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YOU CAN'T HANDLE THE TRUTH: THE EFFECTS OF THE POST-9/11 GI BILL ON HIGHER EDUCATION AND EARNINGS

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You Can't Handle The Truth: The Effects Of The Post-9/11 Gi Bill On Higher Education And Earnings

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ABSTRACT

The Post 9/11 GI Bill (PGIB) is among the largest and most generous college subsidies enacted thus far in the U.S. We examine the impact of the PGIB on veterans' college-going, degree completion, federal education tax benefit utilization, and long run earnings. Among veterans potentially induced to enroll, the introduction of the PGIB raised college enrollment by 0.17 years and B.A. completion by 1.2 percentage points (on a base of 9 percent). But, the PGIB reduced average annual earnings nine years after separation from the Army by $900 (on a base of $32,000). Years enrolled effects are larger and earnings effects more negative for veterans with lower AFQT scores and those who were less occupationally skilled. Under a variety of conservative assumptions, veterans are unlikely to recoup these reduced earnings during their working careers. All veterans who were already enrolled in college at the time of bill passage increase their months of schooling, but only for those in public institutions did this translate into increases in bachelor's degree attainment and longer-run earnings. For specific groups of students, large subsidies can modestly help degree completion but harm long run earnings due to lost labor market experience.

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I. Introduction

Policymakers and social scientists have lauded the original GI Bill (the 1944 Serviceman's Readjustment Act) and the 1984 Montgomery GI Bill (MGIB) as examples of thoughtful policy and engines of social mobility (Burrell 1967, Greenberg 1997, Humes 2006). Among World War II veterans, the GI Bill raised college completion rates by 5 to 6 percentage points and years of schooling by roughly 0.28 years (Bound and Turner 2002). Vietnam-era veterans who took up educational benefits saw schooling increases of 1.4 years (Angrist 1993), which worked to offset the negative earnings effects of lost labor market experience due to consignment (Angrist and Chen 2011). The GI Bill also encourages veterans to attend more selective institutions that they may not have otherwise considered.

In 2008, the GI Bill was significantly revamped and expanded by the Post-9/11 GI Bill (PGIB), constituting one of the largest policy shocks in college subsidies in U.S. history.[1] Rather than replacing the MGIB, the PGIB offers eligible veterans an attractive alternative that increases benefit levels and pays a larger fraction of tuition and living expenses. Under the MGIB, enrolled veterans are directly paid a flat monthly amount to be used towards tuition, books, and living expenses. The PGIB expanded benefits through two channels. Benefits for tuition and fees are based on the highest in-state tuition of public institutions in the state.[2] Those enrolled at least half-time also receive a generous monthly housing allowance (called the Basic Allowance for Housing or BAH), which varies by the zip code of the institution that the veteran attends. As a result, the PGIB roughly doubled the average maximum benefit amount available to eligible veterans, with

[1] Formally, the law is the Post-9/11 Veterans Educational Assistance Act of 2008, Title V of the Supplemental Appropriations Act, 2008 (P.L. 110-252).

[2] Over a thousand U.S. institutions participate in the U.S. military's Yellow Ribbon program (a matching program) which helps cover any gap between the PGIB maximum payment and tuition and fees that the veteran would incur.

significant geographic variation in the expansion of benefits. For the 2009-10 academic year, veterans using the PGIB could have received as much as $48,000 in tuition and fees benefits per term by attending a college in Colorado or Oregon but only up to $5,000 or $6,000 in Wyoming or Arkansas,[3] and the monthly BAH ranged from $630 in parts of Oklahoma and Louisiana to $2,512 in the San Francisco/Bay area. Total PGIB benefits could exceed $60,000 per term if a veteran attended a high-tuition school in an expensive area, but the maximum MGIB benefit was $1,321 per month.[4] An examination of the long-term effects of these generous subsidies seems particularly relevant in an era in which there is widespread and increasing discussion of plans to expand the Pell and separately make community college and potentially other institutions free.[5]

We exploit the intertemporal and cross-sectional variation in veterans' education benefits generated by the PGIB to evaluate the impact of subsidies on education and earnings. We utilize several sources of identification within a difference-in-differences framework. First, we use variation in the level of benefits available to eligible veterans based on when they separated from the Army. Only veterans who are honorably discharged are eligible for either the MGIB or the PGIB, and eligible veterans who separated many years prior to the PGIB expansion are past the natural window of use.[6] We use this variation to compare outcomes for eligible and ineligible veterans who separated before and after the policy. We additionally take advantage of the significant cross-sectional variation in the generosity of the PGIB relative to the MGIB by comparing outcomes for eligible veterans from areas with large expansions in expected benefits

[3] See https://www.benefits.va.gov/gibill/resources/benefits_resources/rates/ch33/tuition_and_fees_2008.asp and www.defensetravel.dod.mil/site/bahCalc.cfm. The Yellow Ribbon Program could further add to this total if the veteran is a student at a participating private school with tuition and fees that exceed the PGIB maximum.
[4] Amounts depend on whether a veteran is enrolled half-time, three-quarters time, or full time.
[5] See https://www.nasfaa.org/uploads/documents/Free_College_Matrix.pdf for an overview of current "free college" proposals.
[6] Roughly half of Army veterans begin using their benefits within 2 years of separation, which is over 80 percent of individuals that ever use education benefits.

with those from areas with small expansions before and after the policy.[7] Using these two sources of variation, we ask whether access to additional financial aid impacts college-going and degree attainment, college choice, and subsequent earnings.

Next, we focus on students who had already enrolled in college prior to the benefit expansion to understand whether the availability of additional funding has measurable impacts on degree attainment and earnings. Eligible veterans who enrolled several years before the PGIB expansion are likely to be less able to utilize the PGIB than those who enrolled just before the additional benefits were announced. We compare eligible and ineligible students enrolled at the same college, but who had initially enrolled in different years. We also combine this variation in the likelihood of benefit access over time with variation in benefit size across colleges.

Our sample comprises U.S. Army veterans who separated between 2002 and 2010. We merge Army personnel records with three administrative data sources. First, the Veterans' Administration (VA) provides information on GI Bill benefit utilization. Second, our measures of degree completion come from the National Student Clearinghouse (NSC). Lastly, we construct several outcomes from administrative tax return data. Our primary college-going measures come from the information return that colleges submit to the IRS (Form 1098-T), which, unlike the NSC, offers complete coverage of for-profit institutions. Tax data also contain information on components of income, the utilization of federal tax benefits, and a limited set of demographic characteristics, and have the advantage of tracking veteran earnings regardless of geographic

[7] Barr (2015) finds positive affects to the PGIB on enrollment and degree attainment using a similar strategy with state-level variation in a smaller data set.

mobility. Because these tax records are available through 2018, we track tax-based outcomes for up to nine years after separation from the Army and initial enrollment in college.

For Army veterans separating during this period, we find modest positive impacts on educational attainment (a 0.17 year increase in time enrolled, and a 1.2 and 0.4 percentage point increase in bachelor's and associate's degree receipt, respectively), but a 2 to 3 percent reduction in earnings 7 to 9 years after separation. We explore several potential explanations for this negative return on educational investments. A Oaxaca-style decomposition reveals that lost labor market experience combined with low-return marginal investments explains much of the negative earnings effect. In addition, heterogeneity analyses show that less advantaged veterans – those in "lower-skilled" military occupations[8] and those with lower AFQT scores – are more likely to enroll in college overall, but this is largely driven by increased enrollments in for-profit institutions. These veterans also experience larger negative earnings effects.

While there is considerable heterogeneity in the benefit expansion across geographies, we find only modest increases in educational attainment for veterans from areas experiencing larger benefit increases relative to those from areas experiencing smaller benefit increases. The effects on earnings are negative across all areas, with some evidence of smaller earnings losses for veterans from areas experiencing larger benefit increases. These differences in the extent to which veterans experience negative returns to education may be driven by somewhat higher quality investments of veterans from high benefit areas, with relative shifts out of for-profit schools and into four-year public and private institutions.

[8] We define occupation skill by building a correspondence between military occupations and Census occupational codes and follow Autor and Dorn (2013) to define high-, middle- and low-skill occupations.

Veterans who were already enrolled when the new policy arrives see modest degree attainment gains, but the overall effect masks larger increases for those initially enrolled in four-year public and community colleges. The corresponding effects on earnings nine years after initial enrollment mirror the effects on degree attainment, with positive effects for those enrolled in four-year public and community colleges, and negative effects for those enrolled in for-profit institutions. In combination, these results suggest that the subsidies are generous enough to draw veterans into additional education and out of the labor market. The longer-term labor market returns depend on the extent to which the resulting periods spent enrolled represent high-quality investments.

Our study contributes to the large literature that examines whether higher education subsidies positively impact college enrollment, degree completion, and earnings. Within the set of education subsidies, the PGIB is a very large program: at over $13 billion a year, it exceeds the amount of grants and scholarships provided by all states combined, is roughly two-thirds of tax expenditures on benefits for higher education, and equates to half of all Pell grant expenditures. Unlike the vast literature that focuses on traditional students, our population of Army veterans comprise an older, non-traditional student group that may respond differently to education subsidies.[9] Further, the provision of benefits at the point of transition out of the military may provide some insight into the effects of proposed efforts to subsidize retraining for those with limited opportunities in the civilian sector, such as those affected by trade exposure or during the recent pandemic.

[9] Seftor and Turner (2002), which estimates the effect of Pell eligibility changes on the enrollment of older students, is a notable exception.

We also shed light on the interaction between the utilization of GI Bill benefits and federal tax benefits also meant to incentivize investments in higher education. There are two relevant changes in these policies that occurred. First, the Hope Tax Credit was replaced by the more generous American Opportunity Tax Credit in 2009. Second, the structure of veterans' benefits changed from a pure conditional cash transfer under the MGIB to an in-kind benefit system. Under the PGIB, tuition and fee benefits are paid directly to institutions, with a commensurate reduction in qualified educational expenses used for the purposes of calculating federal education tax benefits. We find that the net result of these policy changes is a sharp shift away from utilizing tuition and fee deductions towards claiming federal education tax credits. These results suggest that the delivery mechanism for providing aid may be important for tax compliance, although we are unable to disentangle this channel from changes due to the increased generosity of education tax credits.

Overall, our results suggest caution in providing education subsidies of this generosity, at least to non-traditional students. Like Castleman and Long (2016), we observe increases in degree attainment and positive labor market returns for students who were enrolled at four-year public institutions prior to the provision of additional aid. Those initially enrolled at community colleges, many of whom subsequently transferred to four-year public institutions, also benefited in the labor market. However, our results show that the quality of one's investment can matter a great deal for earnings. Policymakers have long been concerned that for-profit institutions engage in aggressive or deceptive recruiting tactics to encourage the use of GI Bill benefits (Martorell and Bergman 2013; Harkin 2014). Those who made their enrollment decisions after the expansion of benefits appear to have made less productive investments, with a greater probability of for-profit enrollment and a lower likelihood of degree attainment conditional on additional enrollment.

While educational attainment increased modestly as a result of the benefit expansion, subsequent earnings fell, especially for the least advantaged groups. Our analyses suggest that it is unlikely that earnings will grow enough to recover the earnings lost during the first nine years following separation. These results may inform recent and widespread discussions to provide free college or subsidize retraining for broad swaths of the population. Without additional guidance or guardrails to help inform student choices, it is not clear that these policies will generate the returns that policymakers might expect.

II. Background and Institutional Features

A. A Brief History of the GI Bill

The original GI Bill, the Servicemen's Readjustment Act of 1944, offered numerous provisions for World War II veterans, including payments to colleges for tuition, unemployment and housing benefits, and zero-down-payment, low-interest loans for home mortgages. There were at least three major motivations for the GI Bill. First, Congress wanted to reward returning veterans. Second, Congress wanted to redeem itself after the debacle of the 1932 World War I veterans march on Washington, in which veterans demanded payment of their veteran's bonus. Finally, there was a belief that reintegrating veterans into the civilian labor force would be a costly adjustment; sending many to college would both smooth out this process and create greater economic opportunities for veterans. The policy contributed to a near-doubling of college enrollment in less than a decade. Historian Sidney Burrell argued that the original GI Bill brought about "what may have been the most important educational and social transformation in American history" (Burrell 1967).

Numerous updates to the GI Bill followed as Congress attempted to keep up with rising real costs of higher education. The 1984 Montgomery GI Bill (MGIB) increased education benefit amounts, but also required military personnel to "pay-in" $100 per month during twelve months of their active-duty service.[10] The MGIB education benefit is a capped monthly payment for eligible veterans who are full-time enrollees at a qualified education or training program, with benefits scaled down for part-time enrollees.[11] The MGIB also includes a provision known as the "kicker" or "Buy-Up" option, which offers an additional $8-for-$1 matching program. If a servicemember makes an additional monthly contribution (totaling $600 over all months) while on active duty, they can then receive an additional $150 per month for 36 months. The benefit is paid directly to the student veteran, is non-taxable, and is intended to be used towards tuition, fees, and living expenses.

B. Key Provisions of the Post 9/11 GI Bill

Noting that military service has been particularly arduous during the wars in Iraq and Afghanistan, Congress wanted to repay, reward, and recognize veterans for their service (Dortch 2018). In addition, the continued rising real cost of higher education required a benefit expansion in order to provide better opportunities for transitioning veterans. The result was a large expansion of benefits under the Post-9/11 Veterans Educational Assistance Act of 2008 (Post-9/11 GI Bill or PGIB), which was passed in June 2008 and took effect August 2009. Along with the new

[10] Servicemembers must elect whether to pay in to the benefit during their first year of enlistment. Choosing not to pay in to the benefit is framed as opting out, likely explaining why 85% of Army veterans choose the default option of paying in.
[11] For details on MGIB benefits, see https://www.benefits.va.gov/gibill/resources/benefits_resources/rates/ch30/ch30rates080108.asp

transferability of benefits to dependents, the benefit expansion was also intended to aid in military recruitment and retention.[12]

Financially, the most significant provisions of the PGIB are the tuition and fees payments and the Basic Allowance for Housing (BAH). Between 2009 and 2011, the PGIB tuition and fees payments were capped at the maximum in-state public tuition in the state where the veteran attends college.[13] The PGIB covers any tuition and fees charged up to these limits, remitted directly to the institution, but does not refund the difference between the maximum and the charges to the veteran. In 2011, this state-by-state maximum changed to a nationwide maximum of $700 per credit or $17,500.[14] Eligible veterans enrolled at least half-time also receive directly a monthly BAH determined by the basic housing allowance for a servicemember at the E-5 rank with dependents in the military service area – similar in size to an MSA – in which the student attends school.[15] In total, the PGIB roughly doubled the average maximum benefit amount relative to the MGIB.

Importantly, the changes in benefit generosity generated by the PGIB vary significantly by geography. Unlike the MGIB, which pays a flat amount regardless of the location of the veteran or the level of tuition and fees of the college attended, the PGIB incorporated variation along both dimensions. Figure 1 shows the degree of geographic variation in predicted average annual benefit levels under the PGIB. To calculate predicted benefits, we focus on eligible veterans who initially

[12] In practice, the transferability provision is largely irrelevant for our sample given very low rates of use and the need to reenlist for four or more years to take advantage of the provision. See Castleman et al. (forthcoming) for an analysis of GI Bill transfer decisions.
[13] The Veteran's Administration also offers a Yellow Ribbon program which provides additional assistance to veterans whose tuition and fees exceed the maximum. This is a one-for-one matching program in which the VA matches contributions from the school to cover tuition and fees above the maximum benefit. Schools participate voluntarily and can designate a maximum number of vets for whom they can offer Yellow Ribbon. In practice, well over 1,000 institutions offer Yellow Ribbon.
[14] This maximum grew to $19,198 by 2016/2017 and $23,672 by 2018/2019. Veterans who were already enrolled in states with higher maximums were grandfathered.
[15] This rate is determined for the purposes of covering off-base housing in the area. See the current tables here: https://militarybenefits.info/bah-rates-state/.

enrolled in college between 2004 and 2008, before the PGIB was available. For each zip code, we calculate implied benefits under the PGIB as the average benefit for which veterans from that home of record, measured at enlistment, would have been eligible had their enrollments occurred when the PGIB was available. We set the tuition and fee benefit amount at an institution to the minimum of (1) the institution's actual in-state institution and fees, and (2) the state maximum tuition and fee level under the PGIB for the 2009-2010 academic year.[16] The BAH was set according to the zip code of the institution enrolled.

