

Paying to Avoid Recession: Using Reenlistment to Estimate the Cost of Unemployment*

Mark Borgschulte[†] and Paco Martorell[‡]

December 9, 2014

Abstract

People who enter the labor market during a period of high unemployment, or lose a job and are forced to search in a recession, suffer significant earnings losses. However, a number of factors, such as the value of leisure and public and private transfers, could mitigate the extent to which these earnings losses lead to welfare losses. In this paper, we use information on the reenlistment choices of military servicemembers, together with longitudinal earnings data for those who leave the military and enter the civilian workforce, to provide revealed-preference estimates of the monetary value of avoiding a high-unemployment labor market. We use administrative personnel records for over a million enlistees from 1993-2009 to estimate reenlistment choice models that include the value of the reenlistment bonus offered to each person (which depends on their military occupation and the time period) and the unemployment rate in their home state. These models imply that servicemembers value each percentage point increase in the unemployment rate in their home state as equivalent to more than \$2000 or a 1.5-2% reduction in earnings over the course of their next contract. We then compare this valuation to the observed earnings losses of those who choose to exit the military, using the offered bonuses to correct for selectivity biases in the earnings of those who

*For comments and other assistance, we thank Beth Asch, Matt Botsch, David Card, Flavio Cunha, François Gerard, Jim Hosek, Pat Kline, Ron Lee, Dave Loughran, Vikram Maheshri, Ulrike Malmendier, Pascal Michaillat, Maury Obstfeld, Carl Nadler, Issi Romem, Jesse Rothstein, Emmanuel Saez, David Silver, Lanwei Yang, and seminar participants at UC Berkeley, San Francisco State, United States Military Academy, University of Miami, University of Virginia, University of Illinois at Urbana-Champaign, Census Bureau-LEHD Group and RAND.

[†]Assistant Professor, Department of Economics, University of Illinois at Urbana-Champaign and IZA Research Affiliate

[‡]Assistant Professor, University of California at Davis School of Education

exit. Consistent with recent work on new college graduates, we find that servicemembers who re-enter a local labor market with a 1 percent higher unemployment rate experience lower earnings for up to 10 years, with peak losses above 2% of annual earnings for cohorts separating from 1993-2004, and above 3.5% when the sample is extended through the Great Recession. Combining our our estimates we conclude that offsetting mechanisms like public and private transfers and the value of leisure likely offset between 13% and 35%, and not more than half, of the observed earnings losses of people who enter the labor market in worse times.

Recent research establishes that business cycle conditions at the time of job search exert a large and persistent effect on the subsequent earnings and occupation choices of displaced workers and new college graduates.¹ Whether these earnings losses imply large or small welfare losses, however, depends on the strength of a wide range of mitigating mechanisms, including public support programs, intra-family transfers, and the value of leisure.² With this problem in mind, the traditional approach to measuring the welfare cost of business cycles is based on secondary measures of welfare—such as consumption responses³ or aggregate worker flows⁴—and not on the variation in observed earnings outcomes over the cycle.

In this paper we present new revealed-preference estimates of the monetary value of avoiding a high-unemployment labor market, based on the reenlistment choices of military servicemembers. The underlying idea is simple: if labor market conditions affect not only earnings, but also welfare, people will be willing to pay more to enter the labor market when unemployment is low. In the case of the reenlistment decision, the costly option takes the form of giving up the (often generous) reenlistment bonuses that are used by the military to balance the supply and demand for different occupation groups. We fit reenlistment choice models that include the value of the reenlistment bonus offered to each person, as well as the unemployment rate in their home state (i.e., the state of residence just prior to joining the military). These models provide estimates of the willingness to pay to avoid a higher unemployment rate, which we then compare to direct estimates of the earnings losses experienced by people who leave the military and (re)-enter the civilian labor market with different levels of the home-state unemployment rate.

Four key features of the military reenlistment setting and the available data are critical to our approach. First, the military recruits workers from all 50 states, but sets a national compensation policy. Thus, we can compare workers facing similar contract offers to stay in the military but subject to distinct, exogenous shocks to their home labor markets. Second, the bonuses used by the military to control occupation-specific reenlistment flows are set based on observable characteristics, allowing us to recover contract offers that were not consummated. Although there is an individual-specific component in the actual received

¹For example, see Oreopoulos et al. (2012) for effects on earnings and employers among Canadian college graduates, Davis et al. (2011) for effects among displaced US workers, and Altonji et al. (2013) for evidence on occupation choice.

²For example, Aguiar et al. (2013) find that 30% of lost market hours in a recession go to home production and 50% to leisure, but note that the home production margin is much more elastic than the leisure margin, as leisure fills far more time.

³As in Lucas (1987), Gruber (1997), Blundell et al. (2008).

⁴As in Shimer (2005), Alvarez and Shimer (2011).

bonuses (primarily reflecting promotion speed), we can use average bonuses paid to an occupation group or other system-level measures of occupation-specific demand as instrumental variables for the bonuses offered to different workers in different periods. Third, reenlistment entails a significant commitment—usually a minimum of 3 or 4 additional years—forcing a discrete decision over the value of employment. A fourth key feature regards the size of the military and the availability of detailed administrative data. The military is a very large national employer (over 1.1 million enlisted service- members at any time, with approximately 180,000 new enlistees each year and an equal number of people leaving the service). Our data permits us to observe the contracts, military pay, and characteristics of enlisted servicemembers for a large number of cohorts (1993-2009) as well as Social Security Administration (SSA) earnings records on post-service earnings for those who leave the military, and Veterans Administration records on college benefit (i.e. GI Bill) usage.

We begin our empirical analysis by presenting estimates of the effect of the local (i.e., home state) unemployment at the time of leaving the military on subsequent SSA earnings outcomes. We find that people who leave the military when their home-state unemployment rate is higher suffer large and persistent earnings losses over the next decade. In their first full year out of the military, young veterans who complete their term of service between 1993 and 2009 could expect to lose 3.6% of their annual earnings for each 1 percentage point increase in their home-state unemployment rate, with effects statistically distinguishable from zero for 7 years. These estimates are notably larger than the estimates reported by Oreopoulos et al. (2012) for new college graduates in Canada, potentially reflecting the greater cyclicity of wages for high-school educated workers than college educated workers.⁵ Part of the difference may also reflect the inclusion of the Great Recession in our study period: when we disaggregate our results by time period, we find that the effects of the home-state unemployment rate are amplified in recessions, and are particularly large in the 2007-2009 period. A concern with these estimates is that there may be selectivity biases in the characteristics of veterans who choose to leave the military when their home-state unemployment rate is higher or lower. We use the bonus offers to each individual's occupation in the year of their reenlistment decision to develop a control function for the degree of selection bias. The corrected estimates are quite similar to our baseline OLS estimates, suggesting that selection biases in our context are small.

We then turn to our main analysis of the reenlistment decision. We estimate choice

⁵See Hoynes et al. (2012) for evidence and citations to the literature on the income and demographic gradients in exposure to business cycles.

models that include the reenlistment bonus and the home-state unemployment rate, as well as a rich set of controls for the characteristics of the service member and his occupation.⁶ A longstanding concern in the military manpower literature⁷ is that the observed correlation between the reenlistment decision and the reenlistment bonus may be biased by a variety of endogeneity and measurement issues. To address this concern we propose an instrumental variable (IV) strategy, using the change in the number of newly recruited enlistees in the same occupation group as an instrument for the bonus offered to potential re-enlistees. This instrument will be valid under the assumption that the same underlying occupation-specific demand shock drives both the target level of new recruits in an occupation and the bonus level for re-enlistees, and that the military controls reenlistment exclusively through the bonus.

We find that reenlistment rates are highly responsive to both reenlistment bonuses and the home-state unemployment rate. Our implied elasticities of the reenlistment rate with respect to bonuses are in the range of 1 to 2.4, similar to the range in the existing literature. The ratio of the estimated effect of the home-state unemployment rate to the effect of the bonus—which roughly equates to the increase in the wage over the next contract required to generate the same reenlistment response as a one point increase in home-state unemployment rate—imply that young veterans are willing to trade no less than 1%, and as much as 2.5% of their military wage to avoid a 1-point increase in the home-state unemployment rate at exit. Since half of the bonus is paid on signing, discounting will increase the value of the bonus payments relative to this baseline.

We then compare the estimated willingness to pay to avoid a higher home-state unemployment rate with the estimated earnings losses experienced by military leavers who are exposed to different home-state labor markets. We calibrate our estimates for a range of discount factors, in order to account for the different timing of bonus payments and earnings losses. The cash-in-hand component of bonuses has a strong influence on the estimates when we apply the discount factors estimated in the previous literature to our calculations. In our preferred estimates, earnings losses move are not much larger than the willingness to pay, and we estimate that mitigating mechanisms like public and private transfers and leisure offset 13 to 35% of the realized earnings losses. We can strongly reject the case of full insurance (i.e. complete offset), but given the range of estimates for the key parameters

⁶These controls include year effects, three-digit fixed effects for primary occupation, controls for the size of the cohort who entered the same occupation, and interactions of age at eligibility for re-enlistment with AFQT, education and other demographic variables.

⁷See Hosek and Martorell (2009) for a detailed discussion of these issues.

we cannot rule out offsetting effects as small as -33% or as large as 80% of the earnings losses. We suspect that the offset effect we estimate for young veterans is likely to be an overestimate of the degree of offset for other similar workers, as US veterans have access to a range of targeted public programs, including generous education/job-training benefits and unemployment insurance.

1 Background on Enlisted Reenlistment, Sample and Estimation

1.1 Enlisted Reenlistment and Labor Market Entry

As with displaced workers and college graduates, young veterans of the US military have limited control over the timing of job search. This occurs due to fixed-length enlistment contract and limited opportunities to voluntarily leave the military before the expiration of the contract. Around 60% of enlisted servicemembers exit the service at the end of the first contract, totaling over 100,000 people per year. To put this in context, this equates to approximately 10% of the flow of new college graduates in the United States.

Although there exists variation in service across the branches, enlisted servicemembers in all branches sign similar enlistment contracts and face common institutional features in their reenlistment choice. Enlisted servicemembers enter the military having selected a branch of service, term of service and occupational specialization. As the end of the initial enlistment contract nears, servicemembers in all four branches enter a reenlistment window, during which they face a menu of reenlistment options over the term and duty assignment in their new contract. The military uses Special Reenlistment Bonuses (SRB) as the primary tool to control reenlistment and incentivize reenlistment into under-staffed occupations. The SRB pays the product of an occupation-specific multiplier, the servicemembers' base pay and the additional years of service commitment, up to a maximum of \$10,000. Given their importance to the supply of military manpower, the SRB program is the focus of a number of economic analyses of reenlistment incentives.⁸ Half of the bonus is paid within a month of reenlistment, and the other half is paid over the course of the new contract. We refer to effects of the payment schedule as the cash-in-hand component of the bonus.

Military labor demand and bonus setting reflect several considerations. The Department

⁸For example, Hosek and Martorell (2009), Asch et al. (2010) and Greenstone et al. (2014) analyze SRB bonuses.

of Defense sets enlistment and reenlistment targets, reflecting projected deployment needs, military doctrine and Congressionally-authorized funding levels. Bonuses are then set to clear the market, with updates occurring as often as several days apart. In addition to controlling occupation-specific reenlistment flows, bonuses may also serve to offset relative changes in working conditions between military and civilian life. Average bonuses increase during overseas conflicts necessitating deployments and increased mortality risk, as well as civilian labor market booms. In the time series, reported in Figure 6, only 40% of reenlistment contracts included a bonus during the drawdown of the mid-1990s, while this increased to 80% at the height of the Iraq and Afghanistan wars. As well, bonuses increased during the robust economy of the late 1990s, and then again during the wars. Cross-occupation variation in demand and bonuses arise due to working conditions, changes in military doctrine and because the military desires a relatively constant tenure pyramid within occupation. The military may also delay separation using “stop-loss” provisions, which extend the contract term through the end of a servicemember’s deployment.

Should a servicemember choose to exit the military (i.e. not reenlist), they are discharged at or near the end of their current contract. Early separation may be negotiable in the final months of the contract, depending on the needs of the military. Upon entering civilian life, veterans become eligible for federal GI Bill benefits, disability benefits and a limited set of means-tested benefits (health care), and a range of state-level programs, such as an unemployment insurance, preferential hiring and housing assistance.

1.2 Data and Sample Characteristics

In order to harness the largest possible group of servicemembers subject to standardized contracts, we study the population of enlisted, active-duty, male servicemembers from the four largest branches of the military, the Army, Navy, Marines and Air Force. We use military administrative data encompassing the universe of enlisted military personnel who served on active duty between 1988 and 2010, linked to their SSA earnings files through 2010 and college benefit usage through mid-2009. We drop female servicemembers from the sample, as our preliminary analysis revealed that the earnings responses of young female veterans differ from the patterns of young male veterans, temporarily rising in response to unemployment at exit, a result consistent with an important fraction of women acting as secondary earners in their household; unfortunately, we lack the sample size to analyze this

group separately.⁹ As well, drop servicemembers who are discharged before the completion of the first year of their contract, because most of these discharges occur involuntarily, e.g. failing to meet standards during basic training. Attrition before the final quarter of the contract is not predicted by state unemployment rate movements. Together, we have just under 1.1 million people in our analysis sample.

We report our primary earnings results for contracts ending between 1993 and 2009, but also report results from our main specification for the 1993-2004 cohorts. The 1993-2009 cohorts offer the largest sample for which we observe the full course of the initial contract for the entire finishing cohort (initial contracts for enlisted servicemembers are 6 years or shorter), allowing us to test for attrition and use cohort-size in our reenlistment analysis. For earnings outcomes, the 1993-2009 cohorts form an unbalanced panel, and as we will see, the impact of the Great Recession may confound the experience-profile of losses. Thus, when we examine dynamics, we drop the final five cohorts for this portion of the analysis. The 1993-2004 cohort sample allows us to observe six years of post-service outcomes for those who separate at the end of the first contract, including behavior at the end of the second contract for those who reenlist and college benefit usage for those who exit. In addition, the military was broadly successful in meeting enlistment and reenlistment goals for first-term recruits during the 1993-2004 period, as opposed to the 2005-2007 period, in which the strain of the Iraq and Afghanistan conflicts led to large-scale shortfalls in enlistment and reenlistment. When we turn to our reenlistment analysis (in subsequent sections), we feel the 1993-2004 period offers the strongest support for our assumption of a stable military labor demand function. In sum, the shorter sample period offers a number of analytical advantages in our subsequent analysis, though naturally, we extend the analysis through the Great Recession-era where appropriate.

The descriptive statistics in Table 1 show enlisted servicemembers to be a young, diverse group of workers. Most enlisted servicemembers reach the end of their first contract between ages 22 and 27, with the average at 24. A large majority of this population enters the military unmarried, with their highest education being a high school diploma or equivalent. Although less than 10% of enlisted servicemembers enroll in college before entering the military, enlisted men are of slightly above average intelligence, as can be seen from the mean and percentiles for AFQT (the percentiles of which are normed to the US population). Looking at the AFQT skew, we can see the effect of entry standards on the lower tail is considerably stronger

⁹ The analysis relies on (residualized) earnings cells from SSA, which must contain a minimum number of observations. Due to this data access constraints, we cannot analyze heterogenous effects by gender, race/ethnicity or other subgroups.

than selection effects at the top of the distribution. Thus, enlisted men are “less-skilled” than college graduates, but not than the general population. Enlisted men are also broadly representative of the racial/ethnic demographics of the US over this period; whites and blacks are slightly overrepresented, while Hispanics are slightly underrepresented. The restriction to the earlier sub-period has no important effects on the sample characteristics.

We employ the same administrative data sources and sample as we used in the earnings analysis to observe the reenlistment decisions that precede the earnings responses estimated in Section 2. Starting with our earnings data, we merge on a number of variables used in Asch et al. (2010) and Hosek and Martorell (2009) (and described below), two recent studies of reenlistment behavior among US servicemembers. Both of these studies focused on reenlistment during the Iraq and Afghanistan conflicts, but the underlying records allows us to extend the data back to the early 1990s. Administrative pay records decompose income into various categories, including the total value of bonus payments, and also information on in-service experience shown to affect reenlistment: receipt of deployment pay, hostile fire pay and stop-loss. To maintain connection to the earnings estimates above, we impose no further sample restrictions with the new data. In our analysis, missing data and the construction of the bonus variable will result in a different sample size in each specification; we discuss several robustness checks below, but have not found important patterns or biases resulting from the dropped observations.

