Student Veterans' Outcomes by Higher Education Sector: Evidence from Three Cohorts of the Baccalaureate and Beyond

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Introduction

As of November 2011, nearly 2.4 million U.S. service members had been deployed to the wars in Iraq or Afghanistan, and nearly 1.4 million of those separated from the military at some point thereafter (Martinez & Bingham, 2011). As wartime service members have returned to civilian life, an important challenge they have faced is assimilation with the civilian labor force—an environment that many, having joined the military shortly after high school, are encountering for the first time.[1] Data suggest that the transition is not always smooth: in 2012, military veterans over age 24 who had served after August 2001 (termed "Gulf War II Era veterans" by the Bureau of Labor Statistics), faced an unemployment rate of 8.6 percent, as compared to only 6.8 percent among non-veterans, and this gap was even larger during the height of the recession (U.S. Bureau of Labor Statistics, 2013).

As is true for their non-veteran counterparts, veterans' employment is closely linked to their level of education. Figure 1 shows that in 2012, the employment gap between Gulf War II Era veterans and their non-veteran counterparts was largest among individuals with some college education but no bachelor's degree, at 3.7 percentage points; and it was smallest among those with no postsecondary education, at 0.4 percentage points (U.S. Bureau of Labor Statistics, 2013).

<Figure 1 about here>

[1] The mean annual age of Active Army enlistees, for example, was 21.3 years, averaged across fiscal years 2001-2011 (U.S. Army, 2013).

The distinction is important because most veterans leaving today's military fall into the "some college but no degree" classification. Enlisted personnel comprised 82 percent of military personnel as of February 2013, representing about 1.14 million individuals (U.S. Department of Defense, 2013). Based on 2005 data from the Defense Manpower Data Center, only about 4.2 percent of enlisted service members held bachelor's degrees, as compared to 95.7% of commissioned officers (Congressional Budget Office, 2004). If enlisted and officer personnel who are exiting today's military are representative of those numbers, we might assume that about 79 percent of recent veterans re-entering the workforce lack bachelor's degrees.

In 2008, Congress passed the Post-9/11 Veterans Educational Assistance Act, commonly known as the Post-9/11 GI Bill, in an effort to streamline the workforce transitions of returning service members. This law, which upgraded the Montgomery GI Bill of 1984, offered the most generous package of veterans' higher education benefits since passage of the Servicemen's Readjustment Act of 1944—the original GI Bill (U.S. Department of Veterans Affairs, 2009). By increasing the amount of funds available to help veterans pursue college degrees, and by creating a federal matching program for private university tuition, the Post-9/11 GI Bill has made a wider array of colleges and universities accessible for returning service members.

That includes proprietary or for-profit colleges, which have come under government scrutiny for deceptive marketing practices and for disproportionately targeting veterans. A two-year study conducted by the Senate Committee on Health, Education, Labor, and Pensions reported that 37 percent of Post-9/11 G.I. Bill benefits were paid to for-profit colleges, and eight of the top ten recipients of Post-9/11 G.I. Bill funding are for-profit education companies (U.S. Senate, 2010). In qualitative and survey data, veterans have explained the draw of for-profit institutions in terms of their vocational focus and their convenience for working adults; they have

also reported relatively high levels of academic advising at these institutions (Steele, Salcedo, & Coley, 2010). But veteran-focused marketing may also play a role. Under a 1998 amendment to the Higher Education Act of 1965, these institutions may receive no more than 90% of their revenue from Title IV federal financial aid, which includes Pell Grants and federally subsidized loans ("Higher Education Act of 1965," 1965). Because GI Bill benefits are not issued under Title IV, they are excluded from this restriction and thus are particularly important streams of revenue for for-profit institutions. According to the nationally representative surveys of bachelor's degree recipients, the share of these degree earners graduating from for-profits rose about five-fold between 1993 and 2008 for both veterans and non-veterans, yielding a 5% share among non-veterans, as compared to a 16% share among veterans (see Figure 2). Due to concerns about student outcomes at for-profit colleges, the Obama administration announced "gainful employment" regulations in 2014, requiring, in part, that the student loan repayments of graduates of for-profit degree programs and cross-sector certificate programs not average more than 20% of discretionary income or 8% of total income (U.S. Department of Education, 2014). In 2017, the Trump administration announced it was reviewing these regulations for possible rollback (U.S. Department of Education, 2017).

<Figure 2 about here>

This article considers how well military veterans fare in the labor force after earning baccalaureate degrees, as compared to observably similar college graduates who lack military experience. It also considers the extent to which graduates' outcomes vary by the sector of the institution from which they graduated—public, private non-profit, or private for-profit. Given the overrepresentation of military veterans in for-profit institutions, the study also examines the extent to which sector effects differ for veterans as compared to non-veterans. To mitigate

selection bias to the extent possible, we use propensity score weights to estimate average effects of treatment on the treated, where the "treated" are defined as graduates with military experience (active or reservist) who have separated from the military. As such, our preferred estimates generalize to college graduates who resemble military veterans within the dataset.

We find that one year after bachelor's degree completion, military veterans were about 8 percentage points more likely than similar non-veterans to be employed full time, and they earned about $1,300 or 2.9% more, conditional on full-time employment. Ten years after bachelor's degree completion, the veteran premium had become a nearly three-percentage-point penalty in terms of full-time employment, but the earnings premium grew to almost $3,000 (4.7%) among those with positive earnings. We speculate that this discrepancy could be due to greater self-reported disability rates of veterans relative to non-veterans ten years after bachelor's degree completion.

Surprisingly, graduation from a for-profit institution was not associated with lower employment or earnings one year post-graduation for veterans or non-veterans. However, graduates of for-profit colleges had much higher predicted odds of graduating with student loan debt. They also borrowed more, conditional on having borrowed at all. Contrary to expectations, veterans graduating from for-profit institutions with student loan debt had actually borrowed about 8.4% more than observably similar non-veterans graduating from these institutions. The data seem to validate policymakers' concerns about veterans' attraction to for-profit institutions, since it is not clear that the availability of GI Bill benefits has circumscribed veterans' borrowing at for-profit institutions. On the other hand, we find no evidence of a labor market penalty for veterans or other graduates for-profit postsecondary institutions.

This article is organized as follows: In the next section, we summarize extant literature on veterans' labor market outcomes and on returns to postsecondary education by higher education sector, and we discuss the potential role of the GI Bill in mitigating postsecondary cost concerns for veterans. We then present our research questions. Next, we discuss the analytic strategy of our paper, followed by a discussion of our data and sample. In the subsequent section, we present the results of our analyses. We conclude with a discussion of what our findings indicate about veterans' performance in the labor force.

Literature Review

There is a substantial literature examining the earnings returns to military service, and a smaller but growing literature on returns to education by higher education sector. However, we are not aware of studies that have looked closely at the intersection of these two issues.

Returns to Military Service

With respect to the first body of research, the estimated effects of military service on subsequent earnings appear to depend on the era of service, the education levels of the service members, and the estimation methods. Descriptive older studies, using simple correlational methods without extensive controls or matching, observed that veterans in the civilian labor market earned as much as or more than non-veterans, especially if they served during the World War II or Korean War eras (Martindale & Poston, 1979; Rosen & Taubman, 1982; Schwartz, 1986) or hailed from minority backgrounds (Martindale & Poston, 1979).

Concerned that some of these apparent earnings benefits were the result of positive selection into military service during the World War II and Korean War eras and not of veteran

status *per se*, Joshua Angrist led a new set of natural experimental studies that used lottery draft data or other quasi-random variables such as month of birth to identify causal effects. That body of work showed earnings penalties rather than advantages for veterans of both the World War II era (Angrist & Krueger, 1994) and Vietnam era (Angrist, 1990; Angrist & Chen, 2011). Angrist and Chen (2011) found that the penalties were at least partially offset by educational gains resulting from use of the GI Bill, which offered veterans education subsidies from 1944 onward, but they still found net earnings advantages for veterans to be close to zero.

Without draft lottery data or other plausibly random assignment variables, studies focusing on veterans from the All-Volunteer Force (AVF) era, which commenced with the end of conscription 1973, have relied primarily on matching or regression approaches, which are less robust to selection bias than lottery-based methods, but which can nevertheless shed light on how veterans perform in the labor market relative to observably similar non-veterans (Angrist, 1998). Some of these studies have used propensity score matching or covariate adjustment (regression) methods, or both, to show earnings benefits for minority veterans and deficits for white veterans (Angrist, 1998; Kleykamp, 2013b; Bryant, Samaranayake, & Wilhite, 1993; Hirsch & Mehay, 2003; Teachman & Tedrow, 2007), or earnings benefits for minorities with no effects on whites (Routon, 2014). This body of work has also found earnings deficits for veterans with higher levels of education, and earnings advantages for those with less than a high school diploma (Bryant, Samaranayake, & Wilhite, 1993; DellaPosta, 2013; Kleykamp, 2013b; Teachman & Tedrow, 2007). Other studies focusing on the AVF era have used regression models to estimate earnings benefits for veterans in lower post-service income brackets and deficits for those in higher brackets (Brown & Routon, 2016).

Insofar as it is possible to generalize across these studies, it appears that the civilian labor market returns to prior military service depend on baseline employment preparation and readiness, such that individuals from minority backgrounds or with lower pre-service educational attainment are more likely to benefit from military service than their counterparts. These studies also clarify the importance of accounting for selection differences between veterans and non-veterans, as individuals with stronger civilian labor market prospects before military service may be less likely not only to pursue military service, but also to benefit from it upon re-entry to the civilian labor force (Bryant, Samaranyake, & Wilhite, 1993; Hirsch & Mehay, 2003; DellaPosta, 2013).

Returns to Education by Higher Education Sector

The literature on returns to education by sector provides useful insight into the importance of sector as a predictor of post-graduation outcomes, but it does not focus specifically on the experience of military veterans. Because individuals self-select into higher education sector, studies of economic returns by sector have used regression analyses descriptively or have used individual-level fixed-effects to estimate the causal effect of sector on post-collegiate earnings.

With regard to descriptive work, differences in returns to four-year degrees between public and private institutions were documented by Thomas (2003), using the first (1993) cohort of participants in the National Center for Education Statistics' (NCES) Baccalaureate and Beyond (B&B) longitudinal surveys. Employing multilevel regression, he found higher debt-to-earnings ratios among graduates of selective and non-selective private institutions than among those graduating from public institutions, though this differential narrowed as graduates gained

labor market experience. Due to the lower prevalence of for-profit institutions in 1993, his analysis was not able to disaggregate findings by for-profit and non-profit private institutions.

Using data from NCES's Beginning Postsecondary Student Survey (BPS), Deming, Goldin, & Katz (2012) also conducted a descriptive analysis, finding that students who first enrolled in for-profit bachelor's degree programs were 12 to 19 percentage points less likely to have earned their bachelor's degrees by 2009 than those who enrolled in public or private non-profit bachelor's programs; they had higher student loan debt and default rates. Among those not still in school, those who began at for-profits were 3 percentage points less likely to be employed, and (partially as a result) were earning about $1800 to $1900 less (in 2009 dollars) than their peers from other institution types.

