# SOCIO-ECONOMIC RETURNS TO VOLUNTARY ARMED FORCES SERVICE

by

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This collection of essays, which serve as my dissertation, is dedicated to my parents, Philip Henry and Marsha Duncan Routon, whose love and support have been the bedrock of all my accomplishments.

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ABSTRACT

This dissertation has three chapters, each one originally being a separate analysis and paper. All three pertain to the socio-economic returns to having served in the United States Armed Forces during the early 21st century wars in Afghanistan and Iraq. By the time they were compiled, Chapter 1 was already published in the *Journal of Labor Research*. The second chapter is coauthored with Dr. Christian Brown, now an economist at the FDA, while the others are solo authored.

In Chapter 1, I estimate the effect of military service during these wars on civilian labor and educational outcomes. I find that veteran status increases civilian wages by approximately ten percent for minorities but has little or no effect on whites in this regard. Veterans of all demographic groups are found to be equally employable and equally as satisfied with their civilian occupation as non-veterans. For females and minorities, veteran status substantially increases the likelihood one attempts college. They are found to be more apt to pursue and obtain a two year degree instead of a four year degree.

With respect to their employment ambitions and perhaps prospects, the average military enlistee is likely to differ from the average American. In Chapter 2, we estimate the impact military service has on civilian wages across the wage distribution. For early 21st century veterans, we find that former military service grants civilian wage premiums at and below the median wage level but perhaps penalties at the high end of the wage distribution. For late 20th century veterans, who were mostly peacetime volunteers, we find evidence that veteran wage premiums were more constant across the wage distribution.

Military service adds additional challenges for married couples. In Chapter 3, I perform a trajectory analysis of the effect of military service on the likelihood of divorce. I find that these individuals were most likely to get a divorce in the first year

following active duty service, with an increased probability of three to six percentage points. A within-racial group analysis shows that these effects are stronger for whites than minorities. I find that veterans who served during an earlier period (1980-1992) were unaffected, implying differing effects for wartime versus peacetime service.

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LIST OF ABBREVIATIONS

| | |
|---|---|
| 9/11 | September 11, 2001 |
| AFQT | Armed Forces Qualification Test |
| ASVAB | Armed Services Vocational Aptitude Battery |
| AVF or All-Vol. Force | All-Volunteer Force |
| BLS | United States Bureau of Labor Statistics |
| BMI | Body mass index |
| CART | Classification and regression tree |
| CI | Confidence interval |
| CPS | Current Population Survey |
| CREDO | Chaplain's Religious Enrichment Development Operations |
| CWHS | Continuous Work History Sample |
| DMDC | Defense Manpower Data Center |
| Edu | Education |
| Eq | Equation |
| FE | Fixed effects |
| FEQR | Fixed effect quantile regression |
| GI Bill | Servicemen's Readjustment Act of 1944 |
| GPA | Grade point average |
| IV | Instrumental variables |
| JAG | Judge Advocate General's Corps |
| ML | Maximum likelihood |
| MSA | Metropolitan statistical area |
| NLS | National Longitudinal Surveys |
| NLS-72 | National Longitudinal Study of the High School Class of 1972 |
| NLSY | National Longitudinal Survey of Youth |
| NLSY79 | National Longitudinal Survey of Youth 1979 |
| NLSY97 | National Longitudinal Survey of Youth 1997 |
| NLSYM | National Longitudinal Survey of Young Men |
| OLS | Ordinary least squares |
| POLS | Pooled ordinary least squares |
| PREP | Prevention and Relationship Enhancement Program |
| PSM | Propensity score matching |
| PTSD | Post-traumatic stress disorder |
| QR | Quantile regression |
| RCS | Reserve Components Survey |
| `twang` | Toolkit for Weighting and Analysis of Nonequivalent Groups |
| U.S. or USA | United States of America |
| USD | United States dollars |
| VA | The United States Department of Veterans Affairs |
| VE | Veteran effect |
| Wk | Week |
| WWII | World War II |

# CHAPTER 1

# The Effect of 21st Century Military Service on Civilian Labor and Educational Outcomes

## 1.1 Introduction

The largest single employer of young adults in the United States is the military (Angrist, 1998).[1] The military is also the largest vocational training institution in the country (Mangum & Ball, 1987). In 2010, the number of military veterans in the U.S. was approximately 21.8 million or roughly seven percent of the total population. The United States Department of Veterans Affairs is the government's second largest department and has an annual budget of over $87.6 billion. Taken together, these facts and figures emphasize the importance of studying the effects of military service on the subsequent lives of veterans.

Labor economists are particularly interested in how military service affects the civilian labor market outcomes of veterans. Are veterans more or less employable than they were pre-service? Are they earning higher or lower wages than they would have had they not joined the military? Some enlistees view service as a substitute for college, but how good of a substitute is it? Or, does military service act as a pathway to higher education and make one more likely to pursue a college degree post-service? The answers to these questions are important not just to labor economists, but to those who are considering enlistment, policy makers, and veterans themselves.

[1] As of January 31, 2013, there were 1,429,995 active duty personnel in the U.S. armed forces with an additional 850,880 personnel in the reserves (U.S. Dept. of Defense 2013).

The effects of military service on civilian labor outcomes in past war eras have been extensively researched. There is also a small body of existing literature on the educational outcomes of veterans of past wars. The estimates for these effects differ greatly by era. This is of little surprise considering that military training, the civilian workforce, and several other relevant factors have changed from era to era. To this author's knowledge, this study is the first to estimate these effects for the most recent generation of veterans, that is, the veterans of the Afghan and Iraqi theaters (coined Operation Enduring Freedom and Operation Iraqi Freedom, respectively). Thus, the effects of military service on civilian labor and educational outcomes are unknown for the veterans of the early 21st century wars.

The modern military offers occupational training in a variety of fields. Among these are several highly technical training opportunities that were not available to soldiers of previous eras. As the military experience and the training received by soldiers changes, so too should the returns to military service. The new technical skills taught in the armed forces should increase these returns. However, the civilian labor force is more educated relative to past war eras (Laing, p.143). The returns to military service may have decreased if the improved quality of military training has not kept up with the increase in civilian education. Unlike many veterans of previous wars, the veterans of the early 21st century wars were all volunteers. Thus, there are several reasons to think the effects of modern military service may differ from past eras.

The goal of this analysis is to estimate the overall effects of military service on civilian labor and educational outcomes during the early 21st century.[2] As the effects of military service have changed with every era and because of the reasons listed above, it is highly likely that these new veterans have also been influenced by different effects.

[2]For clarification, my treatment samples only include those individuals who served on active duty, not in the reserves, during the wars in Afghanistan and/or Iraq.

It is too early to examine the long-run effects military service will have on these new veterans. The short-run effects, however, can be interesting by themselves and may illuminate the path on which veterans have been set. My primary data come from the National Longitudinal Study of Youth 1997 (NLSY97). The raw data suggests that veterans are both more likely to attempt college and earn associate's (two year) degrees, especially minority and female veterans, but that there are no statistical differences regarding civilian wages and employment. To determine whether there are any causal effects, I make use of least squares, sibling fixed effects, and propensity score matching methodologies. I find that veteran status increases civilian wages by approximately 10 percent for minorities but appears to have no effect on whites. All veterans are found to be equally employable and equally as satisfied with their civilian occupation as non-veterans. Military service and the financial benefits provided for student-veterans are found to substantially increase the likelihood minorities and females attempt college. Evidence suggests that these veterans mostly pursue and obtain associate's (two year) and not bachelor's (four year) degrees.

## 1.2 Veteran Outcomes

### 1.2.1 Theoretical Foundations

Economic theory is ambiguous as to the impact of military service on civilian wages (Bryant and Wilhite, 1990). There are several channels through which military service could have positive labor market effects. Browning et al. (1973) hypothesize that service provides a "bridging environment" that helps transition a youth from pre-military life to civilian life. Relative to a high school graduate, veterans likely have higher levels of human capital due to their military training. Veteran status can also act as a positive screen, allowing employers to distinguish more productive workers

from less productive workers (De Tray, 1982). To gain entry into the armed forces, one must pass exams that ensure the candidate is physically, mentally, and morally capable of service. Since most employers are aware of these exams, veteran status signals that the prospective employee has these levels of physical, mental, and moral fortitude (positive selection). Perhaps military service, and more likely the financial assistance the military provides student-veterans, makes one more likely to attend college and this additional education is the channel through which military service increases labor market outcomes.

There are also ways in which military service might harm subsequent civilian labor outcomes. Young people with high civilian wage opportunities are less likely to volunteer for the armed forces (negative selection). Thus, veteran status may act as a negative screen. Berger and Hirch (1985) note that this was the case for Vietnam veterans. Additionally, employers may believe that insuring a veteran may be relatively more costly as they might feel veterans are more prone to physical and mental health conditions as a result of service and participating in warfare. If military experience is of little use in the civilian workplace, then it may create older workers with fewer skills and thus lower wages (Bryant & Wilhite, 1990). It may be the case that the general civilian attitude toward a war (that is, whether the average American believes the war is just or unjust) influences the labor market outcomes of returning veterans. If military service is viewed as a substitute for college by recruits, and the returns to military service are less than the returns to a college education, then the labor outcomes of those veterans who would have otherwise attended college will decrease.

### 1.2.2 20th Century Veteran Outcomes

Hirsch and Mehay (2003) note that the development of the literature on the effects of military service on earnings has in many ways paralleled the (far larger) literature about the effects of schooling on earnings. Like the literature on schooling, researchers of military service have used a wide range of empirical strategies and techniques in the attempt to pin down the magnitudes and signs of the effects.[3] In contrast to the literature on schooling, the literature on military service has reached less of a consensus as to its effects on the subsequent lives of veterans.

These researchers do agree, however, that the effects of military service on civilian labor outcomes have changed over time. The changing returns to military service are due to changes in military training, the overall military experience, the returns to civilian training and education, the macroeconomy, and several other factors. More studies have found labor market premiums for both World War II and Korean War veterans than penalties (e.g., Browning et al., 1973; Little & Fredland, 1979; Martindale & Poston, 1979). The reverse can be said of the veterans of the Vietnam War (Martindale & Poston, 1979; Berger & Hirsch, 1983; Schwartz, 1986; Angrist, 1990). There is now a small but growing body of literature pertaining to the All-Volunteer Force period (1973-present). Bryant et al. (1993) find that veterans from the early years of the All-Volunteer Force earned 1.7 percent less in their subsequent civilian jobs and that non-veterans would have received a 7.9 percent penalty had they chosen to serve in the military. Also examining the early All-Volunteer Force, Phillips et al. (1992) find that earnings were higher for those who served between 1979 and 1984 while they were serving than for non-veterans during the same period. This likely contributes to one's choice to volunteer, assuming volunteers predict this outcome.

[3] Among the methodologies used are comparisons of summary statistics (e.g., Villemez & Kasarda 1976), ordinary least squares (e.g., Fredland & Little 1980; Bryant & Wilhite 1990), fixed effects (e.g., Teachman 2004), instrumental variables (e.g., Angrist 1990; Angrist & Krueger 1994; Teachman & Call 1996) and matching techniques (Hirsch & Mehay 2003; Angrist 1998).

The idea of a veteran wage premium relies partly on the idea that the skills received by military volunteers should be transferable to a civilian occupation. Magnum and Ball (1987, 1989) investigate the transferability of military skill training to civilian careers in the late 1970s and early 1980s. Their results show vast gender differences, but, overall, they find significant amounts of skill transfer and that military training is not significantly different than training from a vocational or technical institute. Perhaps more interesting is that they find military-trained individuals start to exceed civilian-trained individuals in terms of earnings after two years in the civilian labor force.

Labor economists have long known that education is one of the most substantial determinants of future income. The relationship is both strong and positive (see, e.g., Card (2001)). How does military service affect educational outcomes? Since recruits are self-selected on characteristics that are likely related to ability and educational aspiration, veteran status will be associated with educational attainment. Service may also change the educational aspirations of veterans (Teachman & Call, 1996). The introduction of the GI Bill of Rights and its post-9/11 changes (which provide for up to 100 percent tuition coverage, a $1000 per year books and supplies stipend, and a monthly housing allowance) undoubtedly affected their access to education. Additional civilian education may be the primary channel through which modern military service affects civilian employment and earnings. Thus, an analysis of the effects of military service on educational attainment may also help explain the effects on labor outcomes, especially if the analyses are done in tandem.

The body of literature on the relationship of military service and civilian education is quite small. These studies have shown that this relationship also varies across history. For World War II and Korean War veterans, military service seems to have had a strong positive relationship with civilian education (Fligstein, 1976; Mason, 1970).

For Vietnam veterans, however, Cohen et al. (1992) find a negative relationship and that this relationship explained much of the lower occupational attainment of these veterans. In their study of the early days of the All-Volunteer Force, Teachman and Call (1996) find that military service increases the educational attainment of whites but not African-Americans. The authors explain these results by referencing their own previous finding (Teachman et al., 1993) that the military recruits relatively less qualified individuals from the white population but not from the African-American population.

## 1.3 Empirical Methodology

I use three approaches to determine the effect of military service on civilian outcomes. First, I use least squares to estimate the effects of military service with the model

$$Y_i = \alpha + \beta veteran_i + \gamma X_i + \varepsilon_i \tag{1}$$

where $Y$ is the civilian outcome, $\alpha$ is an intercept, $\boldsymbol{X}$ is a vector of controls with $\gamma$ its corresponding vector of coefficients, and $\varepsilon$ is the error term. The variable $veteran_i$ is a veteran status indicator and its corresponding coefficient $\beta$ is of key interest. The set of controls in $\boldsymbol{X}$ include several variables that help address the military selection issue and are fully discussed in Section 4.3.

My second approach takes advantage of the multiple-respondent households in the data. Specifically, I consider those veterans surveyed that have at least one sibling who was also a respondent. Using a sibling fixed effects approach, I am able to control for family-specific unobserved heterogeneity. Thus, the models take the form

$$Y_i = \alpha + \beta veteran_i + \gamma X_i + s_f + \varepsilon_i \tag{2}$$

where $s_f$ represents the fixed effects and the remaining terms are the same as in Equation (1). Controlling for family-specific heterogeneity could be important for several reasons. As examples, military service may be a family tradition and some parents may encourage, while others discourage, service.

My third approach is propensity score matching (PSM). PSM was first proposed by Rosenbaum and Rubin (1983) in their seminal paper published in *Biometrica* and is now a widely used methodology. In a true experiment, one compares treated and untreated units assuming that treatment is randomly assigned. Only then can causal chains be wrought. In this case, treatment is military service which (since the start of the All-Volunteer Force) is an entirely voluntary activity. Hence, veterans and non-veterans are likely to vary in ways other than military service. PSM corrects for observable pretreatment differences between individuals by matching treated and untreated individuals by propensity score, their likelihood of being treated.

PSM is most useful for cases of causal inference (and selection bias) in non-experimental settings where: ($i$) there are relatively few observations in the non-experimental comparison group that are comparable to the treatment units; and/or ($ii$) selecting a subset of comparison observations similar to the treatment group is difficult because observations need to be compared across a large set of pretreatment characteristics (Rosenbaum & Rubin, 1983). With regards to the effects of military service, both of these scenarios are likely. There are probably few non-veterans who are truly comparable to military volunteers and there are a large number of individual-level characteristics that likely affect the probability of joining the military.

The literature now contains numerous propensity scoring methods (e.g., logistic regression, CART). Following McCaffrey et al. (2004), I use generalized boosted regression to find the optimal set of propensity scores which are then used in a propensity score weighting framework.[4]This method of obtaining the propensity scores was

[4]The *R* package `twang` (Toolkit for Weighting and Analysis of Nonequivalent Groups) was used

chosen because of its several advantages. First, it very often achieves a better balance between the treatment and control groups than other methods of obtaining propensity scores (Ridgeway et al., 2012). Second, it constructs weights that also balance rates of missingness in the treatment and control groups. This makes it an ideal choice when dealing with data sets like the National Longitudinal Surveys where missingness is a common issue. Third, this procedure can help the researcher discover and account for any interaction terms that could serve as relevant matching covariates. Lastly, it allows for matching on categorical and ordered variables as opposed to partitioning such variables into dummy variables as is often done in applied research (Ridgeway et al., 2012).

For a detailed discussion of this propensity scoring method, I direct the reader to Ridgeway (2006), McCaffrey et al. (2004), and Ridgeway et al. (2012), though I summarize the method here. Let $v = 1$ and $v = 0$ denote the sample of veterans and non-veterans, respectively, and $\boldsymbol{x}$ represent the set of matching covariates. Individuals can only be matched on pre-treatment observables as post-treatment observables could have been affected by treatment. My goal is to weight the individuals in the non-veteran (control) sample in such a way that, when weighted, there are no observable differences between this group and the sample of veterans. Statistically, I want to weight the joint distribution of the non-veteran features, $f(x|v = 0)$, so that it becomes identical to the joint distribution of the features in the sample of veterans, $f(x|v = 1)$, as shown in the equation

$$f(x|v = 1) = w(x)f(v = 0). \tag{3}$$

Solving for $w(x)$ and applying Bayes theorem to the two conditional distributions of

to calculate the propensity scores in the models presented here. All models were also re-estimated using propensity scores obtained from logistic regression (as a robustness check) and similar results were found.

$x$ gives us

$$w(x) = K\frac{f(v=1|x)}{1-f(v=1|x)} \tag{4}$$

where $K$ is a constant independent of $x$ that will cancel out in the outcome analysis. The term $f(v=1|x)$ is the propensity score, that is, the probability that an individual with features $\boldsymbol{x}$ will join the armed forces.[5] After construction and use of these weights, the only difference between veterans and non-veterans will be treatment and possibly the outcome variables (presented in Section 4.2). Thus, the difference between the average outcome in each group measures the "veteran effect" ( $VE$) as shown in

$$VE = \frac{\sum_{i=1}^{N} v_i y_i}{\sum_{i=1}^{N} v_i} - \frac{\sum_{i=1}^{N} (1-v_i) w(x_i) y_i}{\sum_{i=1}^{N} (1-v_i) w(x_i)}. \tag{5}$$

For the within-race analysis, the sample was restricted by race and the propensity scores were recalculated. The same applies for the within-gender analysis. Therefore, there are five sets of propensity scores used in the PSM analysis, one for the full sample analysis and one for each of the four sub-sample analyses. For all five sets, the balance condition is satisfied. As noted by Imbens (2004) and others, there are several problems with OLS estimation. For example, regression estimators are more often sensitive to changes in the specification. Also, the sample of veterans in the NLSY97 is already relatively small (particularly the female veteran sample) and the sibling fixed effects specification requires reducing the sample to those within multiple-respondent households.[6] Therefore, PSM appears to be the preferred specification in this case.

[5]For a more detailed analysis of these weights, I direct the reader to Wooldridge (2002), Hirano & Imbens (2001), and McCaffrey et al. (2004).

[6]In the interest of full disclosure, all tables relate veteran sample sizes.

# 1.4 Data

## 1.4.1 National Longitudinal Survey of Youth 1997

The data used for this analysis come from the NLSY97, an annual survey of young men and women born in the years 1980-84.[7] The survey is conducted by the United States Department of Labor's Bureau of Labor Statistics and was designed to gather information on the significant life events of United States citizens, especially their labor market activities (NLS website). The NLSY97 consists of a nationally-representative sample of almost 9,000 respondents, initially surveyed in 1997. In this initial round, both the eligible youth and one of their parents were interviewed. The Bureau of Labor Statistics interviews the cohort annually and data are collected on a wide range of topics. These respondents were 16 to 22 years old during 2001, the year of the September 11 attacks on the U.S. and the start of the war in Afghanistan and 18 to 24 in 2003, the beginning of the Iraq War. This age range makes the NLSY97 cohort a useful sample for studying the impacts of military service during these wars on veteran outcomes. Additionally, approximately half of the sample is comprised of multiple-respondent households, making sibling fixed effects a viable empirical strategy.

## 1.4.2 Civilian Labor and Educational Outcomes

With regard to labor outcomes, employment and earnings are of key interest. The NLSY97 contains weekly labor market data. I use outcome data from the 2010 survey as it was the most recent at the time of writing, this was after the Great Recession,[8]it

[7]In a supplemental analysis, I also use data from the CPS July 2010 Veterans Supplement. See Section 6.

[8]Raw NLSY97 data suggests that Iraq and Afghan veterans fared equally as well as non-veterans during the recession in terms of wages and weeks worked. For example, veterans and non-veterans worked on average 36.0 and 37.7 weeks and earned an hourly wage of $20.12 and $21.62 during 2008, respectively. These differences are not statistically different with respective p-values from t-tests of mean equality being 0.136 and 0.795. Wages and weeks worked for 2009 were also not statistically different across veteran status.

is comparable to my secondary data (presented in Section 6), and respondents were at least 25 years of age giving them both time to serve in the armed forces and attempt higher education. The primary labor outcomes are hourly wages and the number of weeks worked throughout the year. The total number of weeks worked (inclusive of military employment) before 2010 is also used as a control in the wage models. Respondents in the NLSY97 are also asked to rate their satisfaction with each job they hold on a one-to-five scale every year.[9] I collect these satisfaction rates to estimate whether veterans are more, less, or equally satisfied with their civilian occupation than comparable non-veterans. Additionally, I examine the effects of veteran status on the total number of jobs individuals have had as adults (inclusive of military occupations) as a measure of job turnover. Lastly, since veterans are thought to be given preferential treatment in the public sector, I examine how modern military service effects the probability of public sector employment.

Four educational outcomes are examined. Of key interest here is whether military service and the financial assistance offered to student-veterans increases the chance that one will attempt college. Thus, an indicator variable for individuals who have attempted college was created. Additionally, two indicator variables for degree completion serve as outcome variables. One variable indicates that an associate's (two year) degree was earned and the other a bachelor's (four year) degree. In 2010, the NLSY97 respondents were between 25 and 31 years of age and have generally had time to serve in the armed forces and complete a two- or four-year degree. This is perhaps not enough time to both serve in the military during wartime and complete a lengthy graduate degree. Thus, the final outcome examined is an indicator for those that have at least begun a graduate program as opposed to a completion indicator.

[9] In the original data, this variable is coded in descending order (where 1 represents extremely satisfied and 5 extremely dissatisfied). I recode the variable in ascending order for ease of interpretation.

Table 1 shows the means of these outcome variables for the different veteran and non-veteran sub-samples. Veterans appear to earn less and work fewer weeks throughout the year (except for minorities who worked more) on average, though these differences are not statistically significant. The difference in weeks worked may be due to new veterans taking time to adjust to civilian life (or a well-deserved break). Veterans appear to be equally as satisfied with their civilian job as non-veterans. The number of adult jobs are, however, statistically different across veteran status groups for all sub-samples excluding females. The fact that veterans have had fewer jobs comes as no surprise as they've been abroad serving in the armed forces. What may be surprising, however, is that the differences in the number of jobs are all relatively small. There are no statistically-significant differences regarding public sector employment.[10]

All veteran sub-samples attempted college at higher rates except for whites who are not statistically different from their non-veteran counterparts. The table also shows that white veterans are less likely to earn a bachelor's or start a graduate program while minority veterans appear unaffected. It may be the case that white veterans find the sum of military tenure and a four year degree as too lengthy a time out of the civilian labor force, where money can be made and families started.[11] Hence, they opt for a two year degree or no higher education. Minority veterans attempted college at much higher rates than their non-veteran counterparts but an associate's degree was the only degree they completed at a higher rate. Separating by gender, both male and female veterans attempt college and earn associate's degrees

[10]In my secondary data (presented in Section 1.6), however, I do find statistically-significant differences regarding public sector employment as well as suggestive evidence that veteran status is causing these differences.

[11]Several previous studies (e.g., Brien (1997) and DaVanzo & Rahman (1993)) have noted that whites tend to marry at younger ages.

at higher rates. The results in Section 5 will assist in the disclosure of any causal effects behind these veteran-outcome relationships.

### 1.4.3 Additional Control Variables

The NLSY97 is a rich set of data. To be used as controls and for calculation of the propensity scores, I gather several types of data from these surveys. Among the general demographics obtained are gender, race, age, an urban residence indicator, and census region. Gender wage differentials are declining in the United States but are still large compared to a number of other countries (Blau & Khan, 2000), making gender an important control. Race is important as, historically, minorities disproportionately apply and serve in the armed forces (Cooper, 1977; Orvis & Gahart, 1990).[12] Region plays an important role in that, again historically, the military is somewhat over-represented by southerners (Kane, 2005).

It is particularly important to control for academic ability. Ability plays an important role in both the decision to volunteer for military service and labor and educational outcomes (Card, 1995). A useful aspect of the NLSY97 is that the respondents were all administered a computer-adaptive form of the Armed Services Vocational Aptitude Battery (ASVAB), regardless of future military service. The ASVAB is a multiple choice test, typically administered by the United States Military Entrance Processing Command, used to determine qualification for enlistment in the United States Armed Forces. The NLSY97 reports the aggregate percent score on the four major tests pertaining to verbal and mathematics skills. Since verbal and math skills are general skills and the ASVAB is the same test used for military enlistment, these test scores are an ideal ability control in this context. As measures of

[12]In the whites-only sub-sample, individuals are effectively exactly matched on race. Thus, race variables are not used as controls or in the matching procedure but were used in the minorities analysis as this sample contains multiple races. Similarly, in the within-gender analysis, the two samples are exactly matched on gender.

scholastic achievement, I also use respondents' high school grade point average (GPA) and highest grade completed (total years of schooling; this variable is not used in the educational outcome models) as controls.

I additionally control for a number of parental characteristics: combined annual income, parental education, the mother's age when the respondent was born,[13] parents' religiosity and an indicator for whether they raised the respondent to be Christian, and indicator variables for parents born in the United States. Parental income and education have been found to be positively correlated with both a child's future income and educational attainment (Altonji & Dunn 1991, 1996, and 2000). The financial compensation for service and inability to pay for one's college education are often determinants of military enlistment. First-generation Americans often have different sets of opportunities than do those whose families have been in the United States for a longer period, may use military service as a "bridging environment" more readily than others, and may also have different levels of patriotism.

Though armed forces volunteers undoubtedly differ from non-volunteers in physical and health characteristics, previous studies have not controlled for these differences. One must be physically fit to enlist in the armed forces. Health status is also correlated with labor market outcomes (Chirikos, 1993; Smith, 2009). Height and weight, especially obese weight, can affect earnings as well (see, e.g., Case & Paxson (2008) and Baum & Ford (2004)). Thus, I control for three physical and health-related characteristics. I use height and weight data to construct a body mass index (BMI) variable for when the respondent was 18 years old.[14] Also included is an index for the youth's general teenage health status, as reported by their parents, on

[13] In her study of childhood residences, Haurin (1992) finds that maternal age when the child is born is associated with several of the child's young adult outcomes such as the likelihood of high school completion, teen pregnancy, and serious illegal activities.

[14] Eighteen is the youngest age one typically enlists in the armed forces. Technically, one may enlist in the armed forces at 17 years of age with parental consent. However, few volunteers are 17 years of age.

a one-to-five scale. Lastly, I match on a variable that describes the number of days per week the respondent typically exercises at least 30 minutes.[15]

Table 2 presents means of these control variables across veteran status for the entire sample and then by each demographic sub-sample. Veterans scored higher on average than non-veterans on the ASVAB except in the white sample where the difference is insignificant. Unlike in previous studies of earlier time periods (e.g., Teachman et al. (1993)), there is no evidence here that white enlistees were relatively less qualified than white non-enlistees. Veterans are shown to more often have American-born fathers though only white veterans more often have American-born mothers. Veterans also come from younger mothers, were more healthy as teenagers than non-veterans, and are more likely to have been raised Christian. Differences in average parental education are small and most are statistically insignificant. Across all samples, veterans are found to be no different than non-veterans in terms of their high school GPAs, family incomes, urban status, BMI, exercise habits, or parents' religiosity.

## 1.5 Results

Table 3 presents the results of the baseline regressions. From the full sample analysis, it would seem that veteran status grants a wage premium of 15.7 percent. The sub-sample analyses, however, show that this premium is only captured by minority veterans and may be higher. For all samples, veteran status does not appear to affect weeks worked, job satisfaction, or public sector employment in a statistically significant way. While the sign and significance level of the effects on the number of

[15]Various types of exercise are staples of military training. Youths who already exercise regularly will be better prepared for military training. Youths that enjoy frequent exercise may be more likely to enlist. Unlike height, weight, and health status, exercise habit data was not collected every survey year. I use the data available to construct a measure of exercise for the year closest to, but not over, 18 years of age.

jobs may not be surprising, the magnitude of the effect is interesting. Young adults more often switch jobs and try different forms of employment. Though veterans of the Afghan and Iraqi theaters were abroad for usually several years, it appears they have only missed out on the experience from approximately one additional employee-type job, with females missing less than one additional job.

Minorities and females are found to be the only demographic groups made more likely to attempt college because of military experience. These effects are fairly substantial with minorities and females being 7.8 and 13.8 percentage points more likely to attempt, respectively. With the exception of the whites-only sample, veterans are all found to be more likely to earn an associate's degree. With the exception of the females-only sample, however, veterans are found to be less likely to earn a bachelor's degree and all samples are less likely to attempt graduate school. These effects are most severe for whites who are found to be made 13.5 percentage points less likely to complete a four year degree.

The results of the sibling fixed effects models are shown in Table 4. As in the baseline regressions, a wage effect is only found for minority veterans. Controlling for family-specific heterogeneity has caused this estimated wage premium to drop to 10.7 percent. Again, there appear to be no effects on weeks worked, job satisfaction, or public sector employment for veterans of any demographic group with a single exception. Female veterans are found to have worked approximately 2.7 additional weeks. While still statistically significant, the effects on number of adult jobs have generally decreased after controlling for family-level heterogeneity.

Regarding the educational outcomes in Table 4, there again appear to be positive effects on college entrance probability and associate's degree attainment for minorities and females. Whites and males are found to be 7.0 and 7.7 percentage points less

likely to have completed a bachelor's degree, respectively. All sub-samples are found to be made less likely to have attempted graduate school.

Table 5 presents the results of the PSM analysis. From the full sample analysis, it appears that modern veteran status grants a wage premium of about 9.1 percent. The within-racial group analyses, however, show that this premium is closer to 9.5 percent and is captured entirely by minorities. Veterans are yet again found to work just as often, be just as satisfied with their employment, and be employed in the public sector just as often as non-veterans. Veteran status appears to have no effect on whether whites attempt college or complete an associate's degree but to decrease the likelihood they earn a bachelor's or graduate degree by around eight and three percentage points, respectively. On the other hand, there appear to be positive educational effects for minorities. They are found to be 7.8 percentage points more likely to attempt college and 7.6 percentage points more likely to complete an associate's degree. Female veterans are shown to be made 10.3 percentage points more likely to attempt college and 5 percentage points more likely to earn an associate's degree.

In terms of sign and statistical significance, several effects were consistent across the three specifications. First, I find evidence of a minority veteran wage premium. Looking at the sibling fixed effects and PSM models, this premium is estimated to be close to 10 percent. Additionally, across all sub-samples and specification, I find that veteran status has no effect on general employment, public sector employment, or civilian job satisfaction. Military service and student-veteran benefits are not found to be enticing white male veterans (the largest veteran group) to attempt college at higher rates. On the other hand, I find positive effects in this regard in the minority and female sub-samples.

Considering that the military will help pay for some or all of a college education for veterans, the estimates for whites and males on college entrance may be unexpected.

As more college students are attending part-time, taking longer to graduate, and enrolling at older ages, it may be premature to estimate the effects of modern military service on *ever* obtaining a college degree. The estimates presented here should be thought of as the short-run effects, or the likelihood of seeking and/or obtaining a degree soon after military service. The estimates of the effects on attempting college and completion of an associate's degree, which takes much less time to complete than other degrees, are likely closer to their long-run effects.

## 1.6 Discussion and Conclusions

Using the sample of veterans and non-veterans from the National Longitudinal Survey of Youth 1997, I set out to estimate the effects of modern military service on civilian labor and educational outcomes. This analysis differs from previous research in that the veterans in this sample served in the wars of the early 21st century. The empirical methodologies used include OLS, sibling fixed effects, and propensity score matching. Within-race and within-gender effects are estimated alongside a full sample analysis. I find that minority veterans receive a wage premium of approximately 10 percent, but find no evidence of either a white veteran wage premium or penalty. In terms of educational outcomes, the effects again vary across demographic groups with minorities and females being more likely to attempt college and complete associate's degrees. White male veterans, which are the largest veteran demographic group, are found to be just as likely to attempt college as their non-veteran counterparts. Modern veterans of all demographic groups are found to be equally as employable and equally as satisfied with their civilian occupation as non-veterans.

As previously mentioned, veteran status can act as a positive screen. Thus, the minority wage premium may be due to veteran status helping mitigate statistical

discrimination. However, educational outcome results presented here suggest that additional civilian education may be the channel through which minority veterans are receiving wage premiums. While no evidence of a white veteran wage premium is found in this short-term analysis, there may be long-term effects. Wage growth may be different across veteran status. In a recent study, Angrist et al. (2011) reexamine the wage effects of Vietnam era military service and find, that while these veterans faced initial wage penalties, their wage growth was faster than non-veterans resulting in the penalties being largely erased by the early 1990s. Also in a recent study, Card and Cardoso (2012) report a similar pattern using data on peacetime draftees in Portugal.

Many of the results in this analysis have policy relevance. For example, there are currently government institutions in place (e.g., VetSuccess) to help veterans find employment and adjust to the civilian labor force. If the goal of these institutions is to make sure veterans work and earn at least as much as their non-veteran counterparts, then the results presented here are evidence that this has indeed been achieved.

The educational outcome analysis provides evidence that military service and the financial benefits it grants student-veterans may not be enticing white male veterans to enter college at higher rates, at least in the short-run. This is not to say that these benefits are not aiding those who take advantage of them, but simply that modern military service on the whole does not appear to make white veterans more apt to pursue a college degree. This finding may stem from improvements in military training. As previously mentioned, modern soldiers are often trained in a variety of technical and specialized fields (e.g., computer science and information technology) that can more easily transfer to a civilian career than those skills learned by veterans of past war eras. Unlike many college graduates, modern veterans have often already put these skills to use in a non-classroom setting and have first-hand experience with

advanced equipment. To certain employers, this may make veterans more attractive than college graduates. Thus, these veterans may not attempt college at higher rates as they realize it would add little to their job prospects and future earnings. The absence of any evidence of negative labor market effects help support this theory.

On the other hand, modern minority veterans do appear to be taking advantage of improvements in the GI bill. They are both attempting college and completing associate's degrees at higher rates than their non-veteran counterparts. I find evidence that this may also be the case for female veterans, though the sample of female veterans is notably small.

This analysis was not without shortcomings. The sample size of veterans in the NLSY97 is somewhat small and may raise questions to external validity. At the time of writing, data through 2010 was all that was available. This puts the age of the veterans used in this analysis between 25 and 31. Thus, these results show the effects of military service for the short-run only. More time is needed to pass for an analysis of the long-run economic returns to serving in the wars of the early 21st century. Magnum and Ball (1989) found that it took two years of civilian life for the veteran wage premiums to take effect for those who served in the early days of the All-Volunteer Force. Thus, though military service was found to have no effect on the hourly wages of white veterans, it is possible that effects may become apparent in a future analysis. With a larger set of veterans, analysis could also be done to determine if these effects differ by military branch.

For comparison of these results to another set of data, Table 6 presents a raw data analysis of the Current Population Survey July 2010 Veterans Supplement. This data unfortunately lacks the depth (e.g., it contains no variable that would help address ability bias) to perform a similar analysis of the effects of modern military service on labor and educational outcomes as done in the body of this paper.[16] Comparison

[16]Also unfortunately, missingness was too high to investigate veteran wages using this data and

of means and simple OLS, however, can be preformed and may prove enlightening. Table 6 shows that, on average, these veterans are more often both part of the labor force and employed. They are also more prone to enter the public sector. The CPS data show that veterans attempt college at higher rates and hold more associate's degrees per capita (with female associate degree completion not being statistically different). Veterans are not found to be significantly different from non-veterans in terms of bachelor's degree completion and graduate school enrollment.

Table 7 shows the results of simple OLS models using this data.[17] Like the previous analysis, I find that modern veterans are equally employable as non-veterans, are more likely to attempt college, and more likely to complete associate's degrees. These results help strengthen the validity of the previous findings. Unlike the NLSY97 analysis, these simple models show that veteran status increases the probability of working in the public sector. These effects are highest for minority veterans who are shown to be 23.6 percentage points more likely to enter the public sector.

Overall, the results of this analysis indicate a positive effect of modern military service on the labor market performance of minorities in the early years of civilian life. These veterans are receiving a substantial wage premium and this appears to have been caused by their military service. Minority and female veterans are also more likely to attempt college and finish a two-year degree. Though white male veterans are not more likely to pursue and complete higher education after military service, they do not appear to be suffering in the labor market as they are found to be doing equally as well as their non-veteran counterparts in terms of employment, wages, and job satisfaction. This may result from military training being a better substitute for college education than it has been in the past.

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the survey did not collect information on job satisfaction or number or jobs.

[17]The controls used in these models are listed in the Table 7 notes.

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**Table 1.1: Means of Outcome Variables across Veteran Status and by Sub-Sample**

| | Full Sample | | Whites | | Minorities | | Males | | Females | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets |
| *Labor* | | | | | | | | | | |
| Hourly wage (USD) | 17.564 | 20.054 | 18.216 | 20.891 | 16.809 | 19.145 | 18.192 | 22.576 | 15.266 | 17.547 |
| Weeks worked | 31.027 | 31.415 | 31.942 | 33.738 | 30.028 | 29.078 | 31.531 | 32.142 | 29.313 | 30.733 |
| Job satisfaction[a] | 3.940 | 3.956 | 3.941 | 4.033 | 3.939 | 3.872 | 3.953 | 3.964 | 3.891 | 3.948 |
| Number of adult jobs[b] | 3.977*** | 4.573 | 4.180** | 4.682 | 3.741*** | 4.455 | 3.856*** | 4.460 | 4.390 | 4.683 |
| Public sector job | 0.093 | 0.090 | 0.091 | 0.088 | 0.094 | 0.092 | 0.090 | 0.084 | 0.104 | 0.096 |
| *Educational* | | | | | | | | | | |
| Attempted college | 0.595*** | 0.521 | 0.568 | 0.590 | 0.627*** | 0.447 | 0.543** | 0.462 | 0.779*** | 0.579 |
| Associate's degree | 0.095** | 0.062 | 0.059 | 0.070 | 0.137*** | 0.055 | 0.081* | 0.051 | 0.143* | 0.073 |
| Bachelor's degree | 0.118*** | 0.194 | 0.118*** | 0.261 | 0.118 | 0.122 | 0.096*** | 0.166 | 0.195 | 0.222 |
| Attempted grad school | 0.012*** | 0.044 | 0.011*** | 0.062 | 0.012 | 0.026 | 0.011*** | 0.033 | 0.013*** | 0.055 |
| $N$ | 348 | 8,455 | 187 | 4,377 | 161 | 4,078 | 271 | 4,183 | 77 | 4,272 |

*Notes*: Data come from the 2010 wave of the NLSY97. Stars refer to p-values from t-tests of sample mean equality across veteran status within each sub-sample. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. [a]On a 1-5 increasing scale. [b]Employee-type jobs worked during or after 18 years of age.

**Table 1.2: Selected Sample Means across Veteran Status and by Sub-Sample**

| | Full Sample | | Whites | | Minorities | | Males | | Females | |
|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets |
| ASVAB score percent | 50.447*** | 44.899 | 55.932 | 56.310 | 43.645*** | 31.329 | 50.883*** | 43.651 | 54.163*** | 46.092 |
| High school GPA[a] | 13.163 | 12.840 | 10.456 | 12.653 | 16.132 | 13.082 | 13.894 | 12.132 | 11.209 | 13.489 |
| Family income ($0,000) | 65.098 | 65.197 | 69.370 | 69.909 | 50.503 | 53.332 | 61.887 | 64.859 | 59.870 | 65.566 |
| Father born in U.S. | 0.898*** | 0.830 | 0.978*** | 0.932 | 0.768** | 0.666 | 0.897*** | 0.827 | 0.899* | 0.834 |
| Mother born in U.S. | 0.863 | 0.845 | 0.981*** | 0.937 | 0.726 | 0.742 | 0.852 | 0.849 | 0.863 | 0.842 |
| Father's education (yrs) | 12.536 | 12.655 | 13.059** | 13.542 | 11.800 | 11.457 | 12.544 | 12.692 | 12.508 | 12.618 |
| Mother's education (yrs) | 12.652 | 12.491 | 13.145 | 13.371 | 12.060*** | 11.505 | 12.602 | 12.571 | 12.824 | 12.413 |
| Male | 0.779*** | 0.495 | 0.807*** | 0.497 | 0.745*** | 0.493 | 1.000 | 1.000 | 0.000 | 0.000 |
| African-American | 0.241 | 0.261 | 0.000 | 0.000 | 0.522 | 0.542 | 0.218 | 0.259 | 0.325 | 0.264 |
| Hispanic | 0.213 | 0.212 | 0.000 | 0.000 | 0.460 | 0.439 | 0.218 | 0.213 | 0.195 | 0.211 |
| Other nonwhite race | 0.009 | 0.009 | 0.000 | 0.000 | 0.019 | 0.019 | 0.007 | 0.009 | 0.013 | 0.010 |
| Age (in 2001) | 19.248*** | 18.961 | 19.135* | 18.944 | 19.379*** | 18.980 | 19.116** | 18.928 | 18.773 | 18.994 |
| Mother's age when born | 24.517*** | 25.549 | 24.943*** | 26.273 | 24.020* | 24.739 | 24.749*** | 25.636 | 24.315** | 25.464 |
| Teenage health[b] | 4.494*** | 4.259 | 4.594*** | 4.429 | 4.371*** | 4.066 | 4.508*** | 4.280 | 4.439** | 4.237 |
| Body mass index | 23.739 | 23.717 | 23.674 | 23.631 | 23.816 | 23.809 | 23.651 | 23.769 | 23.953 | 23.668 |
| Parents' religiosity[c] | 3.712 | 3.776 | 3.382 | 3.509 | 4.083 | 4.075 | 3.708 | 3.761 | 3.728 | 3.791 |
| Raised Christian | 0.967** | 0.942 | 0.946* | 0.913 | 0.993** | 0.975 | 0.971** | 0.945 | 0.955 | 0.939 |
| *N* | 348 | 8,455 | 187 | 4,377 | 161 | 4,078 | 271 | 4,183 | 77 | 4,272 |

*Notes*: Data come from the NLSY97. Stars refer to p-values from t-tests of sample mean equality across veteran status within each sub-sample. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. [a]The NLSY97 has a unique, uniform scale (with a range of 0-100 and mean 12.87) for respondents' high school GPA that takes into account school-specific characteristics and omits non-academic courses like physical education so that GPA comparisons can be made across schools. [b]On a 1-5 increasing scale. [c]On a 0-6 scale with 0 meaning "not religious" and 6 "very religious."

**Table 1.3: Baseline Regression Results: Effects of Modern Military Service on Civilian Labor and Educational Outcomes**

| | Full Sample | Whites | Minorities | Males | Females |
|---|---|---|---|---|---|
| *Labor* | | | | | |
| Log wage | 0.157** | 0.136 | 0.189** | 0.128 | 0.088 |
| | (0.071) | (0.107) | (0.089) | (0.082) | (0.148) |
| Weeks worked | 0.108 | 0.378 | -0.674 | 0.026 | -1.277 |
| | (1.141) | (1.528) | (1.716) | (1.320) | (2.339) |
| Job satisfaction[a] | -0.015 | 0.051 | -0.086 | -0.032 | 0.060 |
| | (0.073) | (0.096) | (0.111) | (0.080) | (0.161) |
| Number of adult jobs[b] | -1.003*** | -1.026*** | -1.040*** | -1.058*** | -0.792** |
| | (0.170) | (0.240) | (0.241) | (0.195) | (0.352) |
| Public sector job | 0.004 | 0.010 | 0.001 | 0.007 | 0.010 |
| | (0.016) | (0.022) | (0.024) | (0.018) | (0.035) |
| *Educational* | | | | | |
| Attempted college | 0.038 | -0.004 | 0.078** | 0.045 | 0.138*** |
| | (0.025) | (0.034) | (0.038) | (0.029) | (0.051) |
| Associate's degree | 0.032** | -0.004 | 0.069*** | 0.028* | 0.075** |
| | (0.014) | (0.020) | (0.019) | (0.014) | (0.031) |
| Bachelor's degree | -0.098*** | -0.135*** | -0.048* | -0.091*** | -0.063 |
| | (0.022) | (0.032) | (0.026) | (0.022) | (0.046) |
| Attempted grad school | -0.040*** | -0.053*** | -0.024* | -0.029** | -0.058** |
| | (0.011) | (0.018) | (0.013) | (0.011) | (0.026) |
| $N$ (veteran) | 348 | 187 | 161 | 271 | 77 |
| $N$ (total) | 8,803 | 4,564 | 4,239 | 4,454 | 4,349 |

*Notes*: Standard errors are in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. [a]On a 1-5 increasing scale. [b]Employee-type jobs during or since 18 years of age.

**Table 1.4: Sibling Fixed Effects Results: Effects of Modern Military Service on Civilian Labor and Educational Outcomes**

| | Full Sample | Whites | Minorities | Males | Females |
|---|---|---|---|---|---|
| *Labor* | | | | | |
| Log wage | 0.050* | 0.041 | 0.107*** | 0.010 | 0.033 |
| | (0.028) | (0.045) | (0.036) | (0.031) | (0.048) |
| Weeks worked | -3.543 | -2.248 | -3.316 | -2.953 | 2.669* |
| | (3.005) | (2.359) | (3.472) | (2.075) | (1.516) |
| Job satisfaction[a] | -0.150 | -0.308 | -0.021 | 0.019 | -0.017 |
| | (0.540) | (0.870) | (0.072) | (0.058) | (0.070) |
| Number of adult jobs[b] | -0.410*** | -0.264 | -1.097*** | -0.279** | -0.743*** |
| | (0.131) | (0.162) | (0.217) | (0.136) | (0.208) |
| Public sector job | 0.041 | 0.025 | 0.060 | 0.057 | 0.024 |
| | (0.032) | (0.018) | (0.046) | (0.042) | (0.021) |
| *Educational* | | | | | |
| Attempted college | 0.051*** | -0.001 | 0.038 | -0.008 | 0.255*** |
| | (0.017) | (0.021) | (0.025) | (0.018) | (0.029) |
| Associate's degree | 0.013 | -0.041 | 0.057*** | 0.004 | 0.120*** |
| | (0.010) | (0.035) | (0.015) | (0.011) | (0.013) |
| Bachelor's degree | -0.038*** | -0.070*** | -0.014 | -0.077*** | -0.057 |
| | (0.013) | (0.018) | (0.019) | (0.014) | (0.041) |
| Attempted grad school | -0.043*** | -0.027** | -0.063*** | -0.049*** | -0.047*** |
| | (0.008) | (0.012) | (0.011) | (0.009) | (0.015) |
| $N$ (veteran) | 216 | 116 | 100 | 171 | 45 |
| $N$ (total) | 8,803 | 4,564 | 4,239 | 4,454 | 4,349 |

*Notes*: Standard errors are in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. [a]On a 1-5 increasing scale. [b]Employee-type jobs during or since 18 years of age.

**Table 1.5: PSM Results: Effects of Modern Military Service on Civilian Labor and Educational Outcomes**

| | Full Sample | Whites | Minorities | Males | Females |
|---|---|---|---|---|---|
| *Labor* | | | | | |
| Log wage | 0.091** | 0.091 | 0.095* | 0.094* | 0.048 |
| | (0.046) | (0.069) | (0.055) | (0.052) | (0.085) |
| Weeks worked | -1.773 | -1.630 | -2.124 | -1.664 | -2.602 |
| | (1.241) | (1.682) | (1.874) | (1.420) | (2.607) |
| Job satisfaction[a] | -0.052 | -0.073 | -0.040 | -0.027 | -0.023 |
| | (0.070) | (0.094) | (0.109) | (0.080) | (0.155) |
| Number of adult jobs[b] | -0.713*** | -0.569** | -0.889*** | -0.821*** | -0.419 |
| | (0.176) | (0.265) | (0.235) | (0.205) | (0.337) |
| Public sector job | 0.005 | 0.010 | 0.001 | 0.007 | 0.013 |
| | (0.016) | (0.022) | (0.025) | (0.018) | (0.036) |
| *Educational* | | | | | |
| Attempted college | 0.068** | 0.025 | 0.078* | 0.049 | 0.103** |
| | (0.028) | (0.039) | (0.041) | (0.032) | (0.049) |
| Associate's degree | 0.028* | -0.020 | 0.076*** | 0.019 | 0.050 |
| | (0.016) | (0.019) | (0.028) | (0.018) | (0.041) |
| Bachelor's degree | -0.055*** | -0.078*** | -0.035 | -0.057*** | -0.062 |
| | (0.018) | (0.026) | (0.028) | (0.019) | (0.047) |
| Attempted grad school | -0.022*** | -0.034*** | -0.014 | -0.015** | -0.052*** |
| | (0.006) | (0.009) | (0.010) | (0.007) | (0.015) |
| $N$ (veteran) | 348 | 187 | 161 | 271 | 77 |
| $N$ (total) | 8,803 | 4,564 | 4,239 | 4,454 | 4,349 |

*Notes*: Standard errors are in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. [a]On a 1-5 increasing scale. [b]Employee-type jobs during or since 18 years of age.

**Table 1.6: Summary Statistics from the Current Population Survey July 2010 Veterans Supplement**

| | Full Sample | | Whites | | Minorities | | Males | | Females | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets | Veterans | Non-Vets |
| *Labor* | | | | | | | | | | |
| Employed | 0.768*** | 0.680 | 0.783*** | 0.698 | 0.695*** | 0.559 | 0.793*** | 0.705 | 0.655** | 0.552 |
| Unemployed | 0.083 | 0.068 | 0.072 | 0.062 | 0.133 | 0.089 | 0.084 | 0.072 | 0.079 | 0.053 |
| Not in the labor force | 0.150*** | 0.252 | 0.145*** | 0.241 | 0.172*** | 0.352 | 0.123*** | 0.223 | 0.266*** | 0.395 |
| Public sector job | 0.311*** | 0.159 | 0.298*** | 0.155 | 0.375*** | 0.176 | 0.305*** | 0.127 | 0.343*** | 0.187 |
| *Educational* | | | | | | | | | | |
| Attempted college | 0.730*** | 0.567 | 0.728*** | 0.555 | 0.742*** | 0.511 | 0.718*** | 0.524 | 0.784*** | 0.565 |
| Associate's degree | 0.152*** | 0.093 | 0.150*** | 0.092 | 0.164*** | 0.076 | 0.161*** | 0.074 | 0.115 | 0.099 |
| Bachelor's degree | 0.260 | 0.286 | 0.258 | 0.283 | 0.273 | 0.244 | 0.246* | 0.277 | 0.324 | 0.276 |
| Attempted grad school | 0.040 | 0.039 | 0.040 | 0.040 | 0.039 | 0.030 | 0.034 | 0.032 | 0.065 | 0.042 |
| *N* | 749 | 84,184 | 621 | 68,648 | 128 | 15,536 | 610 | 37,920 | 139 | 46,264 |

*Notes*: Values are means. Data come from the CPS July 2010 Veterans Supplement. Here, veterans refers to only those individuals who served post 9/11/01 and are no longer serving. The control samples, which are made up entirely of non-veterans, were restricted to those individuals within the same age range of the veteran sample. The labor force categories were calculated using the strict U.S. BLS definitions. Stars refer to p-values from t-tests of sample mean equality across veteran status within each sub-sample. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

**Table 1.7: Estimates from the CPS July 2010 Veterans Supplement (OLS Results)**

| | Full Sample | Whites | Minorities | Males | Females |
|---|---|---|---|---|---|
| *Labor* | | | | | |
| Employed | -0.007 | -0.012 | -0.004 | -0.019 | -0.025 |
| | (0.016) | (0.017) | (0.040) | (0.016) | (0.037) |
| Public sector job | 0.182*** | 0.173*** | 0.218*** | 0.181*** | 0.168*** |
| | (0.015) | (0.016) | (0.037) | (0.015) | (0.038) |
| *Educational* | | | | | |
| Attempted college | 0.178*** | 0.172*** | 0.211*** | 0.181*** | 0.187*** |
| | (0.018) | (0.020) | (0.043) | (0.020) | (0.041) |
| Associate's degree | 0.071*** | 0.067*** | 0.089*** | 0.082*** | 0.015 |
| | (0.010) | (0.012) | (0.023) | (0.011) | (0.025) |
| Bachelor's degree | -0.020 | -0.027 | 0.016 | -0.029 | 0.042 |
| | (0.016) | (0.018) | (0.037) | (0.018) | (0.037) |
| Attempted grad school | 0.007 | 0.007 | 0.010 | 0.003 | 0.025 |
| | (0.007) | (0.008) | (0.015) | (0.007) | (0.017) |
| $N$ (veteran) | 749 | 621 | 128 | 610 | 139 |
| $N$ (total) | 84,933 | 69,269 | 15,664 | 38,530 | 46,403 |

*Notes*: Data come from the CPS July 2010 Veterans Supplement. Values are the effects of veteran status obtained from OLS regression with standard errors in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. Here, veterans refers only to those individuals who served post 9/11/01 and are no longer serving. The control samples, which are made up entirely of non-veterans, were restricted to those individuals within the same age range of the veteran sample. Control variables include age; it's square; marital status; number of children; a MSA resident indicator; regional dummies; disability status; a gender dummy (except in the within-gender samples); race dummies (except in the white sample); and years of schooling (except in the education outcome models).

# CHAPTER 2

# Military Service and Civilian Earnings: Clarification from Quantile Regression

## 2.1 Introduction

The United States Armed Forces is the nation's largest vocational training institution (Magnum & Ball, 1987) and largest single employer of young adults (Angrist, 1998). As of January 2013, the United States' active duty military pool (1,429,995 individuals) is second in the world to the People's Republic of China (U.S. Department of Defense, 2013).[1] Consequently, 22 million people, or about seven percent of the population, are military veterans. The benefits or detriments of military service are thus of great importance. Among other reasons, the effect of service on subsequent wages is especially relevant as evidence of positive or negative effects may incentivize or disincentivize enlistment. Since the end of the draft (1973), incentivizing enlistment has become a major military concern. The length of active duty service can be as little as two years (Today's Military, 2014), making effects on future civilian wages more important than within-service compensation.

Concern with the employability and earnings potential of veterans has become a consistent topic in both the news media and political spheres and evidence of wage effects also has implications on policy. The United States Department of Veteran Affairs is the second largest federal department (after the Department of Defense), with nearly 280,000 employees and an annual budget of over $78.3 billion (USA.gov,

[1] As of this date, there were also 850,880 individuals serving in the U.S. military reserves (U.S. Department of Defense, 2013). There were over 2,285,000 active duty military personnel serving for the People's Republic of China (U.S. Department of Defense, 2013).

2014).[2] Other government programs (e.g., VetSuccess) have been founded to help veterans find work and acclimate (or reacclimate) to the civilian labor force. Estimating veteran wage effects may therefore help disclose the necessity and efficiency of such programs.

Among the reasons to enlist with the armed forces are a steady paycheck, financial assistance offered to veterans (particularly student-veterans), and a lack of viable civilian employment options. Thus, individuals with poorer labor market opportunities may be more likely to enlist; the typical veteran may therefore have an atypical labor market experience before or after service. As a result, regression techniques that estimate the effect of service on the conditional mean wage may not fully characterize the return to service for particularly low or high earnings. More specifically, we expect that prior studies have underestimated veteran wage premiums for low wage earners. To this end, quantile regression (QR) and fixed effect quantile regression (FEQR) may clarify the relationship between military service and future civilian wages.[3]

Further, any service-induced gains or losses may evolve over time: these effects could appear, disappear, grow, diminish, or change sign in the years after service. Theoretically speaking, human capital gains from military service may erode or pay off over time; military training may be substitutable or complementary to different types of civilian training; and statistical discrimination may be an issue. For veterans that served in the early days of the All-Volunteer Force period (1973-present), Magnum and Ball (1989) find that it took two years of civilian life for wage premiums to take

[2]The VA 2014 budget request is $152.7 billion, almost double the budget of the previous year (VA Press Release, 2013).

[3]QR has been applied to various topics in which the effect of a covariate may vary across the distribution of the outcome (Buchinsky, 1998b; Koenker & Hallock, 2001) including infant birth weight and height (Wei et al., 2005), standardized test scores (Eide & Showalter, 1998), earnings growth (Buchinsky, 1998a), the effects of education and experience on earnings (Buchinsky, 1994; Buchinsky, 1998b), and the effect of incarceration on earnings (Brown, 2014).

effect. To address this issue, we also exploit our panel data to estimate the trajectory of the service-wage relationship over time.

In this study, we attempt to clarify the link between military service and subsequent civilian wages, utilizing quantile and wage trajectory techniques, drawing data from two longitudinal surveys initiated in 1979 and 1997. While they are similar in design and structure, the 1997 cohort represents a more recent sample of American veterans, while the 1979 cohort represents veterans that are further removed from their military service. The former cohort can represent veterans who served during the wars in Iraq and Afghanistan; the latter allows one to evaluate the consequences of military service over a longer time horizon.

For veterans of the early 21st century wars, we find that former military service grants wage premiums at and below median wages. However, at the high end of the civilian wage distribution, there is evidence for a wage penalty. For the late 20th century veterans, the wage effects are fairly constant across the wage distribution. Our wage trajectory analysis for the older cohort suggests that veterans often suffer wage penalties in the first year following service and that wage premiums may take until the third year after service to arise in full. Finally, it appears that there have been some minor changes in the characteristics of military volunteers over time.

## 2.2 Previous Findings

There are several reasons to believe military service may affect subsequent civilian wages; economic theory is ambiguous as to the direction of the overall effect (Bryant and Wilhite, 1990). First, military service may provide a "bridging environment" that helps a youth transition from high school and parental dependance to their adult life (Browning et al., 1973). Some researchers (e.g., De Tray, 1982) have found evidence in favor of veteran status acting as a positive screen, allowing employers to

distinguish more productive from less productive workers. Volunteers are physically, mentally, and morally tested before entrance into armed forces is granted. Since employers are often aware of these exams, veteran status can signal these levels of fortitude (positive selection). The financial assistance provided to student-veterans makes civilian post-secondary education (which is well known to increase wages) more attainable (particularly for females and minorities (Routon, 2014)). Discipline, team work, and responsibility are core components of military education and these traits are beneficial in almost all civilian occupations. Lastly, in some occupations, the training provided during military service may prove useful in a civilian setting. In fact, the military claims that currently "91 percent of military jobs have a direct civilian counterpart" (Today's Military, 2014). Table 1 shows the number and percent, as of June 2013, of enlisted military personnel in each broad military occupation group for the U.S. armed forces. Also provided are some example specific occupations within each group. If we assume that combat specialty and unspecified occupations have no civilian counterpart, but all others do, then as of this month 83.7 percent of enlisted military personnel were working in an occupation where there is at least one related civilian job.

There are equally as many reasons to think military service might harm civilian labor outcomes. Veteran status may be a negative screen[4] - youths with higher civilian wage opportunities are less likely to volunteer (negative selection). Insuring a veteran employee may be more costly, or perceived to be so, as service (particularly wartime service) can lead to physical and/or mental health conditions. For occupations where military training and experience are irrelevant, service may simply create older workers with fewer skills and thus lower wage opportunities (Bryant and Wilhite, 1990). The general civilian attitude toward a war may be a factor in the labor market out-

[4]Berger and Hirsch (1985) find that this was indeed the case for Vietnam era veterans.

comes of returning veterans (Routon, 2014). Lastly, if the returns to military training are less than the returns to civilian post-secondary education, then those youths who have substituted college with service are likely to have lower wages.

Previous studies have shown that veteran wage effects are very likely to have varied greatly across historical context. Lack of reliable and quality data has made research on the pre-World War II era difficult. Most studies have found wage premiums or negligible effects for veterans of World War II and Korea (e.g., Browning et al., 1973; Little & Fredland, 1979; Martindale & Poston, 1979; Fredland & Little, 1980; Schwartz, 1986; Angrist & Kreuger, 1994; Hirsch & Mehay, 2003). For Vietnam veterans, however, there appears to have been sizable wage penalties that have eroded over time (Martindale & Poston, 1979; Berger & Hirsch, 1983; Schwartz, 1983; Crane & Wise, 1987; Angrist, 1990; Teachman & Call, 1986; Teachman, 2004; Angrist et al., 2011). The body of research on All-Volunteer Force era (AVF; 1973-present) veterans is small but growing. Phillips et al. (1992) find that early AVF veterans earned higher wages in the military than they would have in the civilian labor market. Bryant et al. (1993) find a 1.7 percent veteran wage penalty for those that served in the early years of the AVF. For the wars of the early 21st century, those in Iraq and Afghanistan, Routon (2014) finds significant minority veteran wage premiums but that white veterans were unaffected.

Table 2 presents a chronological summary of the literature on veteran wage effects. This list is not comprehensive, but provides a picture of previous findings. In addition to the estimated average wages effects, the table discloses the primary data source and empirical methodology used by each set of researchers. Importantly, the service period, both era and specific year range, is given. Lastly, some brief comments pertaining to the results in each study are provided. The table shows that researchers have made use of several different empirical strategies in the pursuit of estimating

veteran wage effects. These include least squares, fixed effects, instrumental variables, and matching procedures. All of these studies attempt to discover the effect of prior military service on the conditional mean wage which may or may not provide an accurate picture of reality. As mentioned, studies on World War II and Korean veterans show either wage premiums or insignificant effects while studies on Vietnam veterans report opposite effects.

## 2.3 Empirical Methodology

To estimate the wage premium or penalty of military service, an equation is specified in the form

$$\ln(w_{it}) = \alpha_0 + \beta_1 Veteran_{it} + \beta_2 Current\ Service_{it} + x_{it}^{'}\beta + \varepsilon_{it} \tag{1}$$

where the outcome is the log of an individual $i$'s wages in time $t$, $Veteran_{it}$ is an indicator variable that equals one when $i$ reports military service in period $t-1$ or before but in the current period, $Current\ Service_{it}$ is an indicator that equals one if $i$ reports military service in time $t$, $x_{it}^{'}$ is a vector of controls, and $\varepsilon_{it}$ is a well-behaved error term. The control variables in $x_{it}^{'}$ are discussed in full in Section 4.1. To estimate the evolution of the service-earnings relationship over time, (1) is respecified in the form

$$\ln(w_{it}) = \alpha_0 + \sum_{j=1}^{3} \gamma_j Military_{it-j} + \theta Current\ Service_{it} + \sum_{k=1}^{3} \delta_k Military_{it+k} + x_{it}^{'}\beta + \varepsilon_{it} \tag{2}$$

where $\sum_{j=1}^{3} Military_{it-j}$ and $\sum_{k=1}^{3} Military_{it+k}$ are vectors of indicator variables that equal one when $i$ reports military service in the $j$ periods before and $k$ periods after $t$.[5] Estimates of (2) form a service-wage trajectory for the three years before

[5]We also estimated models with trajectories spanning greater than three years post service. In

and after a period of service. The coefficient vector $\delta_k$ is a form of falsification test in which significant results may suggest spurious correlation between current wages and present military service (Granger, 1969). Both (1) and (2) are first estimated using ordinary least squares (OLS) and pooled cross-sectional data.

In models (1) and (2), $\beta_1$ and $\gamma_j$ represent the wage premium or penalty associated with prior military service, controlling for both current military service and a vector of observable, reported characteristics. The OLS estimator is likely to be biased, however, if military service is correlated with unobservable characteristics captured in $\varepsilon_{it}$. To address potential endogeneity, we estimate individual- and household- level fixed effects (FE) models for (1) and (2); the former controls for time-invariant, individual-level heterogeneity and the latter controls for household-invariant heterogeneity.[6]

Finally, we specify a quantile regression (QR) model of the form of (1) such that

$$\ln(w_{it}) = \alpha_{0\tau} + \beta_{1\tau} Veteran_{it} + \beta_{2\tau} Current\,Service_{it} + x_{it}^{'}\beta_{\tau} + \varepsilon_{it\tau}$$

with

$$Q_{\tau}(\ln(w_{it})|x_{it}) = \alpha_{0\tau} + \beta_{1\tau} Veteran_{it} + \beta_{2\tau} Current\,Service_{it} + x_{it}^{'}\beta_{\tau} \qquad (3)$$

where $\tau$ denotes the conditional quantiles of interest such that $\tau = \{0.05, 0.25, 0.50, 0.75, 0.95\}$, which represents the 5th, 25th, median, 75th, and 95th wage quantiles. QR models are estimated simultaneously for all $\tau$ of interest and standard errors

these models, not reported here, the coefficients on the time periods greater than three were found to be statistically insignificant and the coefficients for the other parameters remained highly similar to those in the models reported here with a trajectory of three years.

[6]Military service is often a family tradition and some parents may encourage, while others discourage, service. For these reasons and other relevant family-specific traits, estimates from sibling fixed effect models may provide important insight.

are bootstrapped. Individual- and household-level fixed effects quantile regressions (FEQR) are also estimated utilizing the procedure developed by Koenker (2004).[7]

## 2.4 Data

### 2.4.1 National Longitudinal Surveys

To estimate the aforementioned models empirically, we require data that meet several criteria. First, they must contain information on the timing of the entry and exit of military service for a substantial, representative sample of individuals. Second, they must also contain earnings data after service. Finally, to implement panel and trajectory methods, they must have some longitudinal structure. This study draws data from the 1979 and 1997 cohorts of the National Longitudinal Survey of Youth (NLSY), both of which meet our empirical needs. Both surveys interview a sample of American youth on a variety of topics, including demographics, education, health, and labor. The initial survey of the 1979 cohort (NLSY79) interviewed 12,868 young adults with follow-up interviews conducted annually until 1994 and biennially thereafter (Bureau of Labor Statistics [BLS], 2012a). The initial survey of the 1997 cohort (NLSY97) contained 8,984 respondents who have been interviewed annually since 1997 (BLS, 2012b). Both surveys provide an unusually rich set of data on the life course of American youth set about two decades apart. In the NLSY79 (NLSY97), 1,914 (520) respondents report military service over their working careers.

We construct two independent panels drawn from the NLSY79 and NLSY97. The NLSY79 panel contains data from 1979 to 1994 and the NLSY97 panel contains data from 1997 to 2010. We restrict the NLSY79 sample to 1994 and prior since it became

[7]Given that some previous studies have found that veteran wage effects vary across racial groups, we would ideally also estimate within-race effects. However, particularly for the NLSY97 sample, our veteran sample sizes within-race, within-wage quantile were deemed too small to do so and we thus leave this task for future researchers.

a biennial survey afterwords, making military service in the off years unknown and period-to-period changes in the trajectory models incomparable to the annual part of the survey. The primary dependent variable of study is the natural log of reported hourly wages at a respondent's primary job in the previous year. The explanatory service variables are derived from a survey question asking if the respondent is serving in the active armed forces at the time of the survey. This allows us to construct annual histories of active duty military service and labor market outcomes. Before sampling weights are introduced in our regressions, the working panels consist of 10,330 (NLSY79) and 4,674 (NLSY97) respondents, in which 1,914 and 348 respondents report active duty service in at least one survey wave.

Additional variables are drawn from each survey for use as controls in empirical specifications; the similarities between the National Longitudinal Surveys allows us to collect analogous (if not identical) sets of variables for each working panel. For each panel, the control vector $x_{it}^{'}$ contains demographic variables including respondent age, its square, sex, race (black and non-white/non-black, omitted category white), urban residence, and region (northeast, north central, and west, omitted category south). Additional respondent variables include marital status, number of children, number of weeks worked at the respondent's primary job, its square, a dummy for public sector employment, number of years of education, and percentile score on the Armed Services Vocational Aptitude Battery (ASVAB) test as a proxy for cognitive ability.[8]

Table 3 presents selected sample means for both working panels, as well as subsamples split by veteran status (respondents who report military service in previous years and those who do not), as well as t-tested differences of means across subsamples. Several descriptive differences arise in both surveys: veterans are disproportionately

[8] As it is the same test used by the armed forces to determine ability, ASVAB scores are an ideal ability control for the research question here. All NLSY respondents took the ASVAB in the initial survey years, regardless of future military service, as part of the survey instrument.

male and non-white, with small but significant differences across marital status, age, job tenure, and education. Veterans in the 1979 cohort are less likely to be married than non-veterans; the opposite is observed for the 1997 cohort. In total, most differences in means between veterans and non-veterans are small in magnitude and descriptively consistent across National Longitudinal Surveys.

### 2.4.2 A New Type of Soldier?

We take this opportunity to briefly examine how the characteristics of volunteers have changed across the NLSY cohorts. The top panel of Table 4 presents means for each of the two NLSY veteran samples for some relevant variables. T-tests of sample mean equality were also performed to check whether differences were indeed statistically different. Differences in average ASVAB scores are statistically insignificant across the two veteran cohorts, suggesting the average cognitive ability of volunteers has remained constant. Increases in average parental education levels are significant but can likely be explained, at least in part, by national trends. Other significant differences across these two veteran cohorts are apparent. The bottom panel of Table 4 shows some select differences between the NLSY97 veteran and non-veteran samples. These four characteristics were not captured in the NLSY79. Interestingly, T-test results show there are no statistically significant differences between modern volunteers and the general population regarding high school grade point averages, body mass indices, and parental religiosity. However, parents of NLSY97 respondents were asked to rate the respondents' general health status on a one-to-five scale, and military volunteers were notably healthier. Given the physical rigor of military service, this is unsurprising.

## 2.5 Results and Discussion

Table 5 presents results from pooled OLS estimates of (1) and corresponding QR estimates of (3) for all wage quantiles of interest and for both the NLSY79 and NLSY97. The first column suggests that prior military service grants an approximate seven percent wage premium for the 1979 cohort and a wage penalty of about seven percent for the 1997 cohort. This difference could be explained by the effects of wartime versus peacetime service implying a 14 percent veteran wage penalty for combat service.

Across the different points on the wage distribution, however, we find that these wage penalties range from 5 to 14 percent and are found only for above-median (75th and 95th) quantiles. It appears to be the high earners that are harmed by prior military service. These negative effects appear to increase between the 75th and 95th quantiles, and disappear between the 75th and 50th quantiles. Wartime service is a known catalyst of mental/emotional/social issues through post-traumatic stress disorder (PTSD). These issues can range from mild to severe. High wage occupations are generally more demanding in terms of cognitive and often social ability which could explain the larger penalties at the high end of the distribution. For the NLSY79 cohort, the wage effects are much more consistent across the distribution, ranging from an approximate 6 to 7.5 percent premium. In total, mean results do not seem to be representative of many key points on the wage distribution, particularly for the NLSY97 sample. Figures 1a and 1b present the QR results from this table.

Table 6 presents results from fixed effects models of (1) and fixed effects quantile regression models of (3) for both individual- and household-level fixed effects and both working panels. The upper panel reports wage effects for NLSY79 veterans where mean fixed effects results suggest that prior military service has no wage effect after controlling for either individual- or household-level heterogeneity. Fixed

effect QR estimates present a contrasting story, however; showing wage premiums for median and below-median wages that are generally smaller than previous QR estimates. Individual FE results suggest a median wage premium of about one half of one percent, while household FE suggest a higher premium of seven percent. The individual FE model shows that wage premiums are almost five times larger for the lowest earners when compared to the median earners while the household FE model shows much more consistent effects across the distribution which also extend into the 75th quantile. We hypothesized higher wage premiums for veterans at the low end of the distribution. Here, the individual FE model results suggest that that is indeed the case. Figures 2a through 2d present the FEQR results of Table 6 graphically for point estimates at all wage quantiles of interest, individual- and household-level fixed estimates, and corresponding 95% confidence intervals.

The bottom panel of Table 6 presents analogous estimates for NLSY97 respondents. In contrast with the QR models, there is no apparent link between veteran status and depressed wages. Wage premiums are found at all points on the wage distribution excepting the 95th quantile in the individual FE results. In both fixed effects specifications, the largest premiums (approximately 9-11 percent) are found at the 5th quantile. In both specifications, median regression suggests a wage premium of about six to seven percent. Additionally, individual-level FE suggests comparable premiums at the 25th and 75th quantiles. In total, fixed effects specifications of (1) and (3) point toward: a mean effect that either overstates wage penalties or is insignificant, wage premiums for median and below-median earnings, and some evidence of wage penalties of similar magnitudes at the 25th and 75th quantiles. These results suggest that simple mean techniques may not be fully characterizing the relationship between prior military service and earnings, and in some cases, may be overstating any negative effects of service, conditioned on a variety of controls and fixed effects.

Table 7 presents pooled OLS, individual-level fixed effects, and household-level fixed effects service-wage trajectories in the form of (2) for NLSY79 respondents.[9] The coefficients of each column represent the effect of veteran status on wages timed to the periods before and after active duty military service; periods $k = (1, 2, 3)$ represent the effect of future military service on current wages and $j = (1, 2, 3)$ represents the effect of prior military service on current wages. For robustness, we present models both with and without the lead variables, that is $k = (1, 2, 3)$. Differences across these two specifications within each level of control are very small. As mentioned, $k = (1, 2, 3)$ form a type of falsification test and we indeed find no statistical significance for these three indicators in any of the three control level specifications (Granger, 1969). All six models suggest negative effects in the first year after service, though these estimates are only significant when our highest level of control (individual FE) is introduced. Though statistically insignificant, all six models also suggest that it is not until year three post service that wage premiums of any size become apparent.

In total, results from a variety of methods, specifications, and two rich data sets suggest that the relationship between military service and subsequent wages is dynamic, with positive and negative effects across time and for different levels of earnings. Mean techniques suggest a negative relationship between 21st century military service and subsequent earnings, however, quantile regression results suggest that military service may be associated with wage premiums at median and below-median wages. Wage penalties at the high end of the wage distribution are still found, but are substantially smaller than those found with mean regression techniques and are insignificant once unobserved heterogeneity is addressed. Additionally, wage trajectory models suggest that any wage penalties faced by veterans are likely to occur in

[9] Again, the NLSY97 veterans, generally speaking, were not yet far enough removed from military service at the time of the most recent survey to perform an adequate wage-trajectory analysis.

the year immediately after active duty service; wage premiums three years after service provide further evidence of the positive effects of military service on earnings.

## 2.6 Conclusions

We set out to estimate the effects of prior military service on civilian wages across the wage distribution, as well as their wage trajectories post-service. To accomplish this, we use quantile regression and wage trajectory techniques. Our data come from both the 1979 and 1997 cohorts of the National Longitudinal Surveys of Youth (the NLSY79 and NLSY97). The veterans in our NLSY79 sample served during the period 1980-1994 while the NLSY97 veterans served during the wars in Afghanistan and/or Iraq. We find that controlling for unobserved heterogeneity is important. Results suggest that former military service grants civilian wage premiums at and below the median wage and perhaps even up to the 75th quantile. The largest benefits appear to be granted to individuals at the lowest end of the wage distribution, with the wage premium for these veterans being as high as 11 percent. For veterans that served during the late 20th century, veteran wage effects are shown to be more consistent across the wage distribution.

We also perform a civilian wage trajectory analysis for military veterans and use our two veteran samples to see whether the characteristics of those who choose to enlist have changed over the past two decades. Our wage trajectory analysis suggests that it can take up to the third year post-service before wage premiums take effect and that penalties are generally present during the first year following service. We find that these two veteran samples are not statistically different with respect to our ability proxy (ASVAB scores), suggesting that any differences in veteran wage effects must come from other sources.

Our results suggest that those individuals who aspire to median or below-median wage civilian jobs can earn higher wages by first volunteering for the military. On the other hand, those individuals who end up in high-paying civilian occupations appear to be unaffected by prior service after controlling for unobserved heterogeneity. More military volunteers likely fall into the former category than the latter implying a generally positive story for veteran wage effects. Coupling this with the financial assistance offered to student-veterans[10] makes military service appear a quality option for transition from secondary education to the labor force.

Further research is required before a more complete understanding of distributional veteran wage effects can be achieved. Both of the veteran cohorts in this study come from the All-Volunteer Force period. Because of the draft and other historical changes, distributional effects may have differed for earlier eras (e.g., World War II, Korea, and Vietnam). If a larger and more diverse panel was available, distributional effects within-gender and within-race could also be determined. Lastly, it may prove enlightening to estimate distributional effects across prior service in the individual military branches.

[10]Currently, the GI Bill grants up to 100 percent college tuition reimbursement, housing assistance, and a $1,000 per year textbook stipend to recent veterans.

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**Table 2.1: Enlisted personnel by broad occupational group as of June 2013**

| Occupational group | Example job(s) | Personnel | Percent |
|---|---|---|---|
| Administrative | Accountant, lawyer, finance specialist | 53,949 | 4.6 |
| Combat specialty | Infantry, artillery, Special Forces | 175,397 | 15.1 |
| Construction | Plumber, water purification specialist | 34,303 | 2.9 |
| Electronics | Electrician, computer specialist | 136,143 | 11.7 |
| Engineering, science, & technical | Information technology specialist | 163,097 | 14.0 |
| Health care | Doctor, nurse, lab tech | 75,820 | 6.5 |
| Human resource development | Recruiter, training specialist | 31,141 | 2.7 |
| Machine operator and production | Metalworker, shipbuilder, welder | 24,174 | 2.1 |
| Media and public affairs | Photographer, musician, journalist | 19,097 | 1.6 |
| Protective service | Military police, firefighter, security officer | 81,427 | 7.0 |
| Support service | Food service specialist, chaplain | 28,411 | 2.4 |
| Transportation & material handling | Driver, quartermaster | 157,694 | 13.6 |
| Vehicle and machinery mechanic | Auto mechanic, heating/cooling mechanic | 167,869 | 14.4 |
| Unspecified | — | 14,303 | 1.2 |
| Total | | 1,162,825 | 100.0 |

*Notes*: Data source is the U.S. Department of Defense, Defense Manpower Data Center (Retrieved Feb. 28, 2014).

**Table 2.2: Previous veteran wage effect estimates.**

| Study | Data | Service Period | | Method | Estimate (approx.) | Comments |
|---|---|---|---|---|---|---|
| | | Era | Years | | | |
| Little & Fredland (1979) | NLSM | Pre Vietnam | 1925-1965 | OLS | +15% | Minorities only |
| Fredland & Little (1980) | NLSM | WWII & after | 1942-1965 | OLS | +5% | Blue collar only |
| Berger & Hirsch (1983) | Census | Vietnam | 1965-1973 | OLS | -2% | Penalty erodes over time |
| Schwartz (1986) | Census | Vietnam | 1965-1973 | OLS | -38% | Also lower returns to edu. |
| Schwartz (1986) cont. | Census | Korea | 1950-1953 | OLS | 0% | No Korean War effects |
| Crane & Wise (1987) | NLS-72 | Vietnam & after | 1972-1978 | OLS | 0% | Civilian exp. superior |
| Angrist (1990) | CWHS | Vietnam | 1965-1973 | IV | -15% | Whites only |
| Xie (1992) | Census | Post WWII | 1947-1983 | OLS | +14% | Larger for low-educated |
| Angrist & Krueger (1994) | Census | WWII | 1942-1947 | IV | 0% | No WWII effects |
| Teachman & Call (1996) | Multiple | Vietnam & after | 1965-1986 | ML | 0% | Penalties for Vietnam vets |
| Angrist (1998) | DMDC | Post Vietnam | 1974-1991 | PSM/IV | +10% | Minorities only |
| Hirsch & Mehay (2003) | RCS | Korea & after | 1951-1991 | PSM | +3% | For officers, +10% |
| Teachman (2004) | NLSYM | Vietnam | 1966-1981 | FE | -12% | Penalty erodes over time |
| Angrist et al. (2011) | CWHS | Vietnam | 1968-1975 | IV | -15% | Penalty erodes over time |
| Card & Cardoso (2012) | Portuguese | Peacetime | 1988-1991 | OLS | 0% | For the low-educated, +4% |
| Routon (2014) | NLSY97 | Iraq/Afghanistan | 1998-2009 | PSM | +10% | Minorities only |

*Notes*: NLSM = National Longitudinal Survey of Men. NLS-72 = National Longitudinal Study of the High School Class of 1972. CWHS = Continuous Work History Sample. DMDC = Defense Manpower Data Center data. RCS = Reserve Components Survey. NLSYM = National Longitudinal Survey of Young Men. NLSY97 = National Longitudinal Survey of Youth 1997. Method refers to the primary empirical methodology with OLS meaning least squares, IV instrumental variables, ML maximum likelihood, PSM propensity score matching, and FE fixed effects.

**Table 2.3: Selected sample means.**

| Variable | Full sample (*i*) | | Veterans (*ii*) | | Non-Veterans (*iii*) | | Difference (*ii*) - (*iii*) |
|---|---|---|---|---|---|---|---|
| *NLSY79* | | | | | | | |
| Male | 0.51 | [0.50] | 0.89 | [0.44] | 0.48 | [0.50] | 0.41*** |
| African-American | 0.14 | [0.43] | 0.21 | [0.44] | 0.14 | [0.43] | 0.07*** |
| Other non-white | 0.06 | [0.36] | 0.07 | [0.28] | 0.06 | [0.38] | 0.01*** |
| Age | 25.40 | [5.20] | 25.15 | [4.82] | 25.42 | [5.26] | -0.27*** |
| Years of schooling | 13.05 | [2.45] | 12.85 | [2.29] | 13.06 | [2.48] | -0.21*** |
| Married | 0.40 | [0.46] | 0.39 | [0.43] | 0.41 | [0.47] | -0.02*** |
| Job tenure (weeks) | 38.36 | [27.90] | 27.49 | [28.60] | 39.24 | [27.62] | -11.75*** |
| $N_{person-year}$ | 202,976 | | 30,624 | | 172,352 | | |
| *NLSY97* | | | | | | | |
| Male | 0.51 | [0.50] | 0.76 | [0.42] | 0.50 | [0.50] | 0.26*** |
| African-American | 0.14 | [0.44] | 0.16 | [0.43] | 0.14 | [0.44] | 0.02*** |
| Other non-white | 0.13 | [0.41] | 0.14 | [0.41] | 0.13 | [0.41] | 0.01 |
| Age | 21.63 | [4.29] | 21.42 | [4.28] | 21.63 | [4.29] | -0.21*** |
| Years of schooling | 12.40 | [1.53] | 12.34 | [1.35] | 12.40 | [1.54] | -0.06*** |
| Married | 0.17 | [0.32] | 0.22 | [0.38] | 0.17 | [0.32] | 0.05*** |
| Job tenure (weeks) | 39.88 | [21.57] | 36.96 | [21.92] | 39.98 | [21.52] | -3.02*** |
| $N_{person-year}$ | 123,242 | | 4,872 | | 118,370 | | |

*Notes*: Standard deviations in brackets. Survey weights were used. Our NLSY79 (NLSY97) sample consists of the 1979 (1997) through 1994 (2010) survey waves. Stars refer to p-values from t-tests of sample mean equality across veteran status. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

**Table 2.4: Select differences across subgroups.**

| Variable | NLSY97 veterans $(i)$ | NLSY79 veterans $(ii)$ | Difference $(i)$ - $(ii)$ |
|---|---|---|---|
| ASVAB percent score | 50.447 | 49.430 | 1.017 |
| Mother's education (years) | 12.652 | 11.408 | 1.244** |
| Father's education (years) | 12.536 | 11.480 | 1.056** |
| Mother born in U.S. | 0.863 | 0.918 | -0.055*** |
| Father born in U.S. | 0.898 | 0.733 | 0.165*** |
| Male | 0.779 | 0.730 | 0.049** |
| African-American | 0.241 | 0.262 | -0.021 |
| Hispanic | 0.213 | 0.086 | 0.127*** |
| Urban resident | 0.721 | 0.922 | -0.201*** |
| Region: West | 0.221 | 0.242 | -0.021 |
| Region: Northeast | 0.164 | 0.235 | -0.071*** |
| Region: North central | 0.207 | 0.129 | 0.078*** |
| Region: South | 0.408 | 0.394 | 0.014 |
| $N$ | 348 | 1,914 | |

| Variable | NLSY97 veterans $(i)$ | NLSY97 nonvets $(ii)$ | Difference $(i)$ - $(ii)$ |
|---|---|---|---|
| High school GPA[a] | 13.163 | 12.840 | 0.323 |
| Body mass index | 23.739 | 23.717 | 0.022 |
| Teenage health (1-5)[b] | 4.494 | 4.259 | 0.235*** |
| Parents' religiosity (0-6)[b] | 3.712 | 3.776 | -0.064 |
| $N$ | 348 | 8,455 | |

*Notes*: Values in columns $(i)$ and $(ii)$ are means. Stars refer to p-values from t-tests of sample mean equality across the veteran cohorts. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. [a]The NLSY97 has a unique, uniform scale (with a range of 0-100 and mean 12.87) for respondents' high school GPA that takes into account school-specific characteristics and omits non-academic courses (e.g., physical education) so that GPA comparisons can be made across schools.
[b]Increasing scale.

**Table 2.5: Veteran wage effects, POLS/QR.**

| | POLS (mean) | 5th | 25th | 50th | 75th | 95th |
|---|---|---|---|---|---|---|
| | | | *NLSY79 (N = 202,976)* | | | |
| Military veteran | 0.0724*** | 0.0745*** | 0.0600*** | 0.0691*** | 0.0716*** | 0.0652*** |
| | (0.0129) | (0.0195) | (0.0140) | (0.0125) | (0.0159) | (0.0241) |
| | | | *NLSY97 (N = 123,242)* | | | |
| Military veteran | -0.0667*** | 0.0173 | -0.0130 | -0.0132 | -0.0487*** | -0.1110*** |
| | (0.0207) | (0.0182) | (0.0130) | (0.0150) | (0.0150) | (0.0320) |

*Notes*: Standard errors in parentheses. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$. 5th, 25th, 50th 75th, and 95th refer to points on the wage distribution. Dependent variable is log(wage).

**Table 2.6: Veteran wage effects, FE/FEQR.**

| | FE (mean) | 5th | 25th | 50th | 75th | 95th |
|---|---|---|---|---|---|---|
| | | | *NLSY79 (individual FE) (N = 202,976)* | | | |
| Military veteran | -0.0098 | 0.0262*** | 0.0183*** | 0.0056*** | 0.0013 | 0.0063 |
| | (0.0185) | (0.0025) | (0.0084) | (0.0021) | (0.0010) | (0.0334) |
| | | | *NLSY79 (household FE) (N = 107,872)* | | | |
| Military veteran | 0.0174 | 0.0634* | 0.0522** | 0.0704** | 0.0678* | 0.0690 |
| | (0.0150) | (0.0364) | (0.0266) | (0.0285) | (0.0349) | (0.0475) |
| | | | *NLSY97 (individual FE) (N = 123,242)* | | | |
| Military veteran | -0.0126 | 0.0872* | 0.0476** | 0.0581*** | 0.0579** | -0.0092 |
| | (0.0390) | (0.0501) | (0.0226) | (0.0218) | (0.0267) | (0.0525) |
| | | | *NLSY97 (household FE) (N = 54,348)* | | | |
| Military veteran | 0.0236 | 0.1074** | 0.0349 | 0.0674** | 0.0299 | -0.0598 |
| | (0.0246) | (0.0512) | (0.0250) | (0.0269) | (0.0242) | (0.0563) |

*Notes*: Standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. 5th, 25th, 50th, 75th, and 95th refer to points on the wage distribution. Dependent variable is log(wage).

**Table 2.7: Enlistee wage trajectories, NLSY79.**

| | $k=3$ | $k=2$ | $k=1$ | $t$ | $j=1$ | $j=2$ | $j=3$ |
|---|---|---|---|---|---|---|---|
| | | | | *POLS (N = 202,976)* | | | |
| Military service | | | | 0.0330* | -0.0656 | 0.0054 | 0.1183 |
| | | | | (0.0194) | (0.0495) | (0.0480) | (0.0372) |
| Military service | -0.0012 | -0.0051 | 0.0127 | 0.0331* | -0.0663 | 0.0049 | 0.1184 |
| | (0.0457) | (0.0623) | (0.0427) | (0.0194) | (0.0498) | (0.0480) | (0.0372) |
| | | | | *Individual FE (N = 202,976)* | | | |
| Military service | | | | -0.0116 | -0.0612* | 0.0062 | 0.0400 |
| | | | | (0.0274) | (0.0330) | (0.0306) | (0.0290) |
| Military service | 0.0013 | -0.0108 | -0.0070 | -0.0155 | -0.0610* | 0.0060 | 0.0381 |
| | (0.0423) | (0.0528) | (0.0318) | (0.0309) | (0.0329) | (0.0308) | (0.0295) |
| | | | | *Household FE (N = 107,872)* | | | |
| Military service | | | | -0.0383** | -0.0436 | 0.0179 | 0.0412 |
| | | | | (0.0182) | (0.0362) | (0.0373) | (0.0299) |
| Military service | -0.0181 | -0.0170 | 0.0056 | -0.0430** | -0.0430 | 0.0174 | 0.0389 |
| | (0.0351) | (0.0387) | (0.0350) | (0.0187) | (0.0363) | (0.0374) | (0.0300) |

*Notes*: Standard errors in parentheses. $^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. Subscript $k$ ($j$) represents periods prior to (since) service while $t$ represents time during service. Dependent variable is log(wage).

Figure 1a. QR tau plot (NLSY79).

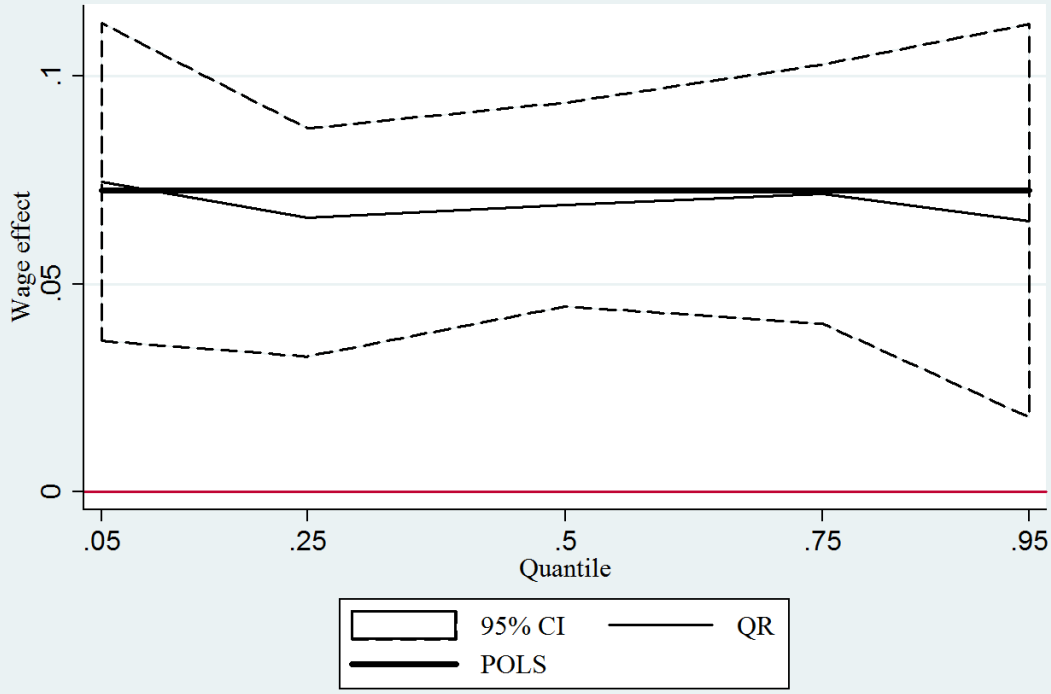



Figure 1b. QR tau plot (NLSY97).

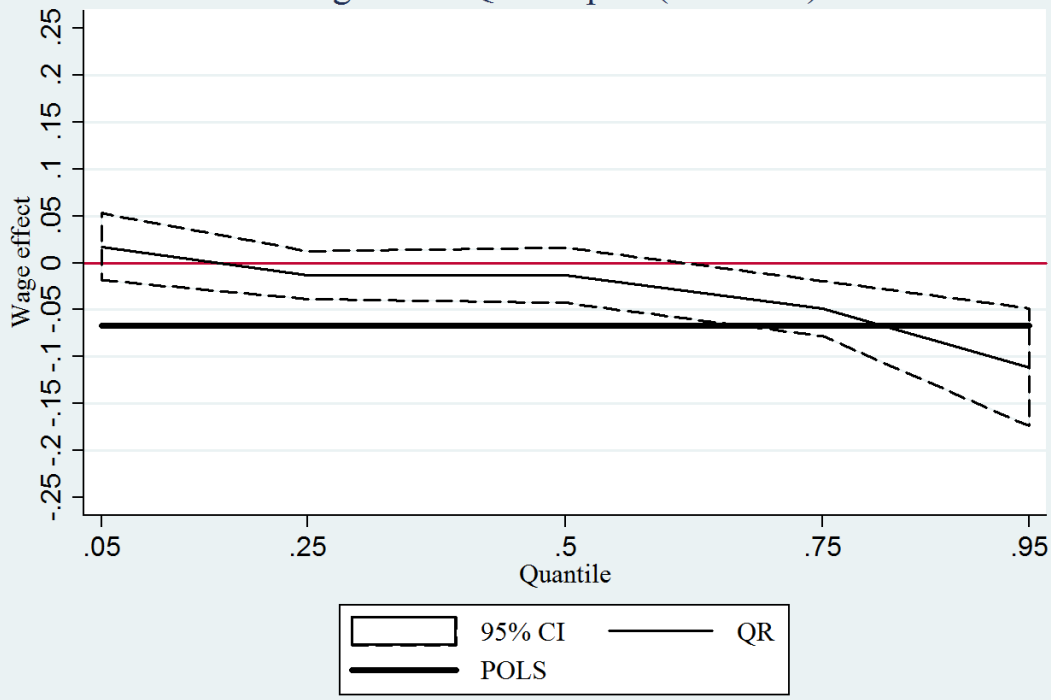


Figure 2a. Individual FEQR tau plot (NLSY79).

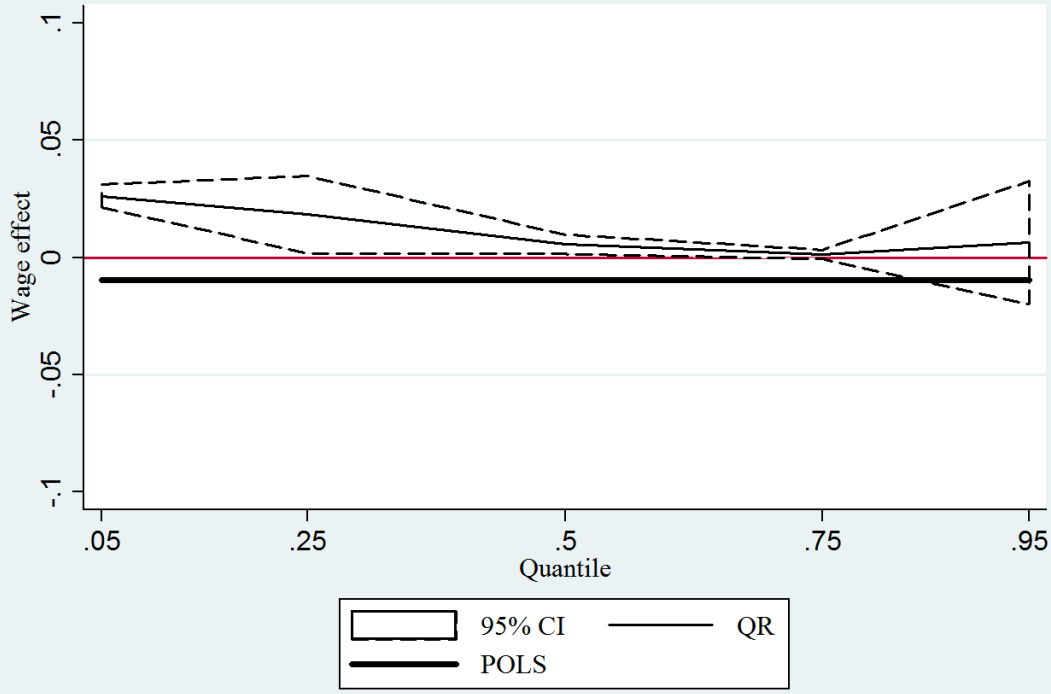



Figure 2b. Household FEQR tau plot (NLSY79).

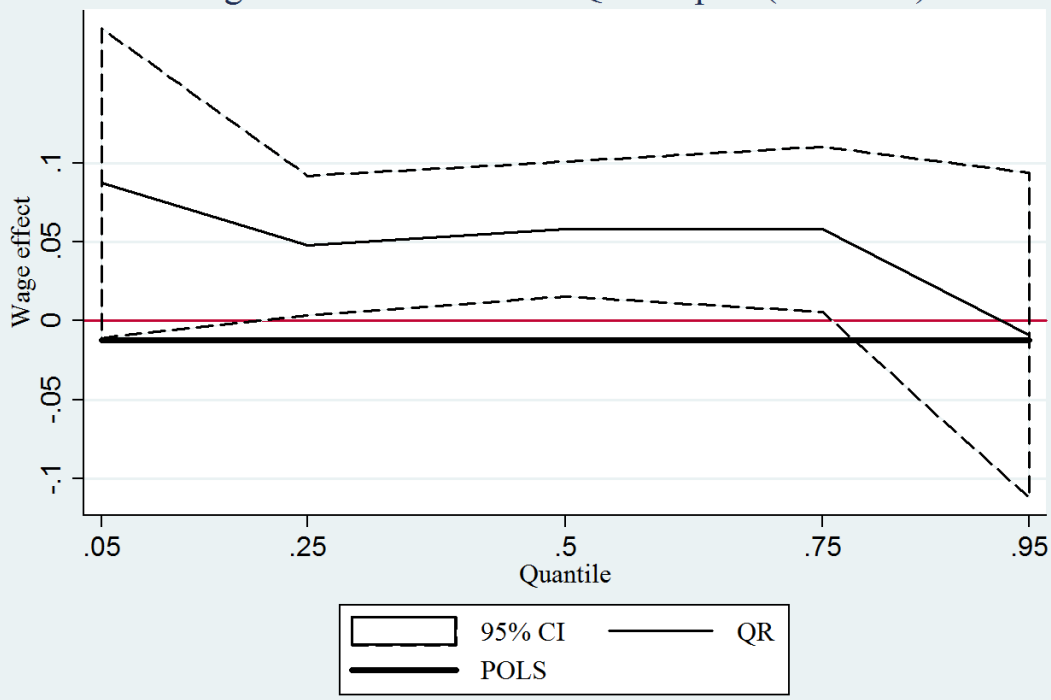


Figure 2c. Individual FEQR tau plot (NLSY97).

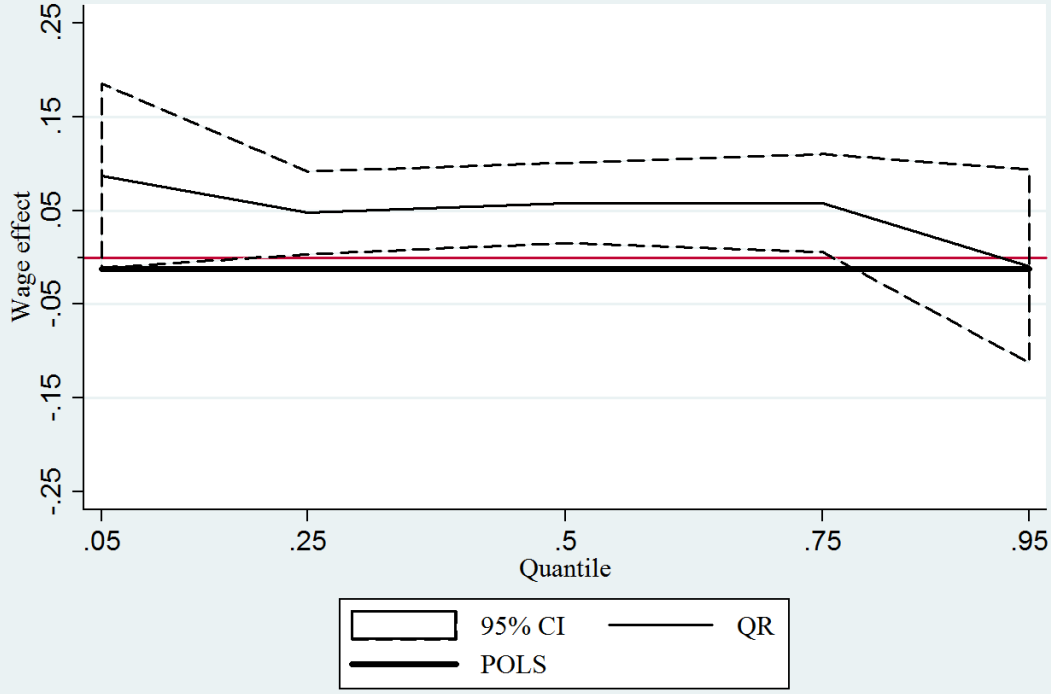



Figure 2d. Household FEQR tau plot (NLSY97).

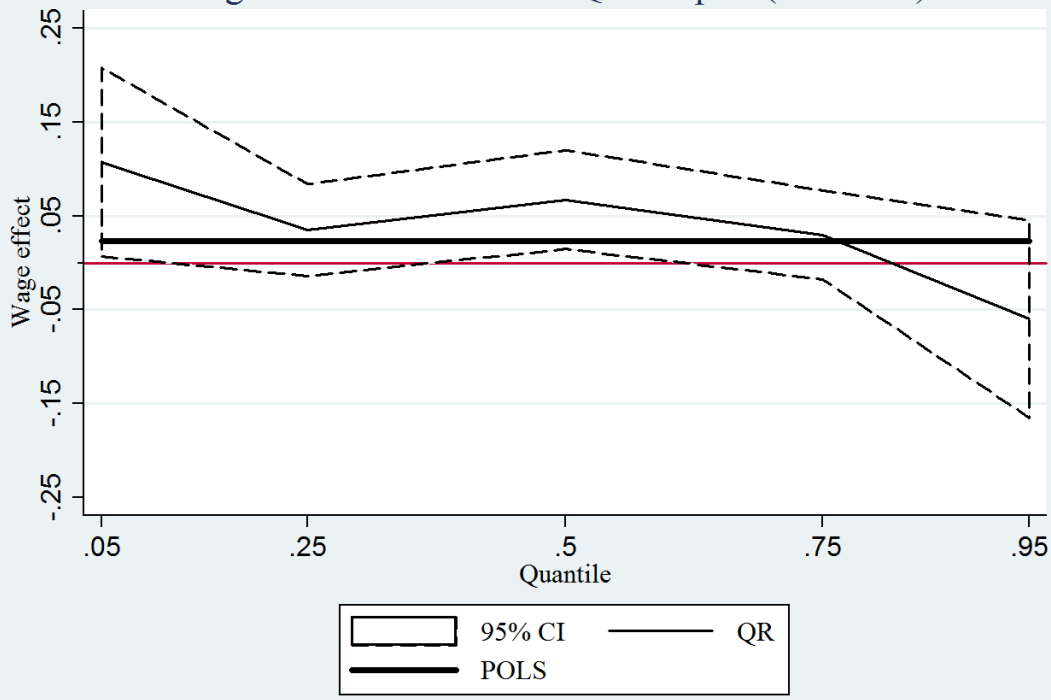


Figure 3a. Service-wage trajectory (POLS).

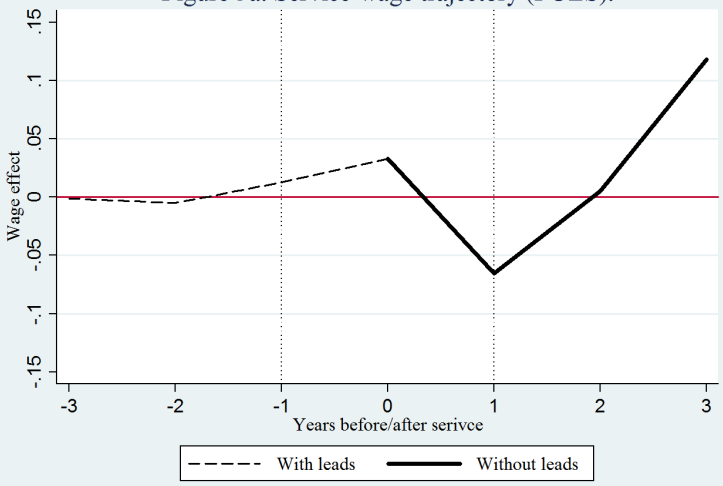



Figure 3b. Service-wage trajectory (individual FE).

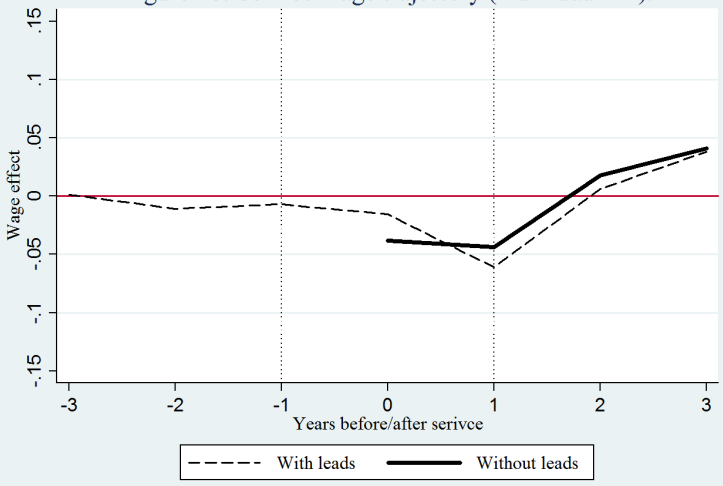
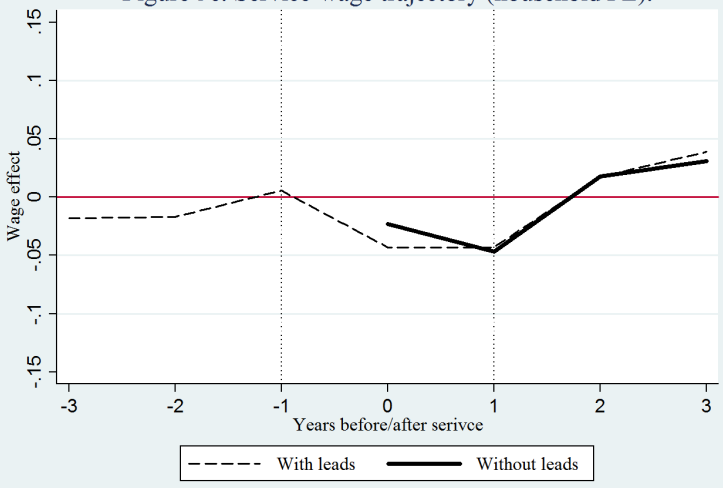



Figure 3c. Service-wage trajectory (household FE).



# CHAPTER 3

# Military Service and Marital Dissolution: A Trajectory Analysis

## 3.1 Introduction

The quality of life for military veterans is a growing national concern in the United States. The U.S. Department of Veterans Affairs (or VA) is the government's second largest department and had an annual budget of 78.4 billion dollars in 2013. The military is the largest vocational training institution in the country (Magnum & Ball, 1987) and the largest single employer of young adults (Angrist, 1998). Approximately seven percent of the American population were military veterans in 2010. For those military volunteers who are married, the quality of their marriage constitutes a significant portion of their overall quality of life. Military service adds additional challenges for married couples. Servicemen and women are often required to spend significant periods of time away from their families (especially during wartime service), are subject to increased risks of injury and death, and military families often have to relocate themselves geographically. Thus, the overall impact of military service on marital stability is an important area of research.

Much, if not all of the current literature on the impact military service has on divorce makes use of cross-sectional or repeated cross-sectional data and empirical techniques. While informative, these types of studies cannot fully characterize the impact in question. Cross-sectional data does not typically disclose the timing of either the respondent's military service or divorce. The key explanatory variable is often simply an indicator for those respondents who have or are currently serving in

the armed forces. More importantly, the divorce variable is most often simply an indicator for those respondents who report being divorced *at the time of the survey*. This illicits several concerns. First, those individuals who became divorced prior to military service as well as those who became divorced long after service (respondents whose divorce likely had little or nothing to do with their military service) are coded the same as those whose divorce occurred during or immediately following service. Second, any veterans whose service caused them to divorce but who later remarried are coded as being married at the time of the survey. Also, cross-sectional data do not allow for the use of panel techniques which can address issues pertaining to unobservables.

This study attempts to add to the literature on military service and marital stability in two important ways. First, my empirical strategy incorporates a trajectory specification to help disclose the timing of the effects of military service on the probability of marital dissolution. Second, to this author's knowledge, this is the first study on the marital stability of the veterans of the early 21st century wars. I find that military service during this era increased the probability of divorce by three to six percentage points in the year immediately following active duty service. After controlling for individual-specific unobserved heterogeneity, these effects remain for white veterans but disappear for minority veterans. Using an alternative panel of veterans from the 1980s-1990s, I find that service during this period did not increase the probability of divorce, suggesting differing effects for war and peacetime. Robustness checks using varied trajectory specifications and a third (cross-sectional) data set support these findings.

The remainder of the paper is organized as follows. The next section discusses the previous literature on the socio-economic effects of military service, focusing on marital dissolution. As a baseline study, a cross-sectional analysis is presented in

Section 3. In Section 4, I explain my primary empirical methodology and data. Results are presented in Section 5. In Section 6, I offer conclusions.

## 3.2 Military Service and Marital Stability

Figure 1 presents two annual time series from 1940 through 2007: the percent of the American population that is serving in the military and the national divorce rate.[1] The initial spike in military personnel (in the 1940s) is due, of course, to the second world war. Not since has such a high percentage served in the armed forces. Immediately following this spike in military personnel is a spike in American divorces and this relationship has been shown to be causal (Pavalko & Elder, 1990). The divorce rate grew rapidly in the 1960s and 1970s. The cause of this increase has been the topic of much debate among divorce researchers (Michael, 1978 and 1988; Oppenheimer, 1997; Preston, 1997, Ruggles, 1997; Friedberg, 1998; Goldstein, 1999; Wolfers 2006; Nunley & Zietz, 2012). Proposed causes include changes in divorce laws (Friedberg, 1998; Wolfers, 2006), the economic empowerment of women (Ruggles, 1997; Bremmer & Kesselring, 2004; Nunley, 2010), and changes in age demographics (Michael, 1978 and 1988; Carlson, 1979; South, 1985; Nunley & Zietz, 2012).

There are two main hypotheses regarding the vulnerability of military volunteers to divorce. Enlistment imposes severe demands on military families, increases risks of death or injury, decreases geographic mobility, and can require long working hours. The first hypothesis is that these characteristics of service increase divorce probabilities and has been called the stress hypothesis. Several studies (Kaylor et al., 1987; Elder et al., 1991; Call & Teachman, 1991 and 1996; MacLean & Elder, 2007) have discussed the psychological effects of military service which, in turn, may alter marital

[1]Military personnel (divorce rate) data come from the Correlates of War Project (U.S. Centers for Disease Control and Prevention).

stability. The second hypothesis is known as the selection hypothesis and postulates that the military tends to recruit individuals from the high risk of divorce population and, through potential additional compensation,[2] encourages them to marry.

The empirical literature on military service and marital stability is comprised of mixed results. Karney and Crown (2007) provide a literature review on the research surrounding military service, marriage, and divorce for the interested reader. Two fairly consistent findings are that marriage rates are higher among military and veteran personnel when compared to the general population and that any effects are likely to vary across race. Cadigan (2000) finds that military personnel are more likely to become married and have children. Lundquist (2004) finds that, though whites have higher marriage rates in the general population than do African-Americans, this gap disappears when looking at the military and veteran populations. Lundquist and Smith (2005) find that women who choose to volunteer for military service marry earlier than others and suggest that this is due to military family-friendly incentives. Teachman (2007) finds that service increases the probability of first marriage in all races, but that the effect is particularly strong for African-American men.

There is yet no consensus as to the impact military service has on the probability of divorce. Zax and Flueck (2003) use data from 1980 to find that military men are more likely to get married and divorced and do both at earlier ages. Pollard et al. (2008) use data from 1995-2002 and find that military men are less likely to get divorced than their civilian counterparts. Cohen and Segal (2009) find that service during the Vietnam era did not affect the probability of divorce after a relatively high level of control was implemented. Teachman and Tedrow (2008) find that military service made African-Americans less prone to divorce in the early days of the All-

[2]The military provides several benefits that are more valuable to volunteers with dependents. For example, military personnel who have dependents receive housing allowances that are approximately 25 percent greater than personnel with no dependents (Hogan & Seifert, 2010).

Volunteer Force. The authors explain this finding by noting that the military is a "relatively race-blind institution," that is, that the military provides an environment within which civilian differences in resources and status across race are minimized.

The military is aware that their personnel face additional marital challenges and is attempting to address the issue. There are several marital aid programs currently offered by the military to active duty, Reserve, and Guard personnel.[3] These programs vary somewhat by military branch and are often a responsibility of a chaplain. The programs offer workshops, "marriage enrichment" classes, and often couples retreats. Military personnel and family all have access to free legal services and advice provided by the military legal office (JAG), which can be useful when considering or going through a divorce. The U.S. Army, through their program entitled Military OneSource, provides free counseling on issues such as couples communication, stress management, and others. The other major military branches have similar but lesser known programs. Therefore, estimates (both their magnitude and timing) of the impact of military service on marital stability also have implications for these programs.

## 3.3 The CPS July 2010 Veterans Supplement

As a baseline analysis, I examine the Current Population Survey (CPS): July 2010 Veterans Supplement. This survey was a joint project of three agencies: the U.S. Department of Commerce's Bureau of the Census, the U.S. Department of Labor's Bureau of Labor Statistics, and the U.S. Department of Veterans Affairs. As the title suggests, these data differ from the typical CPS survey instrument in that those individuals who identified as military personnel or veterans are overrepresented and

[3]Strong Bonds, the Prevention and Relationship Enhancement Program (PREP), the Chaplain's Religious Enrichment Development Operations (CREDO), and MarriageCare are the names of the U.S. Army's, Marines', Navy's, and Air Force's marital aid programs, respectively.

responded to a few additional questions regarding their service. These include in which war era(s)[4] they served and whether they participated in combat. The remaining additional veteran survey questions pertained to their experience with and aid from the Department of Veterans Affairs, this being the supplement's primary purpose. As with other CPS instruments, data were also collected on the individual's general demographics and labor market status.

Summary statistics from these data are presented in Table 1. Means for each of the variables I use are shown for the full sample as well as within each veteran war era subsample. The top panel in Table 1 presents the marital status breakdown (with five categories) of each subgroup at the time of the survey. Both Vietnam and AVF veterans are shown to be divorced at higher rates than the full sample average. Additionally, AVF veterans report being separated from their spouses at a higher rate than the full sample average. The second panel of Table 1 shows several demographic characteristics of each subsample. Females, African-Americans, and other minorities make up larger portions of the AVF sample than any other veteran group. Several other demographics are shown. The third panel of Table 1 presents the labor market variables captured by the survey. AVF veterans are more likely to be either unemployed or employed in the pubic sector than any other veteran cohort or the full sample, as well as belong to a high income household. The last panel in this table presents means from a binary variable relating whether the respondent was ever in a combat scenario while serving.[5] At approximately 26 percent, more AVF

[4]The eras are generally named for the major war that took place in that time period while the All-Volunteer Force era refers to the post-draft period. Specifically, the World War II (WWII) service era refers to pre-July 1950; Korean War era July 1950-July 1964; Vietnam War era August 1964-April 1975; and the All-Volunteer Force era post-April 1975.

[5]Gimbel and Booth (1994) find that Vietnam veterans who saw combat were more likely to have antisocial behaviors which, in turn, decreased their marital stability. Ruger et al. (2002) find that self-reported participation in combat increases the hazard rate for marital dissolution by over 60 percent.

veterans saw combat than the Korean War veterans surveyed (22 percent), but less than the World War II (49 percent) or Vietnam veterans (35 percent) surveyed.

Not being panel in nature, these data only allow for cross-sectional estimation. Also because of its cross-sectional nature, only the individual's *current* marital status (as of July 2010) is captured. Thus, individuals who were previously divorced and later remarried are coded as being married. Additionally, for those that report being divorced, it is unknown whether the divorce occurred before, during, or after active duty military service. Therefore, estimates of the effect of military status on marital dissolution with these data must be considered suggestive. I use these data in logit divorce models which take the form

$$y_i = \alpha + \sum_{j=1}^{4} \beta_j Service\ Era_{ij} + x_i'\theta + \varepsilon_i \tag{1}$$

where $y_i$ represents a divorced indicator; $\alpha$ is an intercept; $x_i'$ is a vector of controls with $\theta$ its corresponding vector of parameters; and $\varepsilon_i$ is the error term. Also included in these models are indicator variables relating to the four war era(s) discussed previously. These variables are represented by $Service\,Era_{ij}$ in Equation (1). Of key interest is the marginal effect of service during the AVF era on divorce.

The results of these models are presented in Table 2. Marginal effects of military service during the AVF period on current divorce status for four models are shown, each with an increasing level of control. From left to right, the first model only includes the service era indicators, the second adds demographic controls, the third labor market controls, and the last combat status. Across all four levels of control, the effect of service during the AVF era on divorce varies little, from approximately 4.1 to 5.1 percentage points. The previous literature on AVF veterans has shown a negative (not positive, as shown here) effect of service on divorce. However, these

studies focused on the early days of the AVF and did not include veterans of the wars in Iraq and/or Afghanistan. For reasons mentioned in Section 1, panel methods will better disclose the true nature of these effects.

## 3.4 Methodology and Data

### 3.4.1 Empirical Methodology

I specify a model of divorce of the form

$$y_{it} = \alpha + \sum_{j=1}^{2} \beta_j Service_{it-j} + \gamma Service_{it} + \sum_{k=1}^{2} \delta_k Service_{it+k} + x'_{it}\theta + \lambda_t + \varepsilon_{it} \quad (2)$$

where $y_{it}$ is an indicator for a marital dissolution event (a legal divorce) for individual $i$ at time $t$ (data are annual); $Service_{it}$ is an indicator for military service; $x'_{it}$ is a vector of controls with $\theta$ its corresponding vector of parameters; $\lambda_t$ are time fixed effects; and $\varepsilon_{it}$ is the error term. Thus, the models contain five parameters relating to military service - one for the years of service, two lags, and two leads.[6] More lags and/or leads could have been used, but the greater the distance between service and the divorce, the more difficult it is to be sure that service was indeed the cause.[7] The control variables included in $x'_{it}$ are fully discussed in the following subsection. Time fixed effects allow for control of year-specific unobservables that might affect the probability of divorce such as any national trends. Since the dependent variable is a binary event, these models are estimated using logistic regression.

[6]Trajectory specifications have been widely used in empirical studies. As examples, they have recently been used to estimate the effects of motherhood on earnings (Fernández-Kranz et al., 2013), the wage effects of incarceration (Brown, 2014), and veteran wages (Brown & Routon, 2014).

[7]Models with three lags/leads as well as models with four lags/leads were also estimated. These additional coefficients were found to be statistically insignificant and the remaining military service parameters were *highly* similar to those in the models presented here (with two lags and two leads).

I then reestimate the models with individual fixed effects to control for individual-specific, time-invariant heterogeneity. Such control allows me to address two important empirical issues. First, there are likely unobservables contributing to one's choice to enlist in the military. Second, unobservables may also play a role in the likelihood of divorce. These models take the form

$$y_{it} = \alpha_i + \sum_{j=1}^{2} \beta_j Service_{it-j} + \gamma Service_{it} + \sum_{k=1}^{2} \delta_k Service_{it+k} + x'_{it}\theta + \lambda_t + \varepsilon_{it} \quad (3)$$

with the primary difference between Equations (2) and (3) being the individual fixed effects (the $\alpha_i$'s). With these fixed effects, the time-invariant controls in $x'_{it}$ are no longer needed and are thusly excluded. For robustness, and for both the pooled logit and fixed effects logit models, I run multiple trajectory specifications. I start by estimating models that only include an indicator for the years of service, that is, models that exclude the terms $\sum_{j=1}^{2} \beta_j Service_{it-j}$ and $\sum_{k=1}^{2} \delta_k Service_{it+k}$. Next, I estimate models that include the service years indicator and leads but not lags. Then, I estimate models that include the service years indicator and lags but not leads. Lastly, I estimate my full and preferred models that include both lags and leads as well as the service years indicator (Equations (2) and (3) in their entirety). The coefficients $\delta_k$ act as a type of falsification test where statistical significance could imply the presence of spurious correlation (Granger, 1969).

Two data sets are used. All models are first estimated for the full sample within each data set and then a within-racial group analysis is performed. Two racial groups are considered, whites and minorities.[8] This was done as some previous studies find differing effects across race (see Section 2). Thus, I present the results of 48 models, one for each of the four trajectory specifications within each of the two racial splits

[8]More specific minority veteran sample sizes (e.g., Hispanic veterans or African-American veterans) were deemed too small to analyze individually, especially with regard to the NLSY97 sample.

and the full sample, for both the pooled logit and fixed effects logit specifications, within each of the two data sets.

### 3.4.2 NLSY79 and NLSY97

I draw data from both the 1979 and 1997 cohorts of the National Longitudinal Survey of Youth, the NLSY79 and NLSY97, respectively. The NLSY79 is an ongoing survey of Americans who were aged 14 to 21 in the initial 1979 interview (BLS, 2011a). The initial survey consisted of 12,868 individuals who responded to questions on a variety of topics including demographics, education, and labor market performance. Overall, there have been nearly 2,000 military personnel surveyed as part of the NLSY79. These individuals generally served during the early and mid 1980s. The NLSY97, a similar survey, consists of respondents who were born in 1980-84 (BLS, 2011b). Follow-up interviews have been conducted annually through 2010. Of the almost 9,000 respondents, 520 have served in the armed forces as of 2010. These respondents were 16 to 22 years old during 2001, the year of the September 11 attacks on the U.S. and the start of the war in Afghanistan and 18 to 24 in 2003, the beginning of the Iraq War. This age range makes the NLSY97 cohort a useful sample for studying the impacts of military service during these early 21st century wars. Both the NLSY79 and NLSY97 were designed to be nationally-representative samples when using the survey weights provided.

There are multiple reasons why one would expect different results from these two panels. Perhaps the most important difference between the two veteran samples is that the NLSY79 veterans served during a period of general peace while the NLSY97 veterans all served during the wars in Afghanistan and/or Iraq. Wartime service is likely to impact marital stability differently than peacetime service. There have also been more general changes between these two time periods including an increased

moral acceptance of divorce. Lastly, as previously mentioned, the military now offers more marital aid and counseling programs to their married personnel.

From these data, I drop the subsample of individuals who have never been married. For use as the dependent variable, I construct an indicator for the period the respondent became legally divorced, a divorce event indicator. This variable is not to be confused with a "divorced indicator" (a variable that equals one in all periods the respondent reports being divorced) as used in some other divorce studies. My control set includes the respondent's race, gender, and both of their parents' education levels (as measured by total years of schooling). As a cognitive ability proxy, I use the respondents' Armed Forces Qualification Test (AFQT) scores.[9] More specific to a study on divorce, I use as controls an indicator for those respondents who were raised by both of their parents, an indicator for those that were raised in a rural area, and religion indicators (one for individuals who were raised Catholic, another for those raised protestant, and a third for those raised in any other religion).

I also include several time-variant controls. Perhaps the most important of these is the length (in years) of the respondent's current marriage (and its square). Other time-variant controls are weeks worked in the prior period (a labor force attachment measure), total years of schooling, and number of children. Several studies (Becker et al., 1977; Ono, 1998; Brines & Joyner, 1999) have shown that total income in the prior period is a strong predictor of divorce. Rodgers (2004) finds that the ratio of the spouses' incomes (also in the prior period) is another such predictor. Thus, I also include these two controls. Only these time-variant controls are needed in the fixed effect logits.

[9]All NLSY79 and NLSY97 respondents were asked to take this multiple choice test as part of the survey, regardless of future military service. As this is the same test used by the U.S. Armed Forces for enlistment qualification, it is a particularly useful control in this case.

Tables 3 and 4 present selected summary statistics from the NLSY79 and NLSY97. At the time of writing, 2010 was the most recent publicly-available survey wave for both samples. Combining answers from all survey waves, I create two indicator variables for each sample. One discloses those respondents who have at any time been married and the other those respondents who have ever completed a legal divorce. The top panels in each table relate means from these two created variables. Veterans and nonveterans in the NLSY79 are not statistically different in their propensity to have been married, but veterans are shown to have divorced more often (at 44 percent versus 35 percent). Veterans in the NLSY97, a much younger sample, are shown to be both more likely to have gotten married (at 60 percent versus 44 percent) and had a divorce (at 18 percent versus 6 percent). Also shown in these tables are means and differences for some other relevant variables. Generally speaking, NLSY79 veterans appear to differ more from their nonveteran counterparts than do NLSY97 veterans. Notably, both veteran groups are shown to be of higher average ability (as proxied by their AFQT scores), have had fewer children, more years of schooling, and higher wages than their nonveterans counterparts.

## 3.5 Results

Table 5 contains the pooled logit results from the NLSY79. The top panel presents results from the full sample; the middle panel shows results from the whites only subsample; and the bottom panel displays results from the minorities only subsample. Columns $k = \{1, 2\}$ represent survey waves (years) prior to service while $j = \{1, 2\}$ represent survey waves after military service. The column $s = 1$ refers to those years of active duty military service for each respondent. For all three subsamples and all specifications, I find no statistically significant effects in the second year following service, but significant effects are found elsewhere in most cases. Effects within each

racial sample but across trajectory specification are highly robust. Small negative effects are found for active service years, with a decreased probability of a divorce event of approximately 2.5 percentage points for whites and 3.5 percentage points for minorities. As both individuals are generally present for a divorce event (signatures are required, negotiations pertaining to wealth and property division are often needed, etc.) and active duty service often displaces volunteers, negative effects within service years are perhaps not surprising. For both racial groups, I find negative effects two years before service. For whites, negative effects are also found in the year immediately prior to service while negative effects are additionally found for minorities in the year immediately following service.

Table 6 presents the pooled logit results from the NLSY97. As these respondents represent the most recent generation of military veterans, those that served during the early 21st century wars, they are perhaps of higher interest. Generally speaking, there are fewer statistically significant effects here than in the older sample. Most importantly, I find evidence that military service does increase the probability of divorce and this most often occurs in the first year following service. These effects are evident in the full sample and within each racial subsample. For white veterans, the effect appears to be at the magnitude of an approximate six percentage points increase. For minority veterans, the effect appears to be smaller at around four percentage points. Like in the older sample, I find no effect during the second year post-service across all subsamples and specifications. Unlike the older sample, there are also no apparent effects during service years. As in Table 5, the models presented in Table 6 are highly robust across trajectory specifications (across models that include/exclude lag and lead variables).

Table 7 contains the fixed effects logit results from the NLSY79. Comparing these results to those in Table 3 shows that, after controlling for individual-specific

unobserved heterogeneity, the estimated effects of military service on divorce increase in magnitude. This implies that those individuals from this sample period who joined the military generally have unobservable characteristics that make them less prone to divorce. All coefficients reported in this table are negative in sign and, with the exception of the second year following service for white veterans, are statistically significant during and post service. There are no longer statistically significant effects in the years pre-service implying any spurious correlation has been controlled through the fixed effects. Military service appears to have not made veterans from this period more likely to have a divorce. In fact, for the first year following service, white veterans are shown to be 2.3-2.6 percentage points less likely to experience a divorce while this effect is higher for minority veterans at 5.1-5.5 percentage points. Though less so when compared to the models without individual fixed effects, the models in Table 7 are robust across trajectory specifications.

Table 8 displays the fixed effects logit results from the NLSY97. Unlike the NLSY79 sample, and generally speaking, here I find that controlling for individual-specific unobserved heterogeneity decreases the magnitude of the effects. This implies that those individuals from this sample period who joined the military generally have unobservable characteristics that make them *more* prone to divorce. Perhaps the key difference between the two cohorts of military volunteers used here is that the NLSY97 volunteers served during a time of conflict, the wars in Afghanistan and Iraq. Many of these veterans enlisted post-9/11 and thus knew they were volunteering for warfare. This may explain differences in unobservables. In the full sample analysis, I again find that these veterans are more likely to obtain a divorce in the first year following service. After splitting the sample by race, however, this effect is shown to only be present for white veterans. In the first year following service, white veterans of the early 21st century wars were made approximately three to four percentage

points more likely to legally dissolve their marriage. Again, a lack of significance in the pre-service years implies a lack of spurious correlation after the fixed effects are employed.

From the estimates in the full and preferred specifications, those that include both lags and leads and are performed within-race, I create trajectory plots of the effects of military service on marital dissolution. These are shown in Figures 2 and 3. Figures 2a and 2b contain the the within-race pooled logit models from the NLSY79. Figures 2c and 2d present the corresponding fixed effects logit models. These plots perhaps make the trajectory of the effects easier to discuss. Figures 3a and 3b show the the within-race pooled logit models from the NLSY97, while 3c and 3d display the corresponding fixed effects logit models. From these four trajectory plots, we can easily see the spike in divorce probability in the first year post-service. Unlike with the NLSY79, the NLSY97 plots are not very dissimilar across race.

## 3.6 Conclusions

I set out to estimate the effect of military service on the probability of marital dissolution with particular interests in the timing of the effects and the most recent generation of veterans, those that served during the wars in Afghanistan and/or Iraq. I find that these veterans were indeed made more likely to obtain a divorce and that this divorce most often took place in the first year following active duty service. For white veterans of this era, the increased probability of divorce was between three and six percentage points. For minority veterans, I first find that this effect was between three and four percentage points, but that the effect disappears after controlling for unobserved heterogeneity. Routon (2014) finds that service during these wars granted minority veterans a 10 percent wage premium and made them more likely to attempt college, but that white veterans experienced no such effects. Since increased income

and education can increase marital stability, this may partially explain the racial difference in divorce effects.

Using a second data set containing veterans from the 1980s-1990s, I find that these veterans were not made more likely to dissolve their marriage by their service. The primary difference between these two veteran cohorts is that the former's service period was a time of war and the latter's a time of peace, suggesting differing effects for war and peacetime. Lastly, using a third set of data, I confirm that veterans of the All-Volunteer Force era were approximately five percentage points more likely to *be* divorced as of 2010 when compared to the average American who has ever been married.

These results show that there is indeed a positive relationship between military service and marital dissolution, at least for the most recent generation of veterans. The effect appears to take place in the year immediately following the last year of active duty service. Additionally considering that legal divorce proceedings can be lengthy, this effect is "fast;" perhaps implying that, in many cases, at least one spouse has decided to dissolve the marriage during the active duty service period. This timing also indicates that any marital instability caused by post-traumatic stress disorder (PTSD) or the "shock" of readjusting to civilian life is generally immediate.

More research is needed before a full picture of the relationship between military service and marital stability can be achieved. First, both veteran cohorts used in the trajectory analyses here come from the All-Volunteer Force era. Using data from prior periods, trajectory analyses on veterans of earlier theaters (e.g., Vietnam, Korea) would shed light on historical effects. Second, if a larger sample were available, it could prove enlightening to estimate within-gender military-divorce effects for the most recent generation of veterans. Third, if program participation information was available, a direct test of the efficacy of the military's marital aid programs may

influence policy. Lastly, I find some evidence here that volunteers are less likely, when compared to the general population, to have experienced a divorce in the years immediately prior to service. Thus, a study on marriage, marital stability, and the choice to volunteer may advance the literature on the determinants of enlistment.

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**Table 3.1: Selected Sample Means (CPS July 2010 Vet. Supp.)**

| Variable | Full Sample | WWII War Era Veterans | Korean War Era Veterans | Vietnam War Era Veterans | All-Vol Force Veterans |
|---|---|---|---|---|---|
| *Marital status* | | | | | |
| Divorced | 0.102 | 0.041 | 0.099 | 0.161 | 0.167 |
| Separated | 0.019 | 0.006 | 0.010 | 0.013 | 0.024 |
| Married | 0.526 | 0.625 | 0.747 | 0.721 | 0.642 |
| Widowed | 0.060 | 0.303 | 0.114 | 0.037 | 0.016 |
| Never married | 0.293 | 0.025 | 0.030 | 0.068 | 0.152 |
| *Demographic* | | | | | |
| Male | 0.479 | 0.965 | 0.981 | 0.971 | 0.869 |
| Age | 45.362 | 82.492 | 74.008 | 63.009 | 45.762 |
| African-American | 0.101 | 0.050 | 0.054 | 0.071 | 0.122 |
| Other non-white | 0.075 | 0.022 | 0.030 | 0.041 | 0.062 |
| Born in U.S. | 0.858 | 0.976 | 0.976 | 0.969 | 0.952 |
| Homeowner | 0.734 | 0.867 | 0.906 | 0.872 | 0.754 |
| Edu.: High school or above | 0.837 | 0.791 | 0.878 | 0.955 | 0.977 |
| Edu.: Associate's or above | 0.354 | 0.297 | 0.348 | 0.400 | 0.415 |
| Edu.: Bachelor's or above | 0.266 | 0.257 | 0.272 | 0.285 | 0.273 |
| Edu.: Master's or above | 0.092 | 0.107 | 0.113 | 0.117 | 0.100 |
| Edu.: Ph.D./prof. degree | 0.026 | 0.044 | 0.040 | 0.034 | 0.026 |
| Children at home | 0.364 | 0.004 | 0.012 | 0.052 | 0.403 |
| Metro. resident | 0.783 | 0.758 | 0.705 | 0.738 | 0.776 |
| Region: Northeast | 0.209 | 0.238 | 0.221 | 0.192 | 0.168 |
| Region: South | 0.310 | 0.294 | 0.310 | 0.318 | 0.357 |
| Region: West | 0.245 | 0.225 | 0.227 | 0.246 | 0.243 |
| *Labor market* | | | | | |
| Unemployed | 0.059 | 0.001 | 0.012 | 0.041 | 0.062 |
| Public sector job | 0.095 | 0.007 | 0.029 | 0.107 | 0.200 |
| Retired | 0.113 | 0.674 | 0.549 | 0.237 | 0.051 |
| Low income | 0.289 | 0.422 | 0.350 | 0.240 | 0.208 |
| High income | 0.186 | 0.080 | 0.093 | 0.195 | 0.220 |
| *Combat status* | | | | | |
| Saw combat | 0.026 | 0.492 | 0.224 | 0.353 | 0.256 |
| *N* | 108,534 | 1,069 | 2,604 | 3,400 | 3,696 |

*Notes*: The CPS sample was restricted to exclude those individuals with missing values for the marital status variable. Low (high) income refers to annual earnings less than (greater than) $30,000 ($100,000).

**Table 3.2: Logit Results (CPS July 2010 Vet. Supp.)**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| All-Vol. Force | 0.0409*** | 0.0512*** | 0.0505*** | 0.0493*** |
| | (0.0073) | (0.0074) | (0.0074) | (0.0076) |
| *Controls* | | | | |
| Demographic | | X | X | X |
| Labor market | | | X | X |
| Combat status | | | | X |
| Pseudo $R^2$ | 0.01 | 0.09 | 0.10 | 0.10 |

*Notes*: Dependent variable is a divorced indicator. The sample was restricted to exclude those individuals who are widowed and those who have never been married. Values are marginal effects with standard errors in parentheses. See Equation 3 for the specification and Section 4.2 or Table 6 for the sets of controls. N = 70,216. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

**Table 3.3: Selected Sample Means (NLSY79, 2010 Survey Wave)**

| Variable | Full Sample (i) | Veteran (ii) | Non-Veteran (iii) | Difference (ii) - (iii) |
|---|---|---|---|---|
| Ever divorced | 0.36 | 0.44 | 0.35 | 0.09*** |
| Ever married | 0.76 | 0.75 | 0.77 | -0.02 |
| *Time invariant variables* | | | | |
| Male | 0.50 | 0.73 | 0.46 | 0.27*** |
| African-American | 0.25 | 0.26 | 0.25 | 0.01 |
| Hispanic | 0.16 | 0.09 | 0.17 | -0.08*** |
| Mother's education | 10.99 | 11.41 | 10.92 | 0.49*** |
| Father's education | 11.15 | 11.48 | 11.09 | 0.39*** |
| AFQT percent score | 42.40 | 49.43 | 41.19 | 8.24*** |
| Raised by both parents | 0.60 | 0.52 | 0.60 | -0.08*** |
| Raised in a rural area | 0.18 | 0.08 | 0.20 | -0.12*** |
| Raised Catholic | 0.34 | 0.30 | 0.34 | -0.04*** |
| Raised Protestant | 0.50 | 0.57 | 0.49 | 0.08*** |
| Raised in other religion | 0.11 | 0.09 | 0.12 | -0.03*** |
| *Time variant variables* | | | | |
| Age | 48.64 | 48.65 | 48.63 | 0.02 |
| Number of children | 2.08 | 1.66 | 2.09 | -0.43*** |
| Years of schooling | 13.42 | 13.63 | 13.28 | 0.25*** |
| Income ($0,000) | 39.59 | 45.03 | 39.08 | 5.95*** |
| *N* | 12,686 | 1,914 | 10,772 | |

*Notes*: Stars refer to p-values from t-tests of sample mean equality across military status. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

**Table 3.4: Selected Sample Means (NLSY97, 2010 Survey Wave)**

| Variable | Full Sample (i) | Veteran (ii) | Non-Veteran (iii) | Difference (ii) - (iii) |
|---|---|---|---|---|
| Ever divorced | 0.07 | 0.18 | 0.06 | 0.12*** |
| Ever married | 0.44 | 0.60 | 0.44 | 0.16*** |
| *Time invariant variables* | | | | |
| Male | 0.51 | 0.78 | 0.49 | 0.29*** |
| African-American | 0.26 | 0.24 | 0.26 | -0.02 |
| Hispanic | 0.21 | 0.21 | 0.21 | 0.00 |
| Mother's education | 12.51 | 12.63 | 12.50 | 0.13 |
| Father's education | 12.79 | 12.69 | 12.79 | -0.10 |
| AFQT percent score | 45.12 | 50.45 | 44.90 | 5.55*** |
| Raised by both parents | 0.49 | 0.45 | 0.49 | -0.04 |
| Raised in a rural area | 0.23 | 0.24 | 0.22 | 0.02 |
| Raised Catholic | 0.31 | 0.32 | 0.31 | 0.01 |
| Raised Protestant | 0.53 | 0.53 | 0.52 | 0.01 |
| Raised in other religion | 0.03 | 0.01 | 0.03 | -0.02*** |
| *Time variant variables* | | | | |
| Age | 27.31 | 27.60 | 27.30 | 0.30*** |
| Number of children | 0.68 | 0.56 | 0.69 | -0.13*** |
| Years of schooling | 12.44 | 12.71 | 12.43 | 0.28*** |
| Income ($0,000) | 31.06 | 33.72 | 30.95 | 2.77* |
| *N* | 8,803 | 348 | 8,455 | |

*Notes*: Stars refer to p-values from t-tests of sample mean equality across military status. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

**Table 3.5: Pooled Logit Results (NLSY79)**

| | $k=2$ | $k=1$ | $s=1$ | $j=1$ | $j=2$ |
|---|---|---|---|---|---|
| | | | *Full Sample ($N_{person-year}$ = 188,418)* | | |
| Military service | | | -0.0289*** | | |
| | | | (0.0030) | | |
| Military service | -0.0372*** | -0.0297*** | -0.0291*** | | |
| | (0.0101) | (0.0088) | (0.0030) | | |
| Military service | | | -0.0291*** | -0.0202*** | -0.0053 |
| | | | (0.0030) | (0.0069) | (0.0071) |
| Military service | -0.0365*** | -0.0285*** | -0.0293*** | -0.0187*** | -0.0044 |
| | (0.0101) | (0.0088) | (0.0030) | (0.0069) | (0.0071) |
| | | | *Whites Sample ($N_{person-year}$ = 111,444)* | | |
| Military service | | | -0.0250*** | | |
| | | | (0.0037) | | |
| Military service | -0.0336** | -0.0396*** | -0.0252*** | | |
| | (0.0135) | (0.0116) | (0.0037) | | |
| Military service | | | -0.0251*** | -0.0150* | -0.0007 |
| | | | (0.0037) | (0.0084) | (0.0087) |
| Military service | -0.0331** | -0.0386*** | -0.0253*** | -0.0129 | 0.0002 |
| | (0.0135) | (0.0117) | (0.0037) | (0.0084) | (0.0087) |
| | | | *Minorities Sample ($N_{person-year}$ = 76,974)* | | |
| Military service | | | -0.0346*** | | |
| | | | (0.0048) | | |
| Military service | -0.0424*** | -0.0193 | -0.0348*** | | |
| | (0.0155) | (0.0133) | (0.0048) | | |
| Military service | | | -0.0348*** | -0.0283** | -0.0122 |
| | | | (0.0048) | (0.0120) | (0.0122) |
| Military service | -0.0413*** | -0.0183 | -0.0350*** | -0.0272** | -0.0112 |
| | (0.0155) | (0.0133) | (0.0048) | (0.0120) | (0.0122) |

*Notes*: Values are marginal effects with standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. Subscript $k$ ($j$) represents periods prior to (since) military service while $s$ represents time during service. See Eq. (1).

**Table 3.6: Pooled Logit Results (NLSY97)**

| | $k=2$ | $k=1$ | $s=1$ | $j=1$ | $j=2$ |
|---|---|---|---|---|---|
| | | | *Full Sample ($N_{person-year}$ = 43,988)* | | |
| Military service | | | -0.0023 | | |
| | | | (0.0063) | | |
| Military service | -0.0227** | -0.0133 | -0.0026 | | |
| | (0.0114) | (0.0104) | (0.0063) | | |
| Military service | | | -0.0020 | 0.0483*** | -0.0048 |
| | | | (0.0063) | (0.0104) | (0.0116) |
| Military service | -0.0256** | -0.0194* | -0.0023 | 0.0518*** | -0.0030 |
| | (0.0114) | (0.0105) | (0.0063) | (0.0105) | (0.0116) |
| | | | *Whites Sample ($N_{person-year}$ = 26,614)* | | |
| Military service | | | 0.0034 | | |
| | | | (0.0087) | | |
| Military service | -0.0226 | -0.0136 | 0.0032 | | |
| | (0.0154) | (0.0144) | (0.0087) | | |
| Military service | | | 0.0039 | 0.0591*** | -0.0001 |
| | | | (0.0087) | (0.0144) | (0.0159) |
| Military service | -0.0261* | -0.0189 | 0.0036 | 0.0620*** | 0.0018 |
| | (0.0154) | (0.0145) | (0.0087) | (0.0145) | (0.0160) |
| | | | *Minorities Sample ($N_{person-year}$ = 17,374)* | | |
| Military service | | | -0.0087 | | |
| | | | (0.0091) | | |
| Military service | -0.0229 | -0.0131 | -0.0089 | | |
| | (0.0170) | (0.0150) | (0.0091) | | |
| Military service | | | -0.0084 | 0.0364** | -0.0101 |
| | | | (0.0091) | (0.0150) | (0.0169) |
| Military service | -0.0250 | -0.0192 | -0.0087 | 0.0405*** | -0.0086 |
| | (0.0170) | (0.0153) | (0.0091) | (0.0153) | (0.0169) |

*Notes*: Values are marginal effects with standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. Subscript $k$ ($j$) represents periods prior to (since) military service while $s$ represents time during service. See Eq. (1).

**Table 3.7: Fixed Effects Logit Results (NLSY79)**

| | $k=2$ | $k=1$ | $s=1$ | $j=1$ | $j=2$ |
|---|---|---|---|---|---|
| | | | *Full Sample ($N_{person-year}$ = 188,418)* | | |
| Military service | | | -0.0400*** | | |
| | | | (0.0042) | | |
| Military service | -0.0591 | -0.0492 | -0.0467*** | | |
| | (0.0400) | (0.0387) | (0.0043) | | |
| Military service | | | -0.0467*** | -0.0348*** | -0.0178** |
| | | | (0.0044) | (0.0071) | (0.0072) |
| Military service | -0.0628 | -0.0522 | -0.0549*** | -0.0385*** | -0.0223*** |
| | (0.0400) | (0.0487) | (0.0045) | (0.0071) | (0.0073) |
| | | | *Whites Sample ($N_{person-year}$ = 111,444)* | | |
| Military service | | | -0.0292*** | | |
| | | | (0.0054) | | |
| Military service | -0.0524 | -0.0518 | -0.0358*** | | |
| | (0.0433) | (0.0416) | (0.0056) | | |
| Military service | | | -0.0336*** | -0.0225** | -0.0058 |
| | | | (0.0058) | (0.0088) | (0.0090) |
| Military service | -0.0549 | -0.0533 | -0.0415*** | -0.0256*** | -0.0100 |
| | (0.0433) | (0.0416) | (0.0060) | (0.0089) | (0.0091) |
| | | | *Minorities Sample ($N_{person-year}$ = 76,974)* | | |
| Military service | | | -0.0537*** | | |
| | | | (0.0065) | | |
| Military service | -0.0668 | -0.0462 | -0.0606*** | | |
| | (0.0451) | (0.0331) | (0.0066) | | |
| Military service | | | -0.0615*** | -0.0506*** | -0.0331*** |
| | | | (0.0067) | (0.0119) | (0.0121) |
| Military service | -0.0715 | -0.0513 | -0.0700*** | -0.0552*** | -0.0379*** |
| | (0.0651) | (0.0432) | (0.0069) | (0.0119) | (0.0121) |

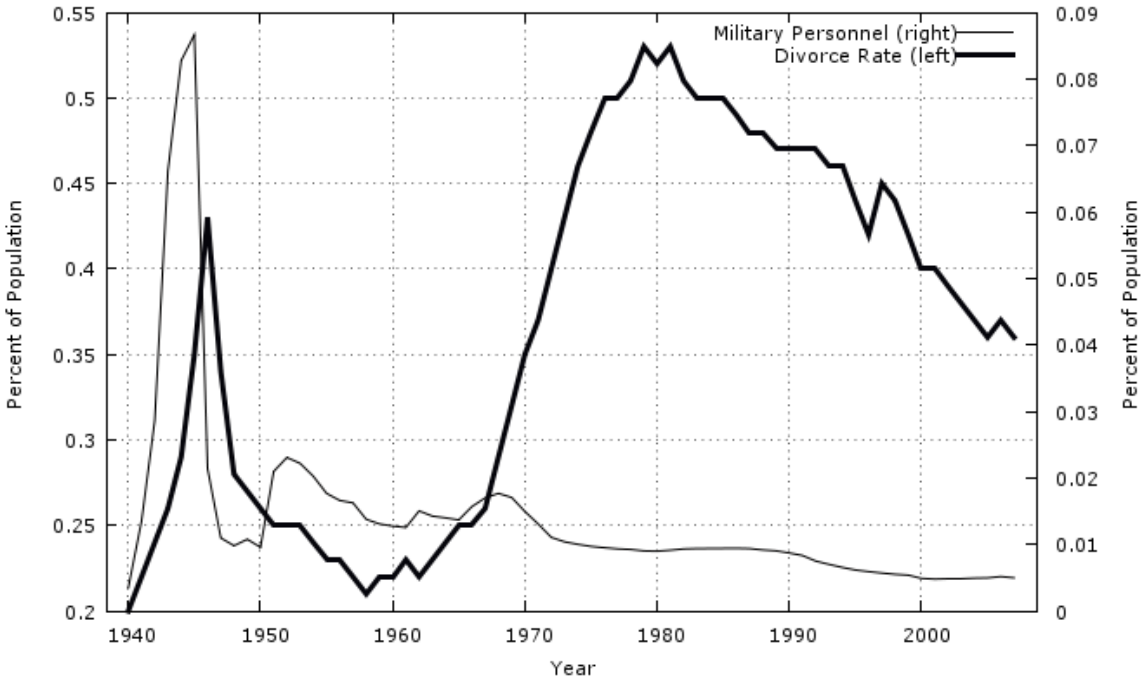
*Notes*: Values are marginal effects with standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. Subscript $k$ ($j$) represents periods prior to (since) military service while $s$ represents time during service. See Eq. (2).

**Table 3.8: Fixed Effects Logit Results (NLSY97)**

| | $k=2$ | $k=1$ | $s=1$ | $j=1$ | $j=2$ |
|---|---|---|---|---|---|
| | | | *Full Sample ($N_{person-year}$ = 43,988)* | | |
| Military service | | | -0.0237*** | | |
| | | | (0.0073) | | |
| Military service | -0.0518 | -0.0447 | -0.0360*** | | |
| | (0.0417) | (0.0308) | (0.0076) | | |
| Military service | | | -0.0217*** | 0.0307*** | -0.0220* |
| | | | (0.0076) | (0.0108) | (0.0118) |
| Military service | -0.0528 | -0.0463 | -0.0361*** | 0.0248** | -0.0312*** |
| | (0.0418) | (0.0309) | (0.0080) | (0.0109) | (0.0119) |
| | | | *Whites Sample ($N_{person-year}$ = 26,614)* | | |
| Military service | | | -0.0172* | | |
| | | | (0.0101) | | |
| Military service | -0.0522 | -0.0489 | -0.0297*** | | |
| | (0.0457) | (0.0350) | (0.0105) | | |
| Military service | | | -0.0139 | 0.0390*** | -0.0183 |
| | | | (0.0104) | (0.0149) | (0.0162) |
| Military service | -0.0519 | -0.0487 | -0.0282** | 0.0320** | -0.0270* |
| | (0.0459) | (0.0350) | (0.0110) | (0.0150) | (0.0164) |
| | | | *Minorities Sample ($N_{person-year}$ = 17,374)* | | |
| Military service | | | -0.0309*** | | |
| | | | (0.0105) | | |
| Military service | -0.0514 | -0.0404 | -0.0429*** | | |
| | (0.0473) | (0.0357) | (0.0110) | | |
| Military service | | | -0.0306*** | 0.0211 | -0.0265 |
| | | | (0.0110) | (0.0157) | (0.0172) |
| Military service | -0.0540 | -0.0436 | -0.0453*** | 0.0162 | -0.0364** |
| | (0.0475) | (0.0357) | (0.0116) | (0.0157) | (0.0173) |

*Notes*: Values are marginal effects with standard errors in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. Subscript $k$ ($j$) represents periods prior to (since) military service while $s$ represents time during service. See Eq. (2).

**Figure 3.1: U.S. Military Personnel and the Divorce Rate**




*Notes*: Military personnel (divorce rate) data come from the Correlates of War Project (U.S. Centers for Disease Control and Prevention).

**Figure 3.2: Military Service-Marital Dissolution Trajectories (NLSY79)**

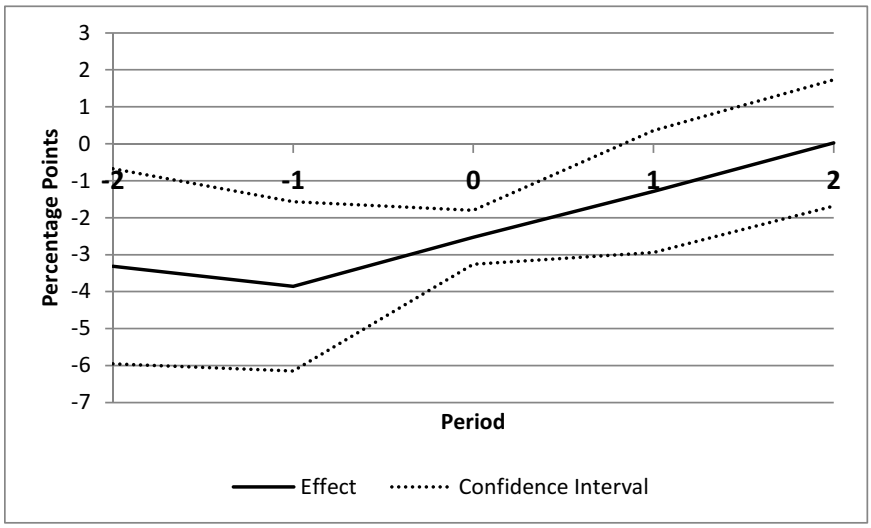



Figure 3.2a: Whites Sample Pooled Logit

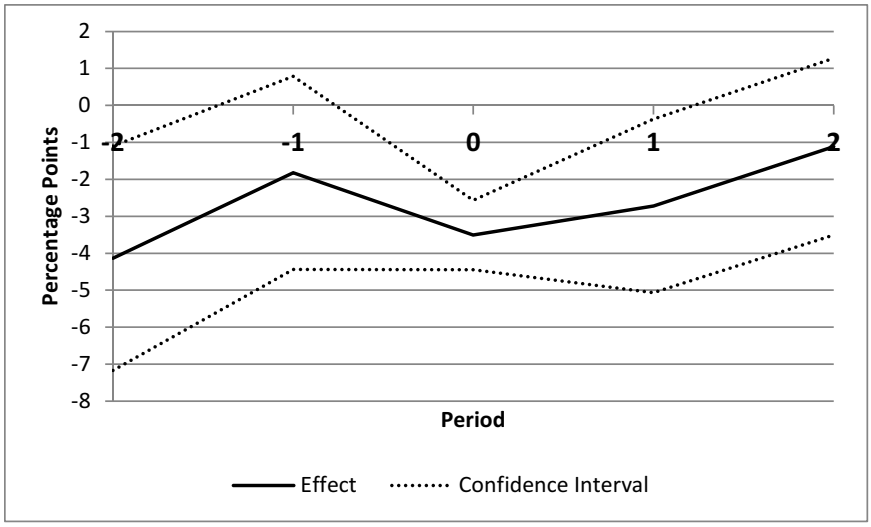



Figure 3.2b: Minorities Sample Pooled Logit

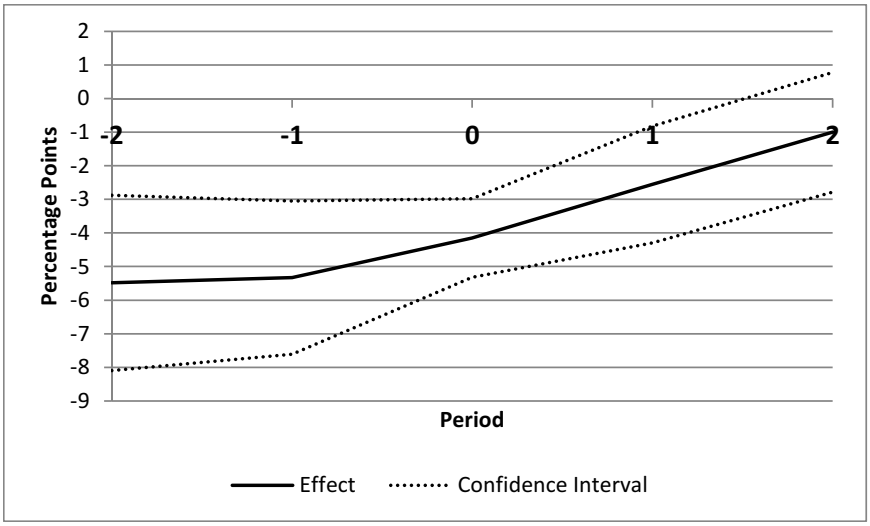



Figure 3.2c: Whites Sample Fixed Effects Logit

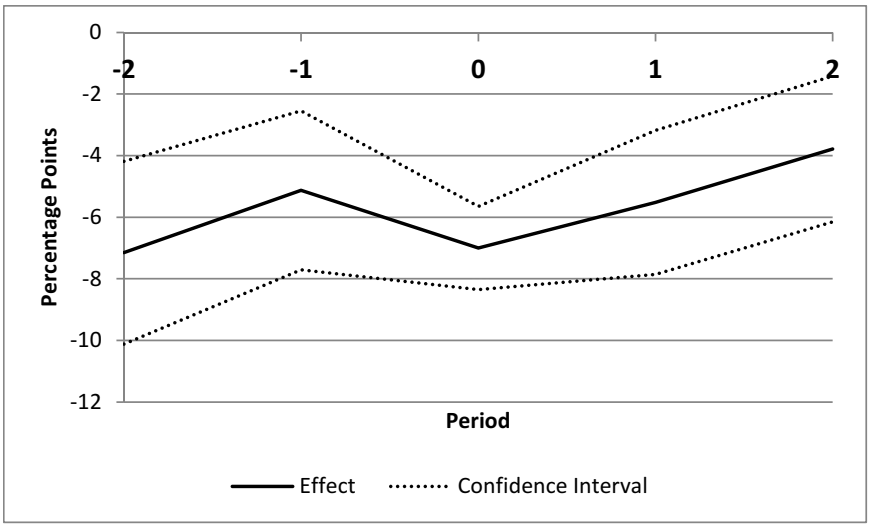



Figure 3.2d: Minorities Sample Fixed Effects Logit

**Figure 3.3: Military Service-Marital Dissolution Trajectories (NLSY97)**

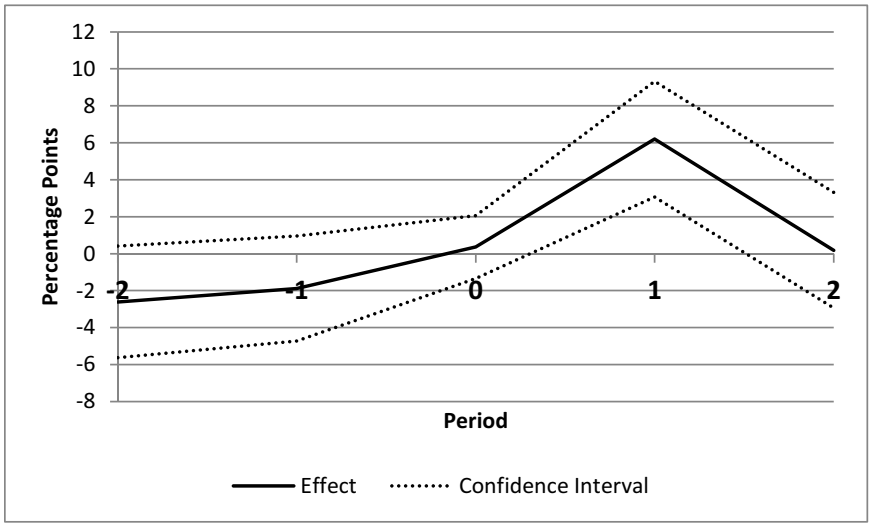



Figure 3.3a: Whites Sample Pooled Logit

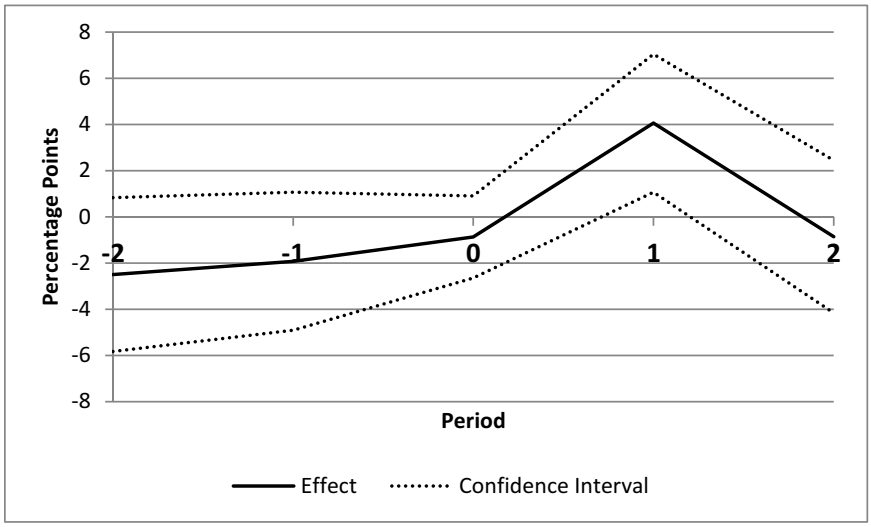



Figure 3.3b: Minorities Sample Pooled Logit

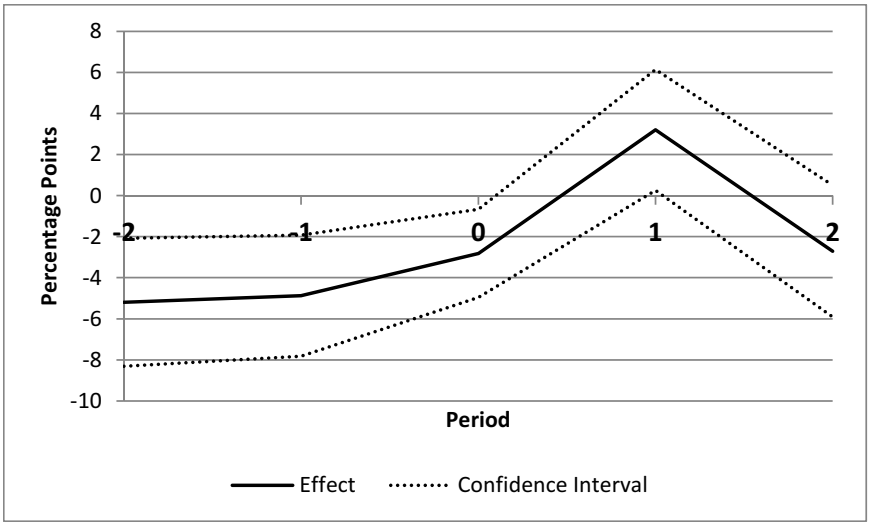



Figure 3.3c: Whites Sample Fixed Effects Logit

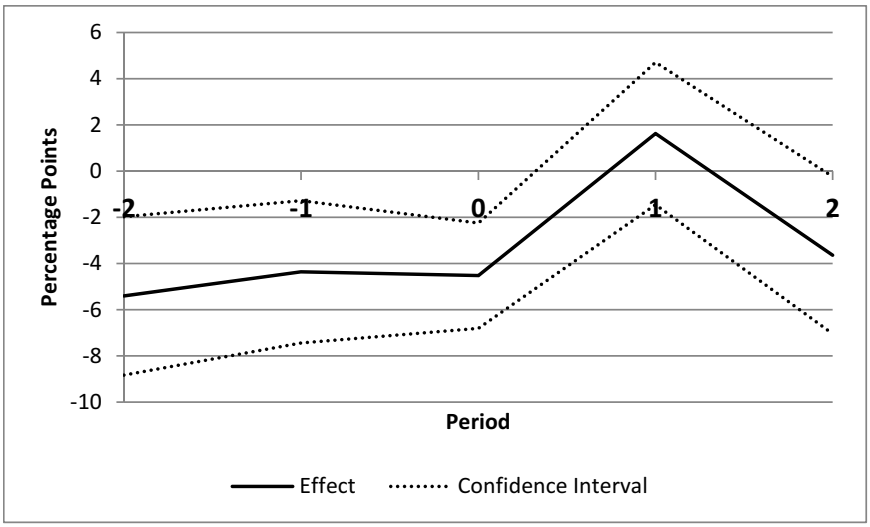



Figure 3.3d: Minorities Sample Fixed Effects Logit