1.3 Modeling Returns to Reenlistment and Civilian Labor Market Entry

The servicemember faces a binary decision to reenlist by the end of the first contract or exit the military. Should he exit at the end of their contract, he receives a payoff, which we summarize with the value function:

$$V_{iost}^{exit} = \alpha^{exit} + \beta UR_{st} + X_{iost}^{exit} + \epsilon_{iost}^{exit}, \quad (1)$$

where α^{exit} represents the average utility of exit, β is the influence of the state unemployment on future utility, including both earnings losses and mitigating mechanisms like leisure, and public and private transfers, and ϵ_{iost}^{exit} is a time-varying shock to the utility of exit, representing idiosyncratic elements, such as a new job offer or the start of a romantic relationship. X_{iost}^{exit} is the vector of controls required to maintain consistency with the earnings analysis, except o is now the 3-digit occupation. We take this linear representation as an approximation

of the total discounted value of exit.

If he reenlists, the servicemember receives a similar payoff:

$$V_{iost}^{reenlist} = \alpha^{reenlist} + \gamma \ln(1 + b_{iot}) + \mu_{iot} + X_{iost}^{reenlist} + \epsilon_{iost}^{reenlist}. \quad (2)$$

In this equation, $\alpha^{reenlist}$ represents the average utility of reenlistment, γ is the effect of a percent change in the pay operating through the bonus b_{iot} , μ_{iot} is the taste for military service, $X_{iost}^{reenlist}$ is our controls, and $\epsilon_{iost}^{reenlist}$ represents idiosyncratic elements of the returns to reenlistment, for example, a particularly desirable posting. We think of $\mu_{iot} = \mu_i + \mu_{ot}$, so that we can decompose the taste for service into the individual’s time-invariant taste for service (for example, the effect of having a parent who served), and a time-varying component that we refer to as working conditions.¹⁰

The response to the bonus in Equation 2, γ , will allow us to monetize the value of reenlistment. In the empirical application, we will use Special Reenlistment Bonuses (SRB) bonuses, which are the primary tool the military uses to control reenlistment, and the focus of many military manpower analyses of reenlistment incentives.¹¹ The SRB bonus formula pays the product of the SRB multiplier, the servicemembers’ base pay and the additional years of service commitment. Half of the bonus is paid within a month of reenlistment, and the other half is paid over the course of the new contract. We refer to effects of the payment schedule as the cash-in-hand component of the bonus. We convert the SRB multiplier to the implied percentage increase in the annual wage, meaning that we can roughly interpret a unit increase in the natural logarithm of $1 + b_{iot}$ as a one percent increase in earnings over the course of the next contract. This is “rough” for two main reasons: first, the bonus carries with it an increase in the option value to reenlist at higher reenlistment points, as there is serial correlation in bonus offers across contracts; and second, the cash-in-hand component of bonuses will lead to a higher value for the bonus (relative to an increase in pay over the course of the next contract) if discount rates exceed the growth rate of earnings by a large amount. Both of these factors will increase the value of the bonus, relative to the hypothetical increase in pay over the course of the next contract. In Section 4 we calibrate our willingness to pay elements to address these issues.

The servicemember chooses to reenlist if $V_{iost}^{exit} > V_{iost}^{reenlist}$. Combining these elements, the

¹⁰Although we did not enter a time-varying occupation-specific component in the returns to exit, they will be captured in μ_{ot} when we difference the two value functions.

¹¹See Hosek and Martorell (2009) and Asch et al. (2010) for a detailed discussion of SRB bonuses.

probability of individual i reenlisting is:

$$\begin{aligned}
\mathbb{P}[D_{iost} = 1] &= \mathbb{P}[V_{iost}^{exit} > V_{iost}^{reenlist}] \\
&= \mathbb{P}[(\alpha^{exit} + \beta UR_{st} + X_{iost}^{exit} + \epsilon_{iost}^{exit}) \\
&\quad > (\alpha^{reenlist} + \gamma \ln(1 + b_{iot}) + \mu_{iot} + X_{iost}^{reenlist} + \epsilon_{iost}^{reenlist})] \\
&= \mathbb{P}[\alpha + \beta UR_{st} - \gamma \ln(1 + b_{iot}) + \mu_{iot} + X_{iost} > \epsilon_{iost}^{reenlist} - \epsilon_{iost}^{exit}]. \quad (3)
\end{aligned}$$

Here, $\alpha \equiv \alpha^{exit} - \alpha^{reenlist}$ and $X_{iost} \equiv X_{iost}^{exit} - X_{iost}^{reenlist}$. In Equation 3, a point of unemployment increases the probability of reenlistment by β , while a 1% increase in the pay through the bonus increases the probability of reenlistment by γ . Dividing γ by the average reenlistment rate (around 40% in our sample) will yield a reenlistment elasticity, which we can compare to previous estimates in the literature. We want to know how large of a bonus would be required to offset (i.e. hold constant the reenlistment rate in the presence of) a one point change in unemployment, $\gamma \Delta \ln(1 + b_{iot}) = \beta * 1$, so $\Delta \ln(1 + b_{iot}) = \beta / \gamma$. A decrease in unemployment at exit roughly corresponds to an increase in earnings of β / γ percentage over the course of the next contract, (where the approximation was discussed in the previous paragraph).¹² Multiplying β / γ by the monetary value of a bonus point returns our estimate of the willingness-to-pay for unemployment at exit.

The final component of the calculation we require is the monetary value of the bonus, i.e. the change in earnings associated with a unit increase in $\ln(1 + b_{iot})$. If we had access to the earnings data, we could estimate the response of earnings to bonus offers, $\sum_{k \geq 0} \rho^k \delta_k^b$ in the framework from Section 2; state unemployment would serve as the excluded variable in the selection correction. This would allow us to add together the mechanical increase in earnings from the bonus formula, increase in earnings coming from the serial correlation in bonus offers across contracts, and any other financial incentives correlated with the bonus. Instead, we use the bonus formula to calibrate the monetary value of the bonus. The details of the calibration can be found in Section 4; for the purposes of illustration, a reasonable guess at the average of the total value of bonus payments resulting from a 1% increase in the bonus is \$1500, as this is approximately 1% of the earnings for the next 4 years for those who reenlist. If $\beta = 1$ and $\gamma = 0.7$, plausible values from our estimation, and $\rho = 1$, then $WTP = \frac{1}{0.7} \$1500 = \2100 .

¹²We are setting aside serial correlation in unemployment at the end of first and second contracts, since virtually all second contracts are 3 years or longer in length, a horizon at which the serial correlation in unemployment is small. Including this in the analysis would increase the willingness to pay for lower unemployment at exit, and reduce the value of the implied mitigating mechanisms.

We define mitigating mechanisms, M , as the difference between the earnings losses and WTP :

$$M = \Delta y(UR_{st}) - WTP = \Delta y(UR_{st}) - \frac{\beta}{\gamma} \Delta y(\ln(1 + b_{iot})) \quad (4)$$

where $\Delta y(UR_{st}) = \sum_{k \geq 0} \rho^k \delta_k y_k$, the discounted sum of earnings losses estimated above, and $\Delta y(\ln(1 + b_{iot}))$ is the discounted value of bonus payments. Although we refer to M as mitigating, nothing constrains $M > 0$. For example, if young veterans engage in costly migration that offsets a large fraction of earnings losses, or if unemployment represents a psychic cost in addition to lost earnings, it is possible that young veterans may be willing to give up more than their earnings losses to avoid entering the civilian labor market. In the back-of-the-envelope calculation using $WTP = \$2100$ and $\Delta y(UR_{st}) = \$4200$ (approximately the earnings loss in the 1993-2004 sample), $M = \$2100$, so mitigating mechanisms account for 50% of earnings losses. We calibrate more precise estimates below, and most refinements will reduce the share of earnings offset by mitigating mechanisms, $m = 1 - \frac{WTP}{\Delta y(UR_{st})}$.

So far, we have set aside the issue of the timing of events, discounting and the cash-in-hand component of bonuses. Warner and Pleeter (2001) is a classic study of the discount rates of servicemembers. This paper finds that 90% of enlisted servicemembers exhibit discount rates above 18% in their choice of an immediate payment versus a large annuity. Other work on enlisted servicemembers find high discount rates, though most estimate somewhat lower values, such as 11-12% in Daula and Moffitt (1995).¹³ The timing of events interacts with the discount rate in two ways to reduce the estimate of M . First, ρ appears in the Δy functions, influencing the mechanical calculation of the annualized present discounted value of the difference in the bonus payment schedule versus the profile of earnings losses. We address this directly in Section 4 by calibrating our estimates for different values of ρ . Second, the reenlistment decision occurs before the end of the contract, in extreme cases, 24 months before the end of the contract. This is a more difficult issue, because of the endogenous

¹³The estimates in Warner and Pleeter (2001) are remarkable: enlisted men with 7 years of service (the least tenured servicemembers included in the program) exhibit mean discount rates of 41% and 71% in the primary specifications. Some have suggested this drawdown-era, cost-cutting program could not credibly commit to the annuity offer, or at least, that the presence of many “involuntary separations” in this era influenced the natural experiment. One such paper, Warner et al. (2011), replicated the previous analysis on a sample of enlisted men with 20 years of expected service choosing between two annuity streams, one of which had a lump-sum component, and found median discount rates of 11% for enlisted men. Based on the patterns of discount rate heterogeneity in these previous papers, our sample would be expected to display higher discount rates than the selected group of enlisted men who reach 20 years of service. In the calibration in Section 4, we will use a discount factor of $\rho = 0.9$ in our preferred estimates, corresponding to a conservative discount factor of 11%.

choice of reenlistment date, and the absence of quality data on the entire sequence of bonus offers. In the primary analysis, we set aside the timing of the decision, and return to this issues in Section 4.3. These issues are quantitatively important. To finish the back-of-the-envelope calculations, if bonus payments arrive on average 2 years before earnings losses, and we use a discount rate of 18%, mitigating mechanisms account for only 22% of earnings losses.

2 Effect of Economic Conditions on Earnings of Young Veterans

Our reduced-form model in this section summarizes the effects of home-state unemployment on veterans who separate at the end of their first contract using a similar regression specification to other research on labor market transitions. In our main specification we estimate regressions for individual i , enlisted for $j(i)$ years on their initial contract, from home state s , with initial enlistment contract ending in year t , observed k years following the end of their initial contract:

$$y_{istk} = \alpha_{sk} + \beta_{tk} + \gamma_{j(i)tk} + \delta_k UR_{st} + X_{istk} + \epsilon_{istk}. \quad (5)$$

Our main outcomes of interest are the percentage response of civilian and total earnings to unemployment at the end of the first contract, UR_{st} ; plotting the δ_k coefficients provides the impulse response to a one-percentage-point change in the unemployment rate at the end of the first contract. In a slight abuse of notation, we use the unemployment rate in the three months following discharge eligibility, rather than the average for the year t implied by the subscript; later in the paper we refer to the average unemployment rate in year t as \overline{UR}_{st} . X_{istk} includes quadratic interactions of age with a rich set of demographic, health and education controls, including AFQT, education at entry, marital status, height, weight, and race/ethnicity. The model is estimated in two stages. First, in an individual-level analysis at SSA, we partial out the influence of individual-level background variables on earnings by computing the residual from a regression of earnings on $\gamma_{j(i)tk}$ and X_{istk} . We then compute average residuals within cells defined by state of enlistment, year of end of contract and year of earnings, (dropping cells with 5 or fewer servicemembers; this is less than 1% of observations). In a second round of analysis, we divide the residual by the average earnings at horizon k (the difference between year of end of initial contract and year of earnings) and

regress this normalized residual on the remaining coefficients. We adopted this strategy to estimate proportional effects without taking the logarithm of zero, (although our subsequent analysis revealed no effect on the zero-earnings margin). Standard errors are clustered at the state level.

Identification in this specification arises from the unexpected evolution of UR_{st} over the course of the initial contract. Lengthy initial enlistment contracts do most of the work, as serial correlation in state unemployment rates is small at the initial enlistment contract horizon. State fixed effects absorb (time-invariant) regional differences across the US, while the year fixed effect absorbs national business cycles, two important predictors of future unemployment. To address any remaining anticipation of the unemployment rate at the time of exit, we include controls for the state of the economy at the time of enlistment: state unemployment at enlistment, fixed effects for the interaction of year of entry and year of eligibility, and the interaction of year of eligibility for discharge and year of earnings. Once state and year effects are included, additional controls do not impact the point estimates, giving us confidence that the response of earnings to UR_{st} represents the causal effect of business cycle conditions at the end of the contract, free of selection effects arising from anticipation.

Applying the above specification to the population of young veterans assumes selection-on-observables in reenlistment, which may be plausible, considering our rich set of controls. We go one step further and make use of variation in reenlistment rates at the 1-digit occupation level to account directly for selection out of the military. We define o to be the first digit of the first observed primary occupation code (this is the military MOS code; we call this the 1-digit level, although it is a letter in the Marines) and use movements in occupation-level reenlistment to construct a control function approach to Heckman’s two-step correction. In the first stage, we estimate a linear probability model of reenlistment, adding occupation-by-year effects to the model. We then include the predicted probability of reenlistment from the first stage in the main earnings equation, using a semi-parametric transformation suggested by Newey (2009):

$$\mathbb{E}[y_{iostk} | D_{iost} = 1] = \psi_{ok} + \alpha_{sk} + \beta_{tk} + \gamma_{j(i)tk} + \delta_k UR_{st} + X_{istk} + \Lambda_k(\widehat{p}_{iost}) \quad (6)$$

$$\widehat{p}_{iost} = \omega_{ot} + \lambda_s + \mu_{j(i)t} + \nu UR_{st} + X_{ist}. \quad (7)$$

Here, $\widehat{p}_{iost} \equiv \mathbb{P}[D_{iost} = 1]$ is the predicted probability of exit, $\Lambda_k(\widehat{p}_{iost}) \equiv \sum_{l=1}^4 \lambda_l^k (2\Phi(\widehat{p}_{iost}) -$

$1)^l$ is the semi-parametric transformation and $\Phi(\bullet)$ is the cumulative distribution function.¹⁴ Due to constraints on our data access, we estimate the X_{istk} contains the same variables as the above specifications. The selection-correction is identified by the exclusion of the ω_{ot} cross-effect between military occupation and the period of discharge from the earnings equation (2), where we include only the independent effects of occupation o and year of eligibility t . The occupation-by-year effects have strong predictive power; we reject the null of $\omega_{1t} = \dots = \omega_{Ot} = 0$ at $p < 0.001$. These ot effects reflect the average reenlistment rate in occupation o at time t , conditional on changes in the composition of servicemembers with contracts ending in the period. Effectively, this requires changes in state-level economic conditions and occupation-level demand to be uncorrelated, which would be the case if servicemembers were randomly distributed across occupations with respect to their state of enlistment. Under this assumption, the fraction who separate from occupation o in time t tells us the degree to which the distribution of exiters has been selected.

In our primary earnings analysis, we estimate lower bounds on the earnings effect for the group of young veterans who do not use their GI Bill benefits. Besides continuation in the current job, further education or job training can provide a secondary insurance option. As such, our estimates using those who forgo this option represent lower bounds on the true effect (i.e. corrected for the endogenous choice to forgo their benefits). Two pieces of additional evidence, drawn from reenlistment analysis and the empirical evidence on earnings, suggest we do not lose much by doing this. First, selection on observables seems to do a reasonable job with the reenlistment margin, while the college margin appears less elastic (with respect to unemployment) than the reenlistment margin. The low elasticity of benefit usage is particularly pronounced for longer spells, the type of usage we would expect to result in large effects on the earnings profile. Second, we see virtually no difference between the effects on the earnings of those who do and do not use their college benefits. Ideally, we would like to estimate a heterogeneous return to further education, in which the agent forgoes some earnings to acquire additional human capital, $r_i(e)y_{e+k}^{exit}$, along the lines of the model of Willis and Rosen (1979). To estimate such a model would require additional sources of excluded variation affecting the schooling decision. In practice, we estimate lower bounds on the effect among those who do not use their GI Bill benefits, and leave these questions for future work.