In a study that used individual fixed-effects models with the National Longitudinal Survey of Youth 1997 (NLSY) to estimate causal changes in students' earnings after they pursued associate's degrees, Cellini & Chaudhary (2014), found that earnings grew about 7% for each year of education students received in a for-profit college (combining employment and earnings effects), as compared to estimates from the literature showing 5% to 8% returns for each year of education in other two-year programs (Kane & Rouse, 1995, cited in Cellini & Chaudhary, 2014). However, Turner (2011) used IRS earnings data with individual fixed-effects to find only a 2% return for each year at a for-profit institution, as against a 6% return for students attending public and private, non-profit institutions.

Lang and Weinstein (2013) used BPS data to focus on students starting certificate or associate's degree programs. They found no statistically significant differences in earnings for those who obtained certificates or associate's degrees from for-profits as compared to public or non-profit community colleges. And Cellini and Turner (2016) linked data from programs

subject to the new gainful employment regulations to IRS records. They found negative earnings effects of 5%-6% for attending a degree program at a for-profit, largely driven by high rates of dropout. For those who obtained degrees from for-profits, earnings gains for each year of enrollment were about 11% for associate's programs, 7% percent for bachelor's programs, and 9% percent for master's programs. As the authors noted, generalized cross-sector estimates of returns to each year of bachelor's degree program enrollment were at least 6% to 9%, but these estimates included those who did and did not complete degrees (Kane & Rouse, 1995; Oreopoulos & Petronijevic, 2013), meaning that students earning degrees from for-profits likely underperformed their counterparts graduating from public and private non-profit institutions.

In an effort to understand how employers actually respond to degrees from for-profit colleges, a few resume-audit studies have randomized institutions to resumes and tracked employer call-backs. In one such study, Deterding and Pedulla (2016) found no difference in employers' responses to associate's degrees from for-profit and public institutions in hiring for administrative positions. Randomizing within six different career fields and three education levels, Darolia and colleagues (2015) also found no statistically significant differences in call-backs between applicants from for-profit and public institutions, though the estimate magnitudes favored public institutions over for-profits. Finally, Deming and colleagues (2016), found a callback penalty in business jobs for bachelor's degrees from large, online for-profits but not from small brick-and-mortar ones; in health jobs, they found a for-profit callback penalty of 57% unless the jobs required occupational licenses.

Taken together, the research on returns to a for-profit college education suggest that students who enroll in for-profits are less likely to complete degrees than those who enroll in public or in private, non-profit institutions. Among those who do earn degrees, those from for-

profits seem to perform no better than, and perhaps slightly worse than, their counterparts graduating from public or non-profit institutional sectors, with those graduating from large, online for-profits being especially vulnerable. Also, the higher debt loads and default rates of those who attend for-profits raise the question of whether and when these institutions represent a reasonable investment.

Education Cost Differentials for Veterans

Military veterans choosing postsecondary institutions may face a modestly different cost curve from their non-veteran counterparts if they are eligible for GI Bill benefits. Since June 30, 1985, active duty service members and veterans with at least three years of service have been eligible for the Montgomery GI Bill (MGIB), which as of 2013 offered $1648 per month in tuition and living expenses for up to 36 months toward full-time enrollment in a higher education program, or $1236 per month for the first six months of an apprenticeship or on-the-job training program, and $906 for the next six months. (U.S. Department of Veterans Affairs, 2013). And G.I. Bill support does not diminish students' eligibility for federal financial aid (U.S. Department of Education, 2012). However, even the maximum rate of reimbursement under the MGIB granted students only $14,832 per academic year, which was not enough to cover the national average cost of tuition, room, and board in the fall of 2013, which was $18,383 in nominal dollars (College Board, 2017).

Motivated by concerns that the MGIB had not kept up with increasing higher education expenses, in 2008 Congress authorized the Post-9/11 GI Bill (Iraq and Afghanistan Veterans of America, 2008), which offers free tuition at public institutions or (as of 2016-17) tuition and fees up to about $22,000 annually at private institutions, in addition to a geographically adjusted

living allowance and book stipend (U.S. Department of Veterans Affairs, 2016a). Moreover, private institutions—including for-profit institutions—can choose to subsidize an even greater share of their tuition and receive matching GI Bill dollars through the Yellow Ribbon GI Education Enhancement Program (U.S. Department of Veterans Affairs, 2016b). Thus, for veterans with full GI Bill eligibility, tuition at many for-profit institutions should be deeply subsidized, reducing the individual risk of choosing this type of institution. Despite this fact, news reports have suggested that veterans are still borrowing to attend for-profit institutions — sometimes larger sums than their non-veteran counterparts (Zarembo, 2015).

Research Objectives

This study uses data from the MGIB era to examine how veterans fare in the labor market after earning bachelor's degrees from public, private non-profit, and private for-profit institutions. In particular, we examine (1) whether veterans differ from observably similar non-veterans in terms of their student debt loads at graduation in 1993; their full-time employment status one year after graduation, and their earnings one year after graduation, conditional on full-time employment. For each of these outcomes, we examine (2) whether performance differs by postsecondary sector (public, private non-profit, or private for-profit), and (3) whether sector effects differ for veterans versus non-veterans. Finally, we examine (4) veterans' employment status and earnings relative to non-veterans four and ten years after graduation, and (5) whether veterans' relative performance over time depends on graduation from a public or private institution. (In the latter analysis, data are insufficient to compare graduates of for-profit institutions to graduates of the other institution types.)

Though our study design does not provide causal identification of the effect of veteran status (which is self-selected) or of the higher education sector from which graduates earned a degree (a choice that occurs after achieving veteran status in our dataset), our intent is to describe veterans' labor market outcomes after earning degrees and to ascertain the extent to which they differ from those of observably similar graduates who are not veterans. Because we are interested in isolating the observed effects of veteran status and institutional sector as completely as possible, our analysis controls for observable differences by veteran status and by institutional sector in two key ways. First, we use statistical control variables in a regression framework to remove the linear (or logistic) effects of variables likely to be correlated with both veteran status and the outcomes of interest. Second, we construct and apply average-treatment-on-the-treated (ATT) propensity score weights to the analytic sample so that non-veterans are observably similar to veterans in the data.

Though it focuses on the Montgomery GI Bill era, our study is the first we are aware of to examine military veterans' outcomes by higher education sector, and thus the intersection of the returns-to-service and returns-to-sector questions. Because our data focus only on bachelor's degree completers, they do not capture the outcomes of those who *began* postsecondary education at for-profit or other institution types.

Yet our approach makes an important contribution to the literature. Earlier studies that have shown that more highly educated veterans suffer greater earnings penalties than their peers with the lowest levels of education (Bryant, Samaranayake, & Wilhite, 1993; Kleykamp, 2013b; Teachman & Tedrow, 2007), but these studies have not taken into account how long participants have held their educational credentials. This is important because veterans may be slower to earn educational credentials (due to time spent in military service) and thus may have less labor

market experience post-credentialing than peers of similar ages and education levels. Our approach holds time since degree constant for all participants and focuses only on the relative performance of recent bachelor's degree recipients.

Analytic Strategy

The challenge in estimating the returns to veteran status with observational data is that individuals are not randomly assigned to veteran status, so veterans may differ from non-veterans in ways that are related to their post-graduation outcomes but that are not fully captured by the covariates in the analysis. To mitigate selection or omitted variable bias concerns, our analyses include statistical controls for characteristics that are likely to be related to both the independent variable of interest (veteran status at graduation) and to the dependent variables of interest (student loan debt, employment, or earnings in the decade after graduation). For instance, relative to other college students and other citizens, military veterans may differ from non-veterans in terms of gender, race/ethnicity, and background socioeconomic status, all variables that are predictive of differences in employment and earnings (National Center for Veterans Analysis and Statistics, 2017; Radford, 2009). In addition, because of their years of military service, military veterans tend to be older than their counterparts when they enter college (Cate, 2014; Radford, 2009). We also control for graduation year, since different cohorts face different labor market contexts. In addition, our models control for characteristics of the graduates' higher education institutions, including Carnegie classification, as well as institutional sector (public, private non-profit, or private for-profit), which is our moderating variables of interest. Since military veterans may be more likely than their peers to major in applied fields (Durdella & Kim, 2012), and because major choice is strongly predictive of postsecondary earnings (Arcidiacono, 2004; Carnevale, Cheah, & Hanson, 2015), we also control for the academic major of the

bachelor degree. In addition, we use state fixed effects to control for the state of the institution from which the individual graduated; the intent is to adjust for unobserved labor market differences among states (Wooldridge, 2002).

An approach that relies strictly on regression controls is somewhat sensitive to the functional form of each variable's relationship to the outcomes of interest, be it linear, quadratic, cubic, etc. (Angrist, 1998; Cook et al., 2009). Combining a propensity score weighting approach with regression controls in a "doubly robust" estimation model reduces our reliance on functional form assumptions in both the propensity score estimation models and the regression models (Funk et al., 2011; Hirano & Imbens, 2002).

Therefore, in addition to controlling for the aforementioned characteristics in a regression model, we use propensity scores (Rosenbaum & Ruben, 1983) to weight the analytic sample so that veterans and non-veterans in the sample are observably similar in terms of the aforementioned characteristics. Studies comparing the effectiveness of propensity score matching or weighting to multivariate regression using the same variables have concluded that these two methods yield similar results, and that the main determinant of their ability to yield causal inferences lies in how well the variables account for selection differences between the treated and the untreated (Cook et al., 2009; Steiner et al., 2010). In addition, Angrist (1998) has shown that the two methods differ in the relative weight they give to comparison-group cases, such that propensity score matching or weighting weights observations proportionally to their probability of being in the treatment group at each covariate value, and regression, in effect, weights observations proportionally to the variance of treatment group membership at each covariate value (p. 256).

We choose propensity score weighting over matching because weighting better ensures common support (i.e., sufficient overlap) of covariates in the treatment and comparison groups and also removes concerns about sensitivity of results to the matching algorithm, such as nearest neighbor, caliper matching, etc. (King & Nielsen, 2016). Using data from a heart catheterization study, Hirano & Imbens (2002) demonstrated that our chosen approach—multivariate regression with propensity score weighting—is more efficient than propensity score matching because it uses more of the data and is more robust to model misspecification than regression or propensity score matching alone.

Our approach allows us to estimate the average effect of treatment on the treated (again, ATT), which in this case represents the effect of veteran status on those who are similar to veterans on observed attributes. Note that this is distinct from estimating the average treatment effect—ATE—or the average treatment on the comparison group—ATC. In other words, we aim to isolate veterans' labor market performance relative to recent college graduates who are observably similar to veterans (Austin, 2011).