[Figure 1 about here]

Both the MGIB and PGIB require that a veteran received an honorable discharge to be eligible. For the PGIB, veterans must have also served on active duty for at least 90 days after September 10, 2001, but unlike the MGIB, they are not required to elect into the program upon enlistment or forego some monthly salary during active-duty service. Veterans can receive up to 36 months of benefits for up to 15 years after separation from the military, with maximum annual amounts and months of benefits adjusted for both length of service and enrollment intensity (Barr 2015, Dortch 2018).[17]

GI Bill benefits can be used at a wide variety of educational institutions, including traditional undergraduate and graduate programs, vocational and technical training, flight school, courses designed for certification in a particular profession, and courses designed for standardized test preparation (e.g., the SAT, GMAT). In Appendix Tables A1 and A2, we provide the 100 most

[16] We also impose a $20,000 maximum because benefits were capped at $20,000 at private schools in 2011.
[17] Veterans must serve at least 90 days on active duty after September 11, 2001 to receive some PGIB benefits. This entitles a veteran to 40 percent of benefits. There are incremental increases at 6,12, 18, 24, and 30 months of service. At 36 months of active duty service, a veteran is entitled to 100% of PGIB benefits. An exception is that veterans who are discharged with a service-connected disability are eligible for 100% of benefits if they have 30 days or more of active service.

common schools attended by veterans based on enrollment data from the National Student Clearinghouse (NSC).[18]

III. Empirical Strategy and Identification

A. Overall Impacts of the PGIB

We first investigate the overall impact of the PGIB on college enrollment, degree completion, and earnings for eligible veterans. Building on Barr (2015, 2019), we use two difference-in-differences strategies that compare cohorts that separated earlier, and thus had limited potential to use the higher level of benefits that became available in 2009, with those who separated just prior to the benefit expansion.

i) Eligible versus ineligible

We begin with a standard difference-in-differences framework, comparing veterans who are eligible and ineligible for GI Bill benefits before and after the PGIB expansion. Specifically, we run regressions of the following form:

$$(1)\quad Y_i = \alpha + \beta_1 Post_i \times Eligible_i + \beta_2 Post_i + \beta_3 Eligible_i + \gamma \boldsymbol{X}_i + \rho Z_i + \varepsilon_i$$

where Y_i is an outcome of interest, $Eligible_i$ is an indicator variable for being eligible for GI Bill benefits and the indicator variable $Post_i$ equals one for servicemembers whose year of separation from the Army is 2008 or later. The vector $\mathbf{X}_i$ represents veteran pre-treatment characteristics, and $\mathbf{Z}_i$ represents a set of 3-digit home-of-record fixed effects. We control for gender, race, education level and marital status upon entry into the Army, and age and military rank upon exit. When

[18] We are unable to release information at the school-level using the 1098-T data.

considering labor market outcomes, we additionally include Primary Military Occupation Specialty (PMOS) fixed effects. Standard errors are clustered at the 3-digit home-of-record zip code.

The key parameter of interest is β_1, which captures the effect of the additional funds provided under the PGIB. The majority of veterans during our period (74%) have an honorable discharge and are thus eligible for GI Bill benefits. This characterization of service is bestowed "when the quality of the Soldier's service generally has met the standards of acceptable conduct and performance of duty for Army personnel or is otherwise so meritorious that any other characterization would be clearly inappropriate." (U.S. Army, 2016). The identifying assumption is that the change in outcomes that would have occurred for eligible veterans in the absence of the program is captured by the change in outcomes for ineligible veterans; any difference is assumed to be a causal effect of the PGIB. We form this counterfactual using veterans who received a general discharge under honorable conditions, which account for roughly 14% of veterans. This characterization of service is "issued to a Soldier whose military record is satisfactory but not sufficiently meritorious to warrant an honorable discharge." These types of discharges often result from a pattern of minor misconduct (e.g., reporting late), a single incident of more serious misconduct (e.g., going absent without leave), illegal drug use, or physical fitness issues. These veterans are eligible for all military and VA benefits with the exception of the GI Bill. We exclude from our comparison group veterans with an other than honorable discharge, bad conduct discharge, dishonorable discharge, or entry-level or medical separations.

To align with the academic calendar, we adjust the Army separation year forward one year for separations that occur between August 15 and December 31. In our main tables, we limit the sample to cohorts separating in 2003, 2004, 2008, or 2009. The first two cohorts separate from

the Army early enough that their educational decisions are less likely to be impacted by the policy change. The second two cohorts separate between August 15, 2007 and August 14, 2009, so their educational investments are potentially heavily impacted, but their separation decision occurs before the policy is enacted. In other words, we wish to minimize the possibility that the separation decision itself is an endogenous response to the availability of benefits. We omit the 2005—2007 cohorts as being partially treated and partially untreated.

We broaden this strategy to use all separation cohorts between 2002 and 2010 in an event study framework. Instead of relying on a single delineation of pre- versus post-expansion year cohorts, we interact year of separation with eligibility status for each year 2003-2010; the 2002 cohort is the excluded group. This strategy is implemented as follows:

$$(2)\ Y_i = \alpha_0 + \alpha_1 Eligible + \sum_{\tau=2003}^{2010} \beta_\tau [\, \tau_i \times Eligible_i] + \gamma X_i + \rho Z_i + \tau_i + \varepsilon_i$$

where $\boldsymbol{\tau_i}$ represents cohort fixed effects that are identified by the ineligible veterans in the sample. This event study framework, which we use across all of our identification strategies, allows us to evaluate whether our estimates can be interpreted causally. A primary threat to validity in any difference-in-difference strategy is that there are underlying differential trends between the treatment and the control group that are not attributed to the GI Bill expansion. We directly assess the extent to which these two groups were on parallel trends prior to the expansion of benefits using coefficients β_t for early separation cohorts, although we note that the delayed availability of benefits for some cohorts may result in partial treatment effects that grow larger the closer the year of separation is to the availability of expanded benefits.

ii) High versus low-benefit areas under the PGIB

Our second identification strategy relies on cross-sectional variation in the generosity of the BAH and tuition benefits, which generates variation in benefits within localities over time. Because the actual benefits that a veteran receives are determined by his endogenous school choice, we rely instead on the exogenous variation that stems from interacting the timing of the PGIB with the benefit generosity variation associated with the veteran's home of record. This strategy compares changes in outcomes over time for eligible veterans from areas with large predicted increases in education benefits to changes for those from areas with low predicted increases.

Specifically, we predict expected available benefits for each veteran using the choices of college-going veterans from his 3-digit home of record zip code who initially enrolled between 2004 and 2008: predicted benefits are calculated as the average hypothetical maximum BAH and tuition benefit that these enrollees would receive under the PGIB in 2009. We then interact the predicted benefit amount, which is constant within 3-digit zip code, with an indicator for separating in the post-PGIB period. Prior to the PGIB, veterans received the same education benefit regardless of the cost of living or education in their area; as a result, the policy change constitutes a much bigger increase in generosity for example, for veterans who attend school in New York City relative to those enrolled in Grand Forks, North Dakota.

The estimating equation becomes:

(3) $Y_i = \alpha + \beta_1 Post_i \times \widehat{Benefits}_{HOR} + \beta_2 Post_i + \beta_3 \widehat{Benefits}_{HOR} + \gamma X_i + \rho Z_i + \varepsilon_i,$

where $\widehat{Benefits}_{HOR}$ is the predicted level of benefits assigned to each 3-digit home of record zip code. We scale expected benefits in thousands of dollars per year. The key identifying assumption is that conditional on the set of observables, unobserved factors that affect enrollment, educational attainment, or earnings are unrelated to the size of the benefit expansion in one's home of record.

To test the validity of this assumption, we run placebo analyses by estimating equation (3) for ineligible veterans used in our previous difference-in-differences analysis. In results not shown (available upon request), we generally find that the effects on benefits, educational attainment, and earnings are economically small and statistically insignificant. As a robustness check, we additionally estimate triple-difference regressions where we include ineligible veterans to control for underlying changes within localities over time (results shown in Appendix Tables A5 – A7).

B. Within-College Estimates of PGIB Effects for Veterans Already Enrolled

For eligible veterans who were just beginning their college careers when the PGIB was announced, the policy constitutes a large, unexpected jump in subsidies, conditional on continued enrollment. Those veterans who had enrolled many years earlier may not be able to utilize the additional funds – they may have already earned a BA or used several months of MGIB benefits. We use this variation to investigate the impact of the PGIB on college persistence, degree completion, and earnings for the set of veterans who had already made their college enrollment choices prior to the announcement of the benefit expansion.

In this quasi-experiment, we compare the outcomes of eligible and ineligible veterans who initially enrolled just before the additional funds became available relative to those who enrolled at that same college well before the PGIB expansion. Specifically, we run the following regression:

$$(4)\ Y_i = \alpha + \beta_1 Post_i \times Eligible_i + \beta_2 Post_i + \beta_3 Eligible_i + \gamma X_i + \delta C_i + \varepsilon_i.$$

Here, Y_i is an outcome of interest, measured relative to the year of initial college enrollment, and the vector $\boldsymbol{X_i}$ contains the same controls as in equation (1). We limit the sample to veterans who were already enrolled prior to 2009, and define the indicator variable, $Post_i$, to equal one for veterans who initially enrolled in college in 2007 or later. We include institution-specific fixed effects, $\boldsymbol{C_i}$, so identification here comes from within- college variation in benefits.

The identifying assumption is that the inclusion of the ineligible veterans removes any differential trend in within-institution outcomes that would have occurred in the absence of the program. In other words, outcomes for the eligible veterans at a given institution would have evolved in a similar way to the ineligible veterans so any differential change can be attributed to the causal effect of the PGIB. Because we rely on within-institution variation, we form this counterfactual using all ineligible veterans to maximize power, but our main conclusions are similar when restricting the ineligible group to veterans with a general discharge.[19]

We estimate equation (4) using four enrollment cohorts: 2003, 2004, 2007, and 2008. Similar to our previous strategies, these limitations are intended to create a comparison between cohorts that likely completed their schooling or largely exhausted their benefits through the MGIB prior to the arrival of the generous PGIB (2003 and 2004) and cohorts that made initial separation and enrollment decisions prior to the PGIB but spent much of their college careers under the influence of the PGIB (2007 and 2008). We also implement the event study counterpart to this strategy using all enrollment cohorts between 2002 and 2008.

Finally, we again use the geographic variation in benefit generosity to identify the impacts of financial aid amounts on persistence, graduation, and earnings. In this within-school framework, we have a tight estimate of expected BAH and tuition benefits because we know the benefits associated with the particular college of each enrolled veteran.[20] Identification comes

[19] As an alternative comparison group for outcomes derived from tax data, we draw a random sample of civilian men who initially enrolled in college between ages 17 and 20 and match the distribution of institution-based enrolled patterns as the eligible veteran population. However, we found that even conditional on initial institution fixed effects, the pre-trends and subsequent years of schooling and earnings levels of this group of traditional students is a poor match for those of eligible veterans.

[20] Of course, a veteran may transfer to a new college or area. Our estimates should thus be thought of as an intent to treat effect.

from interacting benefit generosity with an indicator for initially enrolling in the post-treatment period. We run the following regression using eligible veteran students:

$$(5)\ Y_i = \alpha + \beta_1 Post_i \times \widehat{Benefits}_C + \beta_2 Post_i + \beta_3 \widehat{Benefits}_C + \gamma X_i + \rho C_i + \varepsilon_i.$$

In this equation, Y_i, $\boldsymbol{X_i}$, and $\boldsymbol{C_i}$ are as defined in equation (4). The coefficient β_1 indicates the causal effect of an additional thousand dollars in benefits per year. The key identifying assumption is that conditional on the set of observables, unobserved factors that affect persistence in school or labor market outcomes are unrelated to the size of the benefit expansion within a college, which was determined by the PGIB.

IV. Data Description

We begin with administrative Army personnel data for the 1.2 million veterans who separated from the Army between 1994 and 2017.[21] These data include demographic characteristics, such as birth date and race, as well as home of record zip code, marital status, number of dependents, and educational attainment at the point of enlistment and separation from the Army. The data also include details of a veteran's military experience, including PMOS codes[22], scores on the Armed Forces Qualification Test (AFQT), military rank, assignment and location at each year, dates of entry and separation, and type of discharge, known as the characterization of service.

We merge these military service records with three administrative data sources: the Veteran's Administration (VA), the NSC, and administrative tax returns. Data from the VA

[21] The data were compiled by the U.S. Army of Economic and Manpower Analysis at West Point, to whom we are grateful for their assistance.
[22] https://usarmybasic.com/army-jobs/army-mos-list and https://en.wikipedia.org/wiki/List_of_United_States_Army_careers

contain information on GI Bill benefit take-up, months of benefits used, and cumulative benefit amounts used. We define total benefits received as the cumulative sum of all MGIB and PGIB benefits paid to the veteran, measured in thousands of dollars.[23]

To measure degree completion, we rely on individual-level NSC data, which provides dates of enrollment and degrees obtained for any institution attended. Although the NSC data cover between 90 to 97 percent of all college enrollments during our period, one concern is that the coverage rate for for-profit institutions is low. For this reason, our primary measures of college enrollment are based off of the information return that colleges submit to the IRS to report qualified educational expenses in a calendar year, Form 1098-T. These data do not suffer from measurement error due to differential coverage across institutions. We match both the NSC and 1098-T data to the Integrated Postsecondary Education Data System (IPEDS) to incorporate additional information about each institution that a student attends. These data allow us to partition institutions into four distinct categories: four-year public, four-year private, community college, and for-profit institutions. We also collect information on whether the institution is primarily an online college, and the bachelor's degree graduation rate.

Our key enrollment measures are indicator variables for enrollment (overall and by particular institution type) that occurs within two years of separation from the Army, although we collect all 1098-T-based enrollments between 1999 and 2018. In Appendix Table A3, we document the benefits of using tax data to measure college enrollments. The tax data indicate that 55 percent of veterans enroll within two years of their Army separation, whereas the NSC yields a 10 percentage point smaller rate that can be largely explained by missing enrollments in the for-

[23] The VA did not begin capturing annual amounts used until the last few years.

profit sector. Other differences between the two data sources are likely due to differences in timing and the FERPA blocking in the NSC.[24]

For each veteran, we construct a panel of tax return data spanning 1999 through 2018 to measure tax benefit utilization and labor income measures over time. We collect information on the take-up and amounts claimed for the several federal benefits for higher education that are available through the tax code over this time period. The Hope Tax Credit (HTC) and Lifetime Learning Tax Credit (LLTC) allow for a dollar-for-dollar reduction in taxes for qualified education expenditures, capped at the household's overall tax liability amount (i.e., the credits are nonrefundable). Whereas the HTC is available only for the first two years of college, the LLTC can be used for virtually any postsecondary coursework. In 2009, the HTC was temporarily replaced by the more generous American Opportunity Tax Credit (AOTC) – now permanent as of 2015 – which provides benefits that are partially refundable and can be claimed for the first four years of college enrollment. An alternative to the tax credits is the above-the-line deduction for tuition and fees (DTF) from gross income, up to a cap. The DTF was created under the Economic Growth and Tax Relief Reconciliation Act of 2001, and is available to students who have modified adjusted gross incomes below a specified amount. An individual must choose between the DTF and one of the other education tax credits.

Individual-level measures of labor income are computed as the sum of wage and salary income reported on Form W-2, the information return that employers submit to the IRS to report wage and salary income, and self-employment income reported on Schedule SE, which contains

[24] For NSC data, the sector of enrollment is measured based on the first enrollment measured in the calendar year. For the tax data, we are unable to see when each enrollment occurs if there are multiple 1098-Ts issued in the same year. As a result, we rank institutions based on their sector in the following order: four-year public, four-year private, two-year, and for-profit institutions. If after this rank ordering, a veteran still receives a 1098-T from more than one institution, we randomly select one.

net income from partnerships, S corporations, and sole-proprietorships. W-2 income is available even when a veteran does not file a tax return; in this case, Form 1099-MISC, the information return that businesses submit to the IRS to report gross non-employee compensation, provides information on some sources of self-employment income.