¹⁴In constructing $\widehat{p_{iost}}$, we set extreme values to 1 and 99%; this is less than 0.0003% of observations.

2.1 Earnings Results

Figure 1 graphically displays the earnings losses that result from higher unemployment for young veterans with initial contracts ending in 1993-2009, where we use the selection correction for reenlistment, and drop those who use their college benefit. These are the δ_k regression coefficients that reflect the effect of end-of-contract unemployment UR_{st} on earnings at horizon k after separation. Total earnings drop nearly 4% in the year following separation, and recover slowly. Table 2 reports average earnings, the δ_k regression coefficients and their standard errors and the implied value of horizon k losses in 2010 dollars, where we use the CPI to deflate nominal income. Negative effects on earnings remain statistically significant through the 7th year, and point estimates return to zero between the 8th and 10th year. Summing the stream of earnings losses, young veterans lose \$4875 (over the next 10 years) for each point increase in home-state unemployment; with a discount factor of 0.9, the present discounted value of the earnings losses is \$3450. These results are close to double the effect of unemployment among Canadian college graduates estimated in Oreopoulos et al. (2012).

We evaluate several components of the model in Figure 2 and Table 3, finding that selection into reenlistment, and military income (for example, through the reserves) play small roles in the effects. In Figure 2, we plot effects without the selection correction for reenlistment. Selection plays a very small role in the outcome distribution: as expected, correcting for selection increases the size of the effect, but the increase is economically negligible. This suggests that there is either little heterogeneity in individual-specific shocks to post-service earnings options once we condition on our control variables, or that servicemembers have little information about these shocks at the time of their decisions. By considering the effect on civilian sources of earnings, we can address the role of shorter extensions and residual military income earned after discharge. Figure 2 displays the effect on the civilian component of earnings. Although there are large effects on civilian income in the year before and year of discharge, they come on a small base of civilian income (see Table 3 for average civilian earnings and year 0 effects). After year 1, effects on civilian income closely track the effects on total income, implying that there is no component of the residual military income that insures veterans against shocks after year 1. We take this as evidence that our definition of reenlistment has left little room for further insurance from maintaining a relationship with the military, for example, through shorter contract extensions, re-joining the military after separation, or service in the Reserves.

What explains the magnitude of these losses, nearly twice the size of those estimated for college graduates? Period effects appear to be one answer. Davis et al. (2011) finds larger

earnings losses for workers displaced during national recessions, and most previous studies of entry effects used pre-Great Recession samples. In Figure 3, we disaggregate the analysis by period, revealing that losses are concentrated in years of high national unemployment. These results are striking: effects are effectively zero in the 1995-2000 and 2004-2006 expansions, while the periods of high national unemployment, from 1992-1994, 2001-2003 and 2007-2009, show large and persistent effects. Even though we include fixed effects for year of eligibility, and hence, absorb the first-order effect of national economic conditions at separation, these estimates reveal that young veterans are (much) more vulnerable to state labor market shocks during times of national recession. It is not clear what causes this linkage, and we regard this as a worthy question for future research. The larger losses during times of national recession suggest a role for migration in clearing the labor market, as in the story of Blanchard and Katz (1992); however, recent research has cast doubt on the aggregate analysis in Blanchard and Katz.¹⁵ Clearly, the 2007-2009 years play a large part in the differences between our estimates and pre-Great Recession estimates in the literature. The earnings losses in the 2007-2009 years could be explained by longer unemployment spells among young veterans during the Great Recession, documented in survey data and several research reports.¹⁶ Although period effects can explain a portion of the larger losses we find, even when we exclude the Great Recession when we look in the pre-Great Recession era, we find losses as large or larger than those estimated for college graduates.

The effects in the earlier 1993-2004 cohort sample appear to be more comparable to other estimates in the literature; as well, they allow us to investigate persistence of the effects unconfounded with Great Recession period effects. As we observe earnings through 2010, this panel is balanced through year 6, after which one cohort drops out for each year we advance. Figure 4 graphically displays the earnings losses that result from higher unemployment for young veterans with initial contracts ending in 1993-2004. Total earnings fall by over 2% in the year in which the contract ends, and do not return to their expected path until 7 years later, at which point the two samples completely overlap. Total losses over 10 years in the earlier sample are \$4394, with a present discounted value of \$3101. The time pattern of losses is similar, but it is difficult to judge differences in persistence, given the issues with panel balance and period effects; later, we will conduct our analysis of

¹⁵In a recent working paper, Yagan (2013) finds a very small role for migration in clearing labor market shocks during the 2006-2011 period in the IRS microdata. Young veterans display much higher migration rates than other workers, so it is possible they do not follow the patterns of the general population. More work on linkages between national and local economic shocks is needed.

¹⁶For example, see Faberman and Foster (2013) and Humensky et al. (2013).

reenlistment in the early sample.

One concern with interpreting the measured effect of UR_{st} on losses k years later is that state unemployment rates are persistent. Thus, the measured effect of UR_{st} on earnings k years after leaving the military confounds any “initial condition” effect of UR_{st} on outcomes k years later with the direct effect of $\overline{UR_{st+k}}$. To address this concern, Table 4 presents a model in which we add the contemporaneous state unemployment rate $\overline{UR_{st+k}}$ as an additional control for earnings outcomes k years after leaving the military (note that this is not collinear in year 0 with the unemployment rate we use in throughout the analysis, as UR_{st} is the unemployment rate in the quarter following the end of the first contract, while $\overline{UR_{st+0}}$ is the average unemployment rate in year 0). The results isolating the effect of initial conditions in Figure 4 confirm that a significant portion of earnings losses arise from the persistent effect of initial conditions, and cannot be explained by serial correlation in economic conditions. The remaining analysis will focus on the combined effect of initial conditions and serial correlation in economic conditions, as these are the relevant earnings losses for the reenlistment decision.

We would like to be able to say more about the labor market experiences of young veterans, however, our microdata does not link to outcomes beyond earnings and college benefit usage. We find no evidence of an effect on zero earnings, likely reflecting the high labor force attachment of these workers. Examining the time pattern of losses in the analysis of initial conditions can offer some suggestive evidence. First, results in year 0 appear entirely driven by the unemployment rate in the quarter following the end of initial contract, with the average unemployment rate in year 0 having no role once the more precisely-assigned measure is included. The large year 0 losses due to this narrowly-defined unemployment rate suggest a longer search process for those who exit during low points in the business cycle. Second, the effect of initial unemployment conditions is not statistically significant in year 1. In other words, despite whatever unemployment spells young veterans experience at exit, it appears that they find jobs within a year of exit that allow them to achieve the expected earnings trajectory (conditional on subsequent unemployment rates); however, these jobs do not appear to be of the same quality as the jobs of young veterans who separate during good times. We can see this in the later years of our analysis, as the effect of initial conditions returns in year 2 and comes to dominate the estimated effect by year 5. This is expected: we have already stated that state unemployment is only weakly correlated at a 4 year horizon. Taken together, these results imply that a portion of the initial shock is absorbed through human or search capital, for example, taking a job requiring fewer skills

or with lower expected wage growth.¹⁷

While we focus on the effect of unemployment at t , it is interesting to compare these results to the effect of subsequent unemployment rate. In Table 4, results for the concurrent unemployment rate reveal that young veterans slowly build up protection from unemployment rate movements, with losses falling from 2-2.7% in the first 6 years to below 2% after that. It is unclear whether these diminishing effects reflect migration away from the home state, or could be attributed to some other force, such as a reduced impact of business cycle conditions on older workers. In general, we would like to know more about the migratory behavior of young veterans and responses to these shocks. While it appears the initial unemployment rate has particular importance, subsequent unemployment rates movements also have significant effects on the earnings of young veterans.

2.2 Other Responses, External Validity and Relation to Literature

These losses are large, exceeding the losses to entry among college graduates (as in Oreopoulos et al. (2012)) and displaced workers (as in Davis et al. (2011)), and would be larger if we used a more precise measure the relevant unemployment rate. There are at least 3 reasons to believe UR_{st} is mis-measured.¹⁸ First, servicemembers have the opportunity to seek early discharge and short extensions that would not meet our definition of reenlistment. We can observe the timing of separation in the administrative data, and when we use a Wald estimator to correct for the endogenous timing of separation from the military, the estimates increase in magnitude by 17-18%. Second, veterans are a highly mobile population. We lack data on the post-service migration of young veterans, however, we can use the Current Population Survey, ACS and NLSY to examine aggregate patterns in veterans migration. When we correct for the resulting mis-measurement of the unemployment rate, the earnings effects above are magnified by as much as 20%. Third, we use BLS state unemployment rates as the relevant measure of the business cycle. This introduces measurement error relative to the use of a finer measure of unemployment; for example, were we to able to use (well-measured) unemployment rates for young workers at the county level, we would expect to find larger responses. Together, these corrections will increase the estimated effects above from 3.6% of earnings lost to a point increase in unemployment up to close to 5%. These are large losses

¹⁷Though we lack the data to observe employers, this is broadly consistent with the jobs-ladder model of Oreopoulos et al. (2012), in which entrants during times of weak labor demand begin at lower-quality firms and gradually catchup as they transition to higher-quality firms; however, the estimates for Canadian college grads do not show the year 1 catchup.

¹⁸See the Online Appendix for details on these calculations.

for this literature.

Broadly, there are two hypotheses for the larger losses among young veterans. First, recent research finds evidence of socioeconomic and age gradient in cyclical earnings losses, meaning that it is possible that young veterans differ from the workers in these previous studies in ways that make them more vulnerable to business cycle shocks. Second, it is possible young veterans may have better access to mitigating mechanisms, meaning that they are more able to absorb economic shocks through reduced earnings.

We believe the most likely explanation for the larger effects among young US veterans is an age and socioeconomic gradient in the effect of economic fluctuations. Hoynes et al. (2012) documents heterogeneous effects of state-level business cycle fluctuations on the employment and unemployment of workers in the CPS. Although we are looking at earnings, if we consider the ratio of effects for young vs. old workers, high school vs. college educated or men vs. women estimated in Hoynes et al. (2012), we can easily scale the estimates for college graduates and displaced workers to our estimates here.¹⁹ Once they transition to civilian employment, military servicemembers may experience larger losses due to their industries and occupations of employment. This point would be consistent with the findings of Hoynes et al. (2012), which finds that that most of the cross-group variation can be explained by industry and occupation. Along these lines, Black et al. (2008) shows industry of employment for recent vets as compared to similar workers. While veterans are overrepresented in industries that engage in cyclical hiring (e.g. construction, retail trade, and public administration, especially police and firefighters), the comparable groups of civilian workers (i.e. young men without college degrees) have similar employment patterns. An open question is the degree to which workers in these industries are paid compensating differentials for the risk they bear.²⁰

An important alternative explanation is one based on the presence of mitigating mechanisms, particularly those targeted at young veterans. One of the most important differences between young veterans and similar workers is the specific public programs available to veterans. Young veterans have access to one of the most generous education/job-training benefits in the US economy through the G.I. Bill, which include stipends for living expenses. During this era, veterans have been the target of: federal tax-credits to encourage their hiring, especially if they have a service connected disability (Heaton, 2012); preferential hiring

¹⁹It is not clear these patterns hold in all settings. See Brunner and Kuhn (2009) for evidence of larger losses from labor market entry conditions among white-collar workers than blue-collar workers in Austria.

²⁰Similarly, our findings could be construed as a piece of evidence that explains why the military pays a premium over civilian employment.

by local, state and the federal government; matching grants to schools (the Yellow Ribbon program); as well as numerous community efforts to encourage their hiring. Veterans who fall on hard times can access the means-tested health care services available through the Veterans Administration, as well as a host of services targeted at homeless veterans. Disabled veterans have access to Vocational Rehabilitation and Employment, a program designed to find them work; as well, some observers have suggested veterans' disability applications rise in response to local unemployment conditions. Finally, veterans in good standing can rejoin the military. These options available specifically to veterans amount to additional mitigating mechanisms, and economic theory suggests that greater access to mitigating mechanisms would lead agents to bear larger earnings losses; this is related to moral hazard.²¹

We will not be able to measure responses or answer the external validity question without a more comprehensive dataset; however, we can shed some light on this question by directly measuring the willingness-to-pay for these losses. To the degree larger losses among young veterans reflect greater access to mitigating mechanisms, we should expect reenlistment decisions to be less influenced by the unemployment rate. Based on our discussion here, it should be clear that the estimation of willingness-to-pay for these results is important not just for understanding the costs of business cycles, but also for interpreting the earnings results for the population of young veterans.

3 Reenlistment Responses

In the previous section, we estimated the effect of unemployment movements on earnings of young veterans. In this section, we use reenlistment behavior to estimate willingness-to-pay for exit for different levels of the home-state unemployment rate.

The main analysis of reenlistment responses estimates Equation 2 using linear probability models; we check that our results are robust to using a logit framework. The linear model facilitates our instrumental variables estimates of γ , and requires we assume the unobserved taste for reenlistment relative to exit $\epsilon_t^{reenlist} - \epsilon_t^{exit} \sim U[0, 1]$. In the model, the response to state unemployment β reflects the avoidance of a worsening outside labor market and the response to bonus pay γ allows us to price this avoidance behavior. Of the two, there is considerably more uncertainty in the estimation of γ . In most of the analysis, we average

²¹Taking this one step further, it is possible the emerging evidence on the age and socioeconomic gradient reflects greater access to mitigating mechanisms among the affected groups of workers (especially redistributive social programs), so that our results are consistent with the patterns in the broader economy, which also reflect greater access to mitigating mechanisms.

the bonus offers within ot cohorts to remove the individual-level variation in the bonus, and analyze variation in b_{ot} .

There are three main challenges in the estimation of the bonus effect. First, the bonus is endogenously set in response to changes in μ_{ot} , which includes time-varying components of military working conditions. In order to estimate the response to exogenous monetary incentives to reenlist, we need to isolate movements in the bonus driven by labor demand. Second, the military may use other reenlistment levers in conjunction with the measured bonus, particularly when the bonus falls to zero. We can think of these other levers as military labor demand directly affecting μ_{ot} , which is unobserved (or only partially observed) in the data. Finally, the timing of bonus pay and the reenlistment decision raises concerns we address after the main analysis.

To estimate the response of reenlistment to bonuses, we first pursue the OLS strategy used in recent military manpower studies (such as Hosek and Martorell (2009) and Asch et al. (2010)). Research using wartime samples has primarily focused on the first challenge in the estimation, the effect of unobserved working conditions on the bonus. These researchers have naturally been concerned that the OLS response to increases in the bonus may understate the true effect of pay incentives, due to the negative correlation in b_{ot} and μ_{ot} . As our primary sample, 1993-2004, covers a period of relative peace (the Iraq War did not become a protracted conflict until late 2004 and 2005) this is less of a concern. In one attempt to address this concern, we add control variables for the most likely confounders- deployment history, receipt of hostile fire pay and measures of advancement in rank.²² These controls capture the broadest changes in military working conditions experienced by the service member, but do not address expectations of future working conditions (the relevant μ_{ot}), and further, may be endogenous, if, e.g. deployment is not random conditional on our controls for occupation and year of service.

To investigate further the potential endogeneity of bonuses, we estimate what we consider to be an upper bound on the elasticity using an IV strategy based on occupation-specific shocks to military labor demand. As our instrument, we use the flow of new recruits to the 2-digit occupation in the year t , $L_{O(o)t}$, conditional on the flow (at the 1-, 2- and 3-digit level) at the time of entry, \mathbf{L}_{ot-j} .²³ We explored instrumenting at the 1- and 3-digit level,

²²We have also explored including a control for stop-loss (involuntary extension of the term of service), with little effect on the estimated coefficients, in large part due to the fact that fewer than 2% of individuals experience stop-loss, even during the peak war years. In the end, we chose not include stop-loss controls, due to the high fraction of missing observations in the early years of our sample.

²³To the best of our knowledge, this is the first published paper to employ this strategy. We know of at least one internal analysis at RAND that explored a similar IV strategy, but was never published or formally

and although point estimates were similar, the first stage was not strong enough to report estimates. Our estimates will capture the causal impact of bonus payments on reenlistment under the assumption that the same underlying occupation-specific demand shock drives both the target level of new recruits in an occupation and the bonus level for re-enlistees, and that the bonus represents the military's sole control variable in reenlistment. In defense of the identifying assumption, we highlight three reasons that new recruits should offer a clean measure of the underlying demand shock. First, most newly enlisted men enter the military between ages 18 and 22, with only a high-school diploma and little occupation-specific civilian labor market training. Thus, the minimal skill differentiation and attachment to occupations purges the reenlistment estimates of labor supply effects arising from the value of specific training in the civilian labor market. Second, the military controls the flow of new recruits through the availability of seats in entering occupations (and the schedule of occupation-specific enlistment training), in addition to occupation-specific enlistment bonuses. As a result, the military exerts direct control over enlistment flows in a way that is not possible with reenlistment. Recall that we focus on 1993-2004 in order to exclude the enlistment and reenlistment shortfalls in 2005-2007, giving us confidence that the military chooses the size of the entering class. Third, as an empirical matter, the size of the entering class does not predict the speed of promotion. We interpret this to mean that there is a low degree of substitutability between new recruits and first-term reenlistment.

The primary threat to the validity of this instrumental variables strategy arises due to the second challenge in the estimation of the bonus effect, namely, that the military may use other control variables in conjunction with the bonus. Alternative control variables in the military's reenlistment problem (in our model, the extent to which the military can directly affect working conditions μ_{ot} , for example, the degree to which physical performance standards are enforced) will mean that using the measures of the military's labor demand as an instrument violates the exclusion restriction. This seems to be the case. The military has historically used changes to reenlistment windows (the window before the end of the first contract in which one can sign a new contract), review boards (screening of personnel files), bonus caps (setting a maximum contract length eligible for bonus) and other measures to meet its reenlistment goals. These measures will very likely be positively correlated with the bonus, as they serve essentially the same purpose in controlling reenlistment. The problem will be particularly bad in periods of declining demand (as in the mid-1990's post-Cold War military drawdown), when bonuses are in less frequent use, and the military

cleared for public release. Results in that study were similar to the results reported here.

substitutes towards other reenlistment controls. Note that while this source of endogeneity will upward-bias the OLS estimates, the problem is worse in IV.²⁴ In other words, the IV solves the endogeneity of the bonus due to working conditions by exacerbating another source of (upward) bias into the calculation. If we are willing to assume the use of other reenlistment levers is positively correlated with the bonus, then the IV overstates the pay elasticity in this framework.

We estimate two versions of our first-stage model in order to explore the robustness of our identifying assumptions and results. The basic first-stage equation is:

$$b_{iost} = \psi_o + \sigma_t + \phi L_{O(o)t} \chi_{B(o)} + \Pi \mathbf{L}_{ot-j(i)} \chi_{B(o)} + X_{iost} + v_{iost}, \quad (8)$$

where ψ_o indicates 3-digit occupation, σ_t indicates year of eligibility, $L_{O(o)t}$ is the labor demand shock, the size of the entering class at the 2-digit $O(o)$ occupation level, $\chi_{B(o)}$ is an indicator for branch of service, $\mathbf{L}_{ot-j(i)}$ is a vector of controls for the size of the own 1- 2- and 3-digit cohorts, and X_{iost} contains the full set of controls required to maintain consistency with the previous analysis. The bonus is assigned at the 3-digit level, making this the natural baseline; however, as we assign occupation at entry (before many servicemembers have specialized in their 3-digit occupation) and occupation-switching primarily occurs within the 2-digit level, the 2-digit instrument may be a better reflection of the offers available. Given the threats to the exclusion restriction we discussed in the preceding paragraphs, our preferred IV specification of this model replaces σ_t with a dummy for the cross of eligible year and branch of service, $\xi_{B(o)t}$. This specification absorbs the effects of cross-service movements in demand, capturing some of the branch-level reenlistment policy changes, such as changes to the reenlistment window or the tightening of reenlistment standards.²⁵

To estimate the effects of home-state unemployment on reenlistment, β , we employ the same identification strategy as in the earnings analysis: we control for state fixed effects and state unemployment at entry (along with the other control variables in the reenlistment analysis), and use state unemployment in the first 3 months after eligibility for discharge as the treatment variable. The use of this measure of the unemployment rate has the advantage of mirroring the earnings specifications above, while possibly sacrificing some precision due to errors in expectations regarding the future value of unemployment at the time of the

²⁴The endogeneity is worse in IV, because OLS suppresses the endogeneity during declining military labor demand by allowing the bonus to be zero, while the reduced-form relationship between labor demand and reenlistment will pick up the monetary value of reenlistment incentives regardless of the value of the bonus.

²⁵We do not have the power to add cross-effects in one-digit occupation and eligible year and still pass the first-stage F-test.

decision. Most reenlistment occurs in the 12 months leading up to the end of the first contract, and it seems likely that those who reenlist earliest are not marginal reenlisters; however, some of the information in this measure of unemployment may not be available at the time of the decision. As unemployment is a lagging indicator of the business cycle, and the loss in precision should introduce a downward bias in our estimates, we suspect this compromise leads to mild attenuation in our estimates of the response to unemployment. We explore the role of the timing of the decision by considering only those who make the decision in the last year of their initial contract, and provide more details on the timing of the decision in Section 4.3.

Finally, we report the estimated value of unemployment implied by the ratio of reenlistment responses to unemployment and bonus pay, β/γ . Based on the scaling of the bonus, we can interpret this as a rough estimate of the percentage of the wage on the next contract (on average, 4 years long) that servicemembers are willing to forgo to avoid a one-point higher unemployment rate at exit. Standard errors using the Delta-method approximation allow us to test for the presence of full insurance, and verify we have the power not just to estimate the responses, but also their ratio.

3.1 Reenlistment: OLS Estimates

Table 5 reports OLS estimates of reenlistment responses and the implied willingness-to-pay for unemployment at exit. These estimates include our basic set of control variables from the earnings analysis, with in-service controls (detailed above) included as noted. In the first row of Column 1, we find a coefficient of 0.37 on the log of the bonus, meaning a 10% increase in pay is associated with just under a 4% increase in the probability of reenlistment. Off a base of 40% reenlistment, this implies a reenlistment elasticity just below 1. This estimate is at the lower range of estimates found in the literature; for example, Asch et al. (2010) find a value of 1.75 in the 2002-2006 era, and Daula and Moffitt (1995) finds a value of 2.2 in 1974-1987 (this is before our sample period, but in the volunteer-era). Moving across the columns, our estimate is stable in these OLS specifications. Dropping those who enlist for an initial 3 year term allows us to check for anticipation effects, in either the bonus or unemployment rate, while dropping those who began service in a small occupation-year cohort provides a check on the potential bias introduced in the construction of the bonus variable. Neither has an effect on the OLS estimates.

In the second column, we add in-service controls (determined after enlistment), and report results for base pay, which we believe offers less compelling evidence than bonus pay.

Postenlistment outcomes such as deployment and hostile fire may be endogenous if they are anticipated by enlistees or are the result of their decisions; on the other hand, military service does not offer the same flexibility of employment conditions that can be negotiated in civilian employment. The bonus coefficient, γ , seems unaffected by these controls, suggesting the endogeneity due to time-varying working conditions may not be severe for these cohorts. As this is the primary threat to a (downward) bias in our OLS estimates of γ , this gives us some confidence in these estimates.

In the military manpower literature, the response of reenlistment to base pay is sometimes distinguished (the pay elasticity) from the response to bonus pay (the bonus elasticity). The value of the pay elasticity implied by our estimates, 1.25, is within the range of previous estimates. We have set aside base pay as a useful source of identification, because, once we include year effects most of the variation arises from promotions. The presence of year fixed effects removes most variation over time, as military base pay is set by Congress, and reforms are generally implemented with lags. The remaining cross-sectional variation will be due to individual-specific variation in promotion speed, which is likely influenced by unobservable factors correlated with reenlistment, such as taste for military service.²⁶ In principle, were we willing to assume that promotions are uncorrelated with unobserved characteristics that predict reenlistment, and do not influence reenlistment except through higher pay, we would be capturing the effect of financial incentives on reenlistment. In the end, we included the base pay response because it is a financial incentive to reenlist and may be of interest to some readers; it cannot be statistically distinguished from the OLS effect of bonus pay.

The response to state unemployment is our clearest qualitative evidence of avoidance behavior, and our estimation allows us to reject the null of no response to unemployment in all specifications. The coefficient of 0.86 in Column 1 means that a 1 point increase in state unemployment increases reenlistment by just under 1%, implying an unemployment elasticity of 2-2.5. Estimates of the reenlistment response to state unemployment rates at the end of the contract are similarly stable across the specifications, with the exception of the longer sample period. In the first 5 columns, point estimates range from 0.83-1.03; in Column 6, where we add controls for postenlistment experiences, the effect of 1 point of home-state unemployment increase to a 1.33 percentage point increase in the probability of reenlistment. We have no compelling explanation; some aspect of the Great Recession and Iraq and Afghanistan deployments during this period seem like potential explanations, although any story needs to account for why we do not see a larger effect without the

²⁶Interestingly, state unemployment shocks do not appear to predict promotion speed.

postenlistment controls. We use 0.9 as our median estimate throughout the paper, but accept the possibility that the response to state unemployment has grown in the Great Recession.

Finally, our estimates of the value of unemployment in terms of bonus points, formed by dividing the reenlistment response to state unemployment by the reenlistment response to bonus pay, imply large welfare losses to unemployment. The coefficient of 2.29 in Column 1 means that the marginal reenlistee in our sample is roughly indifferent between a 1% increase in unemployment at exit and a 2.3% decrease in pay over the course of the next contract. Reading across the columns, these estimates are large, approaching or exceeding the earnings effects (from Table 4) for the corresponding time frame. Note that the appearance of a downward-biased pay elasticity in the denominator would result in an upward bias to this estimate. We investigate this bias through the use of IV specifications, below.

Before turning to the IV results, we performed one additional check on the OLS specification, using average bonuses within end-of-contract-year by 3-digit-occupation cells, in order to remove individual-level endogeneity in the bonus variable. This takes us one step towards the IV specifications, where we use aggregate variation to identify the effect of bonus pay. Averaging reduces measurement error from the construction of the variable, and the endogeneity of the individual choice of reenlistment timing. The results, in Panel B of Table 5, do not show large differences from the individual-level bonus analysis. This specification gives us confidence that the primary threats to the OLS identification of the γ coefficient exert a small bias on the estimates.

3.2 Reenlistment: IV Estimates

As discussed above, OLS estimates may be attenuated by the endogeneity of bonus offers or measurement error in our construction of the bonus. For these reasons, we estimate an instrumental variables model, using changes in 2-digit occupation demand over time to measure the effect of additional bonus pay on the probability of reenlistment, were the bonus the only lever available to the military to control reenlistment.

The first stage of the IV model uncovers a positive relationship between changes in occupation-cohort size and bonuses across branches, though statistical power is driven by the Army, Marines, and in the longer sample, the Air Force. Table 6 reports the first-stage results, and the Kleibergen-Paap F-test of the joint significance of the four coefficients. We multiply the coefficients by 100 to aid in the reading of the table. First-stage estimates in 1993-2004 reveal that our demand shock is positive predictor of bonuses in all 4 branches,

however, only in the Army and Marines does the size of the entering occupation-cohort have statistically significant predictive power for the bonus offered those up for reenlistment in the occupation. In the Air Force, the coefficient is significant only in the longer unrestricted sample. Across branches, the average first-stage coefficient is around 0.5. The first-stage F-tests fall just below the rule of thumb value of 10, meaning our estimates may include a small weak instruments bias.²⁷

Our basic IV estimates, without the branch-by-year coefficients, increase the bonus coefficients by a factor of 3 to 4 compared to the OLS estimates, implying a corresponding reduction in the value of unemployment. Note that the unemployment rate coefficient (β) remains robust across the OLS and IV specifications: the uncertainty in our estimates does not arise from the avoidance behavior (higher reenlistment in response to arises from the response to pay), but rather from the price of this avoidance behavior (the increase in reenlistment in response to bonus pay). The coefficient in the first column of 1.46 means that a 10% increase in pay results in a 14.6% increase in reenlistment. The standard error of 0.52 on this estimate gives us a confidence interval considerably above the OLS estimates. Moving across the columns, we cannot reject the equality of bonus coefficients in the 1993-2004 sample period, although the estimates are smaller in the robustness checks. When we move to the longer sample period, where our assumptions of a stable relationship between incoming-cohort size and labor demand may have less support, the estimated bonus response falls back into the range of the OLS estimates, and cannot be statistically distinguished from zero; we will return to this below. Off a base 40% reenlistment rate, these responses implies an elasticity between 3 and 4 in the earlier sample period. This elasticity is on the higher end or above what has been found in previous studies of reenlistment, and evidence from the branch-by-year model will support our contention that the basic IV specification overstates the responsiveness to pay.

The lower panel of Table 7 reports the main results in the branch-by-year specification. In the first column we reestimate the OLS model, finding the same patterns as in our basic OLS specification (i.e. not including the branch-by-year effects). The coefficient is slightly larger than but statistically indistinguishable from the corresponding OLS estimates using the individual-level or group mean bonus, meaning that when we remove the cross-branch variation, the estimate of γ does not change by much. This suggests that there is not a strong correlation between bonuses and other reenlistment policies at the branch level, which would

²⁷The distribution of errors is non-i.i.d., as evidenced by the much higher F-statistics (>20) for tests that assume i.i.d. errors. This divergence between the F-tests is much worse when we instrument at the 3-digit level.

have caused an upward bias in OLS. This does not completely answer our concerns about the exclusion restriction, as it could be that the military uses other levers to control reenlistment during times of declining demand. If alternative measures switch on when the bonus is at zero, the reduced-form military labor demand shock will pick up the combined effect of the bonus and all other reenlistment levers, and the IV will be biased towards finding a larger effect of the bonus.

In the second column of the lower panel, we report the branch-by-year IV results for the unrestricted sample, finding considerably smaller coefficients than in the basic IV model. In the unrestricted specification, our estimate of γ is 0.95 and our estimate of β is 0.90. The implied value of a point of unemployment is just above a 1% increase in pay (from the bonus), approximately one-half of the average earnings losses in these years. As we move across columns, our robustness checks further reduce the size of the bonus response, with implied values around 1.5% of the wage. Finally, looking in the 1993-2009 sample, the branch-by-year IV model appears to fit the data better than the basic IV model, as standard errors fall, and the estimates are more in-line with the overall patterns of results (e.g. IV coefficient nearly twice OLS). This may occur because of branch-level differences in reenlistment behavior and policy during the Iraq and Afghanistan conflicts.

Despite the uncertainty in our estimates of the value of unemployment (arising from the estimates of pay elasticity), these IV estimates allow us to test and reject the presence of full insurance against movements in end-of-contract unemployment. Qualitatively, this is a central result of the paper. Our robust estimates of unemployment response form the bedrock of this test: for plausible values of the reenlistment elasticity, we will find economically-meaningful welfare loss. The various biases we have discussed, in particular the exclusion restriction in the IV model, would lead to an understatement of the value of unemployment. Thus, we reject full insurance for these shocks.

To summarize, we estimated regressions of reenlistment on the unemployment rate and bonus. We found relatively stable coefficients in OLS, with our median estimates of $\beta = 0.9$ and $\gamma = 0.4$. This estimate of γ implies a reenlistment elasticity of 1, at the lower range of previous estimates. The ratio, $\beta/\gamma = 2.25$ means that servicemembers are willing to trade roughly a 2.25% increase in military earnings for a 1 point increase in home-state unemployment at exit. The OLS estimate did not change when we added controls for post-enlistment experiences (deployment, hostile fire pay, promotion speed and base pay), averaged bonuses within occupation-by-year (*ot*) cells, or added branch-by-year fixed effects, suggesting limited scope to biases arising for most of our endogeneity concerns. Estimates from the branch-

by-year model, our preferred IV specification, found point estimates implying reenlistment elasticities of 1.75 to 2.4. These estimates are consistent with previous work on reenlistment, and imply that servicemembers require a slightly smaller 1%-1.6% increase in their wage to reenlist. In the next section, we will refine these estimates by explicitly adjusting for the timing of bonus payments.

4 Willingness To Pay Calibration and Mitigating Mechanisms

In this section, we use our reenlistment model estimates to calibrate the willingness to pay for exit over the business cycle, taking into account the dynamic nature of the reenlistment problem. We use these estimates, along with our earnings estimates from Section 2, to quantify the value of mitigating mechanisms. The important question in the calibration is: how much income does the shock to bonuses represent, i.e., how long is the measuring stick we use to value the avoidance behavior? As discussed above, the dollar value of the bonus appear as a scaling factor in our main calculations. This scaling factor depends both on the discount rate and reenlistment behavior at the second reenlistment point. We begin with a description of the characteristics of second contracts and second-term reenlistment, before calibrating the model for a range of discount rates estimated in the previous literature. We conclude with a discussion of using alternative parameter values in the literature and other extensions to our estimates.

4.1 Second Contracts and Parameters of Calibration

Our primary purpose in investigating second-term contracts is to evaluate the response of their characteristics and resulting behavior to changes in the parameters of the first-term reenlistment problem. Unlike first-term contracts, the majority of which result in discharge at their conclusion, second-term contracts (and beyond) begin to reflect labor market behavior based on the accumulation of firm-specific (i.e. military-specific) capital and career concerns. As such, they may be poorly suited for insuring workers against temporary shocks, with small changes to future option values arising from changes in the parameters of the first reenlistment problem.

The option value to reenlist at the end of the second contract depends on both the movements in the bonus and home-state unemployment rate; however, the bonus is considerably

more persistent than the unemployment rate. In the calibration, we focus on quantifying the total value of the bonus. Persistence of the bonus is expected: the labor demand shock we employ reflects a shortfall in the size of an occupation-cohort, and as it likely reflects, in part, the trend in military occupation-level demand, we should expect serial correlation in the bonus. Empirically, an OLS regression of second-term bonus on first-term bonus for those who reenlist reveals that 25% of first-term bonus shocks persist to the end of the second term. If we assume that temporary shocks to working conditions are not serially correlated at the horizon of bonus contracts, this persistence reflects the persistence of the military labor demand shock. Thus, we consider a 1% increase in the bonus at the end of the first contract to translate into a 0.25% increase in the bonus at the end of the second contract.

We assume new contracts are 4 years on average (average second contract length is 49 months between 1993 and 2004), and reenlistment occurs midway through the year t , so the average willingness-to-pay is:

$$WTP = \frac{\beta}{\gamma} [y_t^{mil}(2\% + \frac{1}{2}0.5\% + \rho 0.5\% + \rho^2 0.5\% + \rho^3 0.5\% + \frac{1}{2}\rho^4 0.5\%) + \rho^4(0.55)(0.25)y_{t+4}^{mil}(2\% + \frac{1}{2}0.5\% + \rho 0.5\% + \rho^2 0.5\% + \rho^3 0.5\% + \frac{1}{2}\rho^4 0.5\%)]. \quad (9)$$

The 4 units of bonus, reflecting the 4 additional years of new service commitment, are paid as 2 units in year 0, and half a unit per year served after that. Since the reenlistment occurs midway through the year, the servicemember receives 2.25 of the 4 bonus units in the year of reenlistment, half a unit for the next 3 years, and a quarter of a unit in the last year of the contract. We set $y_t^{mil} = \$32,000$, average year 0 base military earnings in 2010 dollars. This means that each bonus point equates to a stream of payments (over the course of a 4 year contract) of \$640 and \$80 in year 0, \$160 in years 1 through 3 and \$80 in year 4. After the second contract is concluded, we assume that continuation at the occurs at the average rate, and those who continue are paid at the average persistence of bonuses. This implies that 55% will continue, with 25% of the unexpected component of the 1st contract bonus persisting. The combined effect of these assumptions equates to a 1 unit shock for 4 years, followed by about an 1/8th unit shock for another 4 years for the average service member. Following the second contract, the servicemember exits. This is broadly consistent with the reduced-form evidence, which shows that the increase in military income (in response to home-state unemployment) is statistically significant for 9 years following the end of the first contract.

The choice of discount rate governs the PDV calculations and importance of events

occurring at the conclusion of or after the second contract, as well as the magnitude of the estimated earnings effects for lifetime utility. Moffitt estimates a discount rate between 10% and 14% for enlisted men in 1974-1990 (before our sample period, but during the post-1973 volunteer era), while Warner estimates rates above 18% for 90% of enlisted men in 1996. As discussed above, these values are large, and some researchers have suggested that observed rates may be confounded by credit constraints or behavioral elements, such as impatience. We consider annual discount factors of 95%, 90% and 80%, where we consider these values to represent lower, median and upper bounds on the discount rates found in the literature. For larger values of the discount rate, behavior at higher reenlistment points plays a small role in the model.

The key ingredient in our calibration is the ratio of regression parameters, β/γ , which is estimated in Section 3. Consistent with the range of estimates we found above, we calibrate WTP for values of β/γ ranging from 0.5% of the wage up to 2.5%. These values cover the upper and lower range of our estimates; as well, they cover the range of estimates in the previous literature on the reenlistment elasticity. As will be seen, the wide range of reenlistment elasticity estimates are the primary source of uncertainty in our calibration.

4.2 Calibration

The main purpose of the calibration is to recover the value of the mitigating mechanisms, which in the model is the difference between observed earnings losses and willingness-to-pay for these losses. We also provide estimates of the present discounted value of earnings losses and WTP under a range of discount factors.

Table 8 displays the results of our calibration. We vary the discount rate across the columns and β/γ across the rows. The first row of estimates, labeled $\Delta y(UR)$, reflects the discounted sum of earnings losses estimated in Section 2. In parentheses we translate this into the fraction of permanent income lost to a 1 point increase in the unemployment rate. Naturally, as we increase the discount rate the value of the earnings losses falls, but the importance of one year's income in permanent income rises. Our preferred estimates of the discount factor are at $\rho = 0.9$. At this level, the PDV of earnings losses is \$3101, equating to a 1.31% drop in permanent income. We compare these earnings losses to the implied WTP, calibrated according to the assumptions above. Finally, the share of earnings losses left unexplained by WTP we attribute to mitigating mechanisms, m , so $m \equiv 1 - \frac{WTP}{\Delta y(UR)}$.

Our calibration returns a range of values for the mitigating mechanisms, primarily reflecting uncertainty in estimates of γ in Section 3. Our median OLS estimate, for which we

found little evidence of bias, is $\beta/\gamma \approx 2\%$, where $\text{WTP} = \$2706$ (if $\rho = 0.9$), and mitigating mechanisms account for 13% of earnings losses. At these parameter values, servicemembers are willing to give up 1.14% (87% of 1.31%) of permanent income in exchange for a 1 point decrease in unemployment at the time of exit. Instead, if we let $\beta/\gamma = 1.5$, corresponding to the branch-by-year IV estimates in Table 7 and to a reenlistment elasticity of 1.75 (which is similar to values estimated in the military manpower literature), we find that $\text{WTP} = \$2029$, and mitigating mechanisms account for 35% of earnings losses. For a given of the reenlistment elasticity, the choice of the discount rate has a moderate influence on the calibration of mitigating mechanisms. For example, at $\beta/\gamma = 1.5$, the low and high values of the discount factor result in mitigating mechanisms offsetting between 20% and 41% of earnings losses. The results of the calibration reveal that mitigating mechanisms shield only a small share of earnings losses, and we are quite far from the extreme case of full insurance.

Several final comments on the magnitudes and standard errors deserve mention. First, using Wald estimators to adjust the estimates, as in Section 2, would result in correspondingly larger earnings losses and WTP (since the same mis-measured unemployment rate enters both calculations); however, the resulting share of earnings offset by mitigating mechanisms will not be affected, since it is the ratio of these two. Second, our calibration has set aside standard errors, as we are combining several separately-estimated pieces of analysis; instead, we calibrate the model to cover the range of estimates of β/γ . As the analysis and calibration make clear, the primary uncertainty rests in the estimate of the reenlistment elasticity, γ . A long literature seeks to estimate this object, and although we would like to impose further structure on the model to address finer subtleties, ultimately we believe that the uncertainty in the estimation of this elasticity will swamp any additional precision in the calibration.

4.3 Timing of Decision and Credit Constraints

To this point, our empirical analysis of reenlistment has built upon the assumption that reenlistment can be modeled as a single decision point coming at the end of the first contract. This assumption is standard in the most analyses of reenlistment, and greatly simplifies the analysis; however, it comes at a cost. As we have discussed, previous research on the financial decisions of enlisted servicemembers has found evidence of credit constraints and high discount rates.²⁸ While the military manpower literature has recognized that the reenlist-

²⁸For example, FINRA (2010) surveyed military personnel in 2008, finding 25% report carrying over \$10,000 in credit-card debt, and half report making a minimum payment on a credit card in the previous year. Using Air Force data on readiness and retention, Carrell and Zinman (2008) finds “results (that) are consistent with the interpretation that payday loan access causes financial distress and severe misbehavior

ment elasticity may differ if one estimates a separate elasticity for the effect of bonus pay and base pay, we know of no formal analysis of the difference between these two responses. In general, previous work finds the response to bonus pay exceeds the response to base pay, which is suggestive evidence that these elements influence our estimates.

A related issue concerns the timing of the realization of the end-of-contract unemployment rate. Most reenlistment decisions occur in the final 6 months of the initial enlistment contract, however, in some years and for some occupations, reenlistment windows may open 2 years or more before the end of the contract. In these situations, high discount rates can have serious consequences, inducing reenlistment in response to bonus offers due to the cash-in-hand component, rather than through the comparison of the exit and reenlistment options from the point of view of the agent the end of the contract, as we have modeled servicemembers in this paper. In particular, at a horizon of 2 years, the projection of the state unemployment rate will be far less precise than for decisions made at the end of the contract, limiting the potential response of early reenlistment to this variable. Together, we expect a smaller unemployment rate response (β) and larger response to bonus pay (γ) for decisions made significantly before the end of the contract.

Our evidence on this point is suggestive, however, what we have found corroborates the idea that the timing of the decision may attenuate estimates. Data constraints prevent us from building these directly elements into our model: we lack comprehensive data on the reenlistment windows (usually are determined at the branch-level, but with enough exceptions that we cannot simply recover the window from obvious discontinuities in the monthly hazard); as well, the construction of the bonus variable prevents the recovery of the series of offers for most occupations. Instead, we apply a sample restriction used in Asch et al. (2010), and drop reenlistments occurring before the conclusion of the third year of service. This effectively imposes the $j \geq 4$ sample restriction from above, along with dropping earliest reenlistments for the longer contracts; together, just under 20% of observations are dropped by this sample restriction. As a result, the estimated response to home-state unemployment rises by 50%, to 1.4% (from a median estimate of 0.9-1% for the other specifications), meaning that servicemembers who wait to reenlist until the unemployment rate has been realized show stronger avoidance behavior. This sample restriction causes the estimates of the bonus response to fall by half, although standard errors rise in the smaller sample size, and we cannot reject the equality of the two estimates, or reject that this estimate is different from

for relatively young, inexperienced, and financial unsophisticated airmen,” suggesting a link between the availability of outside credit and reenlistment outcomes.

zero. Lacking a clear model of the harvesting-off of the early reenlistment (and the data to estimate such a model), we did not pursue this line of analysis further; however, the evidence reveals the broad patterns we would expect to see if the timing of the decision influences our estimates towards a smaller WTP.

Although we lack the data to incorporate the timing of the decision in our model, the concerns of previous researchers and the suggestive evidence presented here point to larger welfare effects were servicemembers forced to make the decision at the end of the contract. In future work, we hope to pursue this question further.

5 The Compensated Labor Supply Elasticity Under Rationing

In the absence of data on post-service wages and hours worked, we must make some simplifications in order to estimate labor supply elasticities. One possibility is to follow the assumption of Ashenfelter (1980), and assume that hours are rationed at a constant wage. Ashenfelter derives the share of lost earnings, C^* , required to compensate a worker who is prevented by rationing from the sale of a share, D_1 , of their optimal labor supply:

$$C^* \approx (1/2)D_1/e_{11}, \tag{10}$$

where e_{11} is the compensated elasticity of labor supply. This is a normal triangle estimator, where the proportional welfare loss rises with the size of the distortion.²⁹

In our setting, we have estimated the average C^* , which is the share of earnings that is not offset by mitigating mechanisms, $1 - m$. To recover D_1 requires a little more thought. According to this model, the worker supplies an optimal amount of labor to the market unless constrained; there is never an over-supply of labor. This suggests we take some sort of maximum earnings, and use the average earnings below this maximum as the average D_1 . This is impractical, given worker heterogeneity and data constraints. Another alternative is to use the unemployment rate literally: an average of 5% unemployment rate means that 5% of hours go unsold. We multiply our earnings estimates, $\Delta y(UR_{st})$, by the average raw UR_{st} in the data, about 5%. This tells us that the average lost lifetime hours/earnings (at $\rho = 0.9$) are approximately 6.5%. Using this value as D_1 gives a compensated elasticity of labor supply of 0.037 for $m = 13\%$ and a compensated elasticity of 0.05 for $m = 35\%$. This

²⁹Ashenfelter (1980) extends the analysis to include additional household members, hence the subscripts.

estimate is low, and in fact, is quite close to zero. The calculation appears to be relatively insensitive to the choice of D_1 or the discount factor in the implementation of the model, due to the estimates of m far from 1. A compensated labor supply elasticity near zero is consistent with the evidence for labor supply elasticities of prime-age males discussed in Saez et al. (2012).

6 Conclusion

In this paper, we examine the consequences for the earnings and welfare of young US veterans who enter the labor market in times of high unemployment. We measure the dollar value of utility lost in the transition to civilian employment when unemployment is 1 point higher in the young veteran's home state to be between \$2029 and \$2706 (in discounted 2010 dollars). These values imply that between 13 and 35% of the estimated \$3101 in earnings losses are offset by mitigating mechanisms. Welfare effects among this group of workers likely understate the degree to which earnings losses are offset for other, similar workers, as young veterans have better access to (publicly-provided) mitigating mechanisms than almost any other similar group of young workers in the US economy. For instance, in addition to the usual informal insurance provided by private transfers, young veterans are eligible for very generous college/job-training tuition benefits, a dedicated unemployment insurance system (Unemployment Compensation for Ex-Servicemembers), hiring tax benefits, and health insurance.

The welfare costs of business cycles is an fundamental question in economics, and a number of summary measures of welfare costs precede this study. Consumption-based welfare measures allow for tests of insurance against shocks such as job loss, although magnitudes depend on the specific model of consumption behavior and distinction between temporary and permanent shocks.³⁰ Measures of the extensive margin elasticity also imply the value of leisure relative to work, and hence, the welfare effects of unemployment. Chetty et al. (2013) argues there a growing consensus in the microeconomic literature over relatively low Frisch elasticities of intertemporal substitution, implying important welfare costs of unemployment. The reservation wage in search models also can serve as a summary measure of the value of transitioning to unemployment. Using a search model calibrated with detailed survey and administrative data on labor market activity and benefit usage, Chodorow-Reich and Karabarbounis (2013) find a strong cyclical component of the flow value of employment for

³⁰For example, see Cochrane (1991), Gruber (1997), Chetty and Szeidl (2007), Blundell et al. (2008)

the general population. This paper most closely approaches our question, and the results suggest our findings reflect broader trends in the economy. Taken together, the micro-based literature on welfare costs of business cycles has found significant evidence of welfare losses to job loss, income fluctuations and business cycles.

We add to this literature in three ways. First, we provide a new and direct measure of welfare costs of local labor market shocks based on the observation of the decision to enter a labor market with a well-defined outside option. Our estimate of willingness-to-pay summarizes the welfare losses to job search over the business cycle, without assumptions regarding consumption and savings behavior or a model of the labor market and its parameters. Second, losses may entail more than consumption and labor supply responses, and the relevant measure for public policy takes into account indirect effects of unemployment. We estimate the hedonic value of unemployment, which includes the contributions of unemployment to utility from health, happiness and general equilibrium effects, all of which have been shown to respond to business-cycle conditions.³¹ A recent literature estimates the fiscal costs of job creation,³² however, fewer papers estimate the net benefits of job creation; our measure takes a step in this direction. Finally, we believe military reenlistment can serve as a unique laboratory for the study of local labor market shocks. Large-scale studies of the effects of entry conditions have been limited by data coverage, with a focus on older, tenured and college-educated workers, in part due to data availability. In principle, our model would allow us estimate the hedonic value of any exogenous variable that enters the reenlistment decision. Linking the reenlistment decision with college benefit use would be a natural extension to our current analysis.

We also contribute to a separate literature on the study of inequality and local economic shocks. Moretti (2011) reviews the evidence that local labor market shocks increase inequality, and emphasizes the role of migration costs, as in Topel (1986). Comparing our earnings results to previous estimates for college-educated workers, we conclude that shocks to the local labor market may increase income inequality among young workers. Hoynes et al. (2012) documents heterogeneity in the effects of business cycle conditions on employment

³¹A similar exercise to ours is performed by Blanchflower and Oswald (2004), using self-reported happiness as the outcome (in place of reenlistment), and reports of being unemployed as the explanatory variable of interest, finding unemployment to be worth approximately \$60,000, a very large value. As in our estimation, the uncertainty in this estimate appears to arise from the price elasticity of happiness (or in our case, reenlistment), rather than the correlation between unemployment and (un)happiness, which appears quite strong. See Ruhm (2000) and Stevens et al. (2011) for evidence on counter-cyclical patterns in health and mortality.

³²See, for example, Nakamura and Steinsson (2011), Serrato and Wingender (2010) and Chodorow-Reich et al. (2012).

outcomes, finding larger effects for young, non-white, less-educated, male workers; our findings (for earnings, not employment) fit with the aggregate patterns. However, young veterans are highly mobile, exhibiting migration rates nearly twice the rate of civilians, a fact that is inconsistent with migration costs driving distributional effects. In the final rounds of analysis we will disaggregate the analysis by predicted earnings in order to gauge the distributional effects within this population. Given the classic study of Topel and Ward (1992) finding that young workers change jobs 7 times during 20's, these findings implies persistent welfare effects arising from business cycle conditions at the time of these transitions.

Finally, our estimates of willingness-to-pay for unemployment at job search are conditional on the existing public and private transfer system in the sample period. We focused our analysis on the 1993-2004 period, based on the strength of our identifying assumption in this period and the availability of data; however, the transfer system has undergone significant changes since this period. An important extension of this project will be to study how state-level reenlistment patterns evolved over the Great Recession. In particular, one of the original concepts of this project was to interact earnings effects with state-level policy, particularly the generosity of unemployment insurance for young veterans.

References

- Aguiar, M., E. Hurst, and L. Karabarbounis (2013, September). Time Use during the Great Recession. *American Economic Review* 103(5), 1664–96.
- Altonji, J. G., L. B. Kahn, and J. Speers (2013). Cashier or Consultant? Entry Labor Market Conditions, Field of Study, and Career Success. Working Paper.
- Alvarez, F. and R. Shimer (2011). Search and Rest Unemployment. *Econometrica* 79(1), 75–122.
- Asch, B. J., P. Heaton, J. Hosek, F. Martorell, C. Simon, and J. T. Warner (2010). Cash incentives and military enlistment, attrition, and reenlistment. Technical report, DTIC Document.
- Ashenfelter, O. (1980). Unemployment as Disequilibrium in a Model of Aggregate Labor Supply. *Econometrica* 48(3), pp. 547–564.
- Black, D. A., A. Hasan, P. Krishnamurty, and J. Lane (2008). *The Labor Market Outcomes of Young Veterans*. NORC/University of Chicago.

- Blanchard, O. J. and L. F. Katz (1992). Regional Evolutions. *Brookings Papers on Economic Activity* 1992(1), pp. 1–75.
- Blanchflower, D. G. and A. J. Oswald (2004). Well-being over time in Britain and the USA. *Journal of Public Economics* 88(78), 1359 – 1386.
- Blundell, R., L. Pistaferri, and I. Preston (2008, September). Consumption Inequality and Partial Insurance. *American Economic Review* 98(5), 1887–1921.
- Brunner, B. and A. Kuhn (2009). To Shape the Future: How Labor Market Entry Conditions Affect Individuals’s Long-Run Wage Profiles. Unpublished Working Paper.
- Carrell, S. and J. Zinman (2008). In Harm’s Way?: Payday Loan Access and Military Personnel Performance. Unpublished Working Paper.
- Chetty, R., A. Guren, D. Manoli, and A. Weber (2013). Does Indivisible Labor Explain the Difference between Micro and Macro Elasticities? A Meta-Analysis of Extensive Margin Elasticities. *NBER Macroeconomics Annual* 27(1), pp. 1–56.
- Chetty, R. and A. Szeidl (2007). Consumption Commitments and Risk Preferences. *The Quarterly Journal of Economics* 122(2), 831–877.
- Chodorow-Reich, G., L. Feiveson, Z. Liscow, and W. G. Woolston (2012). Does State Fiscal Relief during Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act. *American Economic Journal: Economic Policy* 4(3), 118–45.
- Chodorow-Reich, G. and L. Karabarbounis (2013). The Cyclicalities of the Opportunity Cost of Employment. Unpublished Working Paper.
- Cochrane, J. H. (1991, October). A Simple Test of Consumption Insurance. *Journal of Political Economy* 99(5), 957–76.
- Daula, T. and R. Moffitt (1995). Estimating Dynamic Models of Quit Behavior: The Case of Military Reenlistment. *Journal of Labor Economics* 13(3), pp. 499–523.
- Davis, S. J., T. V. Wachter, R. E. Hall, and R. Rogerson (2011). Recessions and the Costs of Job Loss [with Comments and Discussion]. *Brookings Papers on Economic Activity* Fall, pp. 1–72.

- Faberman, J. and T. Foster (2013). Unemployment among recent veterans during the Great Recession. *Economic Perspectives* 37(Q I), 1–13.
- FINRA (2010). Financial Capability in the United States: Military Survey Executive Summary. Technical report, FINRA Investor Education Foundation.
- Greenstone, M., S. P. Ryan, and M. Yankovich (2014). The Value of a Statistical Life: Evidence from Military Retention Incentives and Occupation-Specific Mortality Hazards. Unpublished Working Paper.
- Gruber, J. (1997). The Consumption Smoothing Benefits of Unemployment Insurance. *The American Economic Review* 87(1), pp. 192–205.
- Heaton, P. (2012). The Effects of Hiring Tax Credits on Employment of Disabled Veterans. Technical report, RAND Occasional Papers.
- Hosek, J. and F. Martorell (2009). *How Have Deployments During the War on Terrorism Affected Reenlistment?* (1 ed.). RAND Corporation.
- Hoynes, H., D. L. Miller, and J. Schaller (2012). Who Suffers During Recessions? *The Journal of Economic Perspectives* 26(3), 27–47.
- Humensky, J. L., N. Jordan, K. T. Stroupe, and D. M. Hynes (2013). How Are Iraq/Afghanistan-Era Veterans Faring in the Labor Market? *Armed Forces & Society* 39(1), 158–183.
- Lucas, R. E. (1987). *Models of business cycles*, Volume 26. Basil Blackwell Oxford.
- Moretti, E. (2011). Chapter 14 - Local Labor Markets. In D. Card and O. Ashenfelter (Eds.), *Handbook of Labor Economics*, Volume 4, Part B of *Handbook of Labor Economics*, pp. 1237 – 1313. Elsevier.
- Nakamura, E. and J. Steinsson (2011). Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions. NBER Working Papers 17391, National Bureau of Economic Research, Inc.
- Newey, W. K. (2009). Two-step Series Estimation of Sample Selection Models. *The Econometrics Journal* 12, S217–S229.
- Oreopoulos, P., T. von Wachter, and A. Heisz (2012, January). The Short- and Long-Term Career Effects of Graduating in a Recession. *American Economic Journal: Applied Economics* 4(1), 1–29.

- Ruhm, C. J. (2000). Are Recessions Good for Your Health? *The Quarterly Journal of Economics* 115(2), 617–650.
- Saez, E., J. Slemrod, and S. H. Giertz (2012). The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review. *Journal of Economic Literature* 50(1), 3–50.
- Serrato, J. C. and P. Wingender (2010). Estimating local fiscal multipliers. Working Paper.
- Shimer, R. (2005, September). The Cyclical Behavior of Equilibrium Unemployment and Vacancies. *American Economic Review* 95(1), 25–49.
- Stevens, A. H., D. L. Miller, M. E. Page, and M. Filipski (2011, December). The Best of Times, the Worst of Times: Understanding Pro-cyclical Mortality. Working Paper 17657, National Bureau of Economic Research.
- Topel, R. H. (1986). Local Labor Markets. *Journal of Political Economy* 94(3), pp. S111–S143.
- Topel, R. H. and M. P. Ward (1992). Job Mobility and the Careers of Young Men. *The Quarterly Journal of Economics* 107(2), pp. 439–479.
- Warner, J. T. and S. Pleeter (2001, September). The Personal Discount Rate: Evidence from Military Downsizing Programs. *American Economic Review* 91(1), 33–53.
- Warner, J. T., S. Pleeter, and C. Simon (2011). The Virtues of Patience: Saving, Financial Well Being, and Awareness. Unpublished Working Paper.
- Yagan, D. (2013). Moving to Opportunity? Migratory Insurance over the Great Recession. Unpublished Working Paper.

Table 1: Summary Statistics

	All Enlisted Men: 1993-2009				Young Veterans: 1993-2009			
	Mean	SD	p10	p90	Mean	SD	p10	p90
Eligible Age	24.06	2.39	22	27	23.92	2.26	22	27
More than HS	0.09	0.29			0.09	0.28		
Single at Entry	0.88	0.32			0.90	0.30		
AFQT Percentile	58.94	17.89	35	84	58.89	17.92	35	84
White	0.67	0.47			0.70	0.46		
Black	0.16	0.37			0.14	0.35		
Hispanic	0.10	0.30			0.10	0.30		
Initial Term	4.03	0.69			4.00	0.66		
N	1,098,377				673,260			

Notes: Descriptive statistics for All Enlisted Men with initial enlistment contracts ending between 1993 and 2009, and the subgroup of Young Veterans, defined as enlisted men who do not reenlist by the end of the first contract. AFQT, education and marital status measured at entry. Most enlisted servicemembers reach the end of their first contract between ages 22 and 27, with the average of 24; the modal service member enlists for an initial term of 4 years. A large majority of this population enters the military single with their highest education being a high school diploma or equivalent. Servicemembers are of slightly above average intelligence, as can be seen from the mean and 90-10 statistics for AFQT (which is normed to the US population). Based on the skew of AFQT, the effect of entry standards on the lower tail appears considerably stronger than selection effects at the top of the distribution. The population is broadly representative of the racial/ethnic demographics of the US over this period: whites and blacks are slightly overrepresented, while Hispanics are slightly underrepresented. The subgroup of Young Veterans has similar observable characteristics to the larger group of enlisted men.

Table 2: Earnings: 1993-2009 Cohorts

Year (k)	Av. Earn.	δ_k	\$ value	N
0	\$23,375	-1.05** (0.45)	-246** (105)	348,092
1	\$23,194	-3.58*** (0.44)	-831*** (102)	353,264
2	\$26,630	-3.61*** (0.57)	-961*** (152)	335,130
3	\$29,209	-2.25*** (0.76)	-658*** (222)	314,538
4	\$30,928	-1.94** (0.76)	-599** (236)	294,804
5	\$32,327	-1.47** (0.73)	-474** (236)	272,422
6	\$33,676	-1.20** (0.52)	-403** (174)	248,844
7	\$35,033	-0.92** (0.37)	-322** (131)	227,524
8	\$36,313	-0.78* (0.43)	-282* (158)	208,301
9	\$37,311	-0.27 (0.44)	-99 (163)	189,920
10	\$38,164	0.46 (0.51)	176 (193)	169,253

Notes: We run a regression of total annual earnings k years after the end of the initial enlistment contract on the home-state unemployment rate in the 3 months following the end of the contract, using the full set of fixed effects and other controls detailed in the text. Upon exit, veterans lose approximately 3.5% of earnings for each percentage point of unemployment at the time of eligibility for discharge. Negative effects remain statistically significant for 7 years (95% C.I.) following eligibility. Table reports average total earnings, regression coefficients on state unemployment (δ_k , from Equation 6, which is the effect of unemployment in the quarter of discharge on earnings at the k -horizon), and total implied losses for earnings in the 11 years beginning in the year in which the first contract ends. The regression includes year fixed effects, so effects should be interpreted as the effect of state-level unemployment. Unbalanced panel. Sample composed of all enlisted men who separate at the end of their first contract and do not use their GI Bill benefits; we correct for selection into reenlistment. Total earnings defined as SSA earnings plus untaxed military income. Real 2010 \$ (CPI). s.e. clustered by state of enlistment. Significance levels: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 3: Earnings: 1993-2009 Cohorts, Civilian Earnings and Selection

Year (k)	Civilian Earnings		Total Earnings		
	Av. Earn.	δ_k	Av. Earn.	δ_k	δ_k , no $\Lambda(\cdot)$
0	\$9,225	-7.31*** (1.15)	\$23,409	-0.91*** (0.32)	-0.72** (0.33)
1	\$19,192	-4.46*** (0.66)	\$21,878	-3.63*** (0.47)	-3.43*** (0.48)
2	\$23,112	-3.88*** (0.69)	\$25,222	-3.55*** (0.57)	-3.39*** (0.58)
3	\$25,466	-2.85*** (0.84)	\$28,098	-2.47*** (0.72)	-2.35*** (0.73)
4	\$27,523	-2.36*** (0.81)	\$30,477	-1.95*** (0.71)	-1.81** (0.71)
5	\$29,490	-2.01*** (0.73)	\$32,675	-1.52** (0.64)	-1.37** (0.63)
6	\$31,405	-1.66*** (0.57)	\$34,770	-1.25*** (0.48)	-1.08** (0.47)
7	\$33,221	-1.11*** (0.42)	\$36,739	-1.02*** (0.34)	-0.85*** (0.33)
8	\$34,890	-0.83** (0.36)	\$38,517	-0.85*** (0.33)	-0.71** (0.32)
9	\$36,289	-0.24 (0.30)	\$39,997	-0.28 (0.26)	-0.18 (0.25)
10	\$37,664	0.58 (0.56)	\$41,419	0.07 (0.46)	0.13 (0.45)

Notes: To evaluate the role of two components of the empirical model, we look separately at the effect on civilian earnings, and drop the selection correction. We run regressions of earnings (civilian or total) k years after the end of the initial enlistment contract on the home-state unemployment rate in the 3 months following the end of the contract, using the full set of fixed effects and other controls detailed in the text. Table reports average total earnings, regression coefficients on state unemployment in the 3 months following eligibility (δ_k , from Equation 6, which is the effect of unemployment in the quarter of discharge on earnings at the k -horizon), and total implied losses for earnings in the 11 years beginning in the year in which the first contract ends. The regression includes year fixed effects, so effects should be interpreted as the effect of a state-level shock. Looking at the effects on civilian earnings, it appears that shorter (non-reenlistment) extensions, service in the Reserves, and other offsetting mechanisms shield a portion of civilian earnings losses in years 0 and 1, after which the effects on civilian earnings and total earnings (in column 4) become quite close. Looking at the models with and without the selection correction, we see a very small difference between the estimates, suggesting selection into reenlistment does not bias our estimates. The selection correction is identified by the military's occupation-specific demand, as detailed in the text. Unbalanced panel. Sample composed of all enlisted men who separate at the end of their first contract (regardless of college benefit usage). In Columns 2 and 3 we report results on the civilian component of earnings, in Column 5 we report the full sample result (with the selection correction), and in Column 6 we report the effects without the selection correction (i.e. excluding $\Lambda(\cdot)$ from the estimation). Real 2010 \$ (CPI). s.e. clustered by state of enlistment. Significance levels: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 4: Earnings: 1993-2004 Cohorts, Initial Conditions and Persistence of Shock

Year (k)	Av. Earn.	Effect on Earnings		Effect with Controls for UR in year k				N
		δ_k	\$ value	δ_k	\$ value	\overline{UR}_{st+k}	\$ value	
0	\$22,600	-2.43*** (0.40)	-549*** (91)	-3.47*** (1.26)	-784*** (284)	1.33 (1.19)	300 (268)	263,904
1	\$23,709	-2.16*** (0.60)	-513*** (142)	-0.19 (0.61)	-45 (145)	-2.58*** (0.83)	-612*** (197)	269,153
2	\$27,105	-2.25*** (0.60)	-609*** (164)	-0.99** (0.40)	-268** (108)	-2.19*** (0.65)	-595*** (175)	270,691
3	\$29,587	-1.84*** (0.67)	-544*** (200)	-0.99** (0.50)	-293** (149)	-2.19*** (0.68)	-648*** (201)	271,933
4	\$31,235	-1.92*** (0.69)	-599*** (215)	-1.21** (0.49)	-377** (154)	-2.68*** (0.54)	-838*** (170)	272,301
5	\$32,327	-1.47** (0.73)	-474** (236)	-1.15** (0.55)	-370** (177)	-2.30*** (0.51)	-742*** (164)	272,422
6	\$33,676	-1.20** (0.52)	-403** (174)	-0.98** (0.39)	-331** (133)	-2.54*** (0.50)	-857*** (167)	248,844
7	\$35,033	-0.92** (0.37)	-322** (131)	-0.77** (0.36)	-271** (126)	-2.04*** (0.46)	-714*** (161)	227,524
8	\$36,313	-0.78* (0.44)	-282* (158)	-0.58 (0.45)	-212 (164)	-1.76*** (0.43)	-641*** (157)	208,301
9	\$37,311	-0.27 (0.44)	-99 (163)	-0.27 (0.43)	-100 (160)	-1.40*** (0.42)	-521*** (157)	189,920
10	\$38,164	0.46 (0.51)	176 (193)	0.14 (0.54)	52 (207)	-1.68*** (0.33)	-642*** (127)	169,253

Notes: We analyze persistence of the effects for cohorts with initial enlistment contracts ending between 1993 and 2004. We restrict to the earlier time period in order to separate the Great Recession period-effect from persistence. Panel balanced through year 5. Sample composed of all enlisted men who separate at the end of their first contract and do not use their GI Bill benefits; we correct for selection into reenlistment. Total earnings defined as SSA earnings plus untaxed military income. Estimates of the effect of initial conditions control for subsequent unemployment rates, \overline{UR}_{st+k} . δ_k , from Equation 6, is the effect of unemployment in the three months after the end of the initial enlistment contract. on earnings at the k -horizon. Negative effects remain statistically significant (using 95% C.I.) for 7 years following eligibility. Real 2010 \$ (CPI). s.e. clustered by state of enlistment. Significance levels: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 5: Effect of Bonus and Unemployment Rate on Reenlistment: OLS

	1993-2004				1993-2009	
	All	All	$j \geq 4y$	$N_{ot} \geq 15$	All	All
A. Individual-level Bonus						
$\ln(1+\text{Bonus}) (\gamma)$	0.37*** (0.04)	0.34*** (0.05)	0.37*** (0.05)	0.38*** (0.05)	0.36*** (0.04)	0.33*** (0.05)
$\ln(\text{BasePay})$		0.47*** (0.12)				0.49*** (0.14)
UR (β)	0.86*** (0.19)	0.83*** (0.16)	1.03*** (0.18)	0.86*** (0.20)	0.86*** (0.13)	1.33*** (0.14)
β/γ	2.29*** (0.70)	2.41*** (0.75)	2.77*** (0.73)	2.22*** (0.73)	2.34*** (0.59)	4.00*** (0.85)
B. Group Mean						
$\ln(1+\text{Bonus}) (\gamma)$	0.35*** (0.08)	0.42*** (0.04)	0.35*** (0.08)	0.37*** (0.08)	0.43*** (0.06)	0.54*** (0.06)
$\ln(\text{BasePay})$		0.54*** (0.11)				0.54*** (0.14)
UR (β)	0.82*** (0.19)	0.73*** (0.17)	0.98*** (0.17)	0.82*** (0.20)	0.84*** (0.13)	1.34*** (0.13)
β/γ	2.33*** (0.81)	1.72*** (0.45)	2.73*** (0.87)	2.22*** (0.81)	1.93*** (0.40)	2.46*** (0.47)
Postenlist X_i	X				X	
1-dig Occs	110	108	110	54	115	111
3-dig Occs	1572	1302	1522	700	1674	1396
Individuals	766,523	593,280	624,727	747,254	1,063,148	839,059

Notes: The table displays our OLS estimates of the response of reenlistment to bonus pay, basic pay, and home-state unemployment rates, as well as the ratio of the unemployment response to the bonus response, which is our estimate of the value of the unemployment rate at exit in terms of bonus points. Linear probability models include fixed effects for 3-digit primary occupation, controls for log of own cohort size in primary occupation, year of eligibility, and interactions of age at eligibility with a set of demographic, AFQT and education controls. Sample composed of all enlisted servicemembers at end of first contract, with other sample restrictions noted in column headers. Columns 1 through 4 restrict to the 1993-2004 period, the primary sample period for our reenlistment analysis. Columns 1 and 2 use an unrestricted subset of our earnings estimation sample with no account for missing bonuses. Column 3 drops servicemembers with the shortest initial contracts ($j \geq 4$ years), who might have more foreknowledge of bonus and economic conditions. Column 4 drops those in small occupations ($N_{ot} \geq 15$), to test robustness to missing bonus cells. Columns 5 and 6 uses the entire sample in the period through 2009. Columns 2 and 6 add control variables realized post-enlistment (and hence, potentially endogenous): the natural logarithm of base pay, and indicators for deployment in the last 1 and 3 years of the initial contract, receipt of hostile fire pay in the last 1 and 3 years of the contract, and for fast promotion. See text for additional discussion, particularly of the potential endogeneity of bonus and base pay. s.e. clustered by 1-digit occupation. Significance levels: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 6: First-stage Results

	1993-2004			1993-2009
	All	$j \geq 4y$	$N_{ot} \geq 15$	All
Army	0.75*** (0.16)	0.87*** (0.22)	0.80*** (0.18)	0.65*** (0.15)
Navy	0.38 (0.42)	0.50 (0.46)	0.36 (0.44)	0.66 (0.44)
Marines	0.62*** (0.21)	0.62*** (0.21)	0.64*** (0.23)	0.73*** (0.19)
Air Force	0.30 (0.48)	0.32 (0.47)	0.32 (0.50)	1.20*** (0.41)
F-test	8.69	8.72	4.56	9.90

Notes: The table displays the first-stage of our IV estimates of the response of bonus offers to increases in occupation-specific labor demand, as measured by the size of the first-year cohort assigned by occupation. Linear probability models include fixed effects for 3-digit primary occupation, controls for log of own cohort size in primary occupation, year of eligibility, and interactions of age at eligibility with a set of demographic, AFQT and education controls. Kleibergen-Paap F-statistics reported for the test of the joint significance of the instruments. Columns 1 through 4 restrict to the 1993-2004 period, the primary sample period in the IV specifications. Column 1 uses an unrestricted subset of our earnings estimation sample with no account for missing bonuses. Column 2 drops servicemembers with the shortest initial contracts ($j \geq 4$ years), who might have more foreknowledge of bonus and economic conditions. Column 3 drops those in small occupations ($N_{ot} \geq 15$), to test robustness to missing bonus cells. Column 4 uses the entire sample in the period through 2009. The response of the bonus is positive for all branches and specifications, and of the same order of magnitude, however, statistical power in the F-test draws primarily from the Army and Marines in the earlier period, and the Army, Marines and Air Force in the longer period. F-stats fall in the later period, reflecting a weaker relationship between entering cohort size and bonuses. See text for additional discussion, particularly of branch-level differences in the first-stage estimates. s.e. clustered by 1-digit occupation. Significance levels: *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$. Coefficients multiplied by 100.

Table 7: Effect of Bonus and Unemployment Rate on Reenlistment: 2-digit IV

A. without Branch x Year Fixed Effects						
	1993-2004				1993-2009	
	OLS	IV	IV	IV	OLS	IV
	All	All	$j \geq 4y$	$N_{ot} \geq 15$	All	All
1st stage F		8.69	8.72	4.56		9.90
$\ln(1+\text{Bonus}) (\gamma)$	0.37*** (0.04)	1.46*** (0.52)	1.23*** (0.47)	1.22*** (0.43)	0.43*** (0.06)	0.47 (0.35)
UR (β)	0.86*** (0.19)	0.80*** (0.19)	0.98*** (0.18)	0.81*** (0.20)	0.84*** (0.13)	0.93*** (0.15)
β/γ	2.29*** (0.70)	0.55** (0.27)	0.80** (0.38)	0.67** (0.32)	1.93*** (0.40)	1.99 (1.60)
B. with Branch x Year Fixed Effects						
1st stage F		7.43	7.83	7.80		8.39
$\ln(1+\text{Bonus}) (\gamma)$	0.40*** (0.04)	0.90*** (0.24)	0.71*** (0.15)	0.71*** (0.17)	0.34*** (0.05)	0.89*** (0.25)
UR (β)	0.99*** (0.19)	0.95*** (0.19)	1.15*** (0.17)	0.97*** (0.20)	1.02*** (0.12)	0.99*** (0.13)
β/γ	2.50*** (0.63)	1.06** (0.42)	1.63*** (0.45)	1.37*** (0.51)	3.03*** (0.68)	1.11*** (0.40)
1-digit occ	110	95	95	52	115	96
3-digit occ	1533	1307	1272	671	1674	1330
N	718,425	708,368	571,411	694,272	1,063,148	805,407

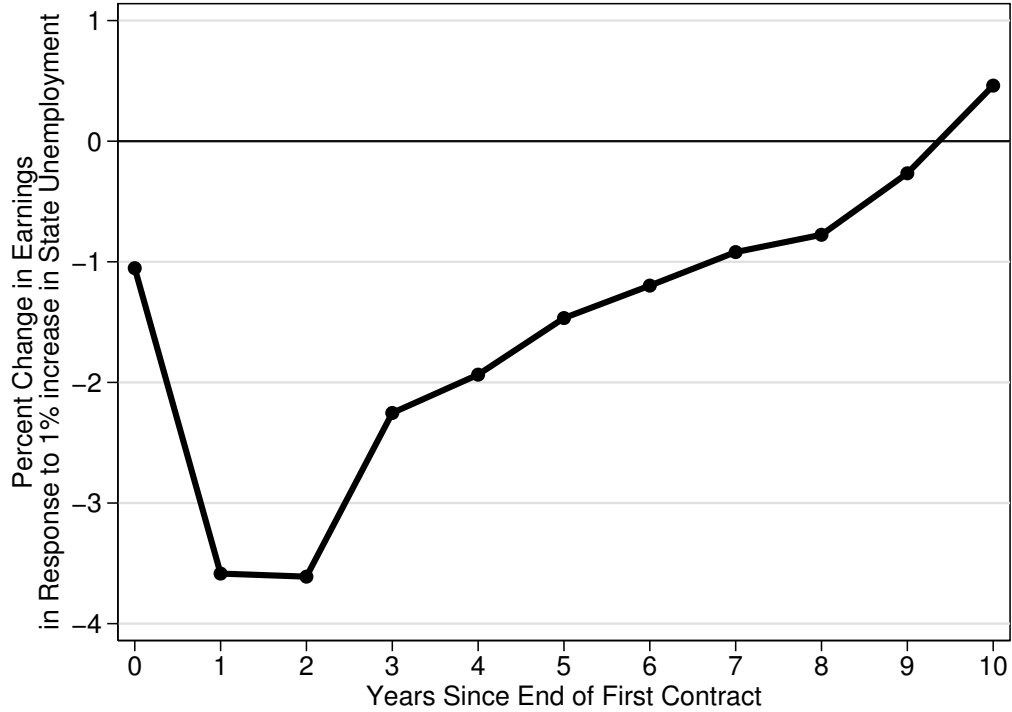
Notes: The table displays our IV estimates of the response of reenlistment to bonus pay and home-state unemployment rates, as well as the ratio of the two, β/γ , which is roughly the the percentage increase in the wage over the next contract servicemembers will trade for 1 point lower unemployment at exit. Sample composed of all enlisted servicemembers at end of first contract with other sample restrictions noted in column headers. Linear probability models include fixed effects for 3-digit primary occupation, controls for log of own cohort size in primary occupation, year of eligibility, and interactions of age at eligibility with a set of demographic, AFQT and education controls. Columns 1 through 4 restrict to the 1993-2004 period, the sample period in which military labor demand most plausibly meets the identifying assumptions of our model. Columns 1 and 2 use an unrestricted subset of our earnings estimation sample with no account for missing bonuses. Column 3 drops servicemembers with the shortest contracts ($j \geq 4$ years), who might have more foreknowledge of bonus and economic conditions. Column 4 drops those in small occupations ($N_{ot} \geq 15$), to account for missing bonuses. Columns 5 and 6 extend the sample period through 2009. Panel B adds branch-by-year fixed effects to the model. The response to state unemployment is robustly significant in all models. Notes: s.e. clustered by 1-digit occupation. Significance levels: *** p<1%, ** p<5%, * p<10%.

Table 8: Mitigating Mechanisms

		$\rho = 0.95$		$\rho = 0.9$		$\rho = 0.8$	
$\Delta y(UR_{st})$		\$3677 (0.78%)		\$3101 (1.31%)		\$2262 (1.91%)	
		WTP	m	WTP	m	WTP	m
β/γ (\approx % wage, next contract)	0.5	\$722	0.80	\$676	0.78	\$603	0.73
	1	\$1443	0.61	\$1352	0.56	\$1207	0.47
	1.5	\$2166	0.41	\$2029	0.35	\$1810	0.20
	2	\$2888	0.21	\$2706	0.13	\$2414	-0.07
	2.5	\$3610	0.02	\$3382	-0.09	\$3017	-0.33

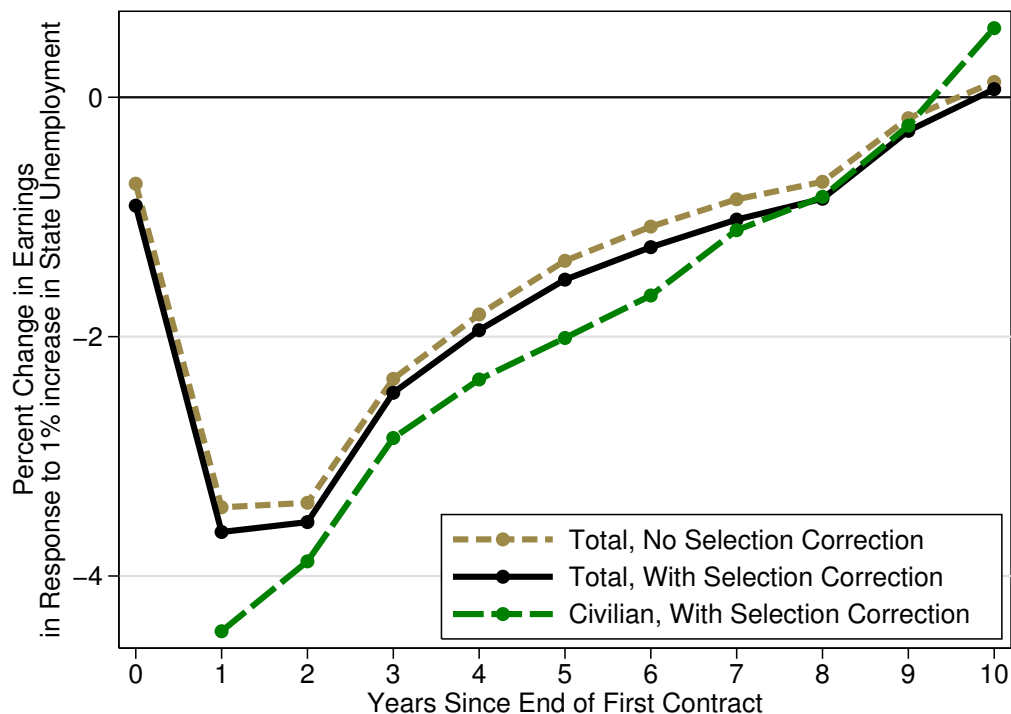
Notes: Mitigating mechanism, m , appear in the model as the residual percentage of the PDV of earnings losses, $\Delta y(UR)$, left unexplained by the willingness-to-pay (WTP), so $m = 1 - \frac{WTP}{\Delta y(UR_{st})}$. Here, we quantify the share of earnings losses offset by mitigating mechanisms, under the range of plausible values of the discount rate and the ratio of regression parameters, β/γ . In the $\Delta y(UR_{st})$ row we report the PDV of earnings losses, and the implied drop in permanent income. Mitigating mechanisms account for between -33 and 80% of earnings losses, with our preferred estimates of 13-35% corresponding to $\rho = 0.9$ and β/γ between 1.5 and 2.

Figure 1: Effect of Unemployment on Total Earnings, 1993-2009 Cohorts



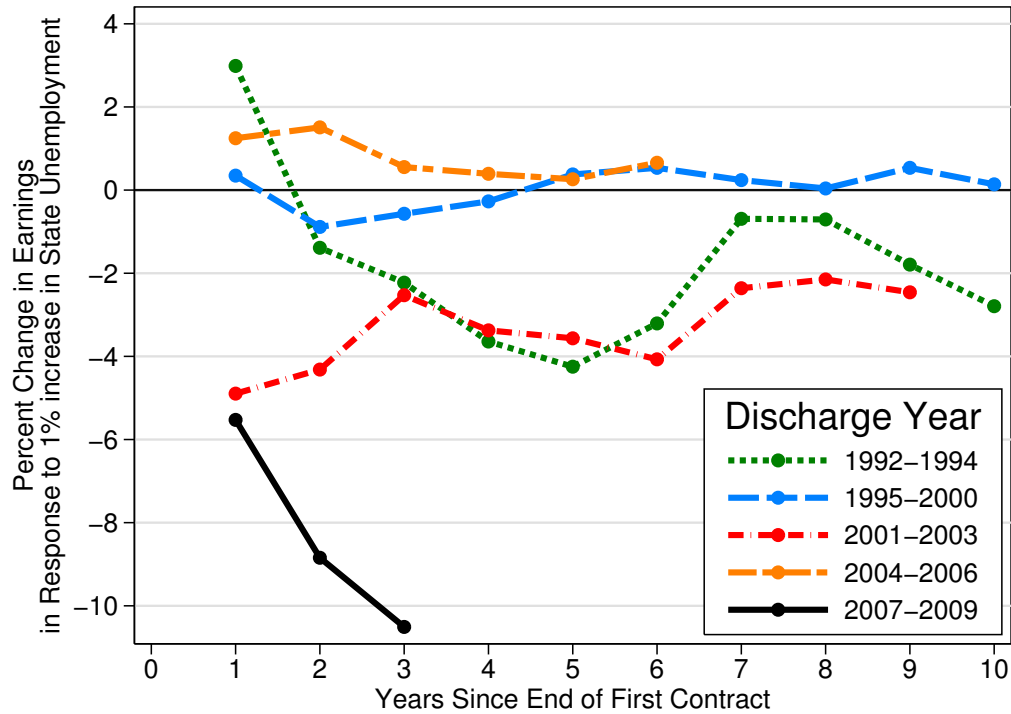
Notes: In the first two full years after the end of their contract, young veterans lose approximately 3.5% of earnings for each percentage point of unemployment at the end of their initial enlistment contract. Effects appear to return to zero around 10 years; regression coefficients reported in Table 2 show statistically significant effects for the first 7 years. Figure plots coefficients from the regression (δ_k in the empirical model) of earnings k years after the end of the initial enlistment contract on state unemployment in the 3 months following the end of the contract. As the regression includes year fixed effects, results can be interpreted as the effect of a local (state-level) unemployment shock. Unbalanced panel. We correct for selection into reenlistment (see Figure 2), and drop those who use the college benefit.

Figure 2: Effect of Unemployment on Earnings, Selection and Civilian Earnings, 1993-2009 Cohorts



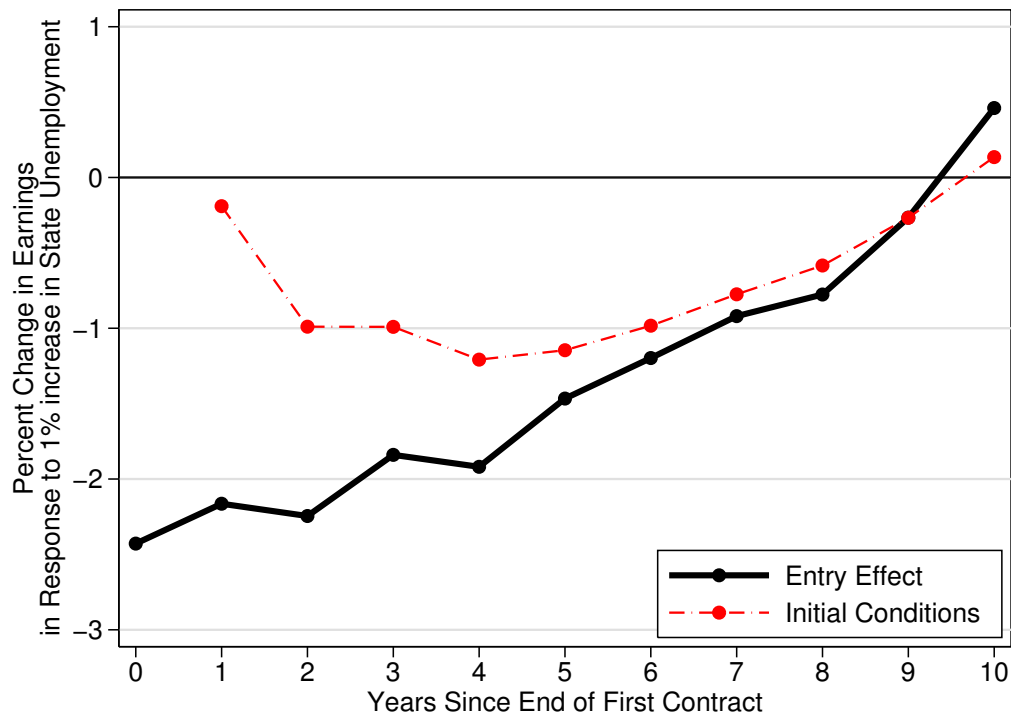
Notes: Figure plots coefficients from a regression of earnings k years after the end of the initial enlistment contract on state unemployment in the 3 months following the end of the contract for our primary specification (Total, with Selection Correction, from Figure 1) and 2 alternative specifications. Unbalanced panel. The first alternative specification drops the selection correction, and reveals that selection into reenlistment plays a small role in our estimates. The second alternative specification, Civilian, With Selection Correction, plots the effect on civilian earnings, defined as SSA earnings minus taxable military income. This civilian earnings specification examines the degree to which young veterans can use their relationship with the military to draw additional earnings to offset civilian earnings lost due to home-state economic conditions (e.g. through the reserves, re-joining the military or contract extensions shorter than 24 months). Effects on civilian and total income appear quite similar after year 0, suggesting these channel play a small role after the year in which the contract ends. Civilian earnings show a large proportional effect in year 0, however, this comes on a small base in income, as can be seen in Table 3; we omit the year 0 civilian estimate from the figure to maintain scale. See text for additional details on regression specification, and Table 3 for average earnings, dollar value of losses, and standard errors.

Figure 3: Effects of Unemployment on Earnings by Period, 1992-2009 Cohorts



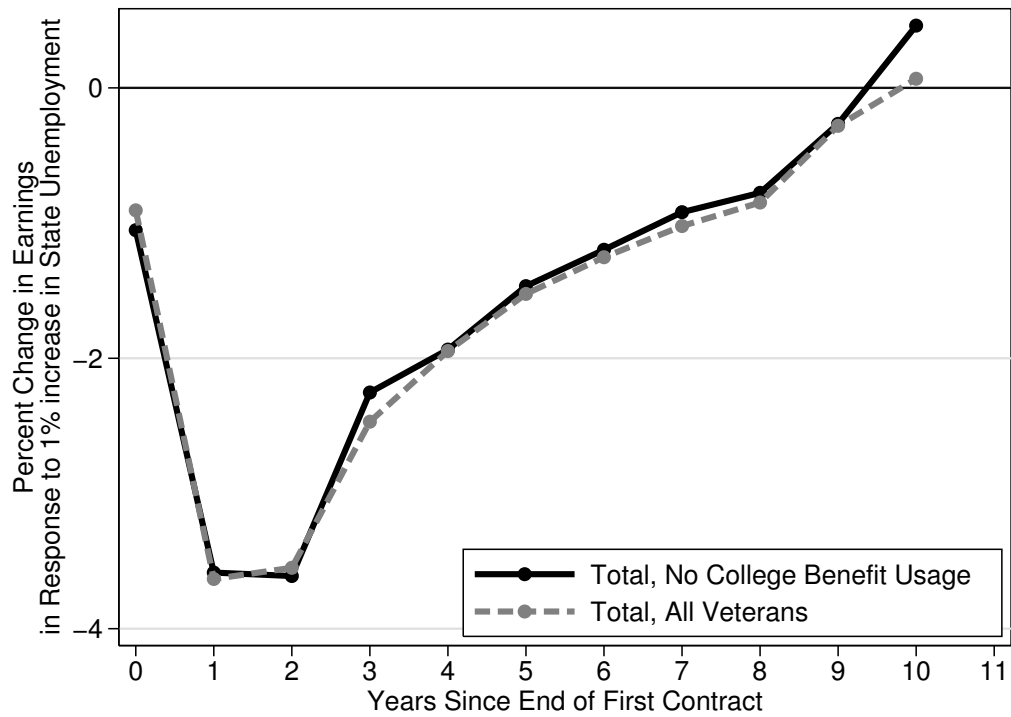
Notes: Figure plots coefficients from a regression of earnings k years after the end of the initial enlistment contract on state unemployment in the 3 months following the end of the contract; regression includes year fixed effects. Unbalanced panel. As in our main estimates in Table 2, we correct for selection and drop those who use the college benefit. Effects (for example, in Figure 1) appear to be driven by recessionary periods; point estimates for entry effects in non-recessions are zero or even positive, while the effects in times of high national unemployment are large and quite persistent. The largest losses are in the Great Recession, and likely reflect longer spells of unemployment experienced by the veterans themselves.

Figure 4: Effect of Unemployment on Earnings and Persistence of Initial Conditions, 1993-2004 Cohorts



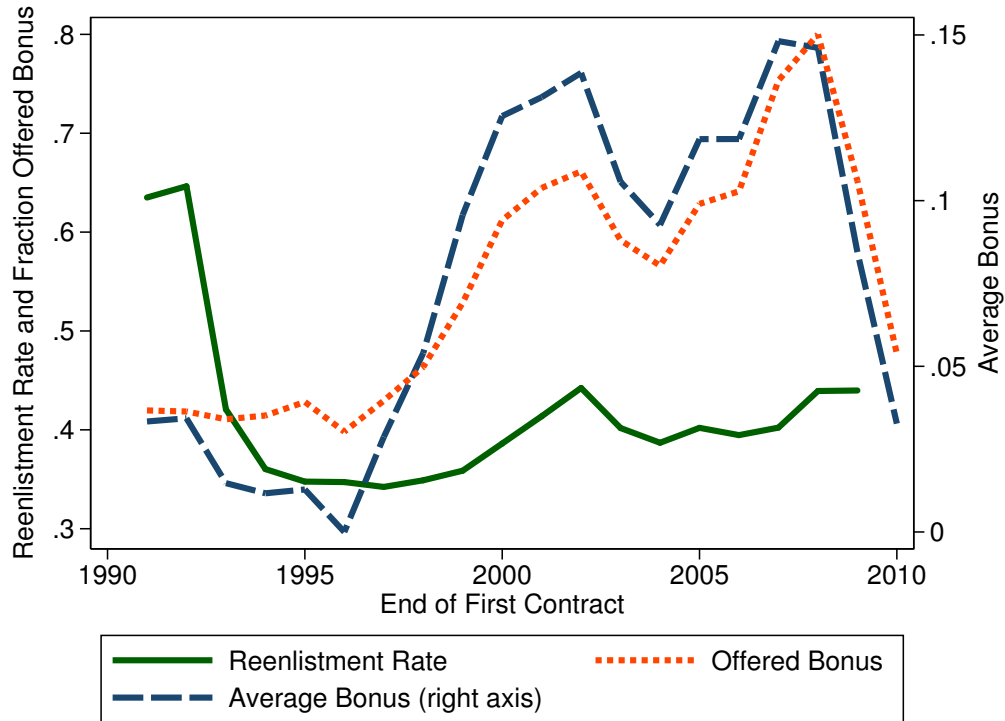
Notes: Figure plots coefficients from a regression of earnings k years after the end of the first contract on state unemployment in the 3 months following the end of the contract; regression includes year fixed effects. Panel balanced through year 5. Initial conditions estimates control for year k unemployment rates to account for the serial correlation between UR_{st} and subsequent unemployment rates. We correct for selection and drop those who use the college benefit. Upon exit, veterans lose over 2% of earnings for each percentage point of unemployment at the time of eligibility for discharge. Initial labor market conditions come to dominate the effect by year 5. Effects appear to return to zero around 10 years; regression coefficients reported in Table 2 show statistically significant effects for the first 7 years.

Figure 5: Effect of Unemployment on Earnings, By College Benefit Usage, 1993-2009 Cohorts



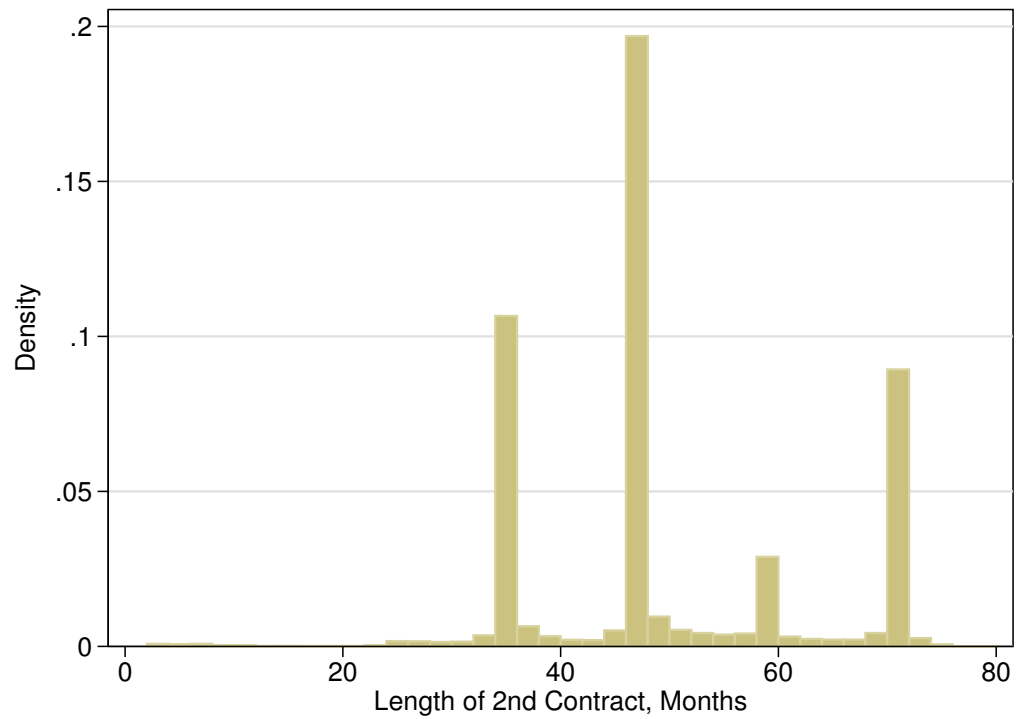
Notes: Figure plots the δ_k regression coefficients separately for all young veterans and those who do not use their college benefits. Unbalanced panel. Dropping those who use their college benefits has little to no effect on our estimates. The coefficients can be compared: Table 2, Column 3 reports results for the sample restricted to those who do not use their GI Bill benefit; and in Table 3, Column 5 can be found estimates for the entire sample.

Figure 6: Bonuses and Reenlistment



Notes: Bonuses grew in importance over the 1991-2010 period, increasing in economic expansions (particularly the late 1990s) and wars, and falling during military drawdowns (for example, in the mid-1990s and at the end of the sample) and recessions. Reenlistment shortfalls (relative to goals) occurred in 2005-2007. Sample composed of all enlisted servicemembers at end of first contract. The bonus formula = months reenlisted (T^*) x bonus multiplier (b_{ot}) x monthly basic salary (y_{it}^{mil}).

Figure 7: Length of New Contract



Notes: Sample composed of all enlisted servicemembers who do not separate within three months following the end of their first contract. Two-month bins. In our empirical specifications, we define reenlistment as an increase in months of service committed of 24 or more months within three months of the end of the first contract. As can be seen, this captures virtually all second contracts.