In propensity score and in regression analyses, it is ideal to condition only on characteristics that predict selection into the treatment group (Cook et al, 2009), and not to weight or control for attributes that occur after selection into treatment, as these may plausibly result from the treatment itself (Austin, 2011; Cook et al., 2009; Schneider et al., 2007). In this case, however, we are tracking labor market outcomes for veterans versus non-vetereans *after bachelor's degree completion*, where completion itself—our start of time—may be affected by veteran status.[2] In effect, our treatment of interest can be understood as *veteran status among*

[2] Similarly, Bryant, Samaranayake, & Wilhite (1993) incorporate a selectivity correction term estimated purely from pre-enlistment characteristics —essentially, a propensity score—into their regression of log wages on veteran status, but they also condition their analysis of log-wages on full-time employment, which may itself be a consequence of veteran status.

new college graduates. As such, we include in our propensity score model characteristics that precede college completion and that may be associated with both veteran status at graduation and with post-baccalaureate outcomes of interest. This choice allows us to achieve high balance on potentially confounding observables between the treatment and comparison groups (Rosenbaum & Rubin, 1983; Steiner et al., 2010). Also, our moderator of interest—institutional sector—clearly varies with veteran status and may be in part affected by veteran status for the reasons described above. Our aim in using propensity score weighting with regression controls is to remove from the analysis attributes that are correlated with veteran status at the point of bachelor's degree completion, so they can be discounted as possible mediators of veterans' relative labor market performance. This places our approach in the context of other AVF-era studies that have used regression controls (Brown & Routon, 2016; Teachman & Coll, 1996; Teachman & Tedrow, 2007), propensity scores matching or weighting (DellaPosta, 2013; Hirsch & Mehay, 2003); regression with controls for a propensity score (Bryant, Samaranayake, & Wilhite, 1993), or a comparisons of regression, matching, and instrumental variables approaches (Angrist, 1998) to gauge civilian labor market earnings differences between veterans and non-veterans. Ours is the only veterans' earnings analysis we are aware of to combine propensity score weighting with regression in a doubly robust regression model, but we also emulate Angrist (1998) in that we compare regression estimates with and without propensity score weighting, discussing the substantive interpretation of the differences in terms of the attributes of each comparison group. In the ATT estimates, the comparison group is weighted to be as similar to the treatment group as possible on pre-graduation characteristics, whereas in covariate adjustment without ATT weighting, the comparison group reflects the characteristics of the broader sample of college graduates (Angrist, 1998; Austin, 2011; Rosenbaum & Rubin, 1983).

The ATT-weighted estimates apply to individuals with similar pre-graduation characteristics to veterans, offering some reassurance that post-graduation differences between veterans and non-veterans are attributable to veteran status and not to other attributes. However, this reassurance is conditioned on the assumption of strong ignorability, meaning that all pre-graduation differences between veterans and non-veterans that are related to the outcomes of interest are fully captured by the observed covariates (Rosenbaum & Rubin, 1983). Given that this is a rather strong assumption (Angrist, 1998), we cannot definitively attribute outcome differences between veterans and non-veterans to the causal effect of veteran status, but we can still isolate differences that are unexplained by a variety of other predictors related to veteran status at graduation and to labor market outcomes.

Constructing ATT Weights

The ATT weights are generated from propensity score estimates of the probability that a college graduate is classified as a military veteran at the time of graduation. The propensity score estimation model is specified as follows in equation 1:

$$\ln\left[\frac{p(vet_{inj})}{1-p(vet_{inj})}\right] = a_0 + \boldsymbol{\mu_0}'\mathbf{C_j} + \boldsymbol{\lambda_0}'\mathbf{P_n} + \boldsymbol{\eta_0}'\mathbf{F_{nj}} + \boldsymbol{\varphi_1}'\mathbf{S_{nj}} + \boldsymbol{\kappa_0}'\mathbf{M_{inj}} + \boldsymbol{\pi_0}'\mathbf{X_{inj}} + \delta_0 w_{inj} \qquad (1)$$

The dependent variable, vet_{inj}, is a dichotomous variable coded 1 if graduate *i* from institution *n* in cohort *j* was a military veteran at the time of graduation, and 0 if he or she was not. The functional form is a logistic regression model in which α_0 is the intercept term, $\mathbf{C_j}$ is a vector of cohort fixed effects, $\mathbf{P_n}$ is a vector of higher education sector fixed effects, $\mathbf{F_{nj}}$ is a vector of Carnegie classification category fixed effects for institution *i* and cohort *j*, $\mathbf{S_{nj}}$ is a vector of fixed effects indicating the state in which institution *i* is located for cohort *j*, $\mathbf{M_{inj}}$ is a

vector of academic major category fixed effects, $\mathbf{X_{inj}}$ is a vector of student-level covariates including age at graduation, gender, race/ethnicity, and a parental education indicator, and w_{inj} is a cross-sectional B&B probability weight for graduate *i*, reflecting his/her probability of sample selection and of response to the survey at wave 1, one year after baccalaureate degree completion. The fitted probabilities of veteran status, $\hat{p}(vet_{inj})$, are then used to construct raw ATT weights, att_wt_{inj} as described in Austin (2011):

$$att\_wt_{inj} = vet_{inj} + \frac{(1 - vet_{inj}) * \hat{p}(vet_{inj})}{1 - \hat{p}(vet_{inj})} \quad (2)$$

The formula in equation 2 gives each veteran a weight of 1 and each non-veteran a weight with an asymptotic range from 0 to ∞ and an observed range, in this case, from 0.0001 to 3.8, with a mean of 0.35.

Adjusting ATT Weights for Probability Sampling and Non-Response

We incorporate B&B design weights that reflect probability sampling and survey non-response by creating survey-adjusted ATT weights. To generate the survey-adjusted weights, we multiply att_wt_{inj} by the appropriate survey weights for each analysis, as described in DuGoff, Schuler, & Stuart (2014). In the analysis of outcomes one year after graduation, we multiply the raw ATT weight by the cross-sectional survey weight for the Wave 1 survey. In the analysis of outcomes up to ten years post-graduation for the 1993 graduating cohort, we multiply the raw ATT weight by the weight for respondents to all three survey waves. In each analysis, we apply these survey-adjusted ATT weights in our preferred model specifications.

Estimating Outcomes One Year Post-Baccalaureate

To estimate the effects of veteran status on the outcomes of interest, we fit linear and logistic regression models within the unweighted, survey-weighted, and survey-adjusted ATT weighted samples. Our particular interest is in estimates from the latter weighting strategy, since it estimates effects of veteran status at graduation on individuals observably similar to veterans. However, we present results from other weighting strategies, especially in our first regression table, to show the sensitivity of our results to the weighting decision.

The logistic regression model for dichotomous outcomes is specified as follows:

$$\ln\left[\frac{p(d_{inj})}{1-p(d_{inj})}\right] = a_1 + \beta_1 vet_{inj} + \boldsymbol{\mu_1}'\mathbf{C_j} + \boldsymbol{\lambda_1}'\mathbf{P_n} + \boldsymbol{\eta_1}'\mathbf{F_{nj}} + \boldsymbol{\varphi_1}'\mathbf{S_{nj}} + \boldsymbol{\kappa_1}'\mathbf{M_{inj}} + \boldsymbol{\pi_1}'\mathbf{X_{inj}} + \boldsymbol{\tau_1}'(vet * \mathbf{P})_{inj} \quad (3)$$

where the dependent variable, d_{inj} is a dichotomous outcome of interest, such as graduating with student loan debt, or being employed full-time one year after bachelor's degree completion. The function form is logistic, meaning that the predictors are linearly related to the log-odds of the dependent variable. The independent variable of interest is vet_{inj}, with the other covariates defined as in equation 1. The interaction effects of veteran status with a vector of institutional sector fixed effects is given by parameter vector $\boldsymbol{\tau_1}$. It represents the differential effects of veteran status for graduates of private non-profit and of private for-profit institutions relative to public institutions.

The predictive model for continuous outcomes is given by:

$$y_{inj} = a_2 + \beta_2 vet_{inj} + \boldsymbol{\mu_2}'\mathbf{C_j} + \boldsymbol{\lambda_2}'\mathbf{P_n} + \boldsymbol{\eta_2}'\mathbf{F_{nj}} + \boldsymbol{\varphi_2}'\mathbf{S_{nj}} + \boldsymbol{\kappa_2}'\mathbf{M_{inj}} + \boldsymbol{\pi_2}'\mathbf{X_{inj}} + \varepsilon_2 \qquad (4)$$

where the dependent variable is the natural log of student loan debt *among those graduating with debt* or the natural log of earnings in 2009 dollars, *among those employed full-time*. Parameter

ε_2 is a normally distributed error term with mean 0 and standard deviation σ. The other terms are defined as above.

Estimating Employment and Earnings Over Time

For the 1993 graduating cohort, we are able to examine veterans' outcomes not only one year after bachelor's degree completion, but also four and ten years after. Due to sample size limitations, we now treat higher education sector, P_{nj}^{d}, as dichotomous—private versus public—instead of three, so we are not able to comment in this analysis on the longer-term effects of graduating from a for-profit institution relative to the other institution types. The logged odds of being employed full-time in any given survey wave are estimated as follows:

$$\ln\left[\frac{p(emp_{int})}{1-p(emp_{int})}\right] = a_3 + \beta_3 vet_{in} + \boldsymbol{\gamma_3}'\mathbf{T_t} + \lambda_3 P_{in}^{d} + \boldsymbol{\eta_3}'\mathbf{F_{in}} + \boldsymbol{\varphi_3}'\mathbf{S_{in}} + \boldsymbol{\kappa_3}'\mathbf{M_{in}} + \boldsymbol{\pi_3}'\mathbf{X_{in}} + \mathbf{v_3}'\mathbf{(vet*T)_{int}}$$
$$+\varsigma_3(vet*P^{d})_{\text{in}} + \boldsymbol{\delta_3}'\mathbf{(vet*T*P^{d})_{int}} + \boldsymbol{\theta_3}'\mathbf{(P^{d}*T)_{int}} + \mathbf{v_3}'\mathbf{(F*T)_{int}} + \boldsymbol{\rho_3}'\mathbf{(M*T)_{int}} \quad (5)$$
$$+\boldsymbol{\psi_3}'\mathbf{(X*T)_{int}}$$

where we now allow the effect of veteran status at graduation, vet_{in}, to vary by a vector of time-period dummy variables, $\mathbf{T_t}$. The effects of other model covariates are also allowed to vary by $\mathbf{T_t}$ except for the vector of state fixed effects $\boldsymbol{\varphi_3}$, which are estimated as time-invariant. The model also estimates the differential effects of veterans status on those who graduate from private versus public institutions (given by ς_3, the coefficient on $(vet*P^{d})_{in}$), and the extent to which any veteran-by-sector interaction effects vary over time (represented by $\boldsymbol{\delta_3}$, a vector of coefficients for the $\mathbf{(vet*T*P^{d})_{int}}$ vector). We fit the specification as a multilevel logistic regression model to reflect the nesting of time-specific observations within individuals.

To estimate earnings over time in the 1993 cohort, we fit a multilevel linear regression model as given in equation 6:

$$\begin{aligned}\ln(earn_{int}) = {} & a_4 + \beta_4 vet_{in} + \boldsymbol{\gamma_4}'\mathbf{T_t} + \lambda_3 P_{in}^{d} + \boldsymbol{\eta_4}'\mathbf{F_{in}} + \boldsymbol{\varphi_4}'\mathbf{S_{in}} + \boldsymbol{\kappa_4}'\mathbf{M_{in}} + \boldsymbol{\pi_4}'\mathbf{X_{in}} + \boldsymbol{\nu_4}'\mathbf{(vet * T)_{int}} \\ & + \varsigma_4 (vet * P^{d})_{\text{in}} + \boldsymbol{\delta_4}'\mathbf{(vet * T * P^{d})_{int}} + \boldsymbol{\theta_4}'\mathbf{(P^{d} * T)_{int}} + \boldsymbol{\nu_4}'\mathbf{(F * T)_{int}} + \boldsymbol{\rho_4}'\mathbf{(M * T)_{int}} \\ & + \boldsymbol{\psi_4}'\mathbf{(X * T)_{int}} + u_{4in} + \varepsilon_{4int}\end{aligned} \quad (6)$$

where the continuous dependent variable is the natural log of earnings for individual *i* from institution *n* observed in time *t*, among those with positive earnings in that time period. The covariates are as specified in equation 5. The model includes a two-part error term to reflect the hierarchical nature of the data, such that u_{4in} is an individual-level error term and ε_{4int} is an observation-level error term, both of which are assumed to be normally distributed with mean 0 and standard deviation σ.

Data and Sample

We conduct these analyses using a nationally representative sample of students who completed bachelor's degrees in 1993, 2000, and 2008. The source is the Baccalaureate and Beyond, which, as noted above, is a set of longitudinal surveys conducted by the National Center for Education Statistics. The base samples of the B&B are pulled from the National Postsecondary Student Aid Study (NPSAS), which is a nationally representative draw of postsecondary students and institutions. The graduating cohort of 1993 was surveyed in the spring of the graduation year and again one, four, and ten years after bachelor's degree completion, in 1994, 1997, and 2003, respectively. The cohorts of 2000 and 2008 were surveyed in the spring of the graduation year and one year thereafter, in 2001 and 2009, respectively.

Therefore, our analysis is able to track the 1993 graduates for a decade after bachelor's degree completion, and to track the 2000 and 2008 graduates for one year after completion.

<Table 1 about here>

In the *first three columns* of Table 1, we display survey-weighted descriptive statistics by B&B cohort, using the cross-sectional survey weights for each cohort. The *middle three columns* provide descriptive statistics overall and disaggregated by veteran versus non-veteran status at the time of bachelor's degree completion, using cross-sectional survey weights. Veterans are defined as those with previous military service—whether as a reservist or on active duty—who are no longer on active duty or reserve status. The *final three columns* also present descriptive statistics overall and by veteran status, this time using survey-adjusted ATT weights, which are constructed as described in the previous section.

The sample sizes refer to the (unweighted) number of survey respondents in the analytic sample in each column: there were 11,192 respondents in the 1993 cohort, 11,702 in the 2000 cohort, and 15,048 in the 2008 cohort. The percentage who were veterans was approximately 4 percent in 1993 and 3 percent in each of the other two cohorts. Similarly, graduates of for-profit institutions constitute just under 3% of the analytic sample, or about 961 graduates, of which 80 were military veterans. The result is that, while our estimates represent the experience of randomly sampled individuals, we must extrapolate the differential effects of veteran status by sector with some caution.

A few patterns are noteworthy in the descriptive statistics. Focusing on the middle three columns, which disaggregate by veteran status and use survey weights but not ATT weights, we observe that veterans were much less likely than other graduates to be female and to have a parent who held a college degree; they were also more likely than other graduates to be black or

Hispanic. As we expected based on prior literature, veterans in the sample were also more likely than their counterparts to have graduated from a for-profit as compared to a public or non-profit institution (7% versus 3%), and less likely to have graduated from a public institution (59% versus 65%). They were also substantially older at the time of graduation, at 35 versus 25 years old, on average. (The pooled standard deviation is 6.6 years.) With regard to the 2005 Carnegie classification of their institutions, veterans were less likely to have graduated from a Research and Doctoral institution than from one of the other classification categories (30% vs. 46%). Importantly, each of the four collapsed Carnegie categories included public, private non-profit, and for-profit institutions, which allows our analysis to separate for-profit effects from Carnegie classification effects. In terms of academic major, veterans were markedly overrepresented in business/health/vocational fields, and slightly overrepresented in STEM, but underrepresented in humanities, social sciences, and education. (The STEM category includes scientific and pre-medical fields, whereas the business/health/vocational category includes nursing and health care support fields.) A slightly lower share of veterans than non-veterans in the sample graduated with student loans (57% versus 59%), and a considerably larger share (77% versus 67%) were employed full-time one year after earning bachelor's degrees. Nearly identical shares—about 20%—of veterans and non-veterans were enrolled at least part-time in post-baccalaureate educational programs one year after finishing bachelor's degrees. The mean loan amounts and incomes in Table 1 are reported for the full analytic sample, not just for those who had loans or who were employed full-time. In the survey-weighted sample, veterans graduated with about $1,700 less in student loan debt and earned at least $10,000 more annually, on average. All monetary values are reported in 2009 constant dollars.

When survey-adjusted ATT weights are applied, observable differences between veterans and non-veterans on the pre-graduation measures largely disappear, meaning that we achieve good balance on observables in the survey-adjusted ATT-weighted sample. For instance, the average age, gender, and institutional profiles of non-veterans in the ATT sample appear similar to those in the veteran sample. Even the post-graduation measures have become more similar, although these were not used in construction of the ATT weights.

Results

Throughout the analyses reported in Tables 2-4, our preferred specification uses the survey-adjusted ATT weights. But because different weighting approaches estimate substantively different parameters, we begin by presenting the results from multiple weighting schemes in our first analysis, which concerns student loan debt at college graduation. In subsequent analyses, we show only two sets of coefficients per outcome: those based on survey weights without ATT weighting (a regression-only approach), and those that include survey-adjusted ATT weights., which remains our preferred approach. In all models, we control for the full set of specified covariates and a vector of state fixed effects corresponding to the state of the bachelor's degree-granting institution.

Student Loan Debt at Graduation

Indebtedness

Logistic regression coefficients estimating the probability of graduating with any student loan debt area shown in Panel A of Table 2. In each pair of columns, the left column shows overall veteran-status effects, and the right column shows differential effects of veteran status by

higher education sector. Columns 1 and 2 present estimates from the full regression model in the unweighted sample. Columns 3 and 4 present estimates from the sample weighted by the cross-sectional survey weights but without ATT propensity scores weights. Columns 5 and 6 show estimates from the ATT-weighted sample, without survey weights. Finally, our preferred estimates in columns 7 and 8 are generated with survey-adjusted ATT weights.

<Table 2 about here>

By comparing estimates in columns 1-2 to those in columns 3-4, and columns 5-6 to columns 7-8, we find that estimates are not highly sensitive to the use of survey weights—that is, to making the sample attributes nationally representative. However, comparing columns that use propensity score weights (5-8) to those that do not (1-4), we find that results *are* somewhat sensitive to the use of ATT weights. This is reasonable given that estimates from the ATT-weighted sample represent the outcomes of veterans relative to observably very similar non-veterans. In contrast, the models without ATT weighting rely solely only on covariate adjustment to make the veteran and non-veteran groups comparable, and covariate adjustment is sensitive to correct functional form specification for all covariates (Allen, 1999; Funk et al., 2011).

Contrary to our expectations, we find that veterans in the analysis graduated with student loan debt at about the same rate as non-veterans. In all weighting schemes, veterans in the sample appear modestly less likely to graduate with debt than non-veterans, but these differences reach marginal significance only in column 3. In the interaction models, we find that they are driven by veteran graduates of private, non-profit institutions, as indicated by the negative and at least marginally significant coefficients on the veteran-by-private interaction terms in all weighting schemes.

Marginal effects from columns 7 and 8 are represented in the left-hand panel of Figure 3, where we find that the estimated share of students graduating with debt was 65.1% among veterans and 66.6% among observably similar non-veterans, holding other terms constant at their sample means. However, this difference was not statistically significant and was driven by graduates of private, non-profit institutions, where the veteran indebtedness rate was only 62.5%, as against 70.4% among similar non-veterans. At public and for-profit institutions, the share of veterans graduating with debt was modestly higher than the comparable figures for non-veterans, though the differences did not approach statistical significance. What is particularly striking, though, about the left-panel estimates in Figure 3 is the difference in the indebtedness rates among graduates of for-profit institutions, at 87-88%, versus only 64-65% at public institutions, and 63-70% at private non-profit institutions.

<Figure 3 about here>

Debt Amount

In Panel B of Table 2, we examine the amounts of student loan debt undertaken by military veterans versus non-veterans among those who graduated with debt. Here we do find that among graduates with debt, veterans' debt loads were lower than those of similar non-veterans on average, and this appears to hold in the overall models across weighting schemes.

As shown in the right-hand panel of Figure 3, which presents fitted dollar-value estimates from columns 7 and 8, veterans at the sample mean had an average student loan debt (in 2009 dollars) of about $18,300 at graduation, versus about $20,500 among non-veterans. This gap of about $2,200 was smaller among graduates of public institutions but was markedly larger among graduates at private non-profit institutions, where non-veterans were borrowing an estimated $5,000 more than veterans. Surprisingly, the effect of veteran status was reversed at for-profit

institutions, where the graduating debt levels of veterans actually topped those of their observably similar non-veteran counterparts, with estimated debt levels of $32,000 and $29,500, respectively.

Importantly, we do not find this difference in the unweighted and survey-weighted samples (columns 1-4), but only in the two ATT-weighting approaches, in which non-veterans and veterans are observably very similar. In other words, veterans' debt levels at for-profits exceed the debt levels of observably similar graduates of these institutions, but not of more traditional students at these institutions. Still, the finding appears to validate policymakers' concerns about excessive borrowing among students at for-profits, including by veterans whose eligibility for the MGIB should mitigate the need for debt.

Employment and Earnings One Year Post Bachelor's

Employment

Panel A of Table 3 displays estimates from models predicting graduates' full-time employment one year after earning bachelor's degrees. From this point forward, we show only the survey-weighted models with and without propensity-score weighting. Estimates from our preferred survey-adjusted ATT-weighted sample are now shown in columns 3 and 4. In this case, the table also includes columns 5 and 6, which use the same survey-adjusted ATT weighting scheme but limit the sample to those *not enrolled* in post-baccalaureate study.

<Table 3 about here>

We see positive and significant effects of overall veteran status in full-sample models with and without the ATT weights (columns 1 and 3), with veteran-by-sector effects that differ only modestly between weighting approaches. When we constrain the sample to graduates who

were not enrolled in post-baccalaureate training one year after graduation, the advantage mostly persists in magnitude, with a full-time employment odds ratio of 1.25 (the antilog of the coefficient, 0.223, shown in column 5), but it is no longer statistically significant ($p<0.11$). This suggests that the effect in column 3 is partially driven by higher rates of post-baccalaureate enrollment among non-veterans.

Using the estimates from columns 3 and 4, the left panel of Figure 4 displays the fitted marginal effects of veteran status and institutional sector, holding constant the other terms in the model. Within the full, survey-adjusted ATT-weighted sample, and controlling for demographic attributes, we find that military veterans were about 5.2 percentage points (or about 7.7%) more likely to be employed than their non-veteran counterparts one year after bachelor' degree completion, corresponding to a logit coefficient of 0.262 ($p<.05$), or an odds ratio of 1.3, as shown in Panel A of Table 3. This ratio means that veterans' estimated odds of employment were 30% higher than those of their counterparts.

<Figure 4 about here>

A few other details are notable in the left panel of Figure 4. First, veterans' employment advantage is driven by graduates of private, non-profit institutions, where the fitted, marginally significant difference ($p<0.10$) is nearly 12 percentage points, at 80.5% versus 68.6% of graduates employed one year after graduation. (The difference becomes non-significant and shrinks slightly in magnitude if the sample is limited only to those not enrolled in college one year post-graduation.) Though veterans also show an observed 3.6-point advantage after graduating from public institutions, this difference does not reach statistical significance. Importantly, the advantage is reversed in magnitude among graduates of for-profit institutions, where veterans are about 2 percentage points less likely to be employed (69% versus 71%),

though this difference also is not statistically significant. Also notable is the fact that employment levels overall among for-profit graduates are comparable to those in the other two education sectors.

A reasonable question is whether the finding of no significant overall for-profit effect depends on students' graduating cohort, since large for-profit chains were much less prevalent in 1993 than in 2000 and 2008. Our estimates (not shown) lend some support to this perspective, as a degree from a for-profit in 1993 predicted about threefold higher employment odds year later relative to a degree from a public institution ($p<0.023$), whereas the relationship of a for-profit degree to employment (relative to a public degree) was modestly negative and non-significant in 2001 and modestly positive and non-significant in 2008. Since the for-profit landscape in 1993 was comprised largely of smaller, vocationally focused institutions, it is possible that the kinds of students choosing those institutions were especially career-focused, leading to the particular employment advantages seen in that year.

Earnings, Conditional on Full-Time Employment

Panel B of Table 3 presents predictors of logged earnings for graduates who were employed full-time one year post-bachelor's degree completion. We focus on this subset in order to distinguish employment effects from earnings effects conditional on employment. We find that positive earnings effects of veteran status are more apparent in the ATT-weighted analysis than in the regression-only analysis. This suggests that veterans' earnings advantages, conditional on full-time employment, are more apparent among those with a high propensity to become veterans than among college graduates in general. Such a finding is consistent with studies showing greater labor market advantages (or fewer disadvantages) for veterans who faced

greater pre-service risk factors (Angrist, 1998) or who had higher propensity for military service (Bryant, Samaranayake, & Wilhite, 1993; DellaPosta, 2013).

Marginal effects of the veteran and veteran-by-sector effects from columns 3 and 4 are displayed in the right panel of Figure 4, which shows a positive and significant earnings advantage for full-time employed veterans of about $1,300 in 2009 dollars, holding other terms constant at their sample means. (Note that the predicted earnings figures are higher than the sample means because they are limited to full-time workers.) This estimated advantage was driven by graduates of public and for-profit institutions, as full-time employed veterans graduating from private non-profit institutions showed a statistically significant earnings disadvantage of about $3,500, possibly because a substantially larger share of these graduates were working full-time. Among employed graduates who observably resembled military veterans, the graduates of for-profits earned more than other groups, and veterans out-earned non-veterans by about $7,500 in 2009 dollars, again, conditional on being employed full-time.

Employment and Earnings in the Decade Post-Bachelor's

Employment Over Time

Panel A of Table 4 presents logistic regression coefficients predicting employment one, four, and ten years post-baccalaureate completion for veterans and non-veterans of both public and private (non-profit and for-profit) institutions, adjusting for the other terms in the models. This analysis does not shed new light on the longer-term trajectories of graduates specifically of for-profit institutions, due to the lower prevalence of for-profit institutions available in 1993. Instead, it examines the persistence of veterans' employment and earnings advantages in the decade after bachelor's degree completion and examines whether they differ between private and

public institutions. Note that estimates for the first-year post-bachelor's degree completion do not perfectly align with those reported above because we are here focused on the 1993 graduating cohort only.

<Table 4 about here>

Examining the probability of full-time employment at each survey wave, we notice in Panel A of Table 4 that veterans were generally more likely to be employed than non-veterans in the year after bachelor's degree completion, and this advantage appeared to hold into 1997, four years after bachelor's completion, in both the regression-only and ATT-weighted models. However, ten years after degree completion, veterans were less likely to be employed than their non-veteran counterparts under both analytic approaches.

<Figure 5 about here>

Fitted marginal effects from the survey-adjusted ATT weights are shown in the left and right panels of Figure 5. On the left, we see the overall effect of veteran status on the probability of employment over time, where military veterans begin in 1994 with roughly a six-percentage point (about 10%) full-time employment rate advantage. The advantage grows to more than 11 percentage points (about 15%) by 1997. However, by 2003, a decade after bachelor's completion, military veterans were modestly less likely to be employed full-time, by about two percentage points or 2.5% ($p<0.05$ in all cases).

In the right panel of Figure 5, which disaggregates the marginal-effect estimates by public versus private sectors, we find that the initial employment advantage for the 1993 cohort was driven by graduates of public-sector institutions, though the veteran advantage in 1997 appeared to be driven by graduates of the private-sector institution. Finally, the drop-off of the

veterans' employment advantage by 2003 appears driven by graduates of the private-sector institutions.

Earnings Over Time

In our longitudinal earnings analysis, again limited to the 1993 graduating cohort, we do not restrict our sample to those employed full-time in a given wave but only to those with positive earnings in that wave. Thus, both full-time and part-time workers are included, conditional on having any earnings at all. Turning to Panel B of Table 4, we note that estimates of veterans' effects in each time period are modestly sensitive to sample weighting, though in both weighting approaches, the effect of veteran status appears to improve over time.

<Figure 8 about here>

Using the survey-adjusted ATT weights, we find a veterans' earnings penalty among graduates with positive earnings one year after degree completion, but the veteran effect is reversed in 1997 and 2003, so that veterans are out-earning their observably similar, working counterparts four and ten years after degree completion. In other words, though they were about two points less likely than their counterparts to be working ten years after degree completion, those veterans who were working were earning about $3000 more in 2009 dollars, an advantage of roughly 4.8 percent, as shown in the left panel of Figure 8. Examining differential earnings trends among graduates of public and private institutions, we find in the right panel of Figure 8 that the veterans' earnings advantage ten years post-bachelors was driven about equally by graduates of private and public institutions.

Discussion

This study contributes to the literature on economic returns to veteran status by focusing on a relatively advantaged but understudied subgroup of veterans: those who complete bachelor's degrees after military separation. It sheds new light on how veterans in the AVF era have performed after earning bachelor's degrees, and also on whether veterans fared differently from non-veterans after graduating from for-profit colleges. With more than a million returning veterans having applied for Post-9/11 GI Bill benefits, the question of the relative labor market performance of veterans who earn degrees is a timely one.

Perhaps the main lesson from our study is that service members who earn bachelor's degrees after leaving the military perform well in the labor market relative to non-veterans, and especially when compared to non-veterans who are observably similar in terms of demographic and institutional characteristics and field of study. These findings show that for the older and less-traditional population of college graduates that veterans represent, veteran status was associated with higher rates of employment and earnings one year and four years after graduation. A decade after graduation, veterans were almost three percentage points less likely than similar counterparts to be employed full-time, but among graduates with positive earnings, veterans earned about 4.7% more than their counterparts.

Interpreting Longitudinal Trends

It is interesting that veterans' employment advantage fades over time, while their earnings advantage increases over time, conditional on having positive earnings. Though it is possible that the lower employment rate contributes to the higher average earnings among those working, the finding implies that the labor market continues to recognize and reward veteran status among those employed. Ten years after bachelor's completion, veteran graduates would

have been about 45, on average, and given that they had separated from the military before graduating, military retirement benefits do not offer a likely explanation for their reduced employment participation a decade after graduation. Still, the employment drop-off could plausibly be due to differences in the health status of graduates more than to any labor market penalty for veteran status.

Research suggests, for instance, that both combat exposure and Post-Traumatic Stress Disorder may contribute to early aging (Trotter et al., 2015; Wolf et al., 2016; Zoroya, 2012), and that reduced sociological and health outcomes are one way in which military service may undermine socioeconomic status over the life course (London & Wilmouth, 2006; MacLean & Elder, 2007). A 35-year-old 1993 college graduate (the mean age for veterans in the sample) would have been 18 in 1976, a year after the end of the Vietnam War. This means that members of the 1993 graduating cohort could potentially have been involved in conflicts from Vietnam through the first Gulf War, which occurred from 1990 through 1991 (Global Policy Forum, 2005). Though the years in between were not a time of major military conflict, smaller scale military engagements may still have left the veteran population more vulnerable than their peers to health problems over time.

Indeed, B&B data show that at the time of graduation, 10.5% of veterans and 3.3% of non-veterans in the 1993 cohort reported having a disability, and that four years later, 6.5% of veterans versus only 1.7% of non-veterans reported a disability that interfered with work. But these early-career differences in self-reported disability were driven almost entirely by veterans who had graduated after age 40, who would therefore have been old enough to have served during the Vietnam era. By 2003—the only time point at which our analysis found lower employment levels among veteran graduates in the 1993 cohorts—8.2% of veterans in the 1993

cohort reported that they had a long-lasting disability that interfered with life activities, as compared to only 2.8% of non-veterans in the cohort, but the gap was now evident among respondents who had graduated in their 20s and 30s as well as their 40s. Thus, it is possible that increasing disability among younger veterans in the 1993 cohort at least partially explains the negative employment effect among veterans in 2003.

Possible Mechanisms Behind Veterans' Labor Market Advantages

Another question implicitly raised by our findings of is the source of veterans' early-career labor market advantages. Though we cannot say definitively why veterans outperformed similar peers in the labor market one and four years after graduation (and ten years after, conditional on working full time), we can conclude that the differences were not attributable to their age, race, gender, parents' educational background, academic major, or the state or Carnegie classification of the institution from which they graduated. One possible explanation offered by the literature is the "bridging" hypothesis, which suggests that military service provides general workforce skills that help prepare individuals with lower baseline employability to transfer successfully into the workforce (Bryant, Samaranayake, & Wilhite, 1993; Hirsch & Mehay, 2003). In this case, however, since we are comparing veteran and non-veteran college graduates, the provision of fundamental workforce skills seems an unlikely mechanism. A more plausible mechanism in this context might be market signaling, in which veteran status signals positive attributes to employers, such as discipline or work ethic (Kleykamp, 2013a). A human capital explanation is also possible, meaning that veterans may bring specialized skills into the workforce that are not captured by controls for their age, academic major, or institution type (Mangum & Ball, 1989).

Implications for Lifetime Earnings

Because our dataset examines point-in-time employment and earnings after bachelor's degree completion, we cannot calculate lifetime earnings by veteran status. What we do know is that veterans earning bachelor's degrees in the sample were nearly a decade older, on average, than non-veterans, which means that they lacked a bachelor's degree for a larger share of their careers. Given that postsecondary education confers earnings advantages of 6-9% per year of education (Kane & Rouse, 1995), and that a bachelor's degree was associated with a 67% earnings premium over a high school diploma at the median in 2011 (Oreopoulos & Petronijevic, 2013), the extra decade that veterans in the sample spent, on average, without a bachelor's degree would have been expected to reduce their lifetime earnings to a greater extent than their post-baccalaureate earnings advantage could fully compensate. On the other hand, GI Bill benefits have been estimated to increase veterans' bachelor's degree completion rates by roughly 5 to 6 percentage points (Angrist & Chen, 2011; Bound & Turner, 2002). For veterans whose military service induced them to complete a degree they would not have otherwise pursued, then military service would boost their lifetime earnings through their eventual bachelor's degree returns as well as through the modest veterans' employment premium (30% increased odds) and earnings premiums (about 3%) we find in our analysis, in addition to any veterans' labor market premiums accrued prior to bachelor's degree completion. In other words, our findings suggest that military service in the AVF era may benefit earnings insofar as it boosts college completion and does not hamper long-term employment (via disability). But the caveat is important. Our findings suggest that veterans' earnings premiums are too modest to compensate for military-related delays to bachelor's degree completion, and ours are among the more-positive estimates

of veterans' employment and earnings premiums in the literature. Also, the data offer a reminder of the physical risks of service that may erode employment longevity (and thus, economic returns to military service), as other studies have cautioned (London & Wilmoth, 2006; MacLean & Elder, 2006). At the same time, the data provide reassurance that AVF-era labor markets do reward a history of military service among the college educated.

Implications of Higher Education Sector Effects

Our analysis of differences by higher education sector also offers some reassurance, along with reason for concern. We find little evidence of employment or earnings deficits among graduates of for-profit colleges, and we even find some evidence of for-profit earnings advantages among graduates of for-profits who were employed full-time. For employment one year post graduation, our disaggregated analyses by cohort were consistent with a decline in the labor market benefits of a for-profit degree as the sector has grown.

Despite the relatively encouraging performance of graduates from for-profits in these datasets, the sharply higher levels of indebtedness among their graduates—both veterans and non-veterans—substantiates policymakers' concerns. Though veterans may choose these institutions for their convenience and their focus on non-traditional students (Steele, Salcedo, & Coley, 2010), the debt levels that veterans are undertaking to do so raises questions about the relative value of these degrees as compared to degrees from public and non-profit institutions.

Conclusion

Our analysis makes a useful contribution to the literature by focusing on the relative labor market performance of veterans in the decade following their bachelor's degree completion.

Though earlier literature has suggested that military service is most costly to individuals with higher levels of education, such studies have typically not taken into account individuals' years of labor market experience post-credentialing (Kleykamp, 2013b; Teachman & Tedrow, 2007). Yet years of post-credential experience is an important consideration, since veterans tend to be older than non-veterans when they earn their education credentials. By focusing on new college graduates, we show that in the ten years after degree completion, veterans out-earned their observably similar counterparts conditional on working full-time, and that they enjoyed higher full-time employment rates one and four years after graduation, with lower full-time employment ten years after graduation. Moreover, we find that veterans' relative performance in the labor market was generally consistent across higher educational sectors.

Based on evidence from the last two decades, veterans who finish college can expect to do well on average compared to observably similar peers. This finding bodes well for recent veterans who are using or planning to use their GI Bills to advance their educations. Contingent on completion of bachelor's degrees, their ability to find employment may depend only slightly on the institutional sector from which they graduate. Where the sector seems to matter in is in the debt burdens they carry. For veterans wishing to capitalize fully on the advantages of prior military service, focused pursuit of a bachelor's degree appears to be a promising pathway. So does choosing an institution at which tuition and living expenses are comfortably offset by available GI Bill benefits.

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Table 1. Sample means, by cohort and veteran status, with survey weights or survey-adjusted ATT weights

	By Cohort (Survey Weights)			By Vet Status (Survey Wts.)			By Vet Status (Adj. ATT Wts.)		
	1993 (n=9,350)	2000 (n=9,344)	2008 (n=15,048)	Overall (n=33,742)	Vet (n=1,093)	Non-Vet (n=32,649)	Overall (n=33,742)	Vet (n=1,093)	Non-Vet (n=32,649)
Demographics & Cohorts									
Military Veteran	0.041	0.036	0.025	0.033	1	0	0.499	1	0
Female	0.549	0.573	0.576	0.568	0.211	0.580	0.221	0.211	0.232
Asian	0.047	0.055	0.059	0.055	0.021	0.056	0.022	0.021	0.023
Black	0.059	0.082	0.088	0.078	0.11	0.077	0.116	0.11	0.122
Hispanic	0.051	0.085	0.094	0.080	0.092	0.079	0.084	0.092	0.076
White	0.831	0.749	0.727	0.761	0.728	0.762	0.731	0.728	0.734
Race Other or Missing	0.007	0.030	0.032	0.025	0.043	0.024	0.042	0.043	0.042
Parent Held 4-year Degree	0.493	0.514	0.550	0.525	0.281	0.533	0.294	0.281	0.306
Age at Graduation	25.407	26.114	25.273	25.554	35.345	25.224	35.349	35.345	35.352
1993 Cohort	1	0	0	0.264	0.332	0.262	0.322	0.332	0.312
2000 Cohort	0	1	0	0.292	0.324	0.291	0.305	0.324	0.285
2008 Cohort	0	0	1	0.444	0.344	0.447	0.373	0.344	0.403
Institutional Sectors & Carnegie Classifications									
Public	0.670	0.648	0.629	0.645	0.594	0.647	0.606	0.594	0.618
Private non-profit	0.316	0.337	0.325	0.326	0.333	0.326	0.304	0.333	0.275
Private for-profit	0.014	0.015	0.046	0.028	0.073	0.027	0.09	0.073	0.107
Research and Doctoral	0.445	0.455	0.452	0.451	0.302	0.456	0.319	0.302	0.337
Master's Granting	0.359	0.356	0.365	0.361	0.452	0.358	0.439	0.452	0.426
Baccalaureate Granting	0.147	0.128	0.135	0.137	0.166	0.136	0.154	0.166	0.141
Special Focus and Other	0.049	0.061	0.048	0.052	0.079	0.051	0.088	0.079	0.096
Academic Major									
STEM	0.171	0.172	0.170	0.171	0.200	0.170	0.21	0.2	0.22
Humanities/Soc Sci/Educ	0.431	0.440	0.356	0.400	0.300	0.404	0.294	0.3	0.287
Business/Health/Vocational	0.267	0.314	0.314	0.302	0.390	0.299	0.384	0.39	0.378
Other or Missing	0.131	0.074	0.160	0.127	0.109	0.128	0.112	0.109	0.114
Post-Graduation Measures									
Graduated with Loans	0.447	0.629	0.656	0.593	0.567	0.594	0.585	0.567	0.603
Employed FT 1 Yr. Postbac	0.731	0.767	0.567	0.668	0.766	0.665	0.735	0.766	0.704
Enrolled 1 Yr. Postbac	0.178	0.209	0.217	0.204	0.199	0.205	0.191	0.199	0.184
Loan Amount at Graduation	5,574.8	14,026.1	16,299.2	12,802.3	11,145.2	12,858.3	12,399.2	11,145.2	13,648.9
Income 1 Year Post-Bac	27,086.6	33,774.4	28,733.1	29,747.2	39,941.8	29,414.9	38,575.9	39,941.8	37,222.9

Table 2. Coefficients predicting graduation with student loans (Panel A) and debt amount among those with loans (Panel B)

	No Weight		Survey Weight		ATT Weight		Adj. ATT Weight	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: Logit coefficients predicting graduation with loans								
Veteran	-0.111	0.104	-0.163+	0.038	-0.064	0.073	-0.067	0.059
	(0.071)	(0.086)	(0.098)	(0.118)	(0.075)	(0.093)	(0.098)	(0.118)
Private Non-Profit	0.354**	0.376**	0.334**	0.355**	0.040	0.238*	0.080	0.292*
	(0.029)	(0.030)	(0.040)	(0.041)	(0.096)	(0.095)	(0.126)	(0.127)
Private For-Profit	1.142**	1.164**	1.312**	1.310**	1.107**	1.300**	1.317**	1.293**
	(0.114)	(0.121)	(0.163)	(0.173)	(0.214)	(0.260)	(0.270)	(0.325)
Veteran * Non-Profit		-0.676**		-0.592**		-0.394*		-0.414+
		(0.149)		(0.204)		(0.167)		(0.221)
Veteran * For-Profit		-0.371		-0.140		-0.394		0.059
		(0.330)		(0.441)		(0.376)		(0.488)
Observations	33,742	33,742	33,742	33,742	33,742	33,742	33,742	33,742
df	71	73	71	73	71	73	71	73
Panel B: Linear regression coefficients predicting logged loan amount, conditional on having loans								
Veteran	-0.130**	-0.063	-0.143**	-0.031	-0.117**	-0.100**	-0.113**	-0.090**
	(0.032)	(0.040)	(0.034)	(0.043)	(0.011)	(0.014)	(0.011)	(0.015)
Private Non-Profit	0.344**	0.351**	0.356**	0.366**	0.192**	0.238**	0.145**	0.215**
	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.019)	(0.015)	(0.019)
Private For-Profit	0.619**	0.616**	0.607**	0.603**	0.558**	0.510**	0.519**	0.443**
	(0.034)	(0.036)	(0.034)	(0.035)	(0.022)	(0.028)	(0.023)	(0.028)
Veteran * Non-Profit		-0.241**		-0.356**		-0.103**		-0.153**
		(0.073)		(0.072)		(0.025)		(0.026)
Veteran * For-Profit		-0.037		-0.051		0.096**		0.170**
		(0.102)		(0.110)		(0.034)		(0.036)
Observations	21,705	21,705	21,705	21,705	21,705	21,705	21,705	21,705
df	71	73	71	73	71	73	71	73

** p<0.01, * p<0.05, + p<0.1

Standard errors in parentheses. All models include controls for cohort fixed effects age, gender, race/ethnicity, parents' highest education level, academic major category, institutional Carnegie category, and state fixed effects.

Table 3. Coefficients predicting full-time employment (Panel A) and conditional earnings (Panel B) one year after graduation

	Full Analytic Sample				Not Enrolled	
Weighting	Survey Weight		Adj. ATT Weight		Adj. ATT Weight	
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: Logit coefficients predicting full-time employment						
Veteran	0.240*	0.070	0.262*	0.164	0.223	0.127
	(0.106)	(0.126)	(0.113)	(0.135)	(0.138)	(0.167)
Private Non-Profit	-0.040	-0.057	0.421**	0.205	0.404**	0.241
	(0.040)	(0.041)	(0.131)	(0.126)	(0.154)	(0.156)
Private For-Profit	0.333*	0.365**	0.203	0.316	0.183	0.193
	(0.136)	(0.139)	(0.293)	(0.319)	(0.318)	(0.350)
Veteran * Non-Profit		0.650**		0.471+		0.368
		(0.227)		(0.249)		(0.301)
Veteran * For-Profit		-0.239		-0.263		-0.024
		(0.484)		(0.531)		(0.602)
Observations	33,656	33,656	33,656	33,656	25,802	25,802
Df	71	73	71	73	71	73
Panel B: Linear regression coefficients predicting earnings, conditional on full-time employment						
Veteran	0.020	0.016	0.029**	0.067**		
	(0.021)	(0.027)	(0.008)	(0.010)		
Private Non-Profit	0.037**	0.038**	0.133**	0.207**		
	(0.009)	(0.010)	(0.010)	(0.014)		
Private For-Profit	0.051*	0.034	0.281**	0.254**		
	(0.025)	(0.026)	(0.018)	(0.021)		
Veteran * Non-Profit		-0.030		-0.141**		
		(0.043)		(0.017)		
Veteran * For-Profit		0.195*		0.067*		
		(0.083)		(0.028)		
Observations	20,178	20,178	20,178	20,178		
Df	71	73	71	73		

** p<0.01, * p<0.05, + p<0.1

Standard errors in parentheses. All models include controls for cohort fixed effects age, gender, race/ethnicity, parents' highest education level, academic major category, institutional Carnegie category, and state fixed effects.

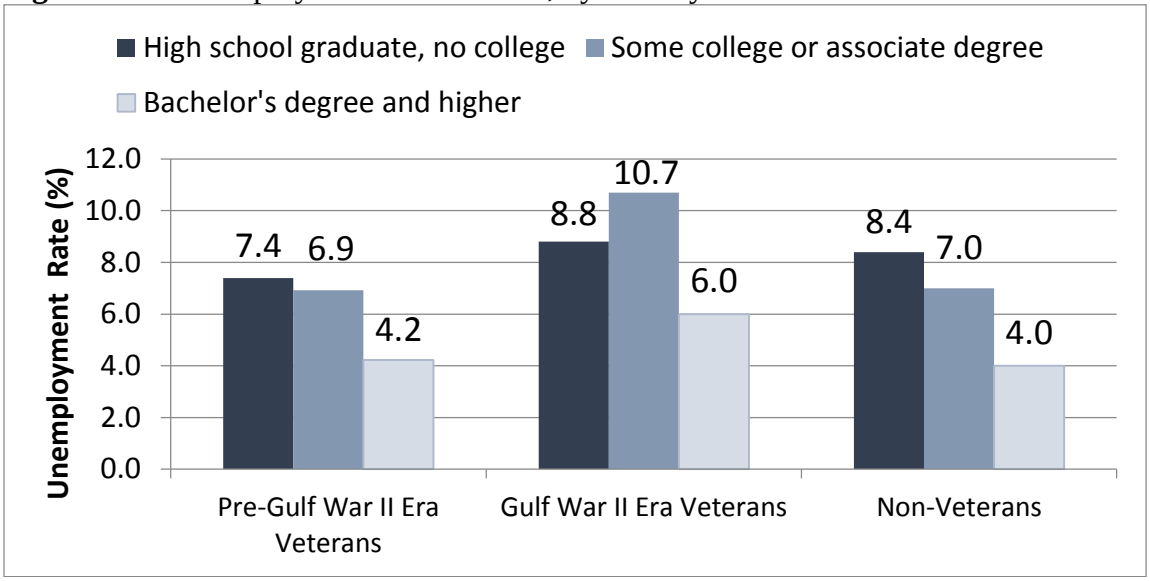
Table 4. Coefficients predicting full-time employment (Panel A) and conditional earnings (Panel B) in the decade after graduation

	Survey Weight		Adj. ATT Weight	
VARIABLES	(1)	(2)	(3)	(4)
PANEL A: Logit coefficients predicting full-time employment				
Veteran	0.341**	0.298**	0.271**	0.372**
	(0.015)	(0.018)	(0.021)	(0.026)
Veteran*1997	0.285**	0.001	0.552**	0.034
	(0.023)	(0.027)	(0.031)	(0.038)
Veteran*2003	-0.941**	-0.761**	-0.445**	-0.363**
	(0.020)	(0.025)	(0.029)	(0.036)
1997 (Wave 2)	1.068**	1.090**	1.448**	1.649**
	(0.017)	(0.017)	(0.062)	(0.063)
2003 (Wave 3)	0.833**	0.816**	2.717**	2.689**
	(0.017)	(0.017)	(0.062)	(0.063)
Private	-0.087**	-0.092**	0.198**	0.349**
	(0.006)	(0.006)	(0.026)	(0.034)
Private*1997	-0.141**	-0.160**	-0.164**	-0.787**
	(0.008)	(0.008)	(0.034)	(0.045)
Private*2003	0.093**	0.110**	-0.477**	-0.375**
	(0.007)	(0.008)	(0.032)	(0.045)
Veteran*Private		0.122**		-0.301**
		(0.030)		(0.045)
Vet*Priv*1997		0.853**		1.534**
		(0.048)		(0.068)
Vet*Priv*2003		-0.477**		-0.169**
		(0.040)		(0.062)
Observations	23,126	23,126	22,675	22,675
Individuals	7,745	7,745	7,594	7,594
Intraclass Correlation	0.244	0.244	0.213	0.217
Panel B: Linear regression coefficients predicting earnings, conditional on having positive earnings				
Veteran	-0.010**	-0.034**	-0.059**	-0.017**
	(0.004)	(0.004)	(0.005)	(0.006)
Veteran*1997	0.065**	0.075**	0.126**	0.072**
	(0.004)	(0.005)	(0.005)	(0.006)
Veteran*2003	0.013**	0.037**	0.106**	0.075**
	(0.004)	(0.005)	(0.005)	(0.006)
1997 (Wave 2)	0.613**	0.611**	0.651**	0.679**
	(0.004)	(0.004)	(0.011)	(0.011)
2003 (Wave 3)	1.282**	1.278**	1.332**	1.350**
	(0.004)	(0.004)	(0.010)	(0.011)
Private	0.042**	0.039**	0.184**	0.243**
	(0.002)	(0.002)	(0.006)	(0.007)
Private*1997	0.001	0.002	-0.095**	-0.172**
	(0.002)	(0.002)	(0.006)	(0.008)
Private*2003	-0.007**	-0.004*	-0.124**	-0.168**
	(0.002)	(0.002)	(0.006)	(0.008)

Veteran*Private		0.067**		-0.117**
		(0.007)		(0.010)
Vet*Priv*1997		-0.029**		0.151**
		(0.008)		(0.011)
Vet*Priv*2003		-0.067**		0.086**
		(0.008)		(0.011)
Observations	20,057	20,057	19,672	19,672
Individuals	7,705	7,705	7,554	7,554
Intraclass Correlation	0.295	0.295	0.343	0.343

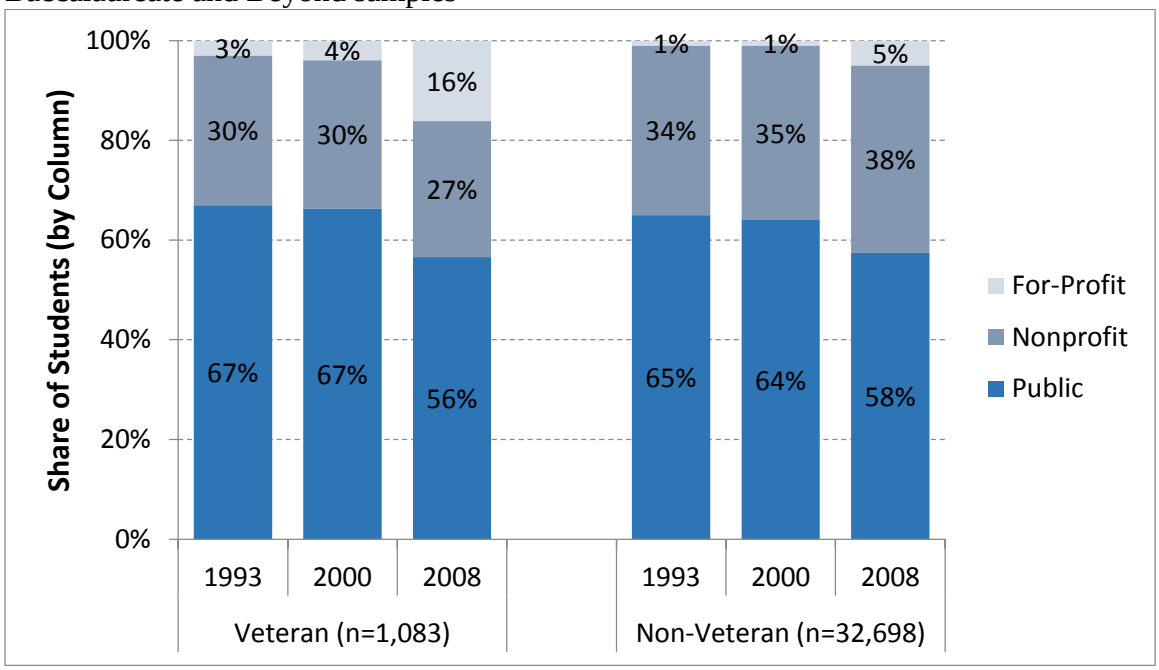
** p<0.01, * p<0.05, + p<0.1
Standard errors in parentheses. All models include controls for age, race/ethnicity, gender, whether parents held a degree, and the interaction of these terms with observation period, as well as fixed effects for state and Carnegie code of bachelor's granting institution.

Fig 1 Annual unemployment rates in 2012, by military veteran status and education level



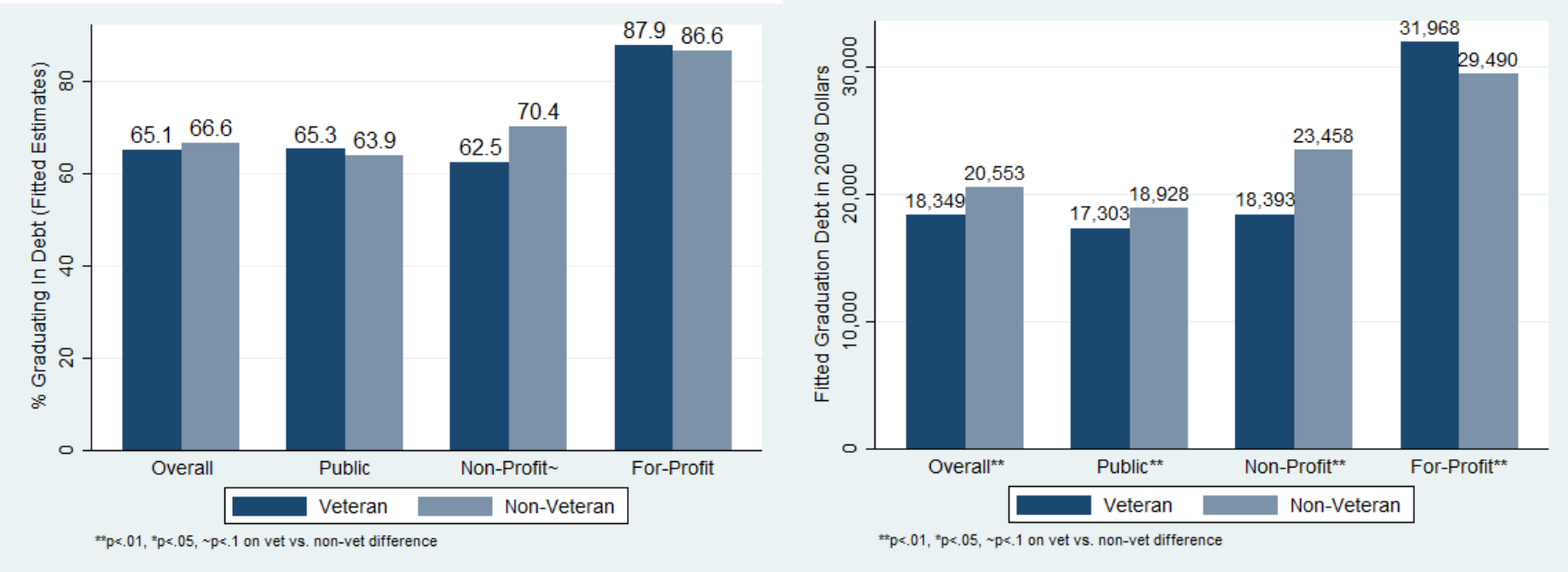
Data Source: U. S. Bureau of Labor Statistics (2013)

Fig. 2 Change in market share of for-profit colleges among bachelor's degree recipients in Baccalaureate and Beyond samples



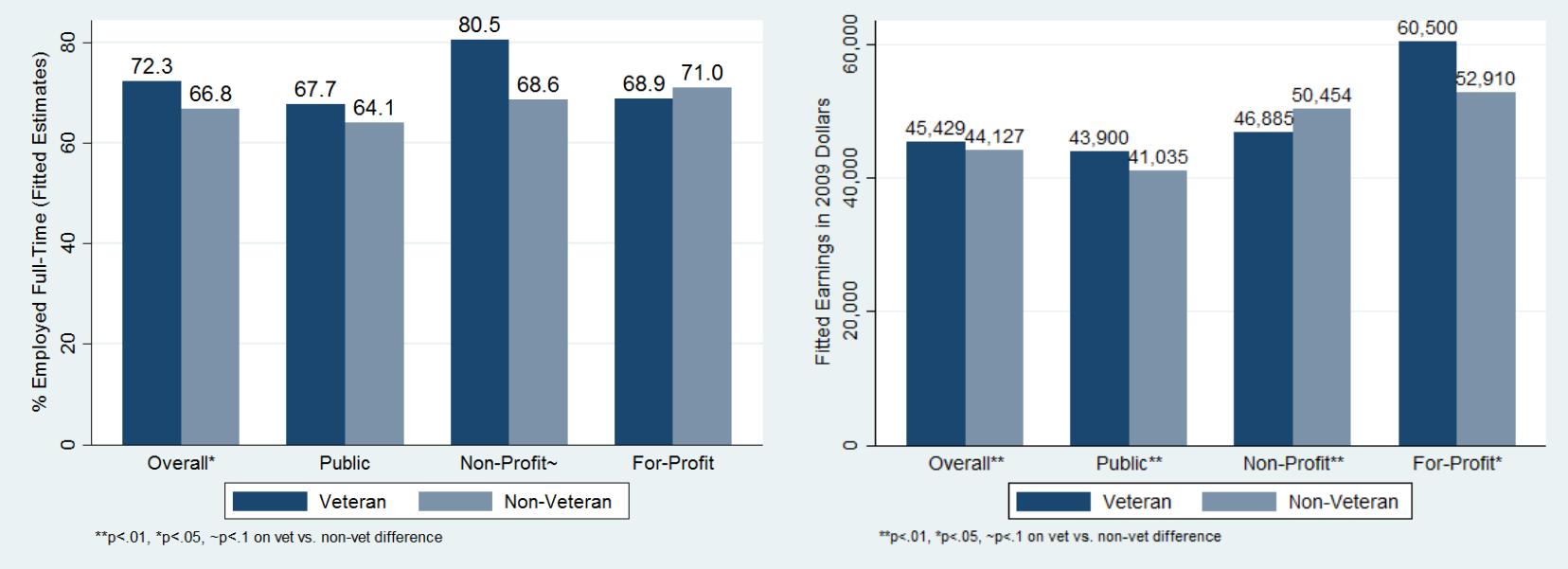
Data Source: NCES Baccalaureate & Beyond data for 1993, 2000, and 2008 cohorts

Fig. 3 Regression-estimated student loan indebtedness rates (left) and debt amounts (if non-zero) at graduation (right)



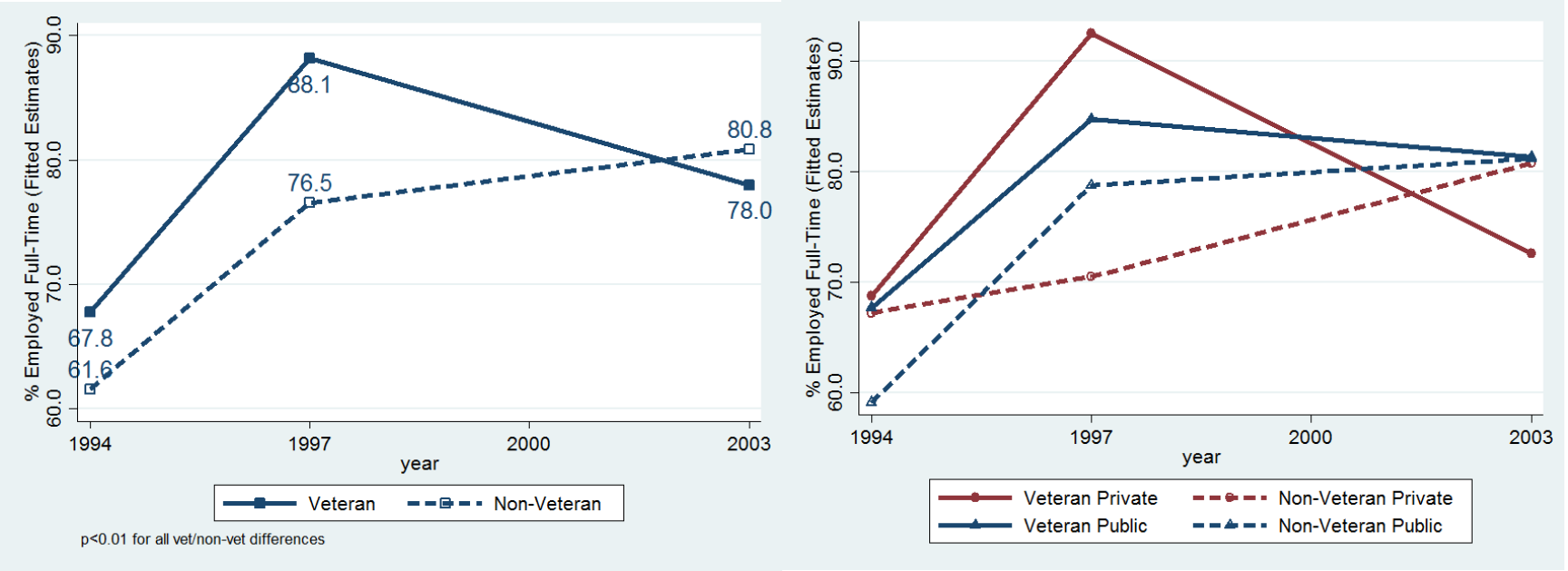
Note: Graphs show marginal effects from sample with survey-adjusted ATT weights, with covariates held at sample means.

Fig. 4 Regression-estimated full-time employment rates (left) and earnings conditional on full-time employment (right) one year after bachelor's completion



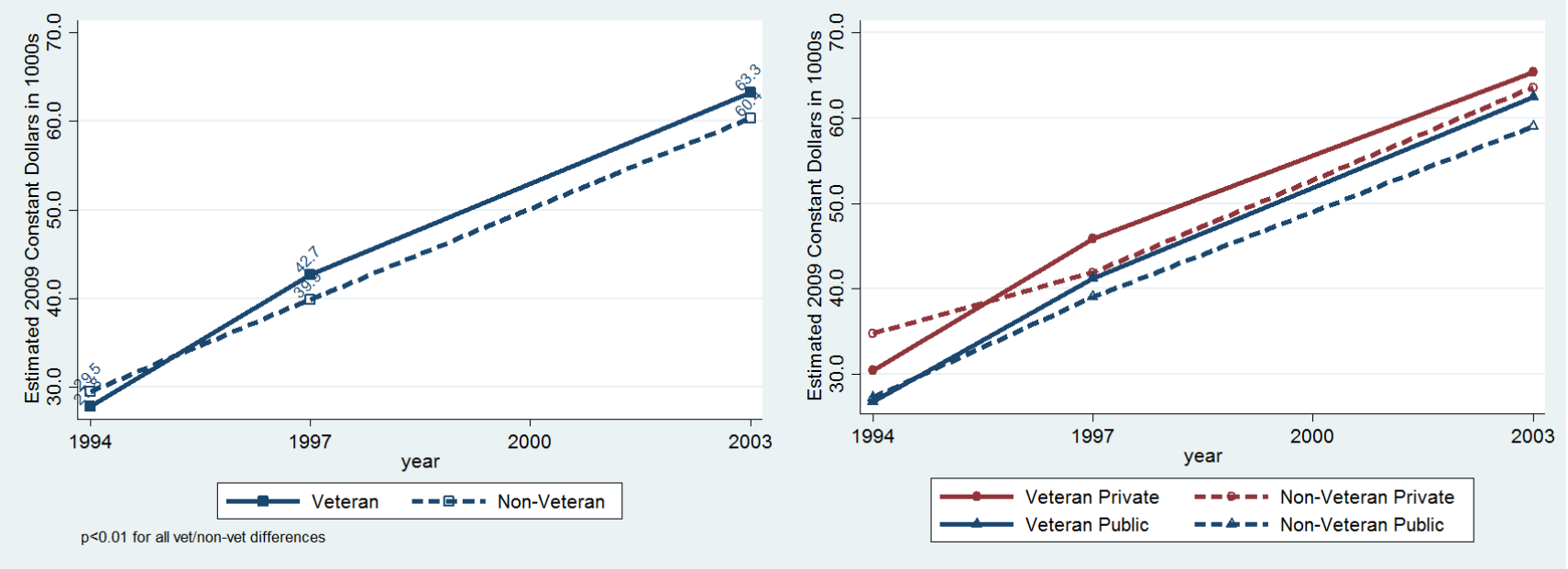
Note: Graph shows marginal effects from sample with survey-adjusted ATT weights, with covariates held at sample means.

Fig. 5 Estimated rates of full-time employment over time for 1993 graduates, overall (left) and by sector (right)



Note: Graph shows marginal effects from sample with survey-adjusted ATT weights, with covariates held at sample means.

Fig. 6 Estimated earnings (where positive) over time for 1993 graduates, overall (left) and by sector (right)



Note: Graph shows marginal effects from sample with survey-adjusted ATT weights, with covariates held at sample means.