To account for the unedited nature of the administrative tax return data, we winsorize all income amounts at the 99th percentile of the distribution of positive values; in the case of variables that can also take on negative values (i.e., self-employment income and AGI), we winsorize at the 1st and 99th percentiles of the distribution of non-zero values. All income measures are adjusted to 2016 dollars using the CPI research series.

We impose several sample restrictions. We limit our analysis sample to the cohorts that separated from the Army between 2002 and 2010. The lower limit is driven by the fact that the fields we use for exact date of separation and characterization of service are only observed in our data starting in 2002. We impose the upper limit so that we have meaningful earnings data on the veterans nine years after separation, when most are in their 30s. We exclude all Army veterans who already had a bachelor's degree upon enlisting. To focus our analysis on younger veterans who are most likely to make significant human capital investment and exclude career military veterans, we limit the sample to veterans who served between 1 and 6 years and who were age 39 or younger upon separation.

Summary statistics for our three main estimation samples are shown in Table 1, restricted to the four cohorts that comprise the pre-treatment and post-treatment groups in our baseline difference-in-differences strategies. Columns (1)—(2) contain eligible and ineligible veterans who separated in 2003, 2004, 2008, or 2009, and columns (3)—(4) contain only eligible veterans from

those same separation cohorts. In Column (5)—(6), we present summary statistics for our enrollment year analysis: these include eligible and ineligible veterans who initially enrolled in college in 2003, 2004, 2007, or 2008. Because the intertemporal variation that we use depends on either when a veteran separated from the Army or when a veteran initially enrolled in college, the outcomes are measured differently across columns. In columns (1)—(4), the enrollment outcomes are measured within two years of separation, bachelor's degree completion is measured within six years of separation, associate's degree completion is measured within three years of separation, and the longer-run outcomes are measured nine years after separation. In columns (5)—(6), enrollment is measured at the time of initial enrollment, degree completion is measured as of 2019, and longer-run outcomes are measured nine years after initial college enrollment.

[Table 1 about here]

Across all estimation samples, 75—80 percent of the veterans are male, 16—19 percent are black, and 11—12 percent are Hispanic. The average age of separation is 24. At the time of enlistment, the highest level of education for the vast majority of the sample was a high school degree, while 5 percent had some college. Turning to our key outcome variables, 55 percent of veterans enrolled in college within two years of their Army separation, with two-year public institutions being the most common type of college chosen. Approximately 9 percent of veterans completed a bachelor's degree within six years of separating from the Army, and their average wage and salary earnings nine years after separation is $32,000 in 2016 dollars. Among those who had already enrolled prior to the PGIB expansion, almost half of veterans enrolled in a two-year public college and slightly over a quarter had chosen a four-year public institution. Nearly 32 percent of these veterans earn a bachelor's degree, and their wage and salary income nine years after their initial enrollment is $36,200.

V. Results

A. Validating the Empirical Approaches

We begin by confirming that the PGIB has a "first-stage" effect on total GI Bill benefits received in each of our identification strategies. First, Panel (a) of Figure 2 shows estimates of the overall impact of GI Bill eligibility on total benefits and on PGIB benefits alone by separation year. Eligible veterans who separated in 2009 received an additional $22,240 in PGIB benefits relative to those who separated in 2002. These veterans received $11,900 more in total benefits after accounting for MGIB payments. Notice that the earlier cohorts of 2003-2006 also see some increases in benefits received. These earlier cohorts are eligible for PGIB benefits even though these benefits only exist as of 2009, though many veterans may have exhausted their MGIB educational benefits before the PGIB arrives. However, there is a modest treatment effect on the earlier cohorts and, as such, our difference-in-differences estimates may understate the total effects of the PGIB.[25] Column (1) of Panel A in Table 2 presents corresponding difference-in-difference results from equation (1) for total benefits: the PGIB increases total benefits received by $7,100 for eligible veterans separating in 2008/09, relative to those separating in 2003/04.

[Figure 2 about here]

[Table 2 about here]

Next, we illustrate the relationship between our estimate of the generosity of PGIB benefits in a home of record and the total benefits actually received by veterans with corresponding homes of

[25] To the extent that earlier cohorts of eligible veterans are using the higher benefits to enroll after 2009, this may result in a slight upward bias in estimated earnings effects as the wages of these pre-period veterans may be slightly depressed 7 to 9 years after separation.

record (Panel (b) of Figure 2). We group homes of record into $1,000 bins based on the average PGIB maximum benefit that would have been received if veteran pre-period enrollment had instead occurred in the post-period. We then plot the average difference in total benefits received between the pre- and post-periods within these bins, overlaid with a linear and fractional polynomial fit. This figure suggests a strong positive relationship between predicted and actual benefits received. The slope of 2.0 indicates that each $1,000 increase in predicted *annual* total benefits available is associated with an increase in *total* benefits received of $2,000.

The remainder of column (1) in Table 2 uses equation (3) to estimate how benefits received responds to geographic variation in expected benefits among eligible veterans separating in the post- versus pre-period. We regress total benefits received on annual measures of expected maximum benefits; this stock versus flow difference explains why our coefficients can exceed 1. A $1,000 increase in annual total benefits available raises cumulative benefits received by $1,700 (Panel D). Total benefits received responds strongly to maximum expected housing benefits. If veterans from prior cohorts and from the same home zip code attended schools that would receive (in 2009) a $1,000 higher BAH per year, total dollars received rises by $2,300 (Panel B).

One potential concern is that these relationships are driven by changes in the composition of soldiers choosing to return to areas with high benefit levels under the PGIB. Recall that the assignment of benefit generosity is based on a soldier's home of record at enlistment, not where they choose to go after separation. To the extent that soldiers move to take advantage of higher PGIB benefits in an area, this would attenuate the effect of our benefit generosity measure on total benefits received. Another possibility is that soldiers who are interested in or are on the margin of benefit use will be more likely to separate if they are from high benefit areas. This type of endogenous separation is unlikely in our difference-in-differences sample because the choice to

separate generally occurred well before the availability of the expanded benefits. Furthermore, this type of endogenous separation does not appear to be the case empirically. In Appendix Table A4, we explore whether the variation in predicted benefit generosity is related to the underlying demographic characteristics of veterans. Specifically, we estimate equation (3) but omit the demographic characteristics and instead use them as dependent variables. We see economically small and mostly statistically insignificant relationships between the benefits available and the change in the composition of soldiers returning there. Although the variation in benefits is positively associated with educational attainment, the coefficient sizes are modest: for example, an additional $1,000 in annual benefits is associated with a 0.3 percentage point increase in the likelihood of having a high school degree.

The magnitudes of the estimated impacts of the PGIB on benefit dollars received are generally consistent between our strategy that relies on variation across veteran eligibility (Panel A of Table 2) and those that rely on variation in generosity across geography (Panels B—D). The average amount of maximum annual PGIB benefits is roughly $8,000 greater than the maximum annual amount available under the MGIB. When we multiply the coefficient in Panel D (1.71) by eight, we obtain 13.7, which is higher than but consistent with the basic difference-in-differences estimate in Panel A.

Lastly, we examine the impacts of the introduction of the PGIB on those who were already enrolled in college at the time of the expansion. Panel (c) of Figure 2 shows that the PGIB has a large impact on benefit amounts received for the eligible veterans who had recently enrolled. Corresponding estimates from equation (4), presented in Panel A of column (2) in Table 2, reveal that eligible veterans who enrolled in 2007 or 2008 received an additional $11,800 in cumulative benefits relative to those who enrolled in 2003 or 2004. In columns (3)—(6), we provide estimates

of the impact of the PGIB on benefit utilization by the type of institution in which a veteran is enrolled. Unsurprisingly, veterans initially enrolled in four-year public and four-year private nonprofit institutions see the largest increases in benefit receipt, with total benefit increases of $15,000 and $16,000, respectively, but there are significant increases across all four college sectors.

The bottom three panels of columns (2)—(6) use geographic variation to estimate the impacts of the generosity on benefits received. For eligible veteran students, an additional $1,000 of BAH raises total benefits received by $970, with similar effects across sectors. Overall and across sectors, housing benefit generosity is more strongly associated with cumulative benefits received than is tuition benefit generosity. This pattern may reflect several possibilities, including that the BAH depends only on the zip code of enrollment and not the specific institution enrolled (which we may incorrectly assign if a veteran transfers), some interaction between the tuition and fee benefit and other available financial aid (which might reduce the actual tuition and fee benefits paid), or differential effects of the benefit types on the months of benefits used (which would influence the *cumulative* benefits received).[26]

B. Effects on Education Choices for Eligible Veterans

To examine the impact of the PGIB expansion on college enrollment for the overall eligible veteran population, we define a series of indicator variables that equal 1 if a veteran is enrolled within x years of separation from the Army and zero otherwise, where $x = 1, \ldots, 6$. Figure 3 presents estimates of β_τ from equation (2) for each of these enrollment outcomes. For the 2009

[26] The modest overall effect of tuition and fee variation can be reconciled with the larger within sector effects by an inverse relationship between average tuition and fees in a sector and the duration and intensity of PGIB receipt for students in that sector. For example, students enrolled in for-profit institutions have high potential tuition and fee benefits, but tend to enroll for shorter periods of time.

separation cohort, we find that that PGIB raised the enrollment of eligible veterans by 5 percentage points within three years relative to the 2002 cohort (panel (c)), and this enrollment boost remains fairly constant when looking across longer windows post-separation up to 6 years after separation. For the 2010 cohort, the enrollment impacts are more immediate, with a 5.6 percentage point rise in enrollment within a year of separation that increases to a 7.9 percentage point gain within six years.[27]

[Figure 3 about here]

Importantly, these figures provide compelling evidence for our overall difference-in-difference identification strategy. The vertical line in each panel indicates the first cohort whose enrollment could have been affected by the higher benefit levels. The trends in college enrollment propensities between eligible and ineligible veterans are quite similar prior to these critical threshold dates, and only begin to increase after the PGIB benefits could have an impact on outcomes. These patterns suggest that our estimates can be attributed to a causal effect of the PGIB, rather than to differential trends across eligible and ineligible veterans. This figure also provides further support for defining the pre-treatment period as comprising cohorts who separated from the Army prior to 2008. As the period over which we define our enrollment measure increases between panels (a) and (f), we consistently find no discernable effect on the decision to enroll for these earlier cohorts even when they could have utilized PGIB benefits.

Table 3 presents estimates for the probability of enrollment occurring within two years of separation, overall (column 1) and by sector (columns 2-5). The first row presents the difference-

[27] In Appendix Figure A1, we use an alternative enrollment measure based on whether a veteran ever enrolls in college according to enrollment data from the NSC. Although these enrollment effects are larger than those obtained using our longest timeframe considered using the tax data, the overall patterns are similar.

in-differences estimates from equation (1). Consistent with the event study graph shown in panel (b) of Figure 3, the impact of the PGIB on overall enrollment within two years of separation is small, at 0.25 percentage points, and statistically insignificant. There are, however, appreciable shifts in the types of institutions in which eligible veterans are enrolled: for-profit college enrollment rises by 1.5 percentage points, while four-year public institution enrollment falls by a nearly corresponding 1.6 percentage points. Enrollments in four-year private colleges and community colleges are largely unaffected. In results not shown, we find that the PGIB caused a 1.8 percentage point increase in enrollments within three years of Army separation, and 2.8 percentage point increase within six.

[Table 3 about here]

These modest increases in enrollment for the eligible veteran population are accompanied by increased college persistence, measured by the cumulative months of GI benefits used and the number of years enrolled in college (i.e., the number of years with a Form 1098-T). Panel (a) of Appendix Figure A2 shows that while there is a small increase in relative benefits used between 2002 and 2004, perhaps as a result of an increase in MGIB generosity in 2003, the difference in months used rises sharply between 2006 and 2009.[28] The corresponding increase in months of benefits used between 2003/04 and 2008/09 is 1.4 months, or 0.17 academic years of additional enrollment (Table 4, Panel A, column 1). Panel (b) of Appendix Figure A1 reveals that following a period where eligible and ineligible veterans' college enrollments are on parallel trends, there is 0.1 year increase in years enrolled in 2008 that continues to rise for subsequent separating cohorts.

[28] This figure is somewhat difficult to interpret because ineligible veterans who remained ineligible would automatically be observed using zero months of benefits.

This impact is a 0.2 year increase in college persistence between the 2003/04 and 2008/09 separating cohorts (Table 4, Panel A, column 2).

[Table 4 about here]

Using the geographic variation in the change in benefit generosity, we find that *within the eligible population*, increased benefits do not elicit additional enrollments, but instead drive longer enrollments. Figure 4 presents estimates analogous to Figure 3, now using geographic variation in total predicted PGIB benefit levels within a soldier's home of record as our source of identification. There appears to be, at most, modest effects of additional benefits on enrollment. Within one to two years of Army separation (panels a and b), the point estimate for the effect of total benefit generosity on enrollment for the 2008 cohort is negative. The point estimate for the 2009 cohort suggests that $10,000 in additional benefits raises enrollment within one to two years by a statistically insignificant 1 percentage point. Although statistically insignificant, the upward trend in benefit utilization again only appears to occur after eligible veterans are able to use the expanded PGIB benefits, providing additional support for the validity of this identification strategy.

[Figure 4 about here]

The bottom three panels of Table 3 present corresponding regression estimates. These estimates indicate at most modest enrollment responses to the geographic variation in the size of the benefit expansion, with suggestive evidence of a relative shift out of for-profit institutions and into two- and four-year public institutions and four-year private institutions. Only the effect on four-year private enrollment is significant at conventional levels; an increase in annual benefits of $8,000 corresponds to an increase in four-year private enrollment of 1.7 percentage points (or 34 percent of the mean).

Instead, we find a strong relationship between predicted PGIB benefit levels and both the months of benefits used and years of college enrollment as the PGIB benefits became available. Figure 5 suggests a modest increase in college persistence measures across separating cohorts between 2004 and 2007 that reflects the partially treated nature of these cohorts. This relative increase in persistence accelerates for the 2008 and subsequent separating cohorts. In Panel D of column (1) and (2) of Table 4, we find that each $1,000 in predicted total benefits raises months of benefits used by 0.3 months (0.033 academic years) and years enrolled by 0.039.

[Figure 5 about here]

These observed increases in enrollment and college persistence for the overall eligible veteran population translate into modest increases in degree attainment. Columns (3) and (4) in Panel A of Table 4 reveal that for eligible veterans who separate in 2008/09, bachelor's degree attainment rises by 1.2 percentage points, and associate's degree attainments rise by 0.4 percentage points relative to those separating in 2003/04. The evolution of degree attainment by separation year is shown in Figure 6, which suggests larger bachelor's degree attainment impacts for the 2010 cohort (which include potentially endogenous separation in response to the PGIB), at 3 percentage points. The trends in educational attainment show that this increase in degree completion coincides exactly with those cohorts for whom we observe the PGIB expansion affecting the decision to enroll in college.

[Figure 6 about here]

Among veterans from areas with relatively high implied PGIB benefits, the observed increases in college persistence translate into statistically significant increases in bachelor's degree attainment. An additional $10,000 in annual total benefits raises bachelor's degree receipt by 3.5

percentage points (Table 4, column 3, Panel D). Given the overall average increase in benefit generosity as the new benefits became available (of around $8,000), these estimates are generally consistent with (though larger than) the difference-in-differences estimates in Panel A.[29]

The pattern of larger effects stemming from the geographic variation in the expansion of benefit generosity versus the overall availability of the expanded benefits is consistent across measures of educational attainment, with larger implied effects on months of benefits used, years enrolled, and bachelor's and associate's degree receipt. One possibility is that this disparity stems from a difference in the salience of the benefit. While veterans and institutions were quite aware that the PGIB massively increased the generosity of the education benefits available to veterans, there was considerably less awareness of how the geographic variation in benefit generosity worked in practice. This may have resulted in an average increase in low-intensity or low-quality enrollments at the national level that resulted from veterans who were not particularly interested in enrolling responding to the incredibly generous benefit expansion, or institutions proactively recruiting these types of veterans. This story is consistent with the nationwide average increase in for-profit enrollment among eligible veterans, and the modest corresponding increase in degree attainment relative to the increase in years enrolled. The less obvious changes in relative benefit generosity occurring across areas were more likely to benefit those who had already planned to enroll, with greater benefits accruing to those enrolled in more expensive, perhaps higher quality, four-year institutions. The relative increase in productive (i.e., degree-producing) enrollments

[29] Appendix Figure A5 and A6 show the geographic variation event study results for bachelor's degree and associate's degree attainment, respectively.

observed in high- versus low-benefit areas (Panel D of Table 4) is consistent with this interpretation.[30]

C. Effects on Education Tax Benefit Utilization for Eligible Veterans

Along with shifting enrollments, the PGIB may also impact how veterans pay for college. Of particular interest is the extent to which GI Bill benefits interact with the federal tax benefits that also aim to promote investments in higher education. There are two important changes that occurred. First, in 2009, the Hope Tax Credit was replaced by the more generous American Opportunity Tax Credit (AOTC). The AOTC increased the maximum credit amounts, extended the income range for eligibility, expanded the set of qualified expenses, and made the credit partially refundable. Second, the delivery mechanism for providing veteran financial aid changed under the PGIB expansion. While veterans are required to enroll in an approved program, MGIB aid is paid directly to veterans and is not restricted to be used for tuition and fees. As such, these amounts need not be deducted from qualified educational expenses for the calculation of education tax credits or the DTF if, for example, they were used for living expenses. In contrast, the VA remits PGIB tuition and fee benefits directly to the institution in which a veteran is enrolled, and qualified educational expenditures are reduced commensurately. Housing benefits are still paid directly to veterans and are nontaxable.

Along with the increased veteran benefits, the change in payment method for GI benefits should result in a reduction in the utilization of all federal education tax benefits conditional on

[30] Another possibility is the presence of some minor confounders correlated with the geographic variation in the size of the benefit expansion. Indeed, while precision is reduced significantly, including ineligible veterans as an additional control group attenuates the resulting estimates somewhat (Table A4-A5). However, this might also be explained by the small share of ineligible veterans who petition for access to PGIB benefits, resulting in modest treatment effects on the educational attainment margin in this group as well.

enrolling. At the same time, if the PGIB induces veterans into colleges that are more expensive than the maximum PGIB benefit amounts, then there could also be a corresponding increase in the utilization of education tax benefits. The increased generosity of available credits under the AOTC generally makes education tax credits more attractive than the DTF, and the expansion of AOTC eligibility for both low-income and higher-income families may increase the utilization of the tax credit. Put together, these changes suggest that DTF utilization conditional on enrollment should fall, but there is an ambiguous effect on education tax credit utilization.[31]

We examine the take-up rates and amount of benefits claimed for both the DTF and education tax credits, measured cumulatively over the first two years after Army separation. As expected, the top panel of Figure 7 shows a marked drop in the utilization of the DTF and the average amount of deductions taken following the introduction of the PGIB. The difference-in-difference estimates in columns (1) and (2) of Table 5 reveal that DTF take-up rates fell by 5.8 percentage points, with a corresponding $240 reduction in deductions taken. Panels (c) and (d) of Figure 7 show that the net effect of the PGIB and AOTC expansions on eligible veterans is an increase in education tax credit take-up and average amounts claimed. The difference-in-difference estimates in Table 5 point to a two percentage point rise in take-up of education tax credits, with the average dollar amount of education tax credits taken rising by about $200.[32]

[Figure 7 about here]

[Table 5 about here]

[31] A related possibility is that the PGIB eliminates the possibility of taking the Tuition and Fees Deduction for student vets (because tuition benefits are paid directly to institutions) and hence student vets find a way to claim the education tax credit for non-tuition qualifying expenses.
[32] This includes zeroes for veterans who did not take a take credit.

Importantly, the introduction of the AOTC impacted both eligible and ineligible veterans. To the extent that ineligible veterans responded to the AOTC expansion, which was relatively more generous for them net of the PGIB, this suggests that our earlier enrollment effects are a lower bound for the impact of the PGIB. Appendix Figure A7 and the bottom three rows of Table 5 explore whether there were changes in education tax benefits between eligible veterans with higher- versus lower- predicted benefit expansions. While there is no differential response in the claiming of education tax credits, there is an increase in the amount of education tax credits claimed for those with higher predicted benefits, consistent with these higher benefits being associated with increased enrollments in four-year private institutions (Table 3).

D. Effects on Labor Market Outcomes for Eligible Veterans

Next, we examine whether the gains in enrollments or degree completions caused by the PGIB expansion improved subsequent labor market outcomes for veterans. Figure 8 illustrates a reduction in labor income nine years after separation of about $900 for cohorts separating in 2008 and 2009, relative to 2003 and 2004 (Panel (a)), and an even larger fall when labor income is measured as average earnings in years 7-9 after separation (Panel (b)).[33] In Table 6, we show the evolution of the effect on labor income between 6 and 9 years since separating and on the average of earnings across years 7-9. Using the difference-in-differences specification (Panel A), the PGIB has consistently negative impacts on wages and labor income for veterans. Six years after separating, eligible veterans earn $3,000 less than ineligible veterans, and this gap reduces to a

[33] These reported effects are conservative relative to the estimated impacts of the PGIB relative to the 2002 separating cohort.

$900 deficit nine years after separating; averaging over years 7 through 9 yields a roughly $1,400 earnings reduction, or about 4.4 percent of mean earnings.[34]

[Figure 8 about here]

[Table 6 about here]

When we instead examine the log of earnings, we find a similar pattern in the coefficients, although the patterns of statistical significance differ slightly (Table 7, Panel A). The PGIB has a consistently negative estimated effect, though the estimates are only statistically significant 6 years after separation. Overall, rather than improving labor market outcomes, the PGIB caused a drop in earnings, though the magnitude of this reduction is shrinking over time.

[Table 7 about here]

One alternative explanation for the negative earnings effect is that the Great Recession may have had differential labor market impacts on eligible and ineligible veterans. In particular, for veterans in our pre-treatment period (2003/04), labor market earnings six years later coincide with the financial crisis (2009/10). We might expect that the recession depressed the wages of ineligible veterans more than those of eligible veterans given the more detrimental effects of recessions on those with lower skills (Heathcote, Perri, and Violante 2020, Yagan 2019). That said, it is possible that ineligible veterans were disproportionately drawn from less affected labor markets or affected differently than expected. Earlier work finds limited evidence of state-level labor market conditions on veteran benefit usage or educational attainment (Barr 2019, Borgschulte and Martorell 2016). In results not shown, we additionally include measures of labor market conditions

[34] We add mean wage and salary income and mean self-employment income shown in Table 1.

in our analyses. Specifically, we compute the following measures of labor market conditions for each year based on a veteran's home of record: median household income for men between 27 and 37 years of age with at least a high school degree or GED but less than a bachelor's degree, median household income when we also include those with a bachelor's degree, and the unemployment rate.[35] We use these labor market measures as controls at the home of record-by-year of observation level, also allowing them to interact with veteran eligibility. In these specifications, the estimates on the impact of the PGIB expansion are virtually unchanged, implying that the earnings reductions we find are not due to ineligible veterans being differentially affected by the recession. We explore other possible explanations for this negative return on human capital investments in detail in Section IV.

The estimates in the bottom panels of Tables 6 and 7, which rely on the geographic variation in the extent of benefit changes within the eligible veteran population, tell a more nuanced story. While higher total benefits have no statistically significant impact on earnings 6 and 7 years after separation, there are statistically significant relative earnings increases within eight years. Averaging over years 7 through 9 after separation, veterans from areas with $1,000 more in total benefits earned $242 more per year (Panel D of Table 6). The corresponding event study figures are presented in Appendix Figure A8. Visually, these figures are somewhat less compelling than the earlier event studies as the increase in earnings 9 years after separation appears to show up between 2004 and 2005, well before the separating cohorts that appear most affected by the benefit expansion. While there is a similar pattern in the years enrolled and degree attainment event studies

[35] Median income is computed as the PUMA by year average of the residualized (controlling for age and education) income for men aged 27 through 37 from the American Community Survey, weighted by ACS person-weights. We map these PUMA-based averages to zip codes. Unemployment rates are based off of the Bureau of Labor Statistics local area unemployment statistics and Census small area income and poverty statistics by county and year, mapped to zip codes.

(Panels (c) and (d) of Figure 5 and Figure A5), it is unclear whether the observed drop in the relative educational attainment and wages of veterans separating in 2003 and 2004 should contribute to the estimated effect as they are part of the pre-period and at most modestly affected by the policy change. Relative to the 2002 separating cohort, the change in wages of those from high- versus low-benefit areas is essentially zero. As such, we interpret the resulting modest positive effects on earnings with caution.

As above, one potential explanation for these effects is that the Great Recession differentially affected veterans from areas with high versus low predicted benefits. For example, if veterans from high-benefit areas were hit harder by the recession, this would reduce the relative wages of eligible veterans separating from high-benefit areas in the pre-period, upwardly biasing our resulting estimates if higher benefit areas rebounded after the recession. In results not reported, we explore this possibility with data from the American Community Survey. Using our measures of civilian labor market conditions, there is a small positive relationship between benefit generosity and the change in some measures of household income and wages between 2012-13 and 2017-18 of around $100 to $150, or 0.2-0.25 percent, roughly half the estimated effect in the eligible veteran sample.[36] However, this relationship disappears when we focus on individuals with at least a high school degree or GED but less than a bachelor's degree, which corresponds to the educational attainment of 90 percent of veterans. Including any of these civilian labor market conditions as controls leaves the estimates unchanged. However, as a result of the modest relative improvement in labor market conditions among civilians and the event study evidence, we view the relative increase in earnings across areas with greater PGIB benefit generosity as suggestive and an upper

[36] 2012-13 and 2017-18 correspond to the years of earnings observation 9 years post-separation for the pre- and post-period veterans respectively.

bound for the true effects. Indeed, Panels C-D of Table 7 contain the analogous estimates for the log of earnings. Here, there is no significant effect of total benefit generosity on average earnings 7 to 9 years post-separation, although the estimates at 8 and 9 years post-separation are positive.

A natural question is how to reconcile the suggestive evidence of a positive relative earnings effect of increased benefit generosity with the consistent evidence of a negative effect of the overall benefit expansion. First, it is important to keep in mind that the geographic variation estimates reflect the *relative* change in earnings among eligible veterans who come from areas with higher and lower predicted PGIB benefit generosity. While eligible veterans from high benefit areas may have done relatively better than those from low benefit areas, it may still be the case that they all earned less as a result of the benefit expansion. Appendix Table A7, which includes the ineligible veterans as a control group, indicates that this was likely the case. The first row of each panel indicates the difference-in-difference estimate at the mean PGIB level, while the subsequent rows allow this estimate to vary across areas with different benefit generosities. While the smaller sample size of the ineligible group limits our precision, the resulting estimates are consistent with our earlier findings. There is a significant reduction in earnings that persists 9 years out in areas with average benefit levels. The coefficient on the interaction with total benefits (TB) is small and not significantly different from zero, indicating at most modestly better relative earnings effects in high-benefit areas. However, even in areas with total PGIB benefits in the 99th percentile (i.e., $23,000 per year), the implied total effect of the PGIB is negative 9 years subsequent to separation.

The patterns of enrollments, persistence, and educational attainment effects observed across the two strategies also provides a natural explanation for the findings. Overall, the benefit expansion generated modest increases in low-return human capital investments, but these marginal investments were larger and of higher quality in areas with higher benefits. While there was an

overall shift away from four-year public and towards for-profit enrollment, the relative shift in higher benefit areas was the reverse, with the most significant increases occurring at four-year public and private institutions. Similarly, while the overall gains in educational attainment were modest, they were larger and more likely to translate into degree attainment in high benefit areas. In combination, the pattern of effects suggests that the overall benefit expansion resulted in veterans making low-quality investments that resulted in lower earnings, but that veterans in high benefit areas made modestly better investments that resulted in smaller earnings reductions. In Section IV, we return to a discussion of why the negative effects on earnings transpired, and whether veterans might be able to recover the lost earnings in the future.

E. Impacts for Veterans Already Enrolled

For veterans who had already enrolled in college by the fall of 2008, the PGIB generated a large and unexpected change in available benefits, and importantly, the extent of these changes is based on their pre-policy choices. The change provides a unique opportunity to evaluate the effect of providing generous subsidies to individuals enrolled in different types of institutions without concern for how the subsidies might have affected veterans' choices to enroll in different types of schools. We first examine whether these additional subsidies encouraged already enrolled veterans to remain in college for longer or produced higher degree completion rates. Panel A of Table 8 presents our difference-in-difference estimates, with corresponding event study analyses in Figures 9 and 10. Relative to veterans who had enrolled several years earlier, the benefit expansion caused eligible veterans who had just enrolled in college to remain enrolled for 0.2 years longer.[37]

[37] Eligible veterans also use 4 additional months of GI Bill benefits, although the control group for these estimates is imperfect given the limited eligibility for benefit use among ineligible veterans.

This increased persistence is accompanied by a 3.1 and 2.7 percentage point increase in the likelihood of earnings a bachelor's or associate's degree, respectively.

[Table 8 about here]

[Figure 9 about here]

[Figure 10 about here]

We examine whether the gains in persistence and attainment differ by the sector of initial enrollment in Panels B through E. Across all four sectors, the PGIB expansion increased years enrolled in college (column 1) and months of benefits used (column 2). However, not all of these persistence improvements lead to increased degree attainment. The impact on bachelor's degree attainment is strongest, by far, for veterans who begin in four-year institutions; these veterans see a 7 percentage point rise in bachelor's degree completion, while those enrolled in community college appear to transfer at higher rates and are ultimately 3 percentage points more likely to earn a bachelor's degree. Estimates for veterans in four-year private institutions are quite imprecise, reflecting the small number of veterans enrolled in these institutions. Those enrolled in for-profit institutions do not see increased bachelor's degree attainment, although they do improve their likelihood of earnings an associate's degree by 6.8 percentage points.

Finally, we ask whether these elevated investments in education improved earnings outcomes. Event study results for our difference-in-difference strategy are presented in Figure 11, with corresponding estimates in Panel A of Table 9. While negative six years after initial enrollment, the average effect of the PGIB on those already enrolled turns positive three years later. The average effects conceal considerable heterogeneity across school types (Panels B—E), with large positive effects for those initially enrolled at two-year and four-year public schools, where we also

observe the largest effects on bachelor's degree attainment. The earnings estimates are negative and insignificant for those initially enrolled in for-profit schools.[38] As before, the effects for those enrolled in four-year private institutions are extremely imprecise, although the point estimate is negative.

[Figure 11 about here]

[Table 9 about here]

Estimates using school-level variation in benefit generosity, presented in Appendix Tables A8 and A9, are less informative. Although individuals who were initially enrolled in schools with higher benefits under the PGIB received larger amounts of aid (Panel D of Table 2), there is no clear evidence of an effect on educational attainment. Overall, we can reject effects on bachelor's degree receipt larger than 0.3 percentage points per $1,000 of aid, with limited evidence of heterogeneity in responses across sectors. The corresponding earnings effects are generally near zero, but are imprecise.

VI. Interpreting the Negative Earnings Effects on Eligible Veterans

To properly evaluate the GI Bill expansion requires understanding why the gains in educational outcomes failed to materialize into improved earnings for the overall veteran population, at least within the timeframe that we are able to analyze. We interpret and decompose the negative earnings impacts of the PGIB expansion by estimating treatment heterogeneity, conducting a Oaxaca-style decomposition, and projecting the potential earnings paths in the very long run.

[38] This negative effect is larger (-$2,800) and statistically significant for those initially enrolled at a for-profit institution when restricting the comparison group to veterans with a general discharge.

A. Heterogeneity of Impacts

The PGIB is likely to increase the human capital and earnings for some veterans and be harmful for others. We are particularly interested in understanding the effects on subgroups of individuals that might be expected to experience greater challenges in transitioning into the civilian workforce. For brevity, we focus on two: above or below median AFQT score, and military job classifications based on PMOS. To create categories for job types, we match PMOS codes to Census occupations and use Autor and Dorn (2013) to classify occupations into high-, middle- and low-skill jobs. The mapping of 2-digit PMOS codes to occupation classifications are presented in Appendix Table A10. Low-skill (PMOS category=0) includes military occupations that do not map to a civilian occupation (e.g., infantryman) or map to a low skill occupation (e.g., food service). Effects on these types of individuals might inform our understanding of the effects of retraining for civilians in lower wage occupations or with skills that have become obsolete. Middle-skill (PMOS category=1) largely includes skilled trades that map to a civilian job, and traditionally do not require post-secondary schooling (e.g., plumber, electrician, police officer). High-skill occupations (PMOS category=2) often correspond to jobs that require some college education in the civilian sector (e.g., computer programmer, intelligence analyst, supply chain manager).

Table 10 shows that the pattern of eligible veterans shifting from four-year public to for-profit institutions is largely driven by those with below-median AFQT scores (second row of Panel A) or those who held low-skill military jobs (first row of Panel B). Lower AFQT veterans have a 2.2 percentage point increase in for-profit enrollments, and low occupational skill veterans have a 2.6 percentage point increase, both of which are large responses relative to the overall sample mean of 16 percentage points. In Table 11, we find that although these shifts are accompanied by increases in college persistence, months of benefits used, and the probability of earnings a

bachelor's degree (columns 1—4), these least skilled veterans also earn $1,600 to $1,700 less 9 years after their separation from the Army (column 5).[39]

[Table 10 about here]

[Table 11 about here]

The impacts for high AFQT and high job skill veterans are quite different. For high AFQT veterans, there are shifts away from four-year public institutions, with increases in both four-year private and for-profit institutions, but these are not associated with significant longer-run effects on earnings. Interestingly, for the veterans who held high-skill jobs in the military, the PGIB expansion did not change their choices over whether to enroll in college, years of enrollment, or whether they ultimately earned a degree. Unlike the other subgroups, these veterans earn nearly $1,300 more nine years after separation, although this result is statistically insignificant. Put together, these results suggest that the PGIB is driving unproductive college enrollments for the least skilled or lower AFQT veterans.

We examine these two dimensions of heterogeneity for veterans who were already enrolled at the time of the PGIB expansion in Table 12. For students with above median AFQT scores, the additional aid from the PGIB induced them to remain enrolled for 0.24 years longer and increased their probability of earning a bachelor's degree. The overall increases in longer-run earnings are also concentrated among these veterans. In contrast, students with below median AFQT scores also remained in school for longer and saw increases in associate's degree completion, but without a corresponding gain in longer-run earnings. There is also suggestive evidence that the bachelor's

[39] Given that the marginal enrollment among the lower-skilled veterans is in the for-profit sector, the lack of a positive return to the additional degree attainment may be explained by the quality of the institutions awarding them.

degree competition and longer-run earnings increases is concentrated among those veterans who held medium- and high-skilled military jobs, although these are generally statistically insignificant.

[Table 12 about here]

B. Decomposing the Negative Earnings Effects into Effects Via Enrollment, Institutional Choice, Work Experience

There are several possible routes through which the PGIB might lower earnings for eligible veterans. In particular, the PGIB might shift veterans into colleges with lower labor market returns, reduce their years of work experience while in college, or shift them into lower-paying occupations. We are able to explore the first and second channels within our data; unfortunately, we do not have data on post-Army occupations.

In Table 13, we show results from a Oaxaca-style decomposition of the negative earnings effect. The "intermediate" or "mediating" outcomes that might explain the impact on earnings that we consider are: years of enrollment, years of work experience (level and squared terms), enrollment in the four college sectors, enrollment in an online institution, and enrollment in an institution with a bachelor's degree graduation rate of less than 30 percent. The first row repeats the impact of the PGIB on earnings nine years after separation that we are trying to explain (Panel A of Table 6). The remainder of column (1) presents estimates of β_1 from estimating equation (1) using one of our mediating variables as the dependent variable. To obtain an estimate of the portion of the negative earnings effect that each mediating variable explains, we multiply the estimate in column (1) by the estimated effect of that variable on earnings for eligible veterans in the pre-treatment period (column 2); the resulting earnings change explained by each mediating variable is presented in column (3).

[Table 13 about here]

The results send a clear message: the negative impacts on work experience drive most of the PGIB's negative impact on earnings. In fact, we over-explain the negative $907 effect using a quadratic function of work experience alone, likely because our OLS regression on work experience and work experience squared has a small positive coefficient on experience squared rather than the small negative coefficient one would expect. Importantly, though, the PGIB-induced drop in four-year public enrollments also explains a portion of the lost earnings.[40]

C. Will the Schooling Investment Pay Off Given Enough Time?

Given that lost work experience appears to largely explain the negative earnings effects related to the PGIB, we next consider whether the higher degree completion rates might ultimately result in veterans making up these lost earnings. The gap in earnings that we find in Table 6 shrinks between 6 and 9 years after Army separation; perhaps if we were able to look over a longer time horizon, veterans would reap earnings gains in future years. To better understand the evolution of earnings patterns for eligible veterans, we first estimate equation (1) using outcomes measured x years after separation as our dependent variables, where $x = 1, \ldots, 9$. Figure 12 plots the estimated difference in outcomes between eligible and ineligible veterans at each year after separation for the pre-PGIB period (grey diamonds) and post-PGIB period (red triangles) separately.

[Figure 12 about here]

Panel (a) shows that after the PGIB, eligible veterans increase their enrollment advantage over ineligible veterans up through year 5, but this increased enrollment response disappears by year 6

[40] The effect on work experience for those already enrolled at the time of the benefit expansion was much smaller, resulting in a net positive effect on earnings for those initially enrolled in two-year and four-year public institutions.

and then even turns slightly negative.[41] The earnings effects, shown in Panels (b) and (c), have the opposite pattern. During the first five years of higher enrollment, the PGIB reduces earnings by $1,000 to $2,000. However, this negative effect remains for several years after the increased enrollment has subsided. By nine years after separation, the negative effects on earnings appear to have moderated, but it is unclear whether these trajectories will eventually cross.

To better understand the potential longer-run earnings effects of the PGIB, we extrapolate earnings trajectories using various assumptions for returns to months of enrollment and bachelor's degree completion. This exercise allows us to examine whether plausible returns to education would predict that the PGIB will cause earnings for more recent PBIG-eligible cohorts to surpass the counterfactual earnings of earlier cohorts. We take actual earnings for years 1 through 7 since separation, and simulate growth in earnings for years 8 through 30 under various scenarios. We then calculate the net present value (NPV) of these different implied earnings paths using a 3 percent discount rate.

As our baseline, we impute earnings trajectories that incorporate returns to additional work experience.[42] We validate the efficacy of this approach by using the first seven years of post-separation earnings to forecast future earnings, which we can compare with actual post-separation earnings in years eight through thirteen for veterans separating in the pre-period. Appendix Figure A11 shows that this approach does a reasonable job in predicting the path of average earnings. We then use two scenarios that generously add returns to years of schooling: a 9 percent return to

[41] This pattern is consistent with eligible veterans exhausting their benefits or completing their education more quickly after the PGIB is available.

[42] We parameterize these returns using estimates of log earnings on work experience in the American Community Survey (ACS) for veterans age 25-55 (7.3 percent per year of experience). Because we estimate minimal effect of work experience squared among veterans in the ACS, we set the coefficient equal to the civilian equivalent (-0.23 percent).

additional years of schooling, slightly higher than the conventional assumption of 7 percent, and an enormous 25 percent return. These boosts in educational returns are included on top of the returns to additional education that are already captured in the first nine years of earnings since Army separation. Finally, rather than returns to additional years of schooling, we add a 20 percent return to bachelor's completion. The results from these simulations are summarized in Table 14 and presented graphically in Figure A12.

[Table 14 about here]

Given the modest increases in bachelor's degree completion and months of education that veterans obtain under the PGIB, we do not find that later cohorts could plausibly surpass the earlier cohorts via returns to education. . The top row of Table 14 shows that the NPV of baseline earnings losses under the PGIB is $38,800. In rows 2-4, we find that the NPV of earnings losses universally continue to be negative under the assumptions over the returns to education, with losses between $17,500 under the most generous scenario to $37,500. These NPVs represent the average post-separation working career earnings losses per veteran, and do not include the costs of the very generous program. Indeed, the average discounted earnings loss is larger than the average increase in benefits received, suggesting that the average veteran was worse off monetarily even after incorporating the transfer.

VII. Discussion and Concluding Remarks

The PGIB constitutes one of the largest expansions to college subsidies in United States history, impacting one of its most generous post-secondary aid programs. Despite its large increase in benefit amounts, the PGIB delivers substantially smaller educational gains than earlier GI Bills. Whereas Angrist (1993) finds that Vietnam veterans' benefits on average raised schooling by

between one and two years, we find average increases of one to two months. Our estimated impact of the PGIB on degree attainment of 1.2 percentage points is also much smaller than the 5 percentage point gain in bachelor's degree completion for Vietnam and Korean War veterans in Angrist and Chen (2011). These differences may be driven by the changing composition of Army enlistees. The Korea and Vietnam eras involved conscription, which likely included large groups of young men who were well-suited to college-going and further education. In contrast, the PGIB was implemented for an all-volunteer Army, and so is offered to a group of young women and men who chose the military over college after leaving high school. Furthermore, the PGIB expanded upon a veteran education benefit that was already quite generous, likely drawing veterans with the lowest returns into additional schooling.

Given the modest increases in bachelor's degree attainment in response to the large increase in GI benefits, the implied cost per additional degree is high. With the average increase in benefits for PGIB-eligible veterans is $7,100 and a corresponding 1.2 percentage point increase in bachelor's degree completion, the implied average cost per degree exceeds $590,000. Using the geographical variation in benefits among eligible veterans, the average cost is somewhat lower, roughly $486,000 per bachelor's degree.[43] Regardless, these estimates are both at the high end of the range of estimates summarized by Dynarski (2003) and Dynarksi, Hyman, and Schazenbach (2013).

Our more remarkable findings are the negative impacts on labor income for the sample of veterans exposed to the program. Averaging across seven to nine years after their separation from the Army, the PGIB lowered annual wages by about $1,400, or 3 percent of the mean. There are

[43] This estimate is produced by dividing the impact of a $1,000 increase in annual benefits offered on total benefits used by the impact on bachelor's degree attainment ($1,700/.0035).

numerous potential explanations for this finding. One possibility is that this estimate constitutes a medium-term effect, caused by the reduced labor market experience reflective of the schooling gains that we find. This is not our preferred explanation because the negative impacts survive when we control for years of experience, and the additional months of schooling are small relative to the average reduction in earnings. Earlier cohorts of GI Bill-eligible veterans see temporary drops in earnings upon school enrollment that last only 2 to 3 years, and these college-educated veterans then quickly surpass their non-college-going peers in earnings. The same is not true for the PGIB compliers, who continue to have lower earnings up to 9 years after separation.

Instead, our hypothesis is that the negative earnings returns stem from two sources: the low value-added of many of the schools chosen by veterans under the PGIB, and the generosity of the policy, particularly the BAH, that induces veterans to forego valuable labor market experience in favor of marginal human capital investments. It is particularly telling that the largest negative effects of the program accrue to the least skilled veterans – measured by their military job experience and AFQT scores – and that these are the groups to see the largest overall enrollment increase, with much of that increase concentrated in for-profit institutions at the expense of enrollments in four-year public institutions. By pursuing schooling of marginal value, veterans may be missing out on opportunities to build occupation- or firm-specific human capital, or to immediately put their Army-refined skills to work in the labor force.

This interpretation is further supported by the effects of the PGIB for veterans who had already elected to enroll in college by the time the additional subsidies became available. Rather than being induced into college-going, the policy expansion keeps these veterans enrolled in college for longer. Only at two-year and four-year public institutions, where the additional time enrolled resulted in additional high-quality degrees, do veterans appear to benefit in terms of

increased earnings. Our findings motivate ongoing work such as Barr et al. (2019) that is providing additional information about the average outcomes and net costs of various frequently-attended colleges for separating veterans.

Ours is the first study of the longer-run impacts of this large and prominent educational subsidy program. Overall, we hope that our study contributes to a deeper understanding of how impacts of college subsidies and college-going can vary a great deal across individuals: returns can actually be negative on average for a large subset of those eligible for additional benefits. These results might be informative as academics and policymakers consider broad-based policies to make college free or subsidize retraining. Without additional targeting of policies or guidance for potential students, it is not clear that these policies will generate positive returns.

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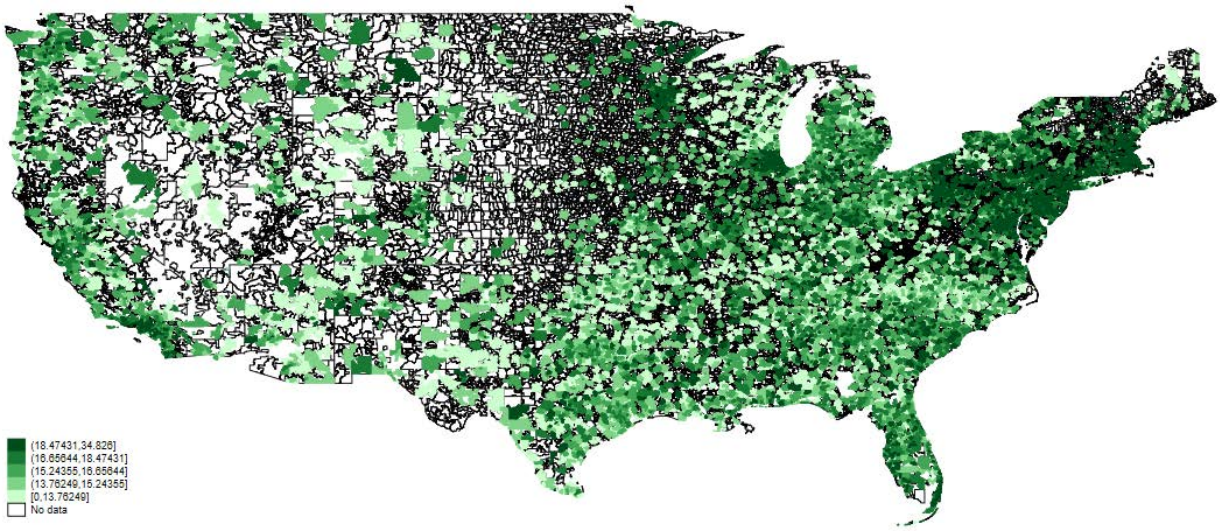
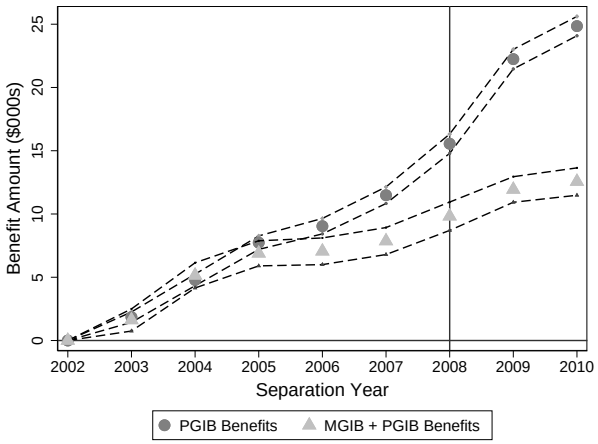
Figure 1: Post 9/11 Predicted Annual Average Benefit Level

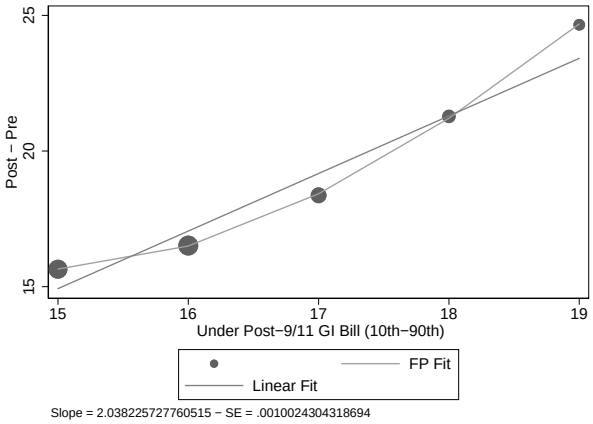


Notes: The map illustrates predicted benefits in each home-of-record zip code. Predicted benefits are computed as the sum of 9 times the average basic housing allowance and average tuition and fee benefits for eligible veterans who initially enrolled in college between 2004–2008.

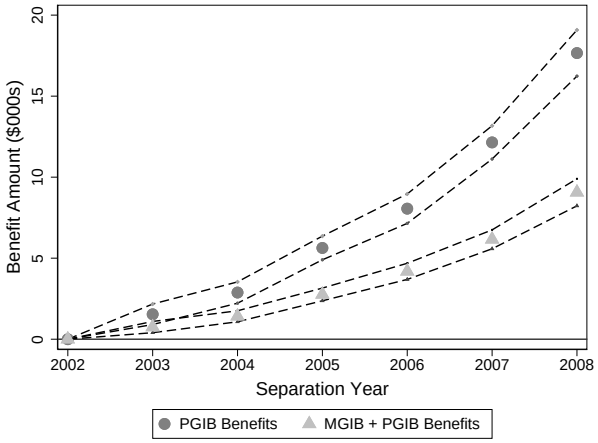
Figure 2: Event Study for the Impact on Benefits Received under PGIB



(a) Overall Impact on Eligible Veterans



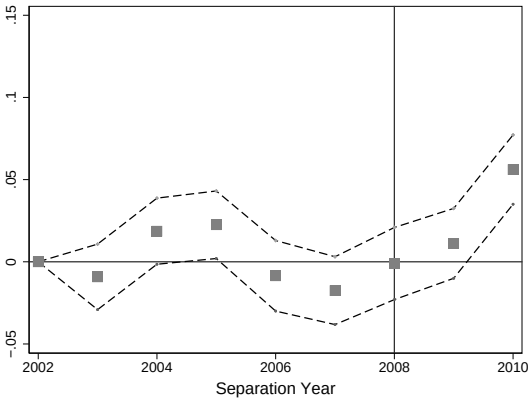
(b) Relationship between Benefits Received and Predicted Total Benefit Levels



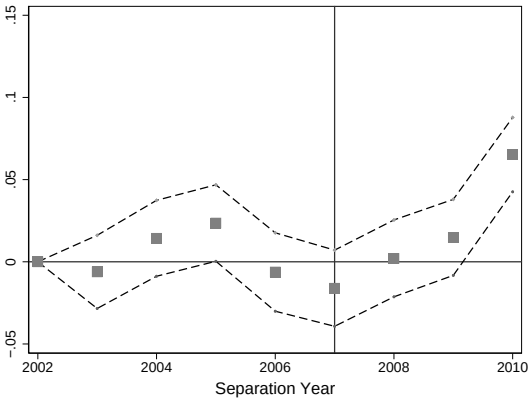
(c) Impact on Veterans Already Enrolled

Notes: Panel (a) presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who separated from the Army between 2002 and 2010. Standard errors are clustered at the 3-digit zip code level. Panel (b) presents the relationship between actual benefits received and predicted benefit levels. Circles indicate the difference in average benefits received (including zeros) between the post (2008/09) and pre (2003/04) periods in $1,000 bins of predicted PGIB benefits based on 3-digit home of record. The figure is restricted to homes of record between the 10th and 90th percentiles of predicted benefit levels. Panel (c) estimated coefficients from difference-in-difference event study regressions from regressions that include eligible and ineligible veterans who first enrolled in college between 2002 and 2012. Enrollment is defined as the first 1098-T observed within two years of Army separation. Benefits used are defined as total GI Bill benefits used as of September 2019.

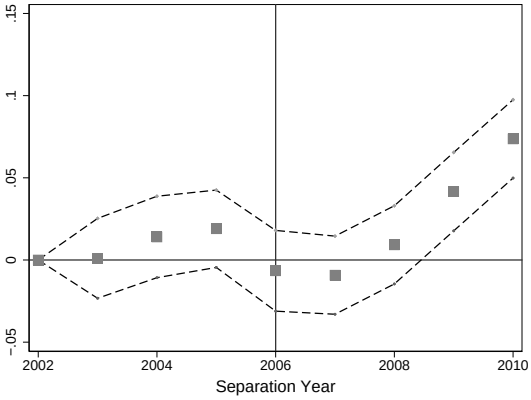
Figure 3: Event Study for Enrollment



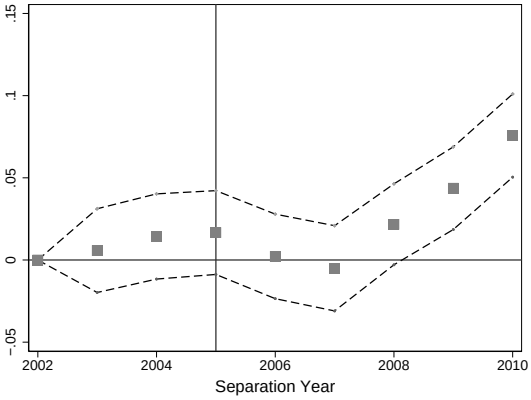
(a) Within 1 Year



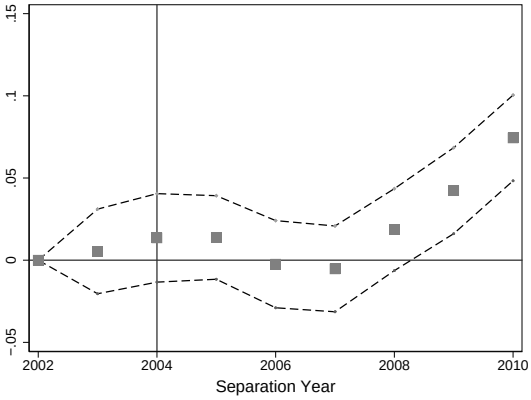
(b) Within 2 Years



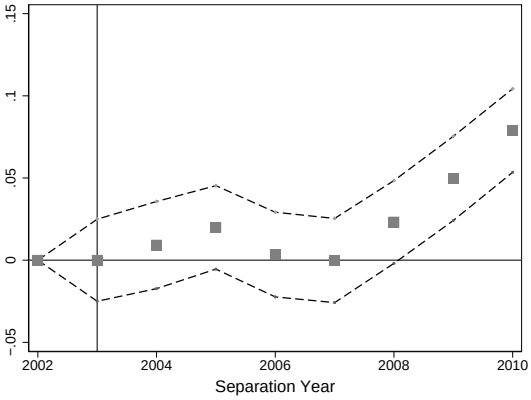
(c) Within 3 Years



(d) Within 4 Years



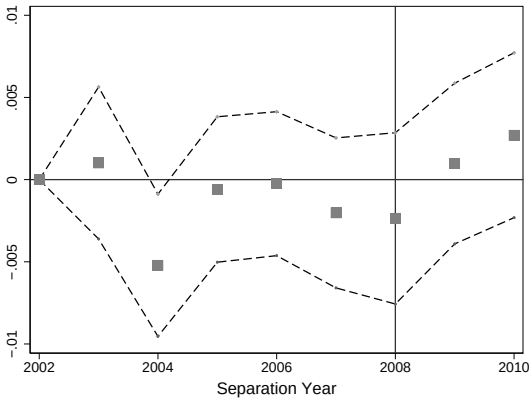
(e) Within 5 Years



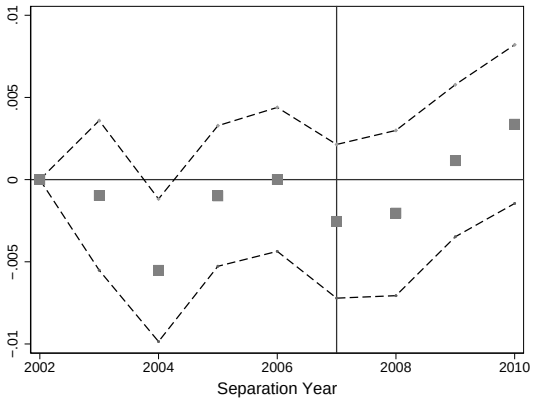
(f) Within 6 Years

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and 3-digit home-of-record zip code. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

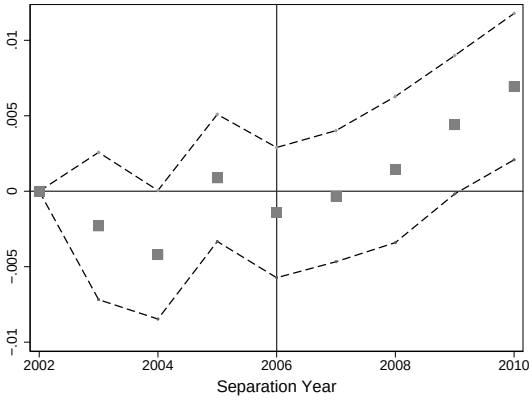
Figure 4: Event Study Using Geographic Variation for Enrollment



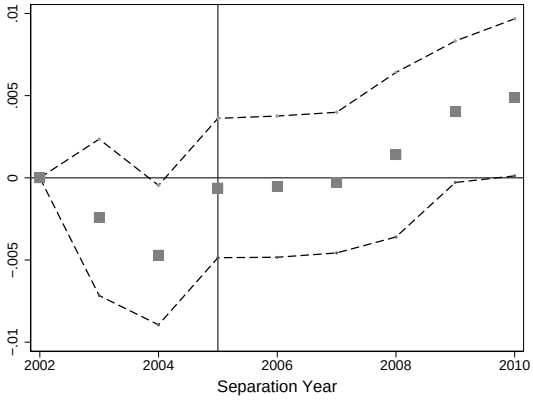
(a) Within 1 Year



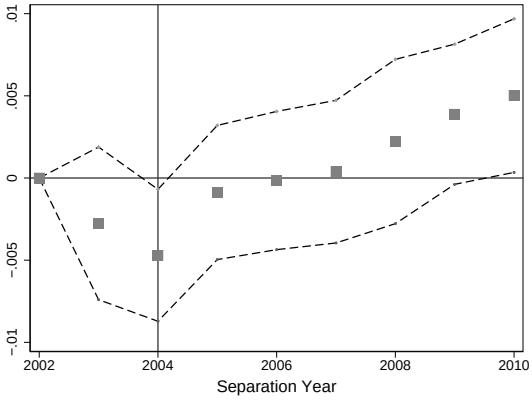
(b) Within 2 Years



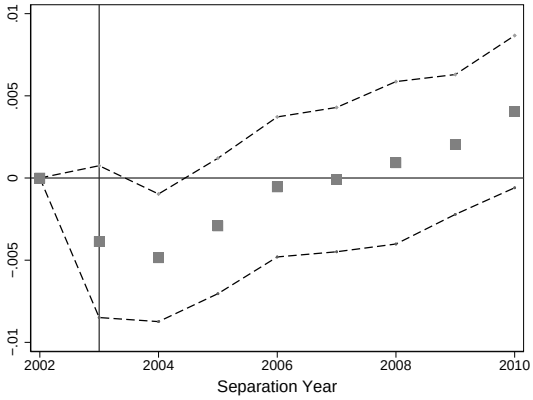
(c) Within 3 Years



(d) Within 4 Years



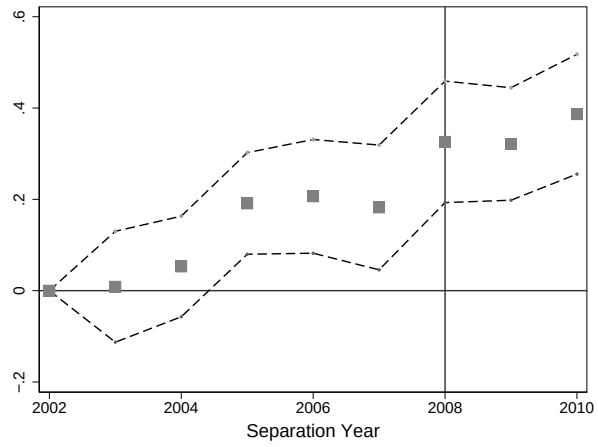
(e) Within 5 Years



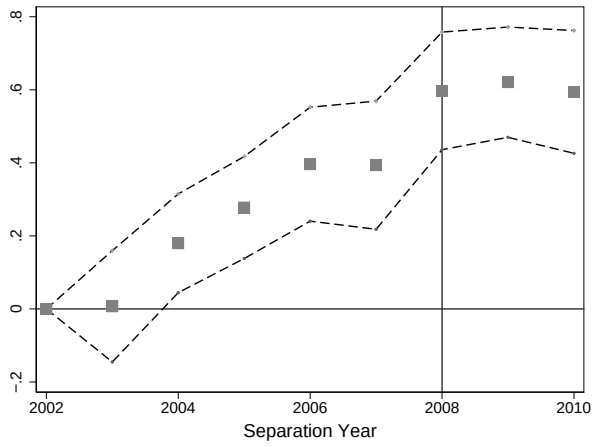
(f) Within 6 Years

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and 3-digit home-of-record zip code. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

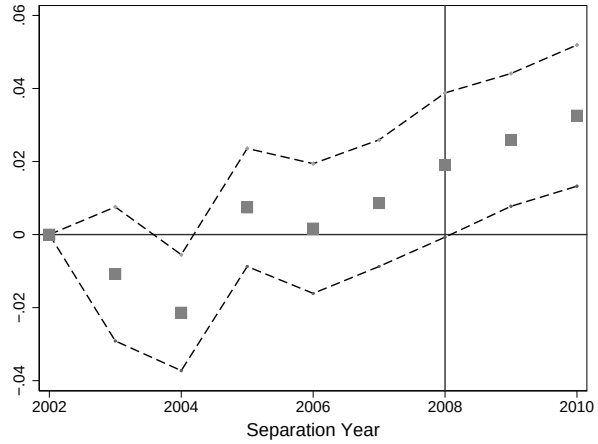
Figure 5: Geographic Variation Event Study for College Persistence



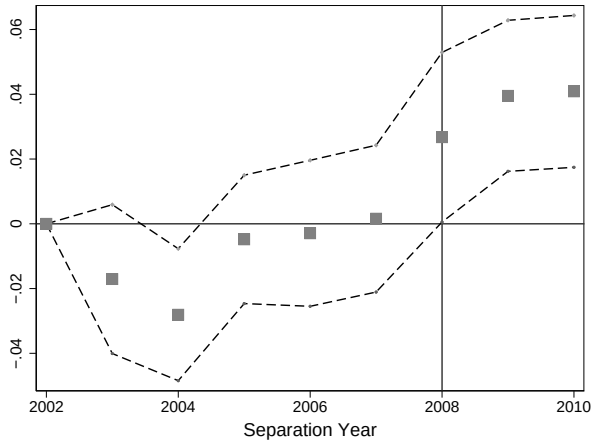
(a) Benefit Months Used, Total Benefits



(b) Benefit Months Used, Basic Allowance for Housing



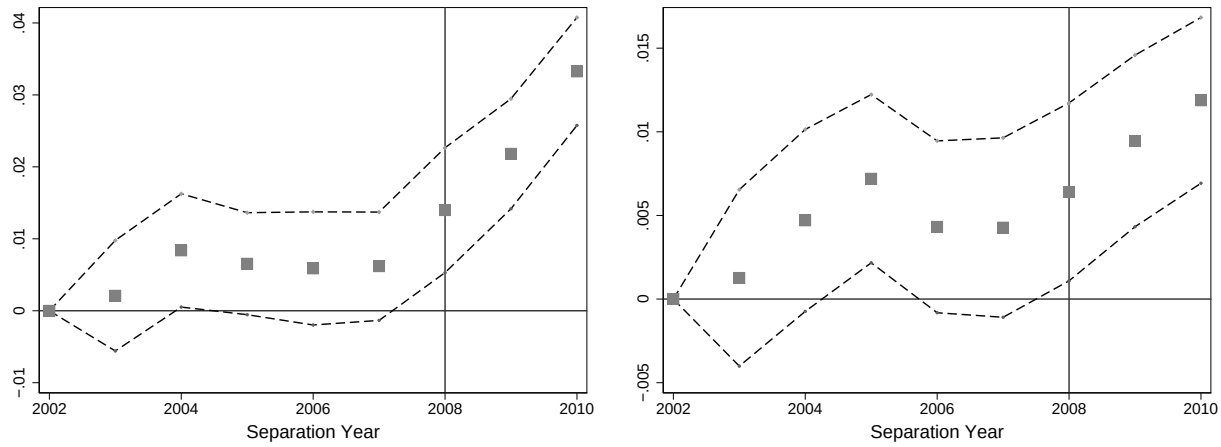
(c) Years Enrolled, Total Benefits



(d) Years Enrolled, Basic Allowance for Housing

Notes: The figure presents estimated coefficients on indicator variables for each year of separation interacted with the generosity of a veteran's home or record predicted total benefits under the PGIB. Benefits are defined as total GI Bill benefits used as of September 2019. The regression includes eligible veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and 3-digit home-of-record zip code. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

Figure 6: Event Study for Degree Attainment

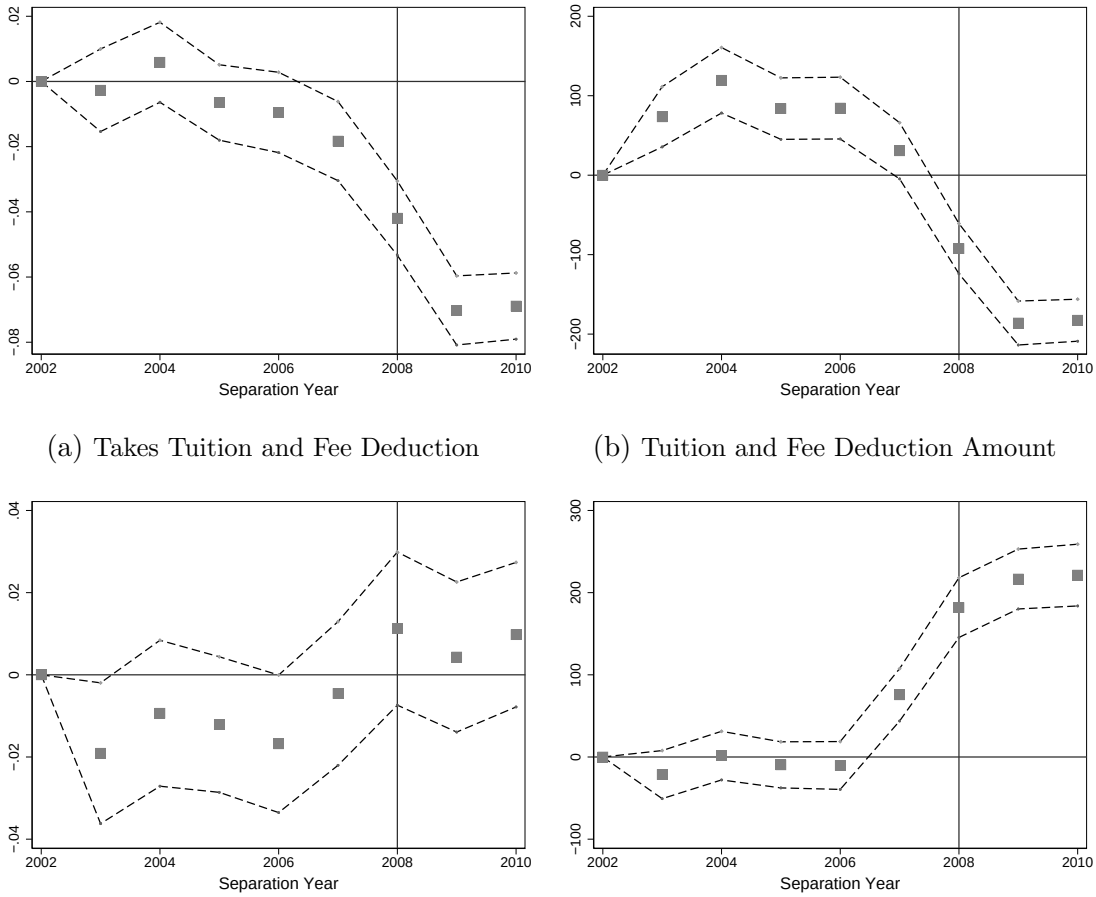


(a) BA Degree Within 6 Years

(b) AA Degree within 3 Years

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and 3-digit home-of-record zip code. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

Figure 7: Event Study for Federal Tax Benefits for Higher Education



(a) Takes Tuition and Fee Deduction

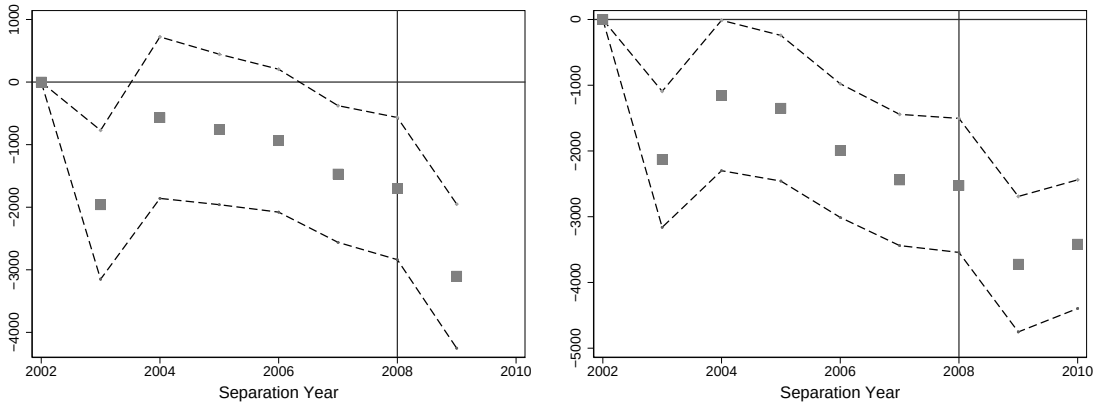
(b) Tuition and Fee Deduction Amount

(c) Takes Education Tax Credit

(d) Education Tax Credit Amount

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: for gender, race, age, educational attainment, marital status, 3-digit home-of-record zip code, and AFQT score interacted with enlistment year. Regressions additionally include military grade at separation, separation month, and military specialization (PMOS) interacted with enlistment year. Standard errors are clustered at the 3-digit zip code level.

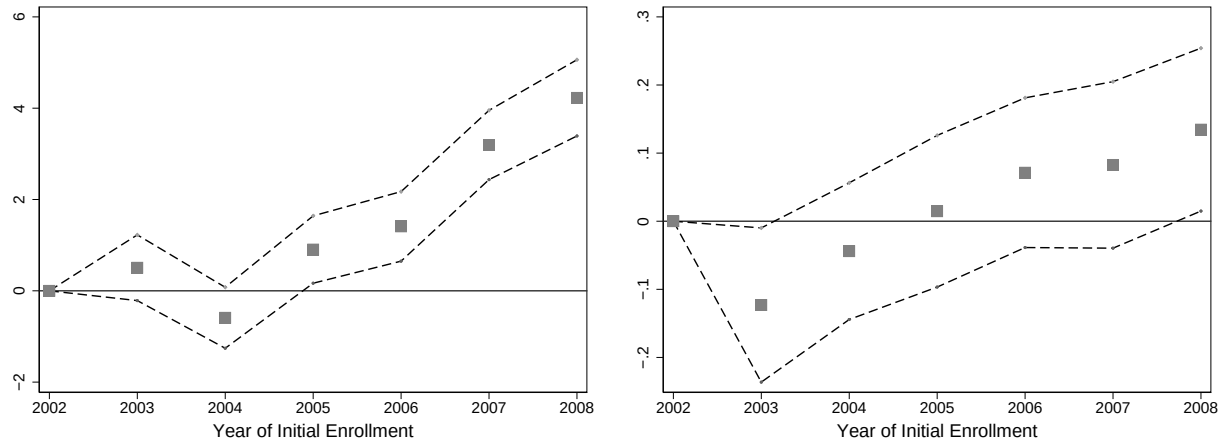
Figure 8: Event Study for Labor Income



(a) 9 Years After Separation (b) Average 7–9 Years After Separation

Notes: The figure presents estimated coefficients on each year of separation year interacted with the generosity of a veteran's home of record predicted total benefits under the PGIB (see text for details). The regression includes eligible veterans who separated from the Army between 2002 and 2010. The 2002 interaction is omitted. Regressions include the following controls defined at the time of enlistment: for gender, race, age, educational attainment, marital status, 3-digit home-of-record zip code, and AFQT score interacted with enlistment year. Regressions additionally include military grade at separation, separation month, and military specialization (PMOS) interacted with enlistment year. Standard errors are clustered at the 3-digit zip code level.

Figure 9: Impact of the PGIB Expansion on College Persistence (Already Enrolled)

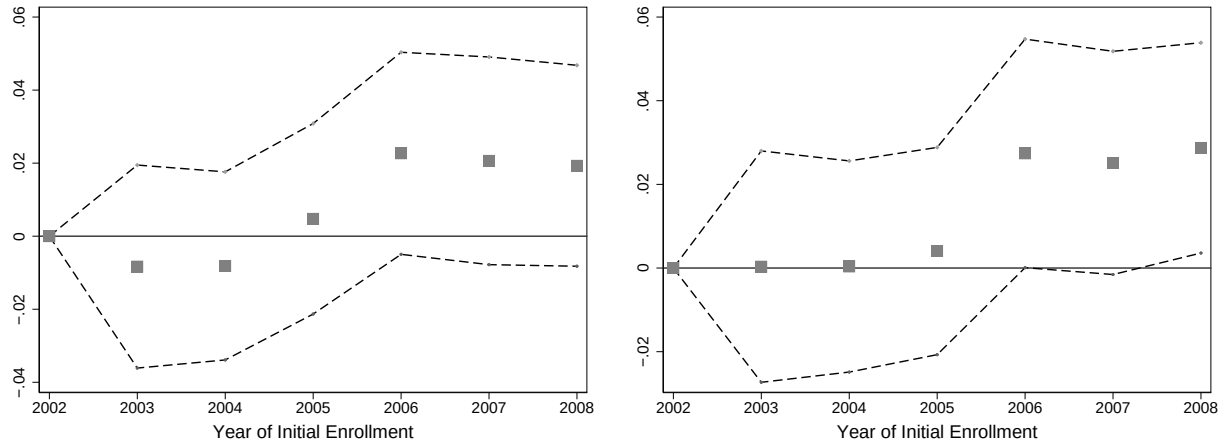


(a) Benefit Months Used

(b) Years Enrolled

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression. Panel (a) includes eligible and ineligible veterans who enrolled in college (within 2 years of Army separation) between 2002 and 2010. Panel (b) replaces the ineligible veteran control group with a civilian control group enrolled at the same institutions between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and institution fixed effects. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

Figure 10: Impact of the PGIB Expansion on Degree Attainment (Already Enrolled)

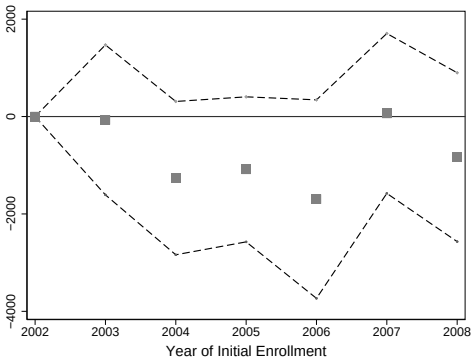


(a) BA Attainment

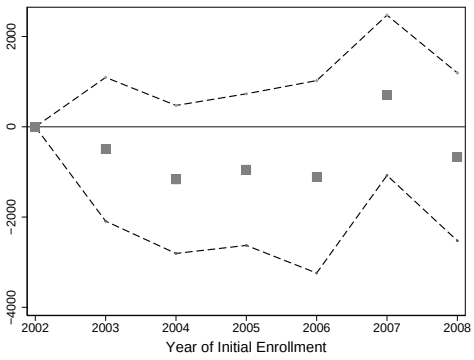
(b) AA Attainment

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible veterans and a civilian control group who enrolled in college between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and institution fixed effects. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

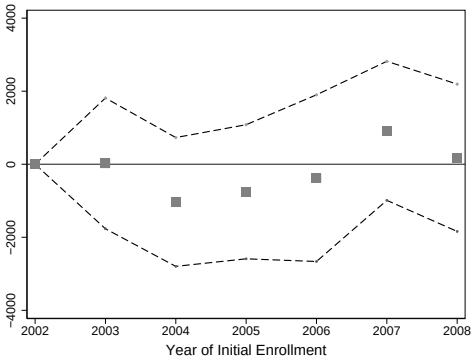
Figure 11: Impact of the PGIB Expansion on Earnings (Already Enrolled)



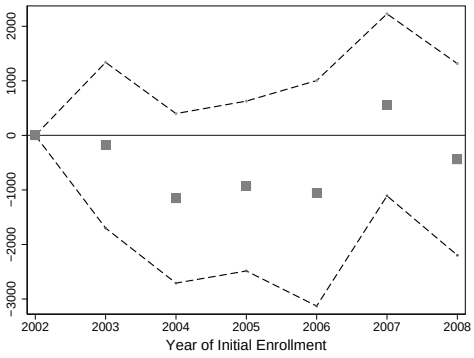
(a) 7 Years After Initial Enrollment



(b) 8 Years After Initial Enrollment



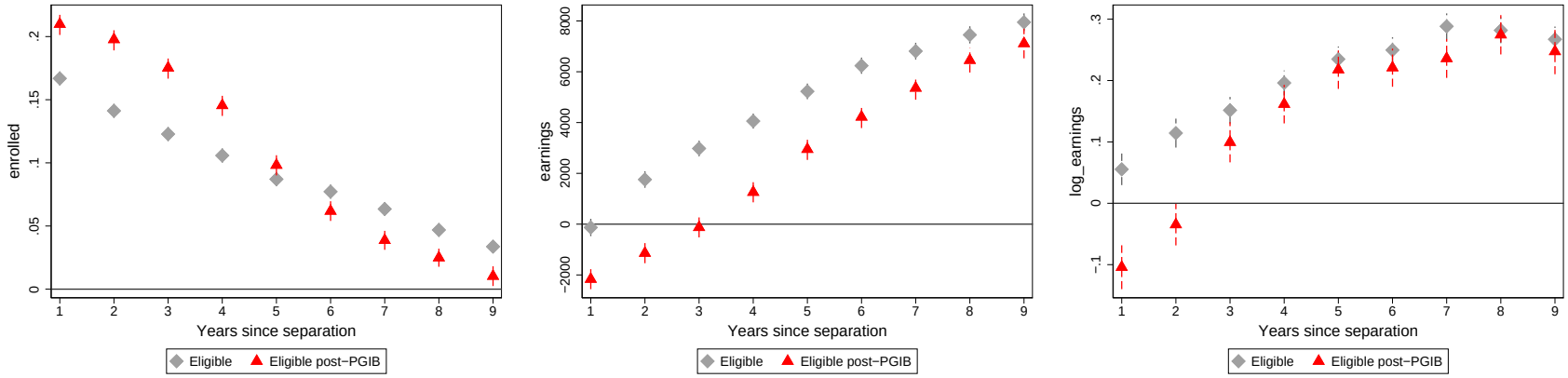
(c) 9 Years After Initial Enrollment



(d) Average 7–9 Years After Initial Enrollment

Notes: The figure presents estimated coefficients from a difference-in-difference event study regression that includes eligible and ineligible veterans who enrolled in college (within 2 years of Army separation) between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: gender, race, age, educational attainment, marital status, and institution fixed effects. Regressions additionally include military grade at separation. Standard errors are clustered at the 3-digit zip code level.

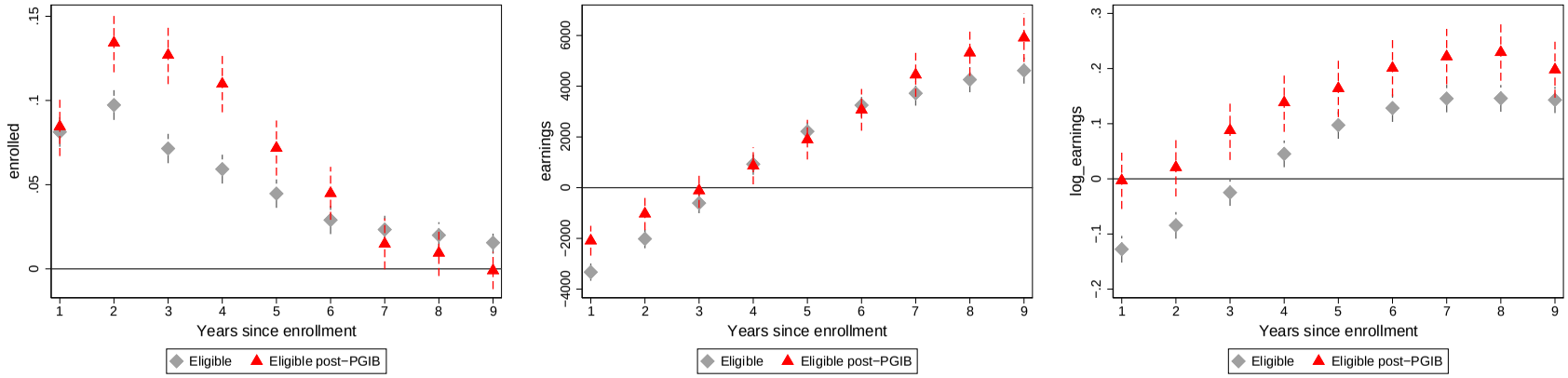
Figure 12: Effect of GI Bill on Earnings and Enrollment Paths



(a) Enrollment (b) Earnings (c) Log Earnings

Notes: The figure presents estimated coefficients on each year of separation year interacted with eligibility for the PGIB (see text for details). The regression includes veterans who separated from the Army between 2002 and 2010. Regressions include the following controls defined at the time of enlistment: for gender, race, age, educational attainment, marital status, 3-digit home-of-record zip code. Regressions additionally include military grade at separation and separation month. Standard errors are clustered at the individual level.

Figure 13: Effect of GI Bill on Earnings and Enrollment Paths (Already Enrolled)



(a) Enrollment (b) Earnings (c) Log Earnings

Notes: The figure presents estimated coefficients on each year of separation year interacted with eligibility for the PGIB (see text for details). The regression includes eligible veterans who separated from the Army and first enrolled in college between 2002 and 2008. Regressions include the following controls defined at the time of enlistment: for gender, race, age, educational attainment, marital status. Regressions additionally include military grade at separation and separation month, and institution fixed effects. Standard errors are clustered at the individual level.

Table 1: Summary Statistics

	Separation Year Analysis					
	Diff-in-Diff		Geographic Variation		Enrollment Year Analysis	
	Mean	SD	Mean	SD	Mean	SD
Demographics, at Enlistment from Army						
Age	24.36	3.34	24.57	3.33	24.12	3.06
Male	0.80	0.40	0.78	0.41	0.76	0.43
Black	0.18	0.38	0.16	0.37	0.19	0.39
Hispanic	0.11	0.32	0.12	0.32	0.11	0.32
Married at separation	0.37	0.48	0.38	0.49	0.32	0.47
Entry AFQT (percentile)	58.32	18.57	59.02	18.73	61.78	18.56
GED	0.15	0.35	0.12	0.33	0.09	0.29
High-school dropout	0.01	0.10	0.01	0.10	0.01	0.10
High-school degree	0.78	0.41	0.80	0.40	0.83	0.38
Some college	0.05	0.22	0.05	0.22	0.05	0.22
GI Bill Utilization						
Eligible	0.83	0.37	1.00	0.00	0.84	0.37
Uses GI Bill	0.64	0.48	0.75	0.43	0.82	0.38
PGIB/MGIB benefits used (000s)	24.86	31.42	29.12	32.30	32.47	32.66
PGIB housing benefits used (000s)	7.61	13.22	8.90	13.93	6.58	11.69
PGIB tuition benefits used (000s)	6.24	11.69	7.28	12.34	5.63	10.71
Months of PGIB/MGIB Used	14.65	14.07	17.12	13.79	19.24	13.43
Enrollment Outcomes						
Enroll Four-Year Public	0.17	0.38	0.19	0.39	0.27	0.44
Enroll Four-Year Private	0.05	0.21	0.05	0.22	0.06	0.23
Enroll Two-Year Public	0.27	0.44	0.29	0.46	0.50	0.50
Enroll For-Profit	0.15	0.36	0.16	0.37	0.17	0.37
Enrolls	0.55	0.50	0.60	0.49	1.00	0.00
Earns Bachelor within 6 Years	0.09	0.29	0.11	0.31	0.32	0.47
Earns Associates within 3 Years	0.04	0.19	0.04	0.21	0.25	0.44
Education Tax Credits	554	1,172	621	1,233	266	541
Tuition and Fee Deduction	267	1,047	301	1,112	294	950
Takes Education Tax Credits	0.31	0.46	0.34	0.48	0.28	0.45
Takes Tuition and Fee Deduction	0.10	0.30	0.11	0.31	0.12	0.33
Longer-Run Outcomes						
Wage and Salary Income	42,503	40,733	46,497	41,512	35,555	30,801
Total Income	42,503	40,733	46,497	41,512	50,252	42,884
Adjusted Gross Income	42,131	40,446	46,086	41,228	49,689	42,561
Married	0.40	0.49	0.44	0.50	0.44	0.50
Number of Kids	0.91	1.18	0.97	1.20	0.95	1.18
Observations	121,323		101,277		59,208	

Notes: The table includes summary statistics for the three estimation samples used in our analysis. Columns (1)–(4) present summary statistics for the estimation samples used in the separation year analysis. The first two columns include all veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The second two columns include only eligible veterans who separated from the Army in the pre- and post-periods. Column (5)–(6) present summary statistics for the estimation samples used in the enrollment year analysis who initially enrolled in college in 2003/04 (pre-period) or 2007/08 (post-period).

Table 2: Impact of the PGIB on Benefits Received

	(1)	(2)	(3)	(4)	(5)	(6)
		Impact on Veterans Already Enrolled				
	Overall Impact	All	Four-Yr Public	Four-Yr Private	CC	For-Profit
Panel A: Eligible vs. Ineligible						
Post * Eligible	7.1021***	11.827***	14.680***	15.824***	11.860***	8.296***
	(0.3018)	(0.653)	(1.100)	(2.418)	(0.750)	(1.306)
R-squared	0.1783	0.190	0.270	0.307	0.166	0.154
Observations	121,323	59,208	16,002	3,332	29,802	9,982
Panel B: Geographic Variation, BAH						
Post * BAH	2.3018***	0.969***	0.678***	1.240***	1.022***	1.420***
	(0.1500)	(0.140)	(0.221)	(0.421)	(0.223)	(0.485)
R-squared	0.1051	0.118	0.189	0.259	0.101	0.080
Panel C: Geographic Variation, Tuition and Fees						
Post * TF	0.6678***	0.154*	0.635***	0.670***	0.574*	0.795***
	(0.2122)	(0.087)	(0.199)	(0.188)	(0.347)	(0.241)
R-squared	0.1019	0.116	0.189	0.255	0.100	0.079
Panel C: Geographic Variation, Total Benefits						
Post * Total Benefits	1.7062***	0.372***	0.711***	0.635***	0.967***	0.779***
	(0.1159)	(0.067)	(0.173)	(0.147)	(0.142)	(0.186)
R-squared	0.1046	0.117	0.190	0.259	0.102	0.080
Observations	101,273	49,472	13,883	2,840	24,650	8,050

Notes: The table presents the estimated impact of the GI Bill expansion for eligible veterans from separate regressions. Column (1) includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period): the top row presents the estimated coefficient on the interaction between eligibility status and the post-treatment period, and the bottom three rows present estimates from regressions that include only eligible veterans. Columns (2)–(6) include veterans who initially enrolled in college after their Army separation in 2003/2004 (pre-period) and 2007/2008 (post-period): the top row presents the estimated coefficient on the interaction between eligibility status and the post-treatment period, and the bottom three rows present estimates from regressions that include only eligible veterans. Enrollment is defined as the first 1098-T observed within two years of Army separation. All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, and marital status. Regressions in column (1) also include home of record 3-digit zip code fixed effects, and standard errors are clustered at the 3-digit zip code level. Regressions in columns (2)–(6) also include institution fixed effects, and standard errors are clustered at the institution level.

Table 3: Impact of the PGIB on Enrollment

	(1) Enrolled	(2) 4-Yr. Public	(3) 4-Yr. Private	(4) CC	(5) For-Profit
Panel A: Eligible vs. Ineligible					
Post * Eligible	0.0025	-0.0163***	0.0020	0.0072	0.0150***
	(0.0072)	(0.0044)	(0.0022)	(0.0058)	(0.0048)
Observations	121,323	121,323	121,323	121,323	121,323
R-squared	0.1062	0.0889	0.0316	0.0823	0.0403
Panel B: Geographic Variation, BAH					
Post * BAH	0.0015	0.0010	0.0021**	0.0015	-0.0001
	(0.0020)	(0.0015)	(0.0009)	(0.0021)	(0.0014)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.0673	0.0791	0.0302	0.0751	0.0402
Panel C: Geographic Variation, Tuition and Fees					
Post * TF	0.0046**	0.0018	0.0011	0.0020	-0.0016
	(0.0022)	(0.0017)	(0.0010)	(0.0021)	(0.0015)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.0674	0.0791	0.0301	0.0751	0.0402
Panel D: Geographic Variation, Total Benefits					
Post * Total Benefits	0.0029*	0.0018	0.0017**	0.0017	-0.0011
	(0.0016)	(0.0013)	(0.0007)	(0.0016)	(0.0012)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.0674	0.0791	0.0302	0.0751	0.0402

Notes: The top panel presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The bottom three rows present estimates from regressions that include only eligible veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 4: Impact of PGIB on College Persistence and Degree Attainment

	(1) Months Used	(2) Years Enrolled	(3) BA Degree	(4) AA Degree
Panel A: Eligible vs. Ineligible				
Post * Eligible	1.4365***	0.1749***	0.0120***	0.0040**
	(0.1340)	(0.0246)	(0.0028)	(0.0018)
Observations	121,323	121,323	121,323	121,323
R-squared	0.2390	0.1344	0.0700	0.0265
Panel B: Geographic Variation, BAH				
Post * BAH	0.5042***	0.0556***	0.0047***	0.0011
	(0.0505)	(0.0083)	(0.0013)	(0.0008)
Observations	101,273	101,273	101,273	101,273
R-squared	0.1096	0.0923	0.0613	0.0242
Panel C: Geographic Variation, Tuition and Fees				
Post * TF	-0.0191	0.0149	0.0011	0.0005
	(0.0572)	(0.0091)	(0.0014)	(0.0008)
Observations	101,273	101,273	101,273	101,273
R-squared	0.1087	0.0917	0.0612	0.0241
Panel D: Geographic Variation, Total Benefits				
Post * Total Benefits	0.2894***	0.0391***	0.0035***	0.0010
	(0.0410)	(0.0062)	(0.0010)	(0.0006)
Observations	101,273	101,273	101,273	101,273
R-squared	0.1092	0.0921	0.0613	0.0242

Notes: The top panel presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The bottom three rows present estimates from regressions that include only eligible veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 5: Impact of the PGIB on Education Tax Benefits

	(1) Takes Tuition and Fee Deduction	(2) Tuition and Fee Deduction	(3) Takes Education Tax Credit	(4) Education Tax Credit
Panel A: Eligible vs. Ineligible				
Post * Eligible	-0.058***	-241.340***	0.020***	199.716***
	(0.003)	(11.202)	(0.006)	(12.413)
Observations	121,323	121,323	121,323	121,323
R-squared	0.038	0.040	0.079	0.081
Panel B: Geographic Variation, BAH				
Post * BAH	0.002	9.930**	0.000	7.366
	(0.001)	(4.250)	(0.002)	(5.168)
Observations	101,273	101,273	101,273	101,273
R-squared	0.033	0.037	0.059	0.070
Panel C: Geographic Variation, Tuition and Fees				
Post * TF	-0.000	-11.796**	0.001	9.063*
	(0.001)	(4.849)	(0.002)	(5.349)
Observations	101,273	101,273	101,273	101,273
R-squared	0.033	0.037	0.059	0.070
Panel D: Geographic Variation, Total Benefits				
Post * Total Benefits	0.001	0.365	0.000	8.942**
	(0.001)	(3.492)	(0.002)	(4.308)
Observations	101,273	101,273	101,273	101,273
R-squared	0.033	0.037	0.059	0.070

Notes: The top panel presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The bottom three rows present estimates from regressions that include only eligible veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 6: Impact of the PGIB on Longer-Run Earnings

	(1)	(2)	(3)	(4)	(5)
Years Since Separation	6	7	8	9	Mean(7,8,9)
			Panel A: Eligible vs. Ineligible		
Post * Eligible	-3,012.860***	-1,950.566***	-1,365.496***	-907.923**	-1,407.995***
	(345.580)	(350.778)	(362.072)	(383.691)	(345.555)
Observations	121,323	121,323	121,323	121,323	121,323
R-squared	0.133	0.132	0.135	0.136	0.149
			Panel B: Geographic Variation, BAH		
Post * BAH	-363.816***	-20.418	207.069*	308.023***	164.891
	(102.997)	(113.387)	(111.735)	(116.085)	(107.393)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.114	0.113	0.116	0.118	0.128
			Panel C: Geographic Variation, Tuition and Fees		
Post * TF	203.721*	219.229*	279.875**	261.533**	253.546**
	(115.346)	(116.440)	(114.613)	(116.265)	(108.364)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.114	0.113	0.116	0.118	0.128
			Panel D: Geographic Variation, Total Benefits		
Post * Total Benefits	-92.182	118.814	279.503***	328.545***	242.287***
	(91.223)	(95.130)	(91.991)	(96.183)	(89.418)
Observations	101,273	101,273	101,273	101,273	101,273
R-squared	0.114	0.113	0.116	0.118	0.128

Notes: The top panel presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The bottom three rows present estimates from regressions that include only eligible veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 7: Impact of the PGIB on Log(Earnings)

	(1)	(2)	(3)	(4)	(5)
Years Since Separation	6	7	8	9	Mean(7,8,9)
			Panel A: Eligible vs. Ineligible		
Post * Eligible	-0.042*	-0.024	-0.002	-0.025	-0.028
	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)
Observations	98,938	98,106	97,585	97,271	106,806
R-squared	0.103	0.103	0.108	0.108	0.108
			Panel B: Geographic Variation, BAH		
Post * BAH	-0.014***	-0.012**	-0.001	0.000	-0.003
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Observations	81,887	81,148	80,727	80,601	87,796
R-squared	0.099	0.095	0.100	0.102	0.102
			Panel C: Geographic Variation, Tuition and Fees		
Post * TF	0.009	0.009*	0.014***	0.011**	0.006
	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	81,887	81,148	80,727	80,601	87,796
R-squared	0.099	0.095	0.100	0.102	0.102
			Panel D: Geographic Variation, Total Benefits		
Post * Total Benefits	-0.003	-0.002	0.007*	0.006	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	81,887	81,148	80,727	80,601	87,796
R-squared	0.099	0.095	0.100	0.102	0.102

Notes: The top panel presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). The bottom three rows present estimