Angrist, 2000, Moretti, 2000, Bernard, Jensen and Schott, 2001, Hanson and Slaughter, 2002, forthcoming).Our results also show substantial differences in relative employment and wages across states related to WWII mobilization.

2. 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.[60]

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

In light of all these caveats, the elasticities we estimate in this paper should be interpreted as short-run elasticities (except when we look at the two-decade change between 1940 and 1960). The majority of the estimates exploit 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.

4.3 Data Sources and OLS Estimates

4.3.1 Data

Our basic data come from the one-percent Integrated Public Use Microsamples (IPUMS) of the decennial Censuses. Samples include males and females ages 14-64 in the year for which earnings are reported who are not residing in institutional groups quarters (such as prisons or barracks), are not employed in farming, and who reside in the continental United States. Throughout the paper, we exclude Alaska, Hawaii, Washington D.C. and Nevada from the analysis. Alaska and Hawaii were not states until the 1950s, while Nevada underwent substantial population changes during the critical period of our analysis.[61] The 1950 sample is further limited to the sample-line subsample because educational attainment is not reported

[60] 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.

[61] 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

in the full sample. Sampling weights are employed in all calculations. Earnings samples include all full-time, full-year workers in paid non-farm employment excluding self-employed who earned the equivalent of $0.50 to $250 an hour in 1990 dollars during the previous year (deflated by CPI All Urban Consumers series CUUR0000SA0). Weekly earnings are computed as total wage and salary income earned in the previous year divided by weeks worked in the previous year, and hourly earnings are computed as wage/salary income divided by weeks worked in the previous year and hours worked in the previous week.[62] Top coded earnings values are imputed as 1.5 times the censored value. We define full-time, full-year employment as working at least 35 hours in the survey week and 40 weeks in the previous year. Because weeks worked in 1940 are reported as full-time equivalent weeks, we do not impose the hours restriction for the full-time 1940 sample and when making hourly wage calculations, we count full-time equivalent weeks as 35 hours of labor input.

Table 4.1 provides descriptive statistics for the 1940, 1950 and 1960 Censuses, which are our main samples. Statistics are given for all the 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 4.4.

As is visible in Table 4.1, 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 considerably lower in these states. However, female labor supply, measured by average weeks worked per woman, does not differ among high-, medium- and low-mobilization states in 1940.

4.3.2 Female Employment and the Level and Distribution of Earnings

In this section, we document the cross-state correlations between female labor supply and their range of labor market outcomes over the five decades between 1940-1990. Table 4.2 presents the relationship between female employment and a variety of aggregate state labor market outcomes including female and male wages, male earnings inequality, and the male college/high school earnings differential. In all these models, our measure of female labor

[62] The 1940 Census does not distinguish between wage/salary and self-employment income and hence both sources are implicitly used in earnings calculations. Restricting the sample to non-farm employed likely substantially reduces the importance of self-employment income in the 1940 sample.

supply is average weeks worked by female state residents aged 14 to 64 (with other sample restrictions as above).

In Panels A and B, we measure wages as log weekly earnings of full-time, full-year workers, and control for year main effects, state of residence and state or country of birth dummies, a full set of education dummies, a quartic in (potential) experience, and dummies for nonwhite and marital status. As in all wage models we report in this paper, all covariates other than the state dummies are interacted with time to allow returns to education, experience and demographics to differ by decade.[63] The results show no consistent relationship between female employment on the one hand and female earnings and male earnings on the other. For example, column 1, which uses data from 1940 and 1990, indicates that greater female employment is associated with an increase in female wages, and a slight decline in male wages. Other columns report results for different subsamples.

Panel C reports the relationship between female labor supply and the male college/high school wage premium. To perform this calculation, we regressed log weekly full-time earnings of males with exactly a college and those exactly a high school degree on year main effects, state of birth and state of residence dummies, and the same set of covariates and their interactions with time, and the measure of average weeks worked by females in their state of residence. Each variable (and the constant) is interacted with a college-graduate dummy and the coefficient reported is the interaction between the female labor supply measure and the college graduate dummy. This coefficient measures the relationship between female labor supply and the earnings of college graduates relative to high school graduates (see equation (4.7) below). Panel C shows a weak negative relationship between female labor supply and the male college/high school wage differential: in the full sample and in 1970-90, a 1 week increase in female employment is associated with a 1 percent decline in the college/high school differential.

Finally, Panel D reports results from regressions of within-state changes in overall male earnings inequality on changes in female weeks worked. The measure of inequality used here is the log difference between the 90th and 10th percentiles of the male earnings distribution. The results show no relationship between overall male wage inequality and female labor supply between 1940 and 1990 or between 1970 and 1990, but during earlier decades, there is a positive association between female employment and male wage inequality, and this

[63] Results without such interactions are similar, and are available upon request.

relationship is significant in the 1940s.

If the results in Table 4.2 corresponded to the causal effect of female employment on female and male wages, we would conclude the 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.[64] 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 demand for male and female labor move together, the OLS estimate of the effect of female employment on male wages will also be biased upward. On the other hand, the OLS estimates of the effect of female labor supply on male wage inequality may be biased upward or downward depending on whether greater labor demand increases wages more at the top or the bottom of the residual earnings distribution.

To obtain unbiased estimates of the effect of female employment on these labor market outcomes, 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.

4.4 Mobilization for World War II

Following the outbreak of the War, the Selective Service Act, also known as the Burke-Wadsworth Bill, was introduced in the Senate in June 1940 to correct flawed conscription policies from the World War I era. The Burke-Wadsworth Bill initiated a mandatory national registration in October 1940 for a draft lottery for all males ages of 21-35 to gather relevant data on potential draftees. By the time the draft ended 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 these either enlisted or were drafted for the War. Men aged 45-64 were registered as part of a civilian workforce management effort by the Selective Service.

[64] An additional problem is composition bias: women who participated at the margin may have been different (say less productive) than the average woman, creating a spurious negative relationship between female employment and female wages. Our instrumental-variables estimates will also be subject to a variant of this bias: IV estimates will identify the market effects of the labor supply of women whose behavior is affected by our instrument; these women may differ from the "average" female labor force participant (see Angrist and Imbens, 1995, for an interpretation of IV estimates along these lines).

Following each of the registrations, there were a series of lotteries determining the order that a registrant would be called to active duty. Local draft boards then classified all of the registrants into qualification categories. 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 left considerable discretion to the local boards.

Due to 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. Consequently, blacks were separated from whites for classification purposes. 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 due to concerns about sending them to battle against their countries of origin.

Our measure of the mobilization rate is the fraction of registered males 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 effectively all men in the relevant age range were registered, our mobilization rate variable is 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 and hence the great majority of those who served, 67 percent, were drafted.[65] Therefore, the main source of variation in mobilization rates is cross-state differences in draft rates.

Table 4.3 shows the cross-state relationship between the mobilization rate and a variety of potential determinants. These right-hand side variables, calculated from the 1940 Census, measure the percent of males ages 13-44 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. We also calculate average years of completed schooling among males in this age bracket since, as Table 4.1 shows, this variable differs significantly among high- and low-mobilization states. We focus on the age bracket 13-44 because men aged 13

[65] According to data from Selective Service System (1956), 4,987,144 men were enlisted and 10,022,367 men were drafted during the War years. 458,297 males were already serving in the military in 1940 prior to declaration of hostilities. 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.

in 1940 would be 18 in 1945, and thus part of the target ("at-risk") group.[66] Finally, we calculate the number of draft registration boards per 1,000 males ages 13-44 using Selective Service (1956) paired to Census population counts. Draft board prevalence might affect the mobilization rate if states with greater mobilization infrastructure were able to conduct the draft more rapidly.

Column 1 of Table 4.3, which includes all of these variables in a regression model simultaneously, shows that the farm, schooling and German-born variables are significant, while the other variables are not. The significant negative coefficient on the farm variable implies that a state with 10 percentage points higher farm penetration is predicted to have a 1.7 percentage point lower mobilization rate. The coefficient on the German-born variable implies that 1 percentage point higher fraction of population born in Germany translates into over 3 percentage points lower mobilization. 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 (also note that the point estimate is significantly smaller in later columns). Interestingly, the percent Italian/Japanese variable has the wrong sign in this regression, but this seems to be because it is correlated with percent German-born, and when entered individually, it is insignificant. Column 2 displays a specification that includes only the farm and nonwhite variables, while column 3 shows a specification only with the farm and education variables. Column 4 combines the farm, nonwhite and schooling variables. Due to collinearity, neither the nonwhite nor schooling variable is individually significant.

To explore robustness, column 5 drops the 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, father and local draft board variables one by one to the model in column 4. The only variables that have additional explanatory power are the age structure and percent German-born variables.

Finally, columns 12 and 13 show specifications that control for the farm, nonwhite, schooling, age composition and the German-born variables simultaneously. These specifications explain a significant part of the variation in state mobilization rates (the R^2's of

[66] The fathers variable refers to 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 and hence we use the percent of women with children, which is presumably highly correlated with the desired variable.

these two regressions are, respectively, 0.62 and 0.70). 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 "non-economic" components, while the residual 30 percent corresponds to idiosyncratic or non-systematic variation. Below we present estimates of the effect of mobilization on female labor supply growth that exploit various combinations of these sources of variation.

4.5 WWII Mobilization and Female Labor Supply

4.5.1 Cross-State Relationships

Figure 4.2 in the Introduction showed male and female labor force participation and the fraction of males ages 14-65 who were in active military duty in each year during the years 1940-1952.[67] The rise of women's labor force participation between 1940 and 1945 closely tracks the mobilization of males. During these five years, male labor force participation declined by 16.5 percentage points while female labor force participation rose by 6.0 percentage points. Hence, the rapid increase in female employment during 1940-1945 appears to be a response to the labor demand shock caused by WWII mobilization.

By 1949, the size of the military was at peacetime levels, male labor force participation slightly exceeded pre-War levels, and the demand shock that had induced the increase in female employment 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 points lower than at the War's peak).[68] If female employment was higher in 1950 than it would have been absent WWII mobilization, this can be thought 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.[69] Alternatively,

[67] Numerators for labor force participation and military active duty numbers in Figure 4.2 are from the Statistical Abstract of the United States (1944/45, 1951, and 1954), which relies on estimates from Census of Populations data for years 1940-1942 and Current Population Reports, Series P-50 and P-57, for years 1943-1952. Denominators are population estimates of U.S. residents ages 14-65 by gender from 1940 and 1950 Census of Populations. Population estimates are interpolated for years 1940-1948 and 1950-52 assuming a constant exponential growth rate over 1939-1952. Due to use of the detailed annual labor force series for 1939-1952 in Figure 4.2 (which are not available for earlier years), our female labor force participation numbers in this figure differ slightly from the series provided by Goldin (1994) and Blau, Ferber and Winkler (2002) displayed in Figure 4.1.

[68] As noted earlier, our data sources do not agree on the exact magnitude of the aggregate rise in female labor force participation during the decade. The Figure 4.1 data place the rise at 6.0 percentage points rather than 5.1 as in Figure 4.2.

[69] In this case, the actual increase in *efficiency units* supplied by female labor may be understated by our

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.

Mobilization for WWII was not uniform across states. In low-mobilization states, less than 44 percent of men between the ages of 18-44 served in the War, in contrast to 51.5 percent of males in high-mobilization states (with a range of 9.2 percentage points between the 10th and 90th percentile states). Figure 4.3 showed that female employment did not systematically vary between high- and low-mobilization states in 1940 (see also Table 4.1). By 1950, however, women worked significantly more in high-mobilization states. In fact, as shown in the second panel of the figure, there is a striking positive relationship between state mobilization rates and the change in average weeks worked by women from 1940 to 1950. Our hypothesis is that this change in the cross-state pattern of female employment between 1940 and 1950 reflects the effects of WWII mobilization on female labor supply. Notably, this positive relationship is unique to the decade of the War. The bottom panel of Figure 4.3 indicates that there is no additional relative growth in female labor supply during 1950-1960 in high-mobilization states (in fact, there is a slight mean reversion).

To investigate the hypothesis more formally, Table 4.4 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:

$$y_{ist} = \delta_s + \gamma_{1950} + X'_{ist} \cdot \beta_t + \alpha \cdot \gamma_{1950} \cdot m_s + \varepsilon_{ist}. \tag{4.2}$$

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, and γ_{1950} is a dummy for 1950. X_{ist} denotes other covariates including state of birth or country of birth, age, race, and share of farmers and nonwhites and average schooling in the state in 1940 interacted with the 1950 dummy, which are included in some of the specifications. The time subscript on β indicates that the effects of the $X's$ on labor supply may differ by decade. The coefficient of interest is α, which corresponds to the interaction term between the 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

labor supply calculations (which are normally expressed in weeks worked), leading to an underestimate of the negative effect of female labor supply on female wages.

1940 to 1950. Since our key right-hand side variable, the mobilization rate, varies only by state and year, all standard errors reported in this paper are corrected for clustering at the state times year level (using STATA robust standard errors).

Column 1 is our most parsimonious specification, including only state dummies, year main effects, 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 13.9 (standard error 1.8) implies that a 10 percentage point higher mobilization translated into a 1.4 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. Subsequent columns add a variety of covariates to this specification.

The addition of a full set of age and marital status dummies interacted with year dummies in column 2 reduces the mobilization rate coefficient by about one-third to 9.6. The difference in the point estimate between the first two columns indicates that age groups with greater increases in labor force participation were more populous in high-mobilization states. Column 3 adds state of birth dummies as a control for cross-state migration (and country of birth dummies for immigrants). These dummies have little impact. As an additional method of controlling for the possible endogeneity of women's location decisions, Appendix Table 4.1 displays a set of specifications comparable to Table 4.4 (columns 3 and 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 4.4. As a final check for migration, Panel B of Appendix Table 4.1 reports results from specifications that use (log) total supply of women, measured in aggregate weeks or aggregate efficiency units, as the dependent variable (see below for definition of aggregate efficiency units). Consistent with the finding that women worked more on average in 1950 in high-mobilization states, total female labor supply also grew more in these states.

4.5.2 Correlation or Mobilization?

The correlations documented above between state mobilization rates and measures of agricultural employment, nonwhite population, and educational attainment raise a concern as to whether we are simply capturing differential trends in female employment in non-agricultural, better-educated, and low-minority states. In that case, the estimated effect of the mobilization rate on female labor supply growth will reflect, at least in part, this correlation. To state the concern more concretely, we can think of the variation in cross-state mobilization rates as arising from three components:

$$m_s = m_s^e + m_s^{ne} + e_s. \tag{4.3}$$

The first of these, m_s^e, is the component of state mobilization rates that is correlated with observable economic factors such as agricultural and educational distributions. The second component, m_s^{ne}, is correlated with non-economic factors that we can potentially measure such as age and ethnicity. 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 these, m_s^e is the most problematic since economic factors that cause differences in mobilization rate could also potentially impact 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 (4.2), thus only exploiting the variation in mobilization rates coming from m_s^{ne} and e_s. To implement this approach, columns 4 and 5 of Table 4.4 add controls for the interaction between the 1950 dummy and the fractions of men who were farmers and who were nonwhite and average schooling among men in 1940.[70] The nonwhite and farm interaction terms are typically only marginally significant while the schooling variable is positively related to growth in female labor force participation. But these variables have little impact on the coefficient on the mobilization rate, which remains between 8 and 10 week and is highly significant. Overall, these estimates also imply that a 10 percentage point higher mobilization rate is associated with an approximately 1 week increase in female employment.

As an alternative check on the influence of racial composition on male employment growth, Panel B of Table 4.4 limits the sample to white females (recall that we have already limited the sample to non-farmers). The results in this subsample are comparable to those reported in Panel A. The baseline estimate is again approximately 9 to 10 weeks, and is

[70] Although the component of mobilization rate correlated with fraction nonwhite may be thought to be "non-economic," given the changes in the economic status of blacks over this time period, we are more comfortable classifying this as an "economic" complement.

similarly robust in magnitude and significance to the inclusion of various covariates.[71]

Another concern is that there may be significant cross-state differences in the importance of occupations or industries with a greater demand for women, explaining the differential growth in female employment between 1940 and 1950. Table 4.5 allows for female labor supply growth to differ by states' initial occupational and industrial structure. In particular, we control (in separate regressions) for the interaction between the 1950 dummy and the fraction of males in 1940 in each of 10 one-digit occupations as well as the fraction of men in defense-related industries.[72] These estimates provide little evidence of differential female labor supply growth by occupational and industrial structure. The occupation and industry variables are insignificant in all but one specification, and their inclusion affects neither the magnitude nor the significance of the relationship between WWII mobilization and female labor supply growth.

In terms of the notation of equation (4.3), the estimates in Tables 4.4 and 4.5 exploit two sources of variation in state mobilization rates: the "non-economic" component, m_s^{ne}, and the "idiosyncratic" component, e_s. An alternative strategy to explore whether these results may be interpreted as a causal effect of WWII mobilization on female labor supply growth is to attempt to isolate the non-economic component of the mobilization rate, m_s^{ne}. To implement this approach, we focus on the variation in mobilization rates accounted for by differences in the age structure and German heritage of the population of males 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 4.3). Conditioning 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, we report results from 2SLS estimation of equation (4.2) in Panel A of Table 4.6, using the 1940 age or ethnic structure (or both) as instruments for the mobilization rate (in these models we also control for percent farmer and male's 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,

[71] We also estimated models with interactions between individual education dummies and year dummies. The results are very similar to those in Table 4.4. Tables of these results are available from the authors.

[72] We define defense-related industries as those contributing to War Stock material directly related to combat missions. Examples of defense industries are: aircraft and ship building, motor vehicles and electronic machinery, metal industries, steel and iron industries, and blast furnaces and rolling mills. A complete list is available from the authors.

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 Panel B of Table 4.6 find insignificant and inconsistently signed effects of mobilization on male labor supply during this decade (see Table 4.8 for a more detailed analysis of the relationship between male labor supply growth and WWII mobilization).[73]

We provide a number of further robustness checks on our main estimates in Table 4.7. Specifically, we present results for a second outcome measure (positive weeks worked), explore the importance of regional variation to the main findings, and compare the 1940-1950 results to estimates for the subsequent decade when there was no mobilization for war. We focus on specifications 3 and 5 from Table 4.4, which are our richest models; the latter includes all state 'economic' controls (i.e., farm, nonwhite, and average years of completed schooling, all interacted with the 1950 dummy).

The first row of the table indicates that our results are not primarily driven by regional trends in female labor supply. Adding region dummies (interacted with the 1950 dummy) corresponding to the 4 Census regions increases the estimated relationship between the mobilization rate and female employment growth, but does not change the overall pattern. Dropping Southern states, on the other hand, reduces the size of the coefficient. In all cases, the relationship remains economically and statistically significant.

The second row of Table 4.7 presents identical models where the dependent variable is an indicator variable equal to 1 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 percent higher mobilization rate is associated with 1 to 3 percentage points additional growth in female labor force participation over this decade.[74]

Panel B of Table 4.7 presents comparable estimates for the years 1950 to 1960, in this

[73]We have also performed a "falsification" exercise for this IV approach in which we regress the change in female (or male) labor supply during 1950-1960 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 age alone or the age and ethnic structures together, p-values range from 0.01 to 0.03, though age variables have the opposite sign to those in Table 4.6.

[74]We do not investigate the effect of the mobilization rate on the Census variable that is coded as in-the-labor-force, since in the 1940 census this is equivalent to having an occupation, and women who worked during the War may still have an occupation even if they are not currently in the labor force. Closely related, however, we show in Appendix Table 4.1 that there is a strong positive relationship between WWII mobilization and the logarithm of total female labor supply to a state.

case interacting the mobilization fraction 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 case do we find a significant positive relationship between the mobilization variable and the growth of female labor supply measured as average weeks worked or any weeks worked over the 1950-60 decade. The cross-state growth in women's labor force participation was significantly correlated with WWII mobilization rates only during the decade of the War.

To supplement these aggregate patterns, Appendix Tables 4.2 and 4.3 present 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 high school graduate women, women between the ages of 14-44 and the cohorts that were 15-24 or 35-44 in 1940. Point estimates for the impact of mobilization on the labor supply of women above 54 and those for the cohorts that were 25-34 or 45-54 in 1940 (Appendix Table 4.3) are sensitive to the inclusion of the aggregate state variables.[75]

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 afterwards). Unfortunately, we are not aware of a data source with information on state labor force participation rates by gender during the intra-Census 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 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 naturally only available for those who survived to 1978 and report valid Social Security numbers. 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 to 1950), we cannot investigate whether women worked more in high-mobilization states during the War.[76] These data nonetheless provide a rare glimpse at women's employ-

[75]In 1940, the educational distribution of non-elderly, non-farm females was: less than 8th grade, 27 percent; exactly 8th grade, 23 percent; 9-11 years, 22 percent; exactly 12 years, 19 percent; 1 or more additional years beyond high school, 9 percent (3 percentage points of which was accounted for by college graduates). In 1950, the corresponding numbers were 22, 17, 23, 26, 12 and 5.

[76]Because we do not have information on respondents' state of birth, we use state of residence as an imperfect proxy. Social Security Numbers (SSNs) are essentially only available for women with positive work

ment in the immediate post-war years.

Figure 4.6 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 ages 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 the 90 percent confidence interval for each estimate (using STATA robust standard errors clustered by state). 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. Reassuringly, 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 (1947-1950). 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 appears to persist for at least 15 years after the War's end.

4.5.3 Supply Shifts or Demand Shifts?

We have so far interpreted the robust cross-state correlation between mobilization rates and growth in female employment between 1940 and 1950 as indicative of a shift in female labor supply. As Figure 4.2 shows, the *aggregate* demand shock, the mobilization for war, that had drawn women into the labor market had almost entirely reversed itself by 1947. There may still have been post-war differences in the demand for female labor across states correlated with WWII mobilization rates, however. For example, men who served in the War may have had difficulty reintegrating into the workforce, or may have taken advantage of the WWII GI Bill by attaining further education rather than working. If this were the case, greater female labor force participation in high-mobilization states could reflect demand for female labor rather than differences in female labor supply.

history and hence we treat missing SSNs as indicating no work history (except in cases where respondents refused to provide a SSN or where 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. Note that although the CPS Exact Match file reports annual quarters worked for 1937-1946, these data are imputed from aggregate income data for these years and hence are not useful for our analysis.

To explore these possibilities, Table 4.8 and Appendix Table 4.1 also provide estimates of labor supply specifications for males comparable to those estimated for women in Table 4.4. These models find no significant correlation between WWII mobilization rates and the growth of male labor supply between 1940 and 1950. Depending on covariates, estimates for male labor supply range from weakly positive to weakly negative and are never significant. As noted above, Panel B of Table 4.6 also shows no relationship between male labor supply growth and the component of WWII mobilization correlated with non-economic factors (age structure and ethnic mix). Hence, it appears that the net growth in male labor supply between 1940 - 1950 was not systematically lower in high-mobilization states.[77]

To probe the relationship between mobilization and men's employment in 1950 further, the final four columns of Table 4.8 provide separate labor supply estimates for males who did and did not serve in the War.[78] These models detect a weak positive relationship between state mobilization and the growth in male labor supply for non-veterans during this decade, but this relationship is insignificant and is only visible in specifications that exclude the interaction between the 1950 dummy and state aggregate measures (farm share, nonwhite population, and educational attainment). For models limited to WWII veterans, there is an insignificant positive effect of mobilization rates on labor supply when we do not control for the state aggregate measures, and an insignificant negative relationship when the state aggregate measures are included. In net, these models do not provide reason to believe that by the 1950s, male labor supply was systematically affected by state mobilization rates.

As a final piece of evidence, note that Table 4.9 (discussed below) documents that relative earnings fell for both genders in high- relative to low-mobilization states. If, contrary to our presumption, state-level labor demand shifts induced by WWII mobilization persisted to 1950, we would expect female wages to have risen in high-mobilization states. Similarly, if cross-state variation in female employment were driven by differences in the overall demand for labor in 1950, we would expect both male and female wages to have been higher in high-mobilization states. These wage results therefore suggest that as of 1950, the enduring effects of WWII mobilization were realized primarily through additional female labor supply rather than greater labor demand for either gender (though this last piece of evidence does

[77] See Stanley (1999) and Bound and Turner (1999) on the effects of GI Bills. As noted by Goldin and Margo (1992, footnote 24), college attendance under the WWII GI Bill peaked in 1947 and declined sharply after 1949.

[78] In these specifciations, the 1940 subsample contains all males from the previous columns, while the 1950 subsample is limited to males who report themselves as WWII veterans (columns 6 and 7) and non-veterans (columns 8 and 9).

not imply that there were no demand-side differences across states).

4.6 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.

4.6.1 Initial Evidence

Figure 4.4 showed 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.

Table 4.9 presents our first set of regression estimates for the impact of female labor supply on wages. For the sake of transparency, we initially take the approach of regressing female and male log weekly earnings on our measure of weeks worked by women from the previous section. We present both OLS models and instrumental-variables (IV) estimates in which the female labor supply measure is instrumented by state mobilization rates. Figure 4.4 above corresponds to the reduced form for the IV estimates (without covariates). More formally, the estimating equation is:

$$\ln w_{ist} = \delta_s + \gamma_{1950} + X'_{ist} \cdot \beta_t + \phi \cdot Y_{st} + u_{ist}. \tag{4.4}$$

The left-hand side variable is log weekly earnings, $\ln w_{ist}$, while 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, a quartic in experience, and a dummy for marital status. Models that include nonwhites also include a nonwhite dummy. The coefficient of interest, ϕ, measures the effect of female labor supply on earnings. As indicated by the time subscript on the coefficient vector, β_t, we allow the wage differential associated with each individual level covariate to differ by decade (similarly to the OLS models of Table 4.2). Standard errors are again clustered to account for the fact that the labor supply measure operates at the state by year level.

We estimate equation (4.4) using both OLS and IV/2SLS models. In the IV models, the first-stage equation is analogous to equation (4.2) above, except that the endogenous variable in this case is not women's individual weeks of work, but average weeks worked per woman in each state. This first-stage relationship is tabulated directly below the point estimate 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. Based upon the evidence presented in the previous section, we believe that this exclusion restriction is plausible.

It is important to bear in mind that estimates of ϕ do not have a direct structural interpretation in terms of our model in Section 4.2. As the theory underscores, the impact of female labor supply on total male and female earnings should depend upon the (log) ratio of female to male labor supply (as well as the supply of other nonlabor factors). Hence, unless female labor supply (in OLS or instrumented form) is uncorrelated with male labor supply, we cannot directly recover the relevant demand and substitution elasticities from estimates of (4.4). We therefore view these results as descriptive and adopt a more structural approach in subsequent tables.

In column 1, we begin with a parsimonious specification which indicates that a 1 week increase in female labor supply is associated with a 10.7 percent decline in female weekly earnings. Given that women's labor supply averaged 10.7 weeks in 1940 (Table 4.1), and assuming no correlation between (instrumented) female labor supply and male labor supply, this point estimate would correspond to a female labor demand elasticity of -1.14. In the next column, we add aggregate measures of female age structure by state to the regression model. These measures control for the correlation between state mobilization rates and female age structure.[79] Inclusion of age controls reduces the estimated wage impact of female labor supply by approximately 25 percent to -7.0 percentage points for a 1-week increase, which remains highly significant. Column 4 adds the interaction between the 1950 dummy and the 1940 aggregate state measures–share farm, share nonwhite, and average education–thus allowing differential wage growth in farming, high-minority and low-education states. These interactions reduce the magnitude of the estimate by one-third and increase the standard error. The lower panel repeats the results for the white sample with similar results but slightly greater precision. The negative estimated impact of female labor supply on mean

[79]Female age structure variables measure the share of female state residents ages 14-64 in each of the following age categories (with one omitted): 14-17, 18-24, 25-34, 45-54, 55-64.

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).

Comparing these IV estimates to the corresponding OLS estimates in the table, which are typically weakly negative and never significant, suggests that the OLS estimates are likely biased upward (i.e., towards zero). It appears, not surprisingly, that cross-state variation in female labor supply during this decade was jointly determined by a combination of demand and supply shifts. By isolating the component of female employment that is plausibly orthogonal to demand, our IV estimates show a substantially larger effect of female labor supply on female earnings.

The subsequent columns of Table 4.9 present corresponding estimates for male earnings, both for the full sample and for the white subsample. 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 negative effects of female labor supply on male earnings. All point estimates are highly significant except those in the final specifications where we control for the interaction between the 1950 dummy and several state aggregate measures. Interestingly, the estimated effects of female labor supply on male earnings are consistently 30 to 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 which we explore in greater detail below. Once again, the IV estimates are more negative than the corresponding OLS estimates, consistent with the view that OLS estimates are biased towards zero due to simultaneity, and that demand for male and female labor are positively correlated.[80]

The final columns of Table 4.9 present wage results estimated separately for male WWII veterans and non-veterans. These columns provide an important specification test. If aggregate wage effects for males were driven exclusively by lower wages for veterans, we would be worried about having primarily detected the adverse effects of war reintegration on vet-

[80] Also notice that the negative effects on female wages are unlikely to be accounted by the labor force participation of women with lower earnings capacity (cf. Smith and Ward, 1984). First, as Appendix Table 4.2 shows, marginal labor market participants were relatively highly educated, so there is no compelling reason to expect that they will be adversely selected on unobserved skills. Second, to rationalize the wage effect we estimate, i.e., over 10 percent decline in wages in response to a 10 percent increase in employment, with the participation of less skilled women, we would need the marginal participants to earn negative wages!

erans' earnings.[81] The estimates in Table 4.9 indicate significant negative wage effects of female labor supply for both non-veterans and veterans. Interestingly, the point estimates for veterans are somewhat more negative than those of non-veterans. This pattern is consistent with Richard Freeman's "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.

Appendix Table 4.4 estimates models similar to those in Table 4.9, while controlling for interactions between the 4 Census regions and year, or dropping all Southern state. These results are quite similar to our baseline estimates, indicating that the negative relationship between the mobilization-induced changes in female labor supply and changes in earnings is not driven primarily by Southern states or regional trends.

Given the substantial convergence in regional wage levels that took place among U.S. states during the 1940s (Wright, 1986), we were also concerned that wage patterns detected in Table 4.9 might reflect a process of "catching up" whereby agricultural states, which generally had low-mobilization rates, gained ground on the rest of the nation during this decade. To check this possibility, we augmented the Table 4.9 models to control for the 1940 level of wages for the relevant demographic group (interacted with a 1950 dummy). Appendix Table 4.5 reports the results of this exercise. This lagged wage variable is negative and generally significant indicating that states with initially higher wage levels experienced smaller wage gains during the decade. The important finding however is that the inclusion of the lagged measure does not affect our general conclusions. In models without state aggregate measures, point estimates of the effect of mobilization on wages are lower than the models without lagged wages, but they are higher when the state aggregate measures are included. In all specifications, the effect of mobilization-induced changes in female employment on female wages is statistically significant, and the effect on male wages is always negative, and is significant in all specifications except for those that control for the interactions between the 1950 dummy and aggregate state measures.[82]

[81] Angrist and Krueger (1994) present evidence that WWII veterans were positively selected, and Angrist (1990) presents evidence that Vietnam Era veterans experienced earnings losses due to foregone civilian experience.

[82] 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 and other price controls are unlikely to be responsible for our results. 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 post-war observations.

4.6.2 Using Mobilization Rates to Estimate Elasticities of Demand and Substitution

The wage estimates in Table 4.9 employ average female weeks as the endogenous regressor. This approach will lead to correct estimates of the elasticity of demand or substitution only if (instrumented) female labor supply is uncorrelated with male labor supply. Although this condition may be satisfied, it would be preferable to exploit exogenous variation in male and female labor supply simultaneously. Using a single instrument, we cannot separately identify both sources of variation. We can, however, use the mobilization rate to instrument the (log) ratio of female to male labor supply in a state. This approach surmounts the problem of treating male labor supply as exogenous and thereby brings us closer to the specification suggested by the theoretical model. The equation that we estimate is:

$$\ln w_{ist} = \delta_s + \gamma_{1950} + f_i + X'_{ist} \cdot \beta^g_t + \chi \cdot \ln\left(\frac{F_{st}}{M_{st}}\right) + \eta \cdot f_i \cdot \ln\left(\frac{F_{st}}{M_{st}}\right) + u_{ist}, \tag{4.5}$$

where the sample now includes all individuals (male and female), f_i is a dummy for female, F_{st} is total labor supplied by women and M_{st} is total labor supplied by men in the state of residence of individual i. As indicated by the super- and subscripts on β^g_t, each of the individual and state aggregate control measures included in the model (contained in X_{ist}) is permitted to impact male and female earnings differentially by gender and decade.

In this equation, there are two coefficients of interest, χ and η. The coefficient χ measures the direct impact of increases in (relative) female labor supply on 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, and the quantity $\chi + \eta$ is an estimate of the inverse elasticity of demand for female labor.

In the first four columns of Table 4.10, we present estimates of equation (4.5) where the labor supply measure is constructed as the log ratio of total female to male weeks supplied in each state and the control variables correspond to those used in the four specifications in Table 4.9. First-stage estimates, tabulated in each column, indicate that WWII mobilization substantially affected relative gender labor supplies. A 10 percent higher mobilization rate is estimated to have induced a 10 to 20 percent increase in the log ratio of female to male

We have also estimated the key labor supply and wage models in Tables 4.4 and 4.9 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 of estimates is available on request.

labor supply, an impact which is always precisely estimated and highly significant.[83]

We next turn to elasticity estimates. The point estimates in the first two rows of Table 4.10, corresponding to χ and η in equation (4.5), are consistently negative, economically sizable, and with few exceptions, highly significant. This set of results confirms the findings above that the demand curve for female labor is downward sloping and that women are relatively close substitutes for men.

Summing χ and η to obtain an estimate of the (inverse) elasticity of demand for female labor, σ_F, we find that a 10 percent increase in relative female labor supply reduced female wages by 6 to 7 percentage points. These wage effects correspond to an own-labor demand elasticity of between -1.4 and -1.7 and are therefore slightly larger in magnitude than the "naive" estimates above. The lower panel of the table presents analogous wage estimates for the subsample of white female wage earners, for whom we find comparable demand elasticities.

The impact of female labor supply on wages is not uniform between the two genders, however. As is visible in the second row of Table 4.10 (and as was suggested by Table 4.9), the wage effects of (relative) increases in female labor supply are uniformly more negative for women than they are for men. A 10 percent increase in female labor supply lowers female wages relative to male wages by about 3 percentage points. By implication, female and male labor inputs are highly, but not perfectly, substitutable. In particular, the point estimates for η, corresponding to the inverse elasticity of substitution σ_{MF}, imply a substitution elasticity in the range of -3, with slightly smaller implied elasticities in the models that include aggregate state controls. Again, elasticities are similar in Panel B, where estimates are limited to white males and females.

4.6.3 Measuring labor supply in efficiency units

As a check on the above results, we also estimate models that replace the aggregate weeks of labor supply measure with a measure of labor supply calculated in efficiency units following the approach of Welch (1969). Conceptually, efficiency unit calculations aggregate various demographic subgroups according to their estimated relative productivities to obtain total labor supply by gender. To implement this approach, we use the 1940 Census sample to calculate average weekly earnings for full-time, full-year workers in the following education

[83] This is a sensible magnitude. For example, an increase in the female to male ratio of labor input from 0.30 to 0.35 percent correponds to a 15 log point increase in the this ratio.

by race by gender categories within each state: 5 age categories (13-23, 24-33, 34-43, 44-53, and 54-63); 5 education categories for whites (<8 years, exactly 8 year, between 8 and 12 years, exactly 12 years, and greater than 12 years of schooling); and 3 education categories for nonwhites (<5 years, exactly 5 years to exactly 8 years, and greater than 8 years of completed schooling).[84] Under the assumption that wages are proportional to marginal productivity, this approach allows us to "quality adjust" aggregate labor input, thereby relaxing the assumption that there is perfect substitutability of labor input within each gender.

Using this matrix of 3,760 efficiency unit weights (47 states by 80 cells), we assign each male and female in our labor supply sample the efficiency weight corresponding to her demographic characteristics and state of residence, α^g_{aers}, where g denotes genders, a indexes age bracket, e indexes education categories, r indexes race, and s indexes states. We calculate aggregate quality-adjusted relative labor supply to a state as

$$\ln\left(\frac{L_s^f}{L_s^m}\right) = \ln\left(\frac{\sum_{a,e,r}\alpha^f_{aers}\cdot F_{aers}}{\sum_{a,e,r}\alpha^m_{aers}\cdot M_{aers}}\right), \tag{4.6}$$

where F_{aers} is the weeks of labor supplied by females in the state with the relevant demographic characteristics and M_{aers} is the corresponding quantity for males (and, as always, all calculations use Census sampling weights).

Estimates of equation (4.5) that use labor supply measured in efficiency units are shown in columns 5-8 of Table 4.10. The first-stage coefficients from these estimates are slightly smaller in magnitude than the corresponding weeks-based estimates, which may imply that marginal female labor force entrants drawn into the labor market by mobilization had lower average productivity than incumbent participants. We find that the elasticities of demand and substitution calculated from these efficiency-unit based estimates are not systematically different from those estimated using the weeks-worked measure. Hence, these results appear to confirm our previous findings. In the subsequent tables, we employ the weeks-worked measure since it is more transparent.

[84] Because non-whites had substantially less schooling than whites in 1940, it was necessary to use fewer education categories with different cutpoints in calculating non-white efficiency units. We normalize each cell by the wages of white, male high school graduates, ages 24-33 in each state in 1940, so each worker's labor input is expressed relative to the weekly full-time labor input of a prime-age white male high school graduate in his or her state. Where cells are empty for given states, we impute them as the population weighted average (normalized) efficiency units of the corresponding cells from other states. Note that the normalization of wages by a given demographic group is for convenience only and does not impact the ratios computed.

4.6.4 Differences between Short-Run and Long-Run Elasticities

As noted in the Introduction and Section 4.2, the estimates reported in this paper are likely to correspond to short-run elasticities. In general, it is of great interest to know whether short- and long-run elasticities differ substantially. Since, as shown in Figure 4.3, there was only a small amount of mean reversion in female employment during the 1950s, states with greater WWII mobilization also had greater female employment in 1960. By exploiting the 20-year changes between 1940 and 1960, we can investigate whether the short-run impact of increased female employment is different from its long-run impact. We perform this exercise in Appendix Table 4.6, by estimating models identical to those in Table 4.10 except that the two decades we now cover are 1940 and 1960.

Comparing the first-stage coefficients on the mobilization rate interaction between Table 4.10 and Appendix Table 4.7, we see that mobilization had a large effect on the growth of relative female labor supply not only between 1940 and 1950, but also between 1940 and 1960. For example, in column 1 of Appendix Table 4.6, the first-stage coefficient is 2.1, compared to 2.05 in Table 4.10. However, the standard error is much larger. Thus, not surprisingly, some of the 2SLS estimates will be less precise. Moreover, in models that control for interactions between the 1960 dummy and share of farmers, nonwhites, and average education in 1940, there is no first-stage relationship between mobilization and female employment growth between 1940 and 1960.

The models in Appendix Table 4.6 that do not control for the state aggregates show that the effect of relative female employment on male wages is broadly comparable between 1940-50 and 1940-60 (columns 1-3 and 5-7). However, the effect of relative female labor supply on male-female differentials is much weaker for the 20-year state level changes. In fact, the estimates are essentially zero and the standard error bands easily exclude the short-run elasticity estimate from Table 4.10 (though the main effect, which captures the impact on the level of male and female earnings, is sometimes as large as those in Table 4.10). This is consistent with the notion that the long-run relative demand curve for women's labor is considerably more elastic than the short-run relative demand curve, or even perhaps perfectly elastic. This could be due to adjustment of nonlabor inputs that are fixed in the short-run (LeChatelier principle), changes in technology or in the organization of production favoring women in areas with greater female employment (Acemoglu, 1998, 2002), or changes in trade patterns that require sufficient time to equilibrate.

4.7 Does Female Labor Supply Raise Male Earnings Inequality?

The results above establish that female labor supply lowers male earnings. But this impact need not be uniform throughout the male wage distribution. Indeed, several of the authors cited in the Introduction 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.[85] Greater female labor supply will generally raise male earnings inequality if women are closer substitutes to low earnings males than high earnings males.

We take three angles of attack to investigate the relationship between female labor supply and male earnings inequality, in all cases exploiting the WWII-induced increase in female employment. First, we ask whether female labor supply affects earnings differentials between males at high, medium and low levels of education—specifically college graduates, high school graduates and those with 8th grade or lower education.[86] Next, we explore how rising female labor supply changes the level of inequality between various quantiles of the male earnings distribution, for example the 90-50 and 50-10 log earnings ratios. Finally, we ask whether female labor supply also affects residual earnings inequality—that is, the inequality that remains after accounting for observable individual characteristics.

4.7.1 The Impact of Female Labor Supply on Male Educational Differentials

We begin with educational differentials. Consider a variant of equation (4.5) in which the dependent variable is log weekly earnings of males of two education groups–initially, college and high school graduates, later high school and 8th grade graduates:

$$\ln w_{ist}^{m} = \delta_s + \gamma_{1950} + c_i + X'_{ist}\beta_t^e + \chi \cdot \ln\left(\frac{F_{st}}{M_{st}}\right) + \eta \cdot c_i \cdot \ln\left(\frac{F_{st}}{M_{st}}\right) + \gamma \ln\left(\frac{C_{st}^{m}}{H_{st}^{m}}\right) + u_{ist}. \quad (4.7)$$

In this equation, c_i is a dummy for whether individual i is a college graduate (the omitted group being high school graduates), F_{st}/M_{st} is relative female labor supply measured in

[85] On rising male earnings inequality see, among many others: Katz and Murphy (1992) and Bound and Johnson (1992) on supply and demand factors; Juhn, Murphy and Pierce (1993) on the role of changing skill premia in the increase in residual inequality; DiNardo, Fortin and Lemieux (1995) and Lee (1999) on the role of labor market institutions; and Berman, Bound and Griliches (1994), Machin and Van Reenan (1998), and Autor, Katz and Krueger (1998) on the role of computerization. Levy and Murnane (1992), Bound and Johnson (1995), Katz and Autor (1999), and Acemoglu (2002) provide surveys of this literature. See the Introduction for cites to studies of the relationship between female labor supply and male earnings inequality.

[86] In 1940, 28 percent of males had less than an 8th grade education, 24 percent had exactly 8th grade, 22 percent had more than 8th grade but less than high school, 15 percent exactly a high school degree, and the remaining 10 percent had more than high school education (5 percent with college or above). In 1950, the corresponding numbers were 24, 18, 22, 20, and 15 (7 percent with college or above).

aggregate weeks worked as above, and C^m_{st}/H^m_{st} is the relative supply of college versus high school male labor input.[87] All covariates are allowed to have different effects on earnings of college and non-college males and to differ by decade.

The coefficients of interest in this equation are χ and η. The coefficient χ measures the impact of female labor supply on the earnings of high school graduates, and η gives 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 males constant, we can think of $\sigma_{fh} = 1/\chi$ as the cross-elasticity of demand between female labor and high school graduates and $\sigma_{fc} = 1/(\chi + \eta)$ as the cross-elasticity of demand between female labor and college graduates. The ratio of cross-elasticities of females for high school versus college graduates, $\sigma^f_{hc} \equiv \sigma_{fh}/\sigma_{fc}$, is therefore $(\chi + \eta)/\chi$. If σ^f_{hc} is less than 1, this implies that female labor has a more (negative) wage impact on high school graduates, so females are closer substitutes to high school than to college males, and vice versa if $\sigma^f_{hc} > 1$.

Consistent with our previous results, we anticipate that the main effect of female labor supply on both college and high school wages, χ, is negative. Since relative supplies of male college versus high school graduates, C^m_{st}/H^m_{st}, should also directly impact the male college/high school premium, we must either control for this measure or assume that instrumented female labor supply measure is uncorrelated with it. We implement both approaches below and find that the choice is not consequential for our results.

While the college/high school wage differential is of great contemporary interest, the vast majority of males in our 1950 sample (85 percent) had high school or less education, with the two modes of the distribution found at exactly high school completers (20.3 percent) and exactly 8th grade completers (18.2 percent). Therefore, it is of interest to ask whether female labor supply raised or lowered earnings inequality between these groups of males as well. After estimating equation (4.7) for the college/high school differential, we perform analogous estimates for the high school/8th grade differential.

Estimates of equation (4.7) for college and high school graduates, shown in panel A of Table 4.11, reveal that growth in female labor supply exerts a small positive effect on

[87] In models that use the male college/high school relative supply, college labor supply is the sum of total weeks worked supplied by college-plus graduates 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/8th 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 8th grade and less than high school education; 8th grade is the sum of weeks worked supplied by those those with 8th grade or less, plus half of that supplied by those with more than 8th grade and less than high school.

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 to 4 log points while reducing college wages by only 1 to 2.5 percentage points. These point estimates imply a relative cross-elasticity of demand σ_{hc}^{f} of 0.4 to 0.6, but this elasticity is imprecisely estimated. This evidence is consistent with the view that females drawn into the labor force by WWII mobilization were more substitutable for high school than college educated men (consistent with the characteristics of female labor force entrants documented in Appendix Table 4.2). But we cannot reject the hypothesis that women's labor supply reduced college and high school wages by equivalent amounts.

This ambiguity does not carry over to the corresponding results for the impact of female labor supply on the male 8th grade/high school differential. These estimates, found in Panel B of Table 4.11, have the opposite sign to and are somewhat larger than those for the male college/high school wage premium. They are also more precisely estimated. A 10 percent increase in female labor supply is estimated to reduce male high school relative to 8th grade earnings by 1.5 to 2.5 percentage points. This relative wage impact is highly significant in specifications that do not control for state aggregate measures. In models that include these aggregates, the estimate is of similar magnitude but is less precise. Interestingly, we cannot reject the hypothesis that female labor supply had no impact on the wages of 8th grade males.

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-educated males, and *particularly* relative to less-educated males. This suggests that during the WWII era, females were closer substitutes to males at the middle of the skill distribution than to males in either of the tails. Given that low-educated males in 1950 were reasonably likely to be employed in manual occupations, it is plausible that women would indeed be worse substitutes for them than for their high school graduate brethren. This result stands in some contrast to Grant and Hamermesh's (1981) and Topel's (1994) OLS findings that high-skill women are strong substitutes for low-skill males. Of course, our findings are from another era and these substitution parameters need not be fixed over long intervals.

4.7.2 The Impact of Female Labor Supply on the Distribution of Male Earnings

Because women's labor force entry during the 1940s appears to have raised earnings inequality between college and high school males while lowering it for high school versus 8th grade educated males, the net impact on male inequality is—at this point—ambiguous. Educational inequality is however only one component of earnings differentials, and the total impact of female labor supply on male inequality could in principle be quite different than its impact on educational inequality.

To provide a more complete picture of these potential effects, we adopt a less structural approach. Using observed male earnings distributions and estimated residual male earnings distributions by state, we define state level inequality metrics as the log difference between various quantiles of the earnings or residual earnings distribution, such as the 90-50 or 50-10 differential. We then explore whether WWII-induced increases in female labor supply raised or lowered these inequality measures. To implement this approach, we first estimate standard wage regressions of the form:

$$\ln w_{ist} = \delta_s + \gamma_{1950} + X'_{ist} \cdot \beta_t + v_{ist}, \tag{4.8}$$

where w_{it} is weekly earnings for male i residing in state s in year t. In the "overall inequality" specification, we include only state dummies and a year main effect. In the "residual inequality" model, the vector X_{ist} includes a full set of education dummies, a quartic in potential experience, nonwhite, state of residence, state or country of birth, veteran status, and marital status dummies, as well as controls for state female age structure by year. A third set of models add state-level macro controls (share farmer and nonwhite, and average education in 1940). The fact that β_t is indexed by t indicates that returns to these observed characteristics are allowed to vary by decade. We also briefly look at measures of wage inequality that combine workers from both genders.

The measures of overall or residual inequality are calculated separately in each state and year as the difference between the 90th and the 10th (or 50th and 10th, etc.) percentile values of the corresponding residual distribution, v_{ist}, and are denoted by v_{st}^{90-10} etc. Observe that the residual distribution in the "overall" inequality model is simply the demeaned log earnings distribution in each state.

We then use these inequality measures as the left-hand side variable in Table 4.12. The typical regression takes the form:

$$v_{st}^{90-10} = \delta_s + \gamma_{1950} + \phi \cdot \ln\left(\frac{F_{st}}{M_{st}}\right) + u_{ist},$$

where v_{st}^{90-10} is the 90-10 differential, the endogenous regressor, F_{st}/M_{st} is again relative female labor supply (in weeks), and the instrument is the state mobilization rate, m_s, interacted with the 1950 dummy. To move from micro- to macro-data (i.e., from individual Census observations to state level aggregates) without losing the information provided by the micro-level controls, we orthogonalize both the instrumental variable and the endogenous regressor with respect to all of the covariates in X_{ist} to form the state level measures used in the second stage of estimation.

The first column in Table 4.12 labeled Mean Δ provides estimates of mean state-level change in earnings inequality from 1940 to 1950. The sizable decline in earnings inequality visible in the table reflects the well-known "Great Compression" studied by Goldin and Margo (1992). Over the decade of the War, male 90-50 and 50-10 differentials each declined by close to 17 log points.

The first set of regression estimates in specification 1 examines the state-level relationship between growth in the log relative supply of female labor input (instrumented with the mobilization rate) and the contemporaneous change in overall state level earnings inequality. The net impact of female labor supply on male earnings inequality is positive and sizable. A 10 percent increase in female labor input is estimated to widen the 90-10 earnings differential by 5.5 log points, which is highly significant. It is noteworthy that this entire impact occurs in the upper half of the male earnings distribution. Consistent with our findings for the impact of female labor supply on the wage gap between middle- and low-education males, increases in female labor supply appear to cause some compression below the median of the distribution, though this impact is not significant. An interesting implication of these estimates is that without the WWII-induced increase in female labor force participation the "Great Compression" would have been even "greater" in the sense that wage inequality among males would have declined even further between 1940 and 1950.[88]

When we look at the impact of female labor force participation on wage inequality among all workers (male and female) in Panel C, we find much larger effects. Now a 10 percent

[88]Interestingly and consistent with our findings, Goldin and Margo (1992, p. 27) notice a similar pattern of *widening* inequality in industries with high female employment: "Industries that were female-intensive (cigars, men's neckwear, woolen and worsted mills) comprise an obvious exception [to the trend of wage compression]... Rather than experiencing a narrowing of the wage structure from the prewar to the wartime or postwar periods, their distributions actually widened."

increase in female labor supply widens the 90-10 earnings differential by 1.4 log points. This much larger effect is not surprising; greater female participation both increases male wage inequality directly, as we have already established, and also adds more women to the distribution who, at this time, were paid considerably less than men.

Panel B of the table presents comparable estimates for the white subsample. In this case the impact of female labor supply on inequality above the median is somewhat less positive while the impact on inequality below the median is somewhat more negative. This estimate suggests that female labor supply primarily reduces male earnings at a lower point in the white male earnings distribution than in the overall earnings distribution, which appears plausible given the substantial racial disparities in education and earnings in this period.

The subsequent two columns of Panels A and B present estimates of the impact of female labor supply on residual male inequality. The impact of female labor supply on residual inequality is about half as large as the impact on overall inequality, but still sizable. A 10 percent increase in female labor supply is estimated to raise the male 90-10 earnings differential by 1.5 to 2 log points. The columns numbered (2) present residual estimates in which state level macro controls (share nonwhite and farm, and average education) are included in the first stage models. The point estimates are typically less precise than previous estimates.

Overall, the results reported in this section show a substantial effect of female employment growth on overall and residual inequality among men at midcentury. These results therefore provide some support to the hypotheses advanced in Fortin and Lemieux (2000) and Topel (1994 and 1997), linking female labor supply to rising male inequality—though distinct from the hypotheses of these authors, female labor appears to increase male inequality not by competing with low-skill males, but by increasing dispersion at the top of the male wage distribution. Interestingly, the effects we find are large enough to "explain" a large fraction of the recent increase in male wage inequality as resulting from the concurrent sizable rise in female employment. This conclusion may be premature, however, for two reasons: first, the education levels and characteristics of women who increased their labor supply during the decade differ substantially from those of the marginal female labor market participants of today. The structure of production has also changed substantially since midcentury. Accordingly, substitution elasticities we estimate from midcentury may not be directly comparable to the elasticities today. And second, as noted before, our estimates correspond to short-run elasticities, which may be quite different from long-run elasticities as the results in Appendix Table 4.6 suggest.[89] To understand the effect of female employment on the increase in male wage inequality over the past three decades, the relevant elasticities are long-run elasticities. Work exploiting additional sources of variation in recent female employment growth is necessary to make progress on uncovering the links between rising female labor supply and recent changes in the structure of earnings.

4.8 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 labor market consequences of increased female labor force participation. 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 where a larger fraction of working-age males 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 1940 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. Our results indicate more downward sloping demand curves for female labor, and a closer degree of substitutability between males and females than suggested by OLS estimates, presumably because OLS regressions are biased towards-zero by simultaneous demand-induced variation in female employment. We also find that, contrary to a common hypothesis in the literature, women are not the closest substitutes to the lowest education males, but to high school graduate males (at least at midcentury). Nevertheless, because greater female participation increases inequality in the top half of the male wage distribution, our estimates suggest

[89]In addition, as noted by, among others, Blau and Kahn (1994, 1997), the gender wage gap closed substantially during the 1980s as female employment was rising, suggesting that demand shifts favoring women may be an important component of the rise in female labor supply.

sizable effects of female labor force participation on male wage inequality. This finding indicates that a more detailed investigation of the relationship between the increase in female labor supply and the recent widening inequality among males would be fruitful.

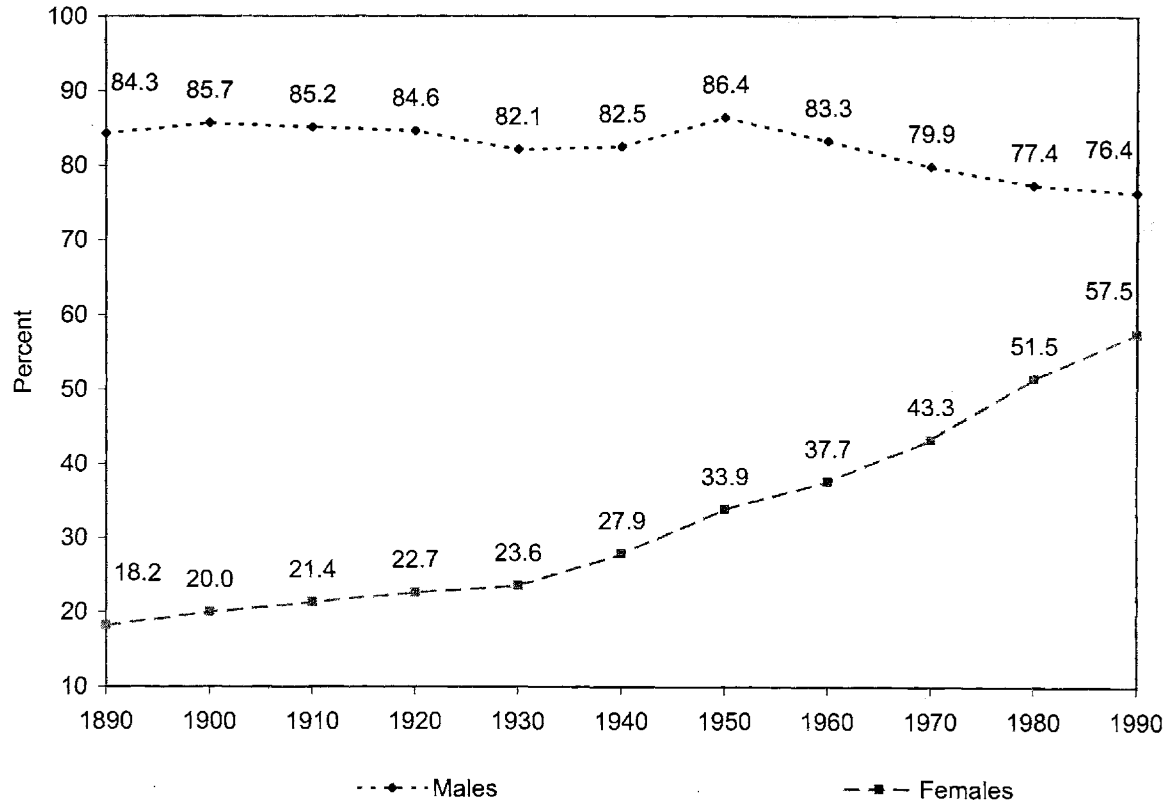

Figure 4.1: Labor Force Participation by Gender of U.S. Residents Ages 16 - 65, 1890 - 1990. Source: Blau, Ferber and Winkler (2000), Table 4.1

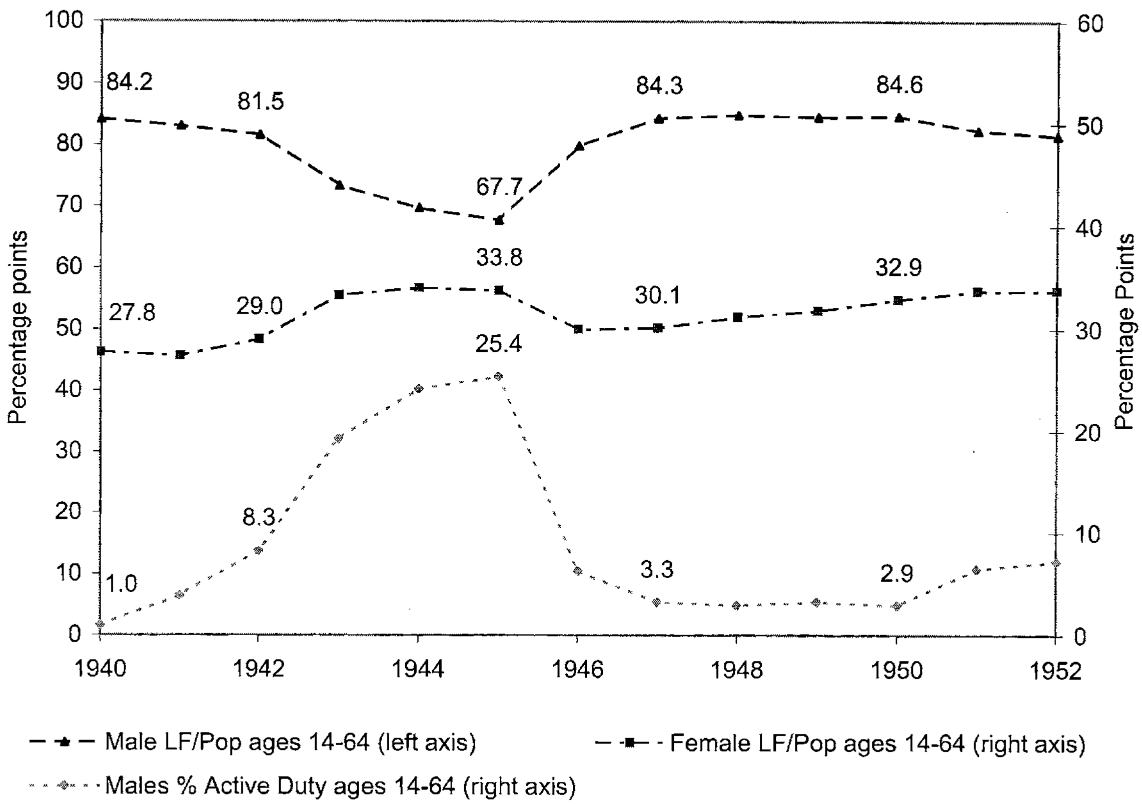

Figure 4.2: Male and Female Labor Force Participation and Military Active Service Personnel, 1940 - 1952.

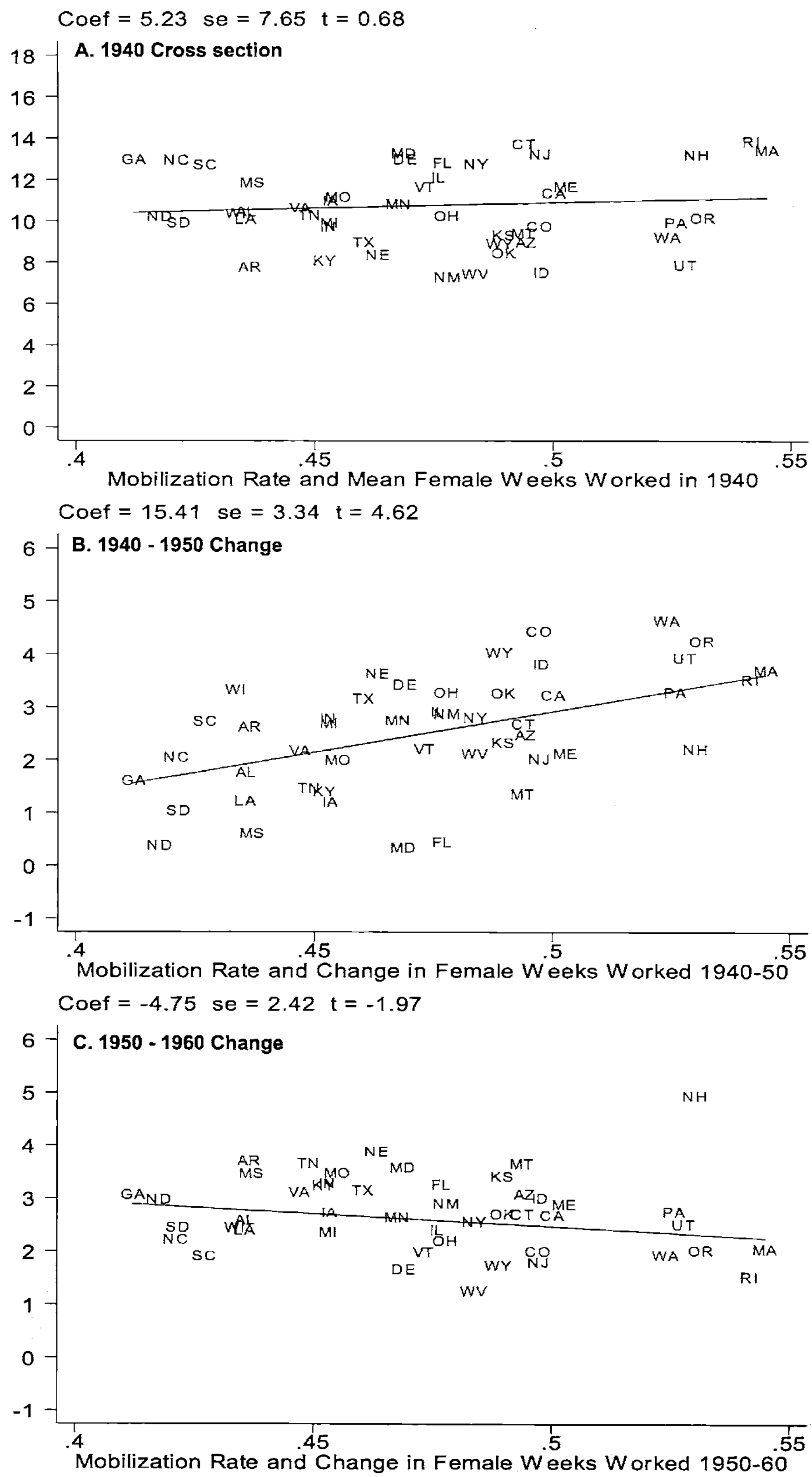

Figure 4.3: State WWII Mobilization Rates and Female Labor Supply, 1940 - 1960

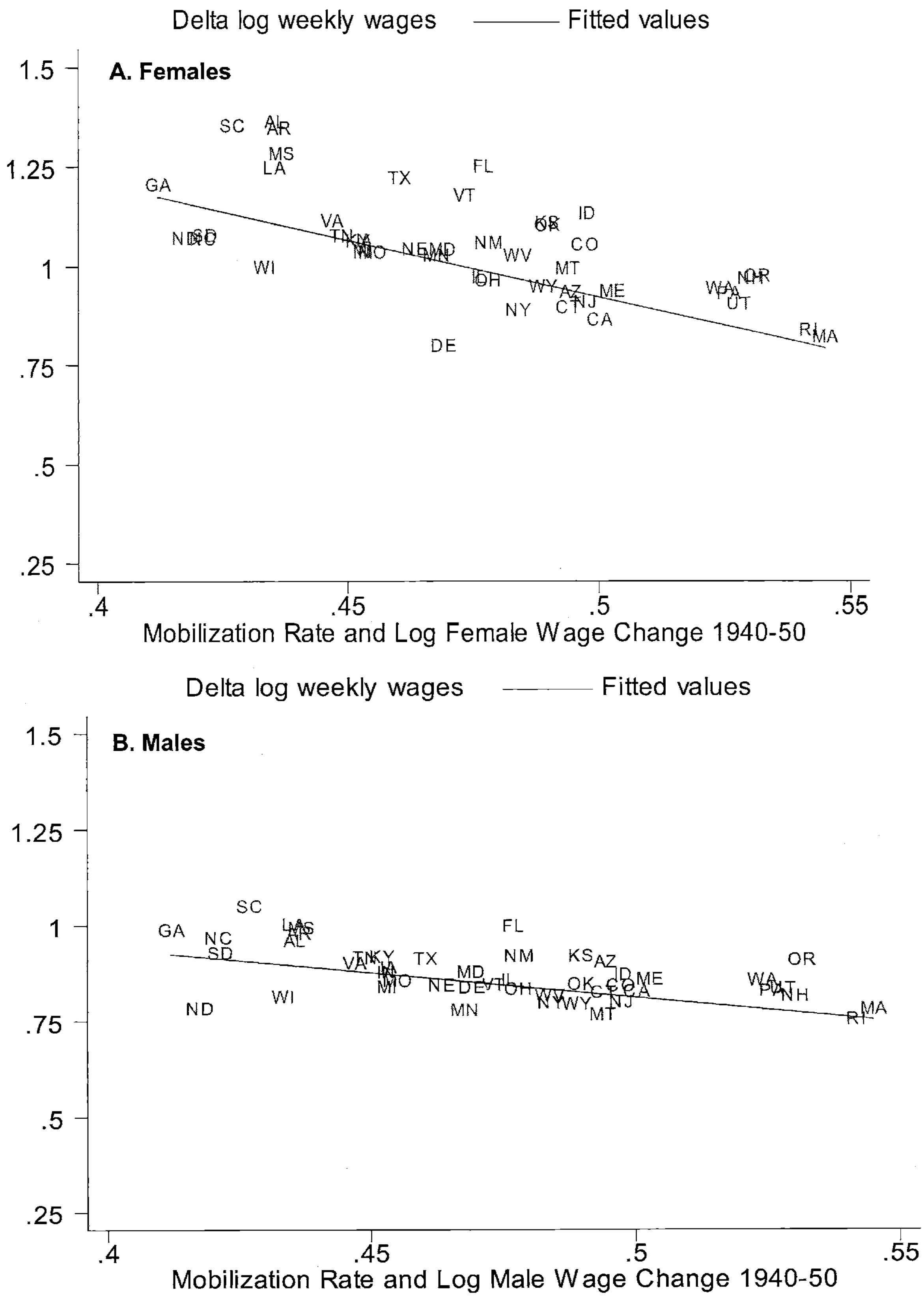

Figure 4.4: State WWII Mobilization Rates and Changes in Male and Female Mean Log Weekly Wages, 1940 - 1950

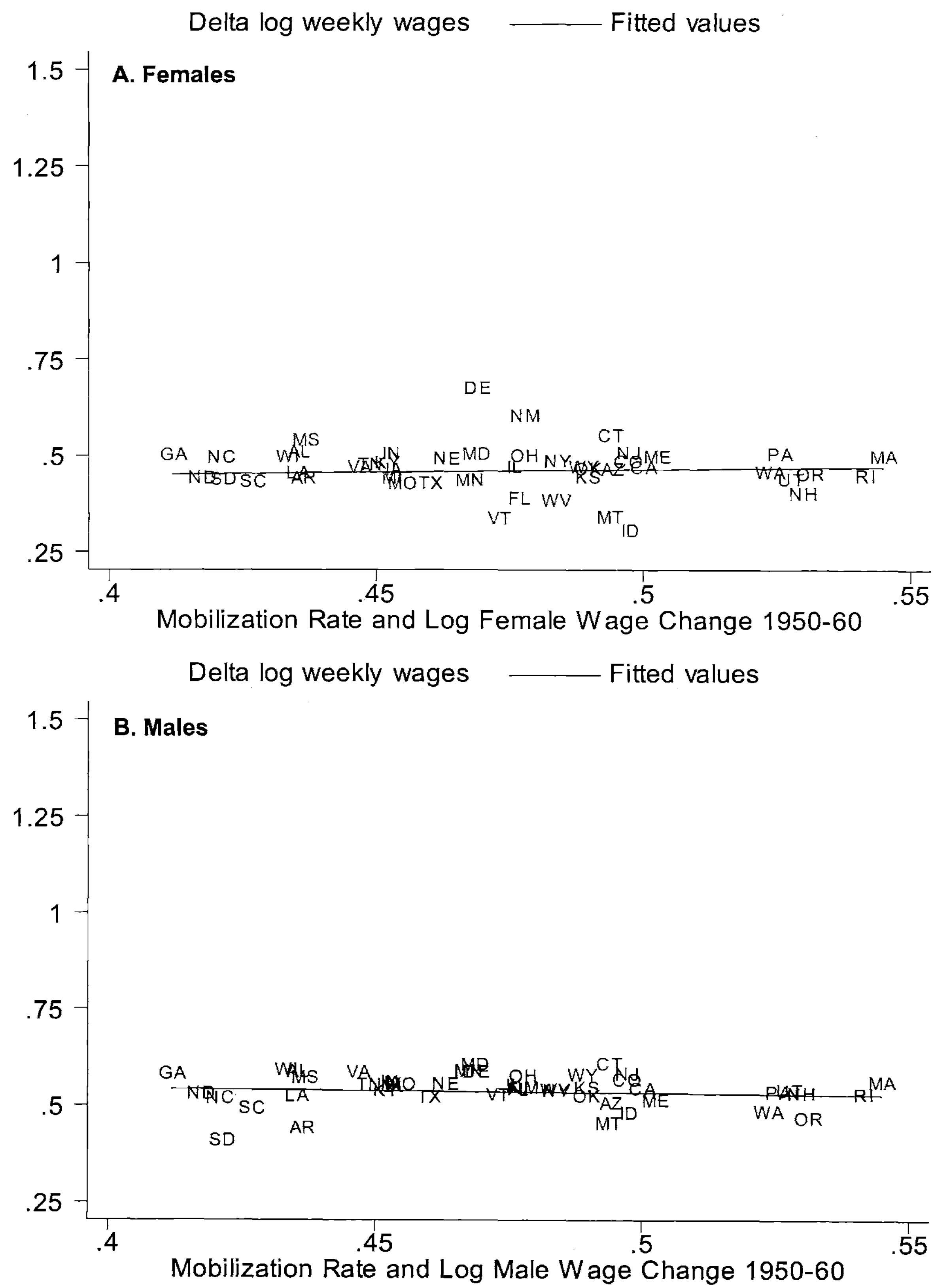

Figure 4.5: State WWII Mobilization Rates and Changes in Male and Female Mean Log Weekly Wages, 1950 - 1960

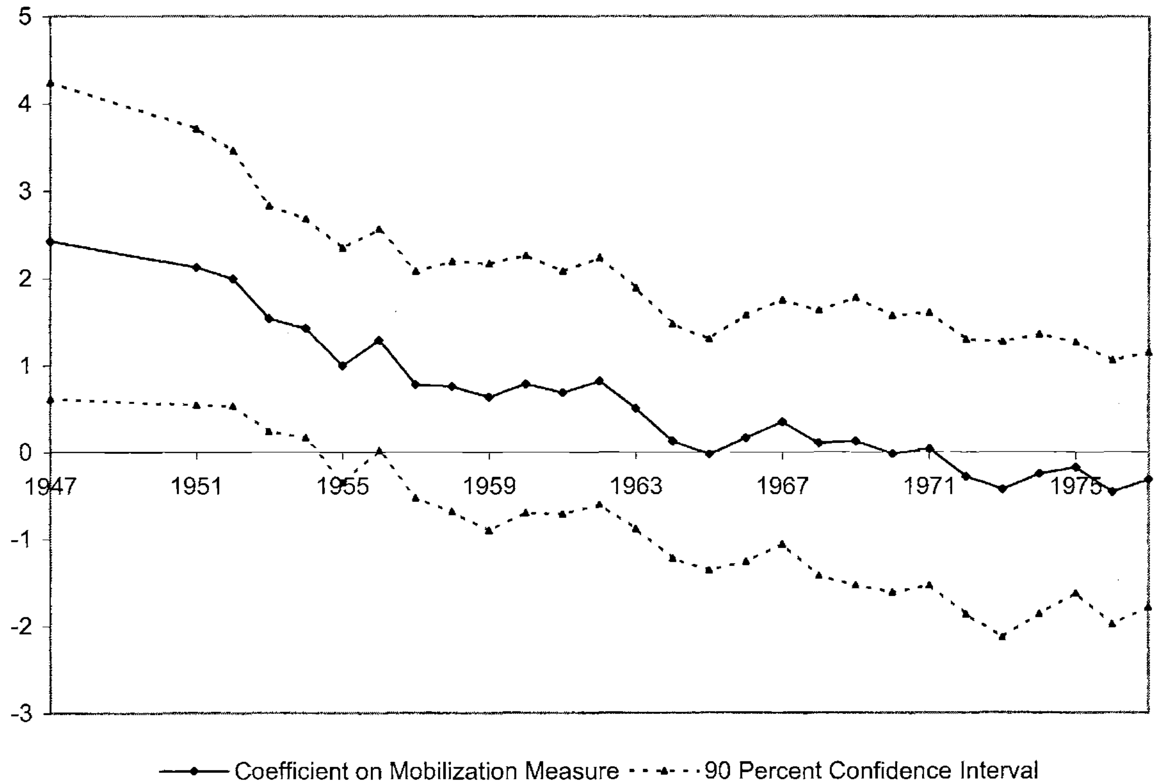

Figure 4.6: Estimated Impact of State WWII Mobilization Rate on Standardized Quarters Worked Annually by Women Who Were Ages 16 - 55 in 1945: 1947 - 1977.

Table 4.1: Characteristics of U.S. State Residents in Low, Medium and High Mobilization Rate States 1940, 1950, and 1960

	1940				1950				1960			
	All	Low	Med.	High	All	Low	Med.	High	All	Low	Med.	High
	A. Non-Farm Females Ages 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
	(0.27)	(0.29)	(0.20)	(0.14)	(0.16)	(0.19)	(0.10)	(0.11)	(0.16)	(0.18)	(0.12)	(0.11)
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)	(0.9)	(0.7)	(1.0)	(1.0)	(1.0)	(0.5)	(0.8)	(0.6)	(0.9)	(0.6)
Mean Year 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
	(0.7)	(0.9)	(0.4)	(0.6)	(0.7)	(0.8)	(0.3)	(0.5)	(0.5)	(0.6)	(0.3)	(0.4)
	B. Non-Farm Males Ages 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
	(0.18)	(0.24)	(0.12)	(0.08)	(0.13)	(0.18)	(0.08)	(0.08)	(0.14)	(0.19)	(0.09)	(0.08)
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)	(0.7)	(1.1)	(1.2)	(0.9)	(0.6)	(1.1)	(1.1)	(1.0)	(0.8)
Mean Year 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
	(0.6)	(0.8)	(0.3)	(0.5)	(0.7)	(0.8)	(0.4)	(0.5)	(0.6)	(0.6)	(0.3)	(0.4)
	C. State Aggregates: Males Ages 13-44 in 1940											
	Percent Mobilized 1940-47				Share Farmers 1940				Share Non-White 1940			
	All	Low	Med.	High	All	Low	Med.	High	All	Low	Med.	High
Percent 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)

Cross-state standard deviations in parenthesis. Data are from Selective Service (1956) monographs and Census PUMS one percent samples for 1940, 1950 (sample line subsample), and 1960. State mobilization rate is the number of males serving in WWII divided by the number registered ages 18-44 during the draft years. The Census PUMS sample includes those ages 14-64 (in earnings year), not living in institutional groups quarters, not employed in farming, and residing in the continental United States excluding D.C. and Nevada. There are 16 states in the low mobilization category (mobilization rate < 45%: GA, ND, NC, SD, SC, WI, LA, AL, AR, MS, VA, TN, KY, IN, MI, IA,), 15 states in the medium category (mobilization rate ≥45% and < 49%: MO, TX, NE, MN, MD, DE, VT, IL, FL, NM, OH, WV, NY, WY, OK), and 16 states in the high category (mobilization rate ≥ 49%: KS, MT, CT, AZ, CO, NJ, ID, CA, ME, WA, PA, UT, NH, OR, RI, MA.) 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 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 highest grade completed. Share non-white and farm are the fraction males in each state ages 13-44 in 1940 with these characteristics (including farm population).

Table 4.2: OLS Estimates of Impact of Female Labor Supply on Earnings 1940 - 1990 at Various Time Intervals
Dependent Variable: Log Weekly Earnings of Full-Time Workers

	Sample: All Full Time Workers							
	A. Female Weekly Earnings				B. Male Weekly Earnings			
	1940 - 90 (1)	1970 - 90 (2)	1940 - 60 (3)	1940 - 50 (4)	1940 - 90 (1)	1970 - 90 (2)	1940 - 60 (3)	1940 - 50 (4)
Weeks Worked per Woman	0.008	-0.004	0.016	-0.006	-0.008	-0.011	-0.001	-0.008
	(0.005)	(0.004)	(0.008)	(0.011)	(0.003)	(0.003)	(0.006)	(0.006)
R^2	0.88	0.70	0.74	0.64	0.89	0.67	0.74	0.58
n	*338,322*	*417,019*	*152,428*	*78,094*	*545,483*	*694,219*	*413,793*	*213,966*
	C. Male College/High School Differential				D. Male 90-10 Differential			
	1940 - 90 (1)	1970 - 90 (2)	1940 - 60 (3)	1940 - 50 (4)	1940 - 90 (1)	1970 - 90 (2)	1940 - 60 (3)	1940 - 50 (4)
Weeks Worked per Woman	-0.010	-0.010	-0.004	-0.003	0.016	-0.017	0.033	0.037
	(0.004)	(0.004)	(0.006)	(0.006)	(0.016)	(0.013)	(0.048)	(0.012)
R^2	0.83	0.65	0.71	0.55	0.64	0.92	0.63	0.98
n	*274,238*	*376,878*	*143,031*	*60,445*	*94*	*94*	*94*	*94*

Standard errors in parentheses account for clustering on state and year of observation. Each coefficient in Panels A - C is from a pooled microdata regression of the independent variable of interest from the two relevant decades regressed on average female weeks worked by state. Additional controls include a quartic in potential experience, a year main effect, a constant, and dummies for: non-white, age, marital status, state/country of birth, state of residence, and years of completed education. All individual demographic variables, aside from state of residence/birth, are also interacted with a year dummy. Birthplace dummies correspond to state of birth (if U.S. born) or German, Italian, Japanese, Other European, Other Asian, African, Latin American, and Other. Models in Panel C are analogous to those in Panels A and B, but are limited to those with exactly a college or high school degree and all individual level covariates are additionally interacted with a college graduate dummy. In these specifications, female weeks worked is both entered directly and interacted with a college graduate dummy, with the coefficient on the interaction reported above. Panel D tabulates separate regressions of estimated state level log 90-10 earnings ratio of male full-time weekly earners on weeks worked per female state resident, state dummies, a year dummy, and a constant. All education values for years 1940-1970 are coded as highest grade completed. Following the recommendations of Jaeger (1997), we define high school graduates in 1990 as those with twelve years of completed schooling, a GED, or a high school diploma and we define high school graduates in 1970 as those with exactly 12 years of completed schooling and no additional uncompleted schooling. Data are drawn from Census PUMS one percent samples (1950 sample line subsample) for years 1940-1970 and 1990. 1980 data is drawn from Census 5 percent sample using a randomly drawn 20 percent subsample. Samples include those ages 14 - 64 in earnings year, not living in institutional group quarters, residing in mainland U.S. state excluding Nevada and the District of Columbia with non-farm paid employment in survey reference week (excluding self-employed) and positive earnings in previous calendar year who earned between $0.50 and $250 an hour in 1990 dollars during the previous year (deflated by CPI All Urban Consumers series CUUR0000SA0) and worked at least 35 hours in the survey week and 40 weeks in the previous year. Top coded earnings values are imputed as 1.5 times the censored value.

Table 4.3: Determinant of State Level WWII Mobilization Rates
Dependent Variable: Mobilization Rate

	Mean (sd)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Share Farm	0.15	-0.17	-0.16	-0.17	-0.17	-0.23	-0.26	-0.22	-0.17	-0.16	-0.17	-0.20	-0.23	-0.25
	(0.11)	(0.05)	(0.04)	(0.03)	(0.04)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Share Non-white	0.10	0.00	-0.07		-0.03	-0.38	0.04	-0.03	-0.03	0.02	-0.03	-0.01		0.02
	(0.11)	(0.05)	(0.04)		(0.06)	(0.27)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)		(0.05)
Avg Completed	8.89	0.02		0.01	0.01	0.01	0.03	0.01	0.01	0.01	0.01	0.01		0.02
	(0.71)	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)		(0.01)
Share Ages 13-24	0.42	0.22					0.73						-0.36	0.38
	(0.03)	(0.34)					(0.24)						(0.24)	(0.33)
Share Ages 25-34	0.31	0.09					0.38						-0.84	-0.03
	(0.01)	(0.49)					(0.48)						(0.53)	(0.54)
Share German	0.007	-3.15						-1.88					-2.19	-1.16
	(0.006)	(0.90)						(0.55)					(0.74)	(0.76)
Share Italian or Japanese	0.010	1.67							0.00					
	(0.012)	(0.52)							(0.42)					
Share Married	0.50	-0.09								-0.22				
	(0.03)	(0.18)								(0.13)				
Share Fathers	0.47	0.07									0.00			
	(0.03)	(0.13)									(0.12)			
Draft Boards/ Pop (1000/s)	0.19	0.06										0.13		
	(0.05)	(0.07)										(0.09)		
Intercept		0.20	0.51	0.40	0.42	0.46	-0.19	0.42	0.42	0.49	0.42	0.42	0.94	0.15
		(0.34)	(0.01)	(0.05)	(0.08)	(0.12)	(0.27)	(0.07)	(0.08)	(0.08)	(0.09)	(0.07)	(0.25)	(0.34)
R^2		0.78	0.57	0.58	0.58	0.39	0.68	0.67	0.58	0.61	0.58	0.60	0.62	0.70
Southern States		Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors are in parenthesis for regression models. Standard deviations are in parentheses for column of means. Columns 1 - 13 contain regression estimates of state WWII mobilization rates (mean 0.475, standard deviation 0.032) on listed variables. Sample includes observations for 47 U.S. states excluding Hawaii, Alaska, Nevada, and the District of Columbia. Regressions are weighted by male population ages 13 - 44 in each state from the 1940 Census PUMS. State mobilization rate is the number of males who served in WWII divided by the number registered males ages 18-44 during 1940 to 1945 from Selective Service (1956) monographs. The Percent Farm, Non-white, Married, and Average Education variables are state averages for these variables for males ages 13 - 44 calculated from the 1940 Census PUMS. Percent German, Italian, and Japanese are the fraction of male state residents ages 13-44 born in these countries. Percent Fathers is the fraction of women ages 14 - 44 with any children in 1940 (a proxy for paternity). Draft Boards per Population is the number of state local draft boards divided by the number of men registered in each state (in thousands) ages 18 - 44 during 1940 to 1945. Southern states excluded from column 5 include VA, AL, AR, FL, GA, LA, MS, NC, SC, TX,KY, MD, OK, TN, WV.

Table 4.4: Impact of World War II Mobilization Rates on Female Labor Supply 1940 - 1950
Dependent Variable: Annual Weeks Worked

	A. All Females					B. White Females				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Mobilization Rate x 1950	13.89	9.59	9.06	10.22	8.28	11.17	10.42	9.85	10.64	8.51
	(1.78)	(2.38)	(2.35)	(2.61)	(2.39)	(1.89)	(2.02)	(2.05)	(2.65)	(2.37)
1940 Male Fraction Farmers x 1950				2.04	1.45				1.74	1.04
				(1.13)	(1.13)				(1.08)	(1.05)
1940 Male Fraction Non-white x 1950				-2.04	0.70				-1.96	-0.72
				(1.24)	(1.86)				(1.15)	(1.37)
1940 Male Average Years of Education x 1950					0.51					0.52
					(0.18)					(0.16)
R^2	0.01	0.17	0.17	0.17	0.17	0.01	0.18	0.18	0.18	0.18
n			*585,745*					*530,026*		
Age & Marital Status	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 and 1950 microdata regression of weeks worked by female state of residence on WWII state mobilization rate interacted with a 1950 dummy, state of residence dummies, a non-white dummy (where relevant), a year main effect, and a constant. Specifications in columns 2 - 5 also include dummies for marital status and years of age. Specifications in columns 3 - 5 contain state/country of birth dummies. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy for columns 2-5. As indicated, models also control for state fraction farmers, non-white, and average years of completed schooling among males ages 13 - 44 in 1940 in women's state of residence (each interacted with a 1950 dummy). Data are from Census PUMS one percent samples for 1940 and 1950 (sample line sub-sample) and include females ages 14 - 64, not living in institutional group quarters, not in farm employment, and residing in mainland U.S. states excluding Nevada and District of Columbia. State mobilization rate is assigned by female state of residence.

Table 4.5: Impact of World War II Mobilization Rates on Female Labor Supply 1940 - 1950
Controlling for the Fraction Males in Occupations in 1940
Dependent Variable: Female Annual Weeks Worked by State

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1940 Occupation Share Controlled	Prof/ Tech	Mngrs	Clerks	Sales	Craft	Oper-atives	Svcs/ Private	Svcs	Labor-ers	Defense Indust.
					A. Main specification					
Mobilization Rate x 1950	8.44	8.24	10.90	9.90	8.77	10.61	7.88	10.87	9.06	8.86
	(2.35)	(2.12)	(2.31)	(2.43)	(2.50)	(2.52)	(2.41)	(2.46)	(2.33)	(2.29)
1940 Male Occ. Share x 1950	2.86	5.82	-5.57	-3.56	0.60	-2.45	-51.81	-12.66	-5.27	0.45
	(7.23)	(6.92)	(3.86)	(6.42)	(3.89)	(2.09)	(61.05)	(5.71)	(4.12)	(1.24)
R^2	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17
	B. Controlling for 1940 Share Farm and Non-White, and Average Years of Education									
Mobilization Rate x 1950	8.11	7.79	5.97	7.78	8.48	8.45	8.30	7.11	8.42	10.56
	(2.44)	(2.59)	(2.47)	(2.32)	(2.63)	(2.30)	(2.35)	(2.46)	(2.62)	(2.48)
1940 Male Occ. Share x 1950	-2.87	-7.25	-10.26	-15.74	1.14	3.88	-19.08	-13.84	-0.57	2.29
	(8.68)	(8.49)	(5.20)	(6.80)	(8.02)	(2.60)	(73.70)	(7.52)	(3.69)	(1.19)
R^2	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17
n						*585,745*				

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 and 1950 microdata regression of weeks worked by female state of residence 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: non-white (where relevant), age, marital status, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy. Models in panel B also control for state fraction farmers, non-white, and average years of completed schooling among males ages 13 - 44 in 1940 in women's state of residence (each interacted with a 1950 dummy.) Data are from Census PUMS one percent samples for 1940 and 1950 (sample line sub-sample) and include females ages 14 - 64 not living in institutional group quarters, not in farm employment, and residing in mainland U.S. states excluding Nevada and District of Columbia. State mobilization rate is assigned by female state of residence. Occupation and Industry codes correspond to major (1-digit) occupational and industry categories. The defense industries correspond to IPUMS 1950 industry codes 326 - 388.

Table 4.6: Instrumental Variables Estimates of the Impact of World War II Mobilization Rates on Female Labor Supply 1940 - 1950
Dependent Variable: Annual Weeks Worked

	A. Females						B. Males					
	All			White			All			White		
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Mobilization Rate x 1950	16.65	13.73	11.38	15.78	13.19	11.42	14.60	-17.47	1.48	10.93	-17.00	-0.04
	(6.15)	(6.11)	(4.42)	(5.38)	(5.49)	(3.97)	(13.17)	(15.03)	(12.82)	(12.23)	(13.98)	(11.94)
1940 Male Fraction Farmers x 1950	3.11	2.61	2.20	2.65	2.19	1.87	3.27	-2.50	0.91	2.66	-2.45	0.65
	(1.39)	(1.21)	(1.07)	(1.30)	(1.11)	(1.01)	(1.82)	(2.53)	(1.66)	(1.75)	(2.45)	(1.65)
1940 Male Avg. Education x 1950	0.34	0.38	0.40	0.36	0.39	0.41	0.66	1.01	0.80	0.65	0.96	0.77
	(0.14)	(0.17)	(0.14)	(0.14)	(0.15)	(0.13)	(0.38)	(0.37)	(0.37)	(0.37)	(0.36)	(0.36)
	1st Stage Results						*1st Stage Results*					
1940 Male Fraction 13-24 x 1950	0.49		0.21	0.56		0.27	0.69		0.41	0.73		0.44
	(0.14)		(0.15)	(0.14)		(0.15)	(0.15)		(0.15)	(0.14)		(0.15)
1940 Male Fraction 25-34 x 1950	-0.04		-0.30	0.04		-0.22	-0.04		-0.23	-0.01		-0.20
	(0.23)		(0.25)	(0.22)		(0.25)	(0.21)		(0.22)	(0.21)		(0.21)
1940 Male Fraction German x 1950		-1.73	-1.36		-1.83	-1.33		-1.96	-1.30		-2.03	-1.30
		(0.39)	(0.46)		(0.39)	(0.46)		(0.38)	(0.41)		(0.38)	(0.41)
P-value: 1st Stage	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
n		*585,745*			*530,026*			*479,867*			*441,343*	

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 and 1950 microdata 2SLS regression of weeks worked by female state of residence on instrumented WWII state mobilization rate interacted with a 1950 dummy, a year main effect, a constant, and dummies for: non-white (where relevant), age, marital status, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy. Instruments used in the first stage of these models are the fraction of males ages 13-44 in 1940 who are of German birth or who are in the listed age categories (each interacted with a 1950 dummy). Models also control for state fraction farmers and average years of completed schooling among males ages 13 - 44 in 1940 in women's state of residence (interacted with a 1950 dummy). Data are from Census PUMS one percent samples for 1940 and 1950 (sample line sub-sample) and include those ages 14 - 64, not living in institutional group quarters, not in farm employment, and residing in mainland U.S. states excluding Nevada and District of Columbia. State mobilization rate is assigned by state of residence.

Table 4.7: Impact of World War II Mobilization Rates on Female Labor Supply
1940 - 1950 and 1950 - 1960
Dependent Variable: Female Weeks Worked

	Coefficient on Mobilization Rate Variable x 1950							
	A. 1940 - 1950							
	All				White			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weeks Worked	9.06	8.28	12.21	6.54	9.85	8.51	11.28	6.34
	(2.35)	(2.39)	(2.98)	(2.31)	(2.05)	(2.37)	(2.97)	(2.40)
R^2	0.17	0.17	0.17	0.19	0.18	0.18	0.18	0.20
Any Weeks Worked	0.019	0.184	0.282	0.148	0.063	0.174	0.253	0.130
	(0.076)	(0.072)	(0.079)	(0.080)	(0.069)	(0.071)	(0.080)	(0.081)
R^2	0.17	0.17	0.17	0.19	0.18	0.18	0.18	0.20
n	*585,745*	*585,745*	*585,745*	*410,794*	*530,026*	*530,026*	*530,026*	*393,820*
	Coefficient on Mobilization Rate Variable x 1960							
	B. 1950 - 1960							
	All				White			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weeks Worked	-3.85	2.70	0.09	3.66	-7.25	2.15	0.39	3.56
	(1.95)	(2.15)	(2.43)	(2.11)	(1.81)	(1.95)	(2.23)	(1.86)
R^2	0.14	0.14	0.14	0.16	0.15	0.15	0.15	0.17
Any Weeks Worked	0.044	-0.009	-0.017	0.032	-0.057	-0.006	0.002	0.035
	(0.054)	(0.069)	(0.071)	(0.073)	(0.049)	(0.067)	(0.064)	(0.069)
R^2	0.13	0.13	0.13	0.14	0.14	0.14	0.14	0.15
n	*683,976*	*683,976*	*683,976*	*480,545*	*615,590*	*615,590*	*615,590*	*449,275*
Fraction Farm / Non-white/ Avg Ed	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Region x 1950	No	No	Yes	No	No	No	Yes	No
Excluding South	No	No	No	Yes	No	No	No	Yes

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 - 1950 or 1950 - 1960 microdata regression of individual weeks worked on WWII state mobilization rate interacted with a 1950 or 1960 dummy (in Panels A and B respectively), a year main effect, a constant, and dummies for: non-white (where relevant), marital status, age, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 or 1960 dummy. As indicated, models also control for state fraction farmers, non-white, and average education among males ages 13 - 44 in 1940 in women's state of residence (each interacted with a 1950 dummy.) State mobilization rate is assigned by female state of residence. Data are from Census PUMS one percent samples for 1940, 1950 (sample line sub-sample) and 1960, and include females ages 14 - 64, not living in institutional group quarters, not in farm employment, and residing in mainland U.S. state excluding Nevada and District of Columbia. Weeks worked for 1960 is calculated using the midpoint of the intervalled weeks worked. Any weeks worked is defined as weeks worked greater than zero. Region x 1950 dummies refer to 4 main Census geographic regions. Southern states excluded from columns 4 and 8 are VA, AL, AR, FL, MS, NC, SC, TX, KY, MD, OK, TN, and WV.

Table 4.8: Impact of World War II Mobilization Rates on Male Labor Supply 1940 - 1950
Dependent Variable: Annual Weeks Worked

	A. All Males								
	All					Non-Vets		Vets	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mobilization Rate x 1950	4.09	5.33	5.09	-5.50	-5.41	7.79	-2.82	0.53	-10.63
	(5.96)	(5.63)	(5.69)	(7.56)	(8.01)	(6.51)	(9.27)	(5.40)	(7.35)
1940 Male Fraction Farmers x 1950				2.30	2.33		2.50		2.40
				(1.34)	(1.47)		(1.88)		(1.35)
1940 Male Fraction Non-white x 1950				-9.15	-9.32		-10.59		-7.52
				(1.24)	(2.66)		(3.51)		(1.40)
1940 Male Avg Years of Schooling x 1950					-0.03		-0.21		0.29
					(0.47)		(0.59)		(0.27)
R^2	0.02	0.35	0.35	0.35	0.36	0.36	0.36	0.34	0.34
n			*479,867*			*442,606*		*397,338*	
	B. White Males								
	All					Non-Vets		Vets	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mobilization Rate x 1950	0.46	4.09	3.60	-6.56	-6.56	7.37	-4.15	-4.03	-10.84
	(5.98)	(5.34)	(5.43)	(7.12)	(7.59)	(6.46)	(9.02)	(4.71)	(6.64)
1940 Male Fraction Farmers x 1950				1.97	1.97		1.97		2.74
				(1.27)	(1.41)		(1.86)		(1.39)
1940 Male Fraction Non-white x 1950				-9.11	-9.11		-10.56		-6.50
				(1.21)	(1.46)		(2.04)		(0.85)
1940 Male Avg Years of Schooling x 1950					0.00		-0.19		0.36
					(0.44)		(0.57)		(0.23)
R^2	0.02	0.36	0.36	0.36	0.36	0.37	0.37	0.35	0.35
n			*441,343*			*406,591*		*366,737*	
Age & Marital Status	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	Yes	No	Yes	No	Yes

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 - 1950 microdata regression of individual weeks worked on WWII state mobilization rate interacted with a 1950 dummy, a year main effect, a constant, and dummies for non-white (where relevant) and state of residence. Specifications in columns 2 - 5 add dummies for married and age. Specifications in columns 3 - 5 add state/country of birth dummies. Columns 6 - 9 include all 1940 males and only WWII Veterans or Non-Veterans in 1950 as noted. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy for columns 2-9. As indicated, models also control for state fraction farmers, non-white, and average education among males ages 13 - 44 in 1940 in male's state of residence (each interacted with a 1950 dummy). State mobilization rate is assigned by male state of residence.

Table 4.9: IV Specifications: Impact of Female Labor Supply on Female and Male Earnings 1940 - 1950
Dependent Variable: Log Weekly Earnings

	A. Full-Time Weekly Earnings: All											
	Females				Males				Male Non-Vets		Male Veterans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IV - Weeks Worked per Woman	-0.107	-0.103	-0.103	-0.068	-0.062	-0.060	-0.061	-0.023	-0.046	-0.025	-0.087	-0.014
	(0.024)	(0.024)	(0.023)	(0.041)	(0.012)	(0.013)	(0.014)	(0.019)	(0.011)	(0.018)	(0.020)	(0.022)
1st Stage Coefficient (Mobilization Rate x 1950)	13.21	13.03	13.02	9.35	13.36	13.11	13.10	9.76	13.08	9.72	13.45	10.16
	(1.61)	(2.39)	(2.39)	(2.70)	(1.80)	(2.37)	(2.37)	(2.67)	(2.36)	(2.65)	(2.52)	(2.81)
OLS -Weeks Worked per Woman	-0.006	-0.006	-0.007	0.007	-0.009	-0.008	-0.008	0.004	-0.004	0.004	-0.015	0.005
	(0.011)	(0.010)	(0.010)	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.008)	(0.006)
n	*78,094*				*213,966*				*192,256*		*174,494*	
	B. Full-Time Weekly Earnings: Whites											
	Females				Males				Male Non-Vets		Male Veterans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IV - Weeks Worked per Woman	-0.124	-0.108	-0.108	-0.072	-0.080	-0.070	-0.071	-0.021	-0.052	-0.024	-0.102	-0.014
	(0.029)	(0.025)	(0.025)	(0.038)	(0.018)	(0.015)	(0.015)	(0.017)	(0.012)	(0.016)	(0.024)	(0.020)
1st Stage Coefficient (Mobilization Rate x 1950)	10.22	11.70	11.68	11.20	10.23	11.66	11.65	11.44	11.70	11.42	11.82	11.74
	(1.81)	(2.15)	(2.14)	(2.72)	(1.90)	(2.17)	(2.16)	(2.73)	(2.17)	(2.72)	(2.26)	(2.82)
OLS -Weeks Worked per Woman	-0.004	-0.012	-0.012	-0.001	-0.006	-0.009	-0.009	0.000	-0.005	0.000	-0.016	0.001
	(0.011)	(0.010)	(0.010)	(0.007)	(0.006)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.009)	(0.005)
n	*69,335*				*198,385*				*178,163*		*162,484*	
Female Age Structure	No	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fraction Farm/Non-white/Avg Ed	No	No	No	Yes	No	No	No	Yes	No	Yes	No	Yes

Standard errors in parentheses account for clustering on state and year of observation. Each column is from a separate pooled 1940 - 1950 microdata regression of log weekly earnings on weeks worked per female by state of residence. All regressions control for state of residence, a year main effect, education, a quartic in potential experience, a constant, and dummies for non-white (where relevant), marital status, and WWII Veteran (for males). All individual demographic variables, aside from state of residence/birth, are interacted with a 1950 dummy. All columns except 1 and 5 control for female age structure (the first two age categories in five year increments, the remaining four in ten year increments over age range 14 - 64). As indicated, models control for state/country of birth and fraction farmers, non-whites, and average education of males ages 13 - 44 in 1940 by state of residence (each interacted with a 1950 dummy). Columns 9 - 12 contain all males in 1940 and only WWII Veterans or WWII non-Veterans in 1950 as noted. Weeks worked per woman is calculated for all non-farm female state residents ages 14 - 64. In OLS regressions, this variable is treated as exogenous. In IV regressions, it is instrumented by state WWII mobilization rate interacted with a 1950 dummy variable. All individual and aggregate controls are included in first stage models. The first stage coefficient and standard error on the mobilization rate variable are given below the regression. Data are from Census PUMS one percent samples (1950 limited to sample line observations) and include those ages 14 - 64, in paid non-farm employment in survey reference week (excluding self-employed), with positive earnings in previous calendar year, who earned between $0.50 and $250 an hour in 1990 dollars (deflated by CPI All Urban Consumers series CUUR0000SA0), who worked at least 35 hours in the survey week and 40 weeks in the previous year, did not live in institutional group quarters, and resided in mainland U.S. states excluding Nevada and the District of Columbia.

Table 4.10: Impact of Female Labor Supply on Female/Male Earnings Differential 1940 - 1950
Dependent Variable: Log Weekly Earnings

	A. Full-Time Weekly Earnings: All							
	Labor Supply Measured in Weeks				Labor Supply Measured in Efficiency Units			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(LS_{Female}/LS_{Male})$	-0.390	-0.386	-0.391	-0.226	-0.471	-0.440	-0.446	-0.197
	(0.075)	(0.074)	(0.076)	(0.190)	(0.098)	(0.092)	(0.095)	(0.165)
$\ln(LS_{Female}/LS_{Male})$ x Female	-0.328	-0.278	-0.272	-0.354	-0.432	-0.332	-0.324	-0.309
	(0.115)	(0.105)	(0.099)	(0.182)	(0.157)	(0.126)	(0.119)	(0.161)
1st Stage Coefficient (Mobilization Rate x 1950)	2.05	2.03	2.03	1.09	1.67	1.77	1.77	1.24
	(0.19)	(0.25)	(0.25)	(0.25)	(0.19)	(0.24)	(0.24)	(0.29)
Implied Female Demand σ_F	-1.39	-1.51	-1.51	-1.73	-1.11	-1.30	-1.30	-1.98
Implied M/F Substition $\sigma_{M/F}$	-3.05	-3.60	-3.68	-2.83	-2.32	-3.02	-3.08	-3.24
p-value of H_0: $(\sigma_F)^{-1} = 0$	0.00	0.00	0.00	0.08	0.00	0.00	0.00	0.08
n	292,060							
	B. Full-Time Weekly Earnings: White							
	Labor Supply Measured in Weeks				Labor Supply Measured in Efficiency Units			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(LS_{Female}/LS_{Male})$	-0.513	-0.463	-0.469	-0.254	-0.527	-0.473	-0.480	-0.237
	(0.107)	(0.089)	(0.092)	(0.200)	(0.113)	(0.096)	(0.099)	(0.185)
$\ln(LS_{Female}/LS_{Male})$ x Female	-0.315	-0.264	-0.254	-0.421	-0.331	-0.277	-0.266	-0.380
	(0.126)	(0.117)	(0.116)	(0.191)	(0.139)	(0.124)	(0.122)	(0.170)
1st Stage Coefficient (Mobilization Rate x 1950)	1.56	1.75	1.75	1.14	1.51	1.70	1.70	1.22
	(0.19)	(0.22)	(0.22)	(0.30)	(0.20)	(0.24)	(0.24)	(0.34)
Implied Female Demand σ_F	-1.21	-1.38	-1.38	-1.48	-1.17	-1.33	-1.34	-1.62
Implied M/F Substition $\sigma_{M/F}$	-3.18	-3.78	-3.94	-2.37	-3.02	-3.61	-3.76	-2.63
p-value of H_0: $(\sigma_F)^{-1} = 0$	0.00	0.00	0.00	0.06	0.00	0.00	0.00	0.06
n	267,720							
Female Age Structure	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	No	No	Yes	Yes
Farm/Non-white/Avg Ed	No	No	No	Yes	No	No	No	Yes

Standard errors in parentheses account for clustering on state and year of observation. Each column is from a separate pooled 1940 - 1950 microdata regression of log weekly male and female earnings on the log ratio of female to male non-farm labor supply measured in weeks (columns 1-4) or efficiency units (columns 5 - 8). All models include controls for veteran status, nonwhite, marital status, education, a quartic in potential experience, (each interacted with a female dummy and a 1950 dummy), state of residence, and a year main effect. As indicated, models control for state female age structure and state/country of birth, and state fraction farmers, non-whites, and average education among males ages 13 - 44 in 1940 (each interacted with a 1950 dummy.) All variables are interacted with a female dummy. Log of (female/male) labor supply measure is calculated for all non-farm state residents ages 14 - 64. This measure and its interaction with a female dummy are instrumented by state WWII mobilization rate and its interaction with a female dummy. The first stage coefficient on the mobilization rate main effect is tabulated below each regression. All individual and aggregate controls are included in first stage models. See text for efficiency unit measure.

Table 4.11: Impact of Female Labor Supply on Male Educational Earnings Differentials 1940 - 1950
Dependent Variable: Log Weekly Earnings

	A. Male College / High School Graduate Log Weekly Earnings Differential									
	All Males					White Males				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
$\ln(LS_{Female}/LS_{Male})$	-0.382	-0.424	-0.267	-0.401	-0.265	-0.471	-0.466	-0.269	-0.428	-0.270
	(0.096)	(0.101)	(0.225)	(0.115)	(0.229)	(0.126)	(0.118)	(0.228)	(0.141)	(0.228)
$\ln(LS_{Female/Male})$ x College	0.139	0.154	0.162	0.189	0.175	0.206	0.261	0.233	0.283	0.231
	(0.097)	(0.114)	(0.222)	(0.119)	(0.215)	(0.126)	(0.117)	(0.210)	(0.131)	(0.210)
$\ln(LS_{College\text{-}Male}/LS_{HS\text{-}Male})$ x College				-0.098	-0.069				-0.042	-0.004
				(0.094)	(0.086)				(0.087)	(0.080)
1st Stage Coefficient (Mobilization Rate x 1950)	2.06	2.04	1.12	1.94	1.11	1.62	1.79	1.18	1.64	1.19
	(0.21)	(0.27)	(0.25)	(0.28)	(0.25)	(0.20)	(0.23)	(0.30)	(0.24)	(0.30)
Implied relative cross-elasticity $\sigma_{HS/CLG}$	0.64	0.64	0.39	0.53	0.34	0.56	0.44	0.14	0.34	0.15
n			*60,445*					*58,885*		
	B. High School Graduate/8th Grade Log Weekly Earnings Differential									
	All Males					White Males				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
$\ln(LS_{Female}/LS_{Male})$	-0.170	-0.205	-0.074	-0.162	-0.098	-0.192	-0.220	-0.115	-0.173	-0.129
	(0.088)	(0.088)	(0.194)	(0.139)	(0.196)	(0.109)	(0.107)	(0.205)	(0.149)	(0.203)
$\ln(LS_{Female/Male})$ x High School	-0.212	-0.219	-0.194	-0.181	-0.161	-0.279	-0.246	-0.154	-0.153	-0.140
	(0.070)	(0.086)	(0.184)	(0.119)	(0.179)	(0.097)	(0.095)	(0.178)	(0.117)	(0.173)
$\ln(LS_{High\text{-}School\text{-}Male}/LS_{8th\text{-}Male})$ x High School				-0.036	-0.104				-0.070	-0.145
				(0.064)	(0.065)				(0.057)	(0.072)
1st Stage Coefficient (Mobilization Rate x 1950)	1.99	2.01	1.14	1.54	1.13	1.64	1.80	1.20	1.56	1.20
	(0.21)	(0.27)	(0.26)	(0.26)	(0.25)	(0.20)	(0.24)	(0.30)	(0.26)	(0.30)
Implied relative cross-elasticity $\sigma_{8th\text{-}Grade/Hs}$	2.24	2.07	3.63	2.12	2.63	2.45	2.12	2.34	1.89	2.09
n			*96,797*					*93,244*		
p-value of H_0: $\Delta W_{HS} = 0$	0.00	0.00	0.24	0.02	0.00	0.00	0.00	0.24	0.04	0.24
Female Age	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Birth State, Married,	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Farm/Non-white/ Avg Ed	No	No	Yes	No	Yes	No	No	Yes	No	Yes

Standard errors in parentheses account for clustering on state and year of observation. Samples and specifications are analogous to Table 4.10 except: sample in Panel A is restricted to males with exactly a college or high school degree; sample in Panel B is restricted to males with exactly a high school degree or 8th grade completion; in addition to time interactions on all individual level controls, all variables are interacted with college graduate dummy in Panel A and high school graduate dummy in Panel B. Log (female/male) labor supply measured in weeks and its interaction with college or high school dummy and a 1950 dummy are instrumented by state mobilization rate and its interaction with college or high school dummy. All individual and aggregate controls are included in first stage of IV models.

Table 4.12: IV Estimates: Impact of Female Labor Supply on Overall and Residual Earnings Inequality 1940 - 1950
Dependent Variable: Change in State Level Estimated Log Earnings Differentials

	Coefficient on $\ln(LS_{Female}/LS_{Male})$											
	A. Overall Inequality: Males				B. Residual Inequality: Males				C. Overall Inequality: Males & Females			
	All		Whites		All		Whites		All		Whites	
	Mean Δ	(1)	Mean Δ	(1)	(1)	(2)	(1)	(2)	Mean Δ	(1)	Mean Δ	(1)
Δ 90-10	-0.336	0.548	-0.316	0.359	0.311	0.502	0.456	0.470	-0.399	1.398	-0.335	0.456
	(0.011)	(0.156)	(0.008)	(0.171)	(0.103)	(0.278)	(0.123)	(0.282)	(0.020)	(0.225)	(0.011)	(0.234)
Δ 90-50	-0.170	0.596	-0.156	0.455	0.230	0.259	0.346	0.205	-0.188	0.489	-0.182	0.251
	(0.010)	(0.122)	(0.007)	(0.147)	(0.081)	(0.220)	(0.098)	(0.220)	(0.010)	(0.112)	(0.008)	(0.155)
Δ 50-10	-0.166	-0.048	-0.160	-0.096	0.081	0.243	0.109	0.265	-0.210	0.910	-0.152	0.205
	(0.009)	(0.129)	(0.008)	(0.178)	(0.086)	(0.211)	(0.096)	(0.204)	(0.015)	(0.202)	(0.013)	(0.274)
Δ 80-20	-0.222	0.447	-0.216	0.550	0.250	0.295	0.378	0.210	-0.262	0.648	-0.227	0.180
	(0.010)	(0.123)	(0.008)	(0.147)	(0.071)	(0.194)	(0.094)	(0.219)	(0.012)	(0.141)	(0.007)	(0.130)
Δ 80-50	-0.117	0.425	-0.106	0.340	0.171	0.125	0.256	0.054	-0.137	0.195	-0.134	0.114
	(0.009)	(0.104)	(0.007)	(0.138)	(0.057)	(0.158)	(0.075)	(0.174)	(0.008)	(0.104)	(0.007)	(0.148)
Δ 50-20	-0.105	0.022	-0.110	0.210	0.079	0.170	0.122	0.156	-0.125	0.454	-0.093	0.066
	(0.008)	(0.120)	(0.006)	(0.115)	(0.055)	(0.132)	(0.070)	(0.147)	(0.009)	(0.119)	(0.008)	(0.167)
Fraction Farm/ Non-white/ Avg Ed	n/a	No	n/a	No	No	Yes	No	Yes	n/a	No	n/a	No

n = 94 in each model: 47 states x two years. Standard errors in parentheses. Each coefficient is from a separate 2SLS regression of the state level 1940 - 1950 change in the log ratio of overall or residual male weekly full time earnings at the specified percentiles on contemporaneous state level change in log(female/male) labor supply measured in weeks instrumented by the state mobilization rate (interacted with a 1950 dummy). All models include state of residence dummies, a 1950 dummy, and a constant. Overall inequality in column 1 is calculated using samples in Table 9 from a regression of male log weekly full-time earnings on year and state of residence dummies. Regression used to calculate residuals for column 2 additionally controls fo: a quartic in potential experience, education, non-white, state of residence, state/country of birth, veteran status, marital status, and state female age structure by year. All individual level controls in residualizing regressions, except for state of birth/residence, are interacted with a 1950 dummy. The regression for column 2 of Panel B adds fraction farmers, non-white, and average years of completed schooling for male state residents 13 - 44 in 1940 (each interacted with a 1950 dummy.) Both the endogenous and instrumental variables in the above regressions are orthogonalized with respect to the microdata variables.

Appendix Table 4.1: Alternative Estimates of Impact of World War II Mobilization Rates on Decadal Changes in State Labor Supply, 1940 - 1950.

Coefficient on Mobilization Rate x 1950 Variable Tabulated in Each Panel

A. Dependent Variable: Weeks Worked Per Woman: Mobilization Rate Assigned by State of Birth

	All		White	
	(1)	(2)	(3)	(4)
Weeks worked in previous year	8.72	11.24	9.25	10.82
	(2.29)	(3.02)	(1.83)	(3.16)
Positive weeks in previous year	0.002	0.274	0.033	0.251
	(0.065)	(0.078)	(0.053)	(0.082)
n	*524,634*		*470,326*	

B. Dependent Variable: Total (Log) Female Labor Supply by State, 1940 - 1950

	All		White	
	(1)	(2)	(1)	(2)
Aggregate Weeks	1.77	1.14	0.96	1.64
	(0.64)	(0.88)	(0.71)	(0.94)
Aggregate Efficiency Units	1.35	1.33	1.04	1.70
	(0.62)	(0.88)	(0.67)	(0.91)
n	*94*			

C. Dependent Variable: Total (Log) Male Labor Supply by State, 1940 - 1950

	All		White	
	(1)	(2)	(1)	(2)
Aggregate Weeks	-0.54	0.11	-0.73	0.42
	(0.59)	(0.72)	(0.62)	(0.72)
Aggregate Efficiency Units	-0.49	0.21	-0.57	0.41
	(0.57)	(0.71)	(0.59)	(0.70)
n	*94*			
Fraction Farmer/Non-white/Avg Ed	No	Yes	No	Yes

Notes for Panel A. Standard errors in parenthesis account for clustering on state of residence and year of observation. Each column is from a separate pooled 1940 - 1950 microdata regression of weeks worked by female state residents on WWII state mobilization rate interacted with a 1950 dummy, a year main effect, a constant, and dummies for: non-white (where relevant), age, marital status, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy. State mobilization rate is assigned by female state of birth. As indicated, models also control for state fraction farmers, non-white, and average education among males ages 13 - 44 in 1940 in state of birth (each interacted with a 1950 dummy.)

Notes for Panels B and C. Each coefficient is from a separate regression of log total state labor supply in weeks or efficiency units by gender and year on the state mobilization rate (interacted with a 1950 dummy), state dummies, a year main effect, and a constant. Regressions are weighted by state-gender population ages 18-64 in each year.

Appendix Table 4.2: Impact of World War II Mobilization Rates on Female Labor Supply by Age and Education: 1940 - 1950. Dependent Variable: Annual Weeks Worked

	Coefficient on Mobilization Rate Variable x 1950			
	All Females		White Females	
	A. Weeks Worked by Age Group			
	(1)	(2)	(3)	(4)
All 14 - 64	9.06	8.28	9.85	8.73
	(2.35)	(2.39)	(2.05)	(2.39)
Ages 14 - 17	1.38	1.20	1.41	0.88
	(0.35)	(0.42)	(0.32)	(0.39)
Ages 18 - 24	2.35	4.85	2.24	5.06
	(0.94)	(1.19)	(0.99)	(1.32)
Ages 25 - 34	1.26	2.16	0.57	2.00
	(0.95)	(1.55)	(0.88)	(1.58)
Ages 35 - 44	1.89	2.32	2.41	2.31
	(0.72)	(0.93)	(0.73)	(0.93)
Ages 45 - 54	1.54	-2.10	2.23	-1.44
	(1.10)	(1.56)	(1.05)	(1.55)
Ages 55 - 64	0.65	-0.15	1.00	-0.08
	(0.99)	(1.08)	(0.96)	(1.05)
	B. Weeks Worked by Education Group			
8th Grade and Below	-2.27	-0.79	-1.05	-0.82
	(1.97)	(1.78)	(1.47)	(1.76)
9th Grade to 11th Grade	-2.44	1.93	-2.75	1.75
	(1.53)	(1.56)	(1.51)	(1.49)
12th Grade and Above	13.78	7.13	13.65	7.81
	(1.77)	(1.79)	(1.85)	(2.02)
n	*585,745*		*530,026*	
Fraction Farm, Non-white, Avg Ed	No	Yes	No	Yes

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each entry is from a separate pooled microdata regression for the relevant demographic subgroup of female weeks worked by state of residence on state mobilization rate interacted with a 1950 dummy, a year main effect, a constant, and dummies for non-white (where relevant), age, marital status, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy. As indicated, models also control for state fraction farmers, non-white, and average education among males ages 13 - 44 in 1940 in women's state of residence (each interacted with a 1950 dummy). Education categories reflect the highest grade completed.

Appendix Table 4.3: Impact of World War II Mobilization Rates on Female Labor Supply by Cohort: 1940 - 1950. Dependent Variable: Annual Weeks Worked

Cohorts	Coefficient on Mobilization Rate Variable x 1950 All		White	
Ages 14 - 24 (1940)	6.11	19.02	4.16	18.14
Ages 24 - 34 (1950)	(5.24)	(5.48)	(5.25)	(5.71)
n	*155,272*		*138,870*	
Ages 25 - 34 (1940)	-3.52	16.84	-2.33	16.21
Ages 35 - 44 (1950)	(4.04)	(6.14)	(3.55)	(6.53)
n	*135,893*		*122,083*	
Ages 35 - 44 (1940)	21.16	15.19	23.42	17.76
Ages 45 - 54 (1950)	(5.39)	(7.38)	(5.40)	(7.23)
n	*115,025*		*103,918*	
Ages 45 - 54 (1940)	11.56	-8.19	13.91	-6.45
Ages 55 - 64 (1950)	(5.82)	(5.22)	(5.46)	(4.75)
n	*100,125*		*92,550*	
Fraction Farm, Non-white, Avg Ed	No	Yes	No	Yes

Standard errors in parenthesis account for clustering on state of residence and year of observation. Each entry is from a separate pooled 1940 - 1950 microdata regression of individual weeks worked by females from the listed cohort on state mobilization rate interacted with a 1950 dummy, a year main effect, a constant, and dummies for: non-white (where relevant), age, marital status, state of residence, and state/country of birth. All individual demographic variables, aside from state of residence/birth, are also interacted with a 1950 dummy. As indicated, models also control for state fraction farmers, non-white, and average education among males ages 13 - 44 in 1940 in female's state of residence (each interacted with a 1950 dummy). Mobilization rate is assigned by female state of residence.

Appendix Table 4.4: IV Specifications: Impact of Female Labor Supply on Female and Male Earnings 1940 - 1950: Dropping the South and Adding 4 Region x 1950 Dummies
Dependent Variable: Log Weekly Earnings

	Females						Males					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Full-Time Weekly Earnings: All												
IV - Weeks Worked per Woman	-0.103	-0.068	-0.041	0.018	-0.158	-0.145	-0.061	-0.023	-0.030	0.012	-0.064	-0.032
	(0.023)	(0.041)	(0.021)	(0.022)	(0.035)	(0.075)	(0.014)	(0.019)	(0.015)	(0.013)	(0.020)	(0.023)
1st Stage Coefficient	13.02	9.35	14.70	14.05	11.22	7.21	13.10	9.76	15.34	14.07	11.19	7.65
(Mobilization Rate x 1950)	(2.39)	(2.70)	(4.15)	(3.76)	(1.96)	(3.00)	(2.37)	(2.67)	(4.01)	(3.97)	(1.93)	(2.80)
OLS -Weeks Worked per Woman	-0.007	0.007	0.000	0.010	-0.035	0.005	-0.008	0.004	-0.005	0.004	-0.014	0.006
	(0.010)	(0.006)	(0.007)	(0.006)	(0.015)	(0.008)	(0.006)	(0.003)	(0.004)	(0.003)	(0.008)	(0.007)
n	*78,094*				*58,727*		*213,966*				*161,683*	
B. Full-Time Weekly Earnings: Whites												
	Females						Males					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IV - Weeks Worked per Woman	-0.108	-0.072	-0.018	0.001	-0.155	-0.135	-0.071	-0.021	-0.024	0.014	-0.063	-0.031
	(0.025)	(0.038)	(0.028)	(0.022)	(0.033)	(0.071)	(0.015)	(0.017)	(0.017)	(0.013)	(0.018)	(0.022)
1st Stage Coefficient	11.68	11.20	11.84	15.05	11.64	8.04	11.65	11.44	11.76	14.46	11.41	8.28
(Mobilization Rate x 1950)	(2.14)	(2.72)	(3.80)	(4.03)	(1.99)	(3.08)	(2.16)	(2.73)	(3.81)	(4.08)	(1.99)	(2.86)
OLS -Weeks Worked per Woman	-0.012	-0.001	0.008	0.008	-0.033	0.002	-0.009	0.000	0.001	0.003	-0.016	0.002
	(0.010)	(0.007)	(0.008)	(0.006)	(0.016)	(0.007)	(0.006)	(0.003)	(0.005)	(0.003)	(0.008)	(0.006)
n	*69,335*				*55,847*		*198,385*				*155,743*	
Fraction Farm/Non-white/Avg Ed	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Region x 1950 Dummies	No	No	Yes	Yes	No	No	No	No	Yes	Yes	No	No
Excluding South	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

Standard errors in parentheses account for clustering on state and year of observation. Samples and specifications are identical to Table 4.9 columns 3 and 4 (females) and columns 7 and 8 (males) except as noted for addition of 4 Census main geographic dummies (interacted with a 1950 dummy) and dropping of the Southern region in columns 5, 6, 11 and 12 (VA, AL, AR, FL, MS, NC, SC, TX, KY, MD, OK, TN, and WV).

Appendix Table 4.5: Lagged Dependent Variable IV Specifications: Impact of Female Labor Supply on Female and Male Earnings 1940 - 1950
Dependent Variable: Log Weekly Earnings

	A. Full-Time Weekly Earnings: All							
	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IV - Weeks Worked per Woman	-0.095	-0.078	-0.078	-0.131	-0.068	-0.051	-0.052	-0.029
	(0.038)	(0.030)	(0.030)	(0.049)	(0.022)	(0.017)	(0.018)	(0.018)
Lagged Dependent Variable	-0.033	-0.085	-0.084	-0.402	0.025	-0.048	-0.046	-0.062
(1940 State Mean Log Wage x 1950)	(0.084)	(0.063)	(0.062)	(0.153)	(0.073)	(0.059)	(0.060)	(0.071)
1st Stage Coefficient	9.40	10.24	10.23	8.87	7.82	9.38	9.37	11.85
(Mobilization Rate x 1950)	(2.07)	(2.59)	(2.59)	(2.74)	(2.08)	(2.29)	(2.29)	(3.19)
n	*78,094*				*213,966*			
	B. Full-Time Weekly Earnings: Whites							
	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IV - Weeks Worked per Woman	-0.070	-0.057	-0.057	-0.097	-0.060	-0.047	-0.049	-0.029
	(0.028)	(0.023)	(0.024)	(0.034)	(0.018)	(0.015)	(0.016)	(0.017)
Lagged Dependent Variable	-0.284	-0.275	-0.277	-0.360	-0.102	-0.148	-0.143	-0.093
(1940 State Mean Log Wage x 1950)	(0.074)	(0.052)	(0.053)	(0.113)	(0.063)	(0.048)	(0.051)	(0.077)
1st Stage Coefficient	10.22	11.73	11.71	11.10	8.90	10.78	10.77	12.88
(Mobilization Rate x 1950)	(1.64)	(2.49)	(2.49)	(2.60)	(1.84)	(2.21)	(2.21)	(3.22)
n	*69,335*				*198,385*			
Female Age Structure	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	No	No	Yes	Yes
Fraction Farm/Non-white/Avg Ed	No	No	No	Yes	No	No	No	Yes

Standard errors in parentheses account for clustering on state and year of observation. Each column is from a separate pooled 1940 - 1950 microdata regression of log weekly earnings on weeks worked per female by state of residence instrumented by state mobilization rate (interacted with 1950 dummy). The lagged dependent variable is equal to the 1940 state mean log weekly full-time wage for the relevant gender/race group interacted with a 1950 dummy. Specifications and control variables are otherwise identical to Table 4.9.

Appendix Table 4.6: IV Estimated Impact of Female Labor Supply on Female/Male Earnings Differential 1940 - 1960

Dependent Variable: Full-Time Log Weekly Earnings

	A. All				B. Whites			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
$\ln(LS_{Female}/LS_{Male})$	-0.385	-0.579	-0.583	20.1	-0.707	-0.837	-0.845	-1.410
	(0.089)	(0.168)	(0.171)	(311.8)	(0.194)	(0.259)	(0.267)	(1.265)
$\ln(LS_{Female}/LS_{Male})$ x Female	-0.039	-0.028	-0.014	13.0	0.094	0.138	0.156	-1.733
	(0.105)	(0.122)	(0.114)	(291.5)	(0.171)	(0.200)	(0.191)	(4.184)
1st Stage Coefficient (Mobilization Rate x 1960)	2.10	1.77	1.77	-0.03	1.17	1.06	1.06	0.24
	(0.39)	(0.47)	(0.47)	(0.42)	(0.32)	(0.35)	(0.35)	(0.40)
Implied Female Demand σ_F	-2.36	-1.65	-1.68	0.03	-1.63	-1.43	-1.45	-0.32
Implied M/F Substition $\sigma_{M/F}$	-25.48	-36.36	-69.99	n/a	n/a	n/a	n/a	-0.58
p-value of H_0: $(\sigma_F)^{-1} = 0$	0.02	0.03	0.03	0.96	0.07	0.10	0.10	0.55
n	*566,221*				*517,458*			
Female Age Structure	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State of Birth	No	No	Yes	Yes	No	No	Yes	Yes
Farm/Non-white/Avg Ed	No	No	No	Yes	No	No	No	Yes

Standard errors in parentheses account for clustering on state and year of observation. Each column is from a separate pooled 1940 and 1960 microdata regression of log weekly male and female earnings on the log ratio of female to male non-farm labor supply in weeks instrumented by the state mobilization rate interacted with a 1960 dummy. Sample definition and specifications are identical to Table 4.10 columns 1 - 4 except that 1960 Census data is used in place of 1950 Census data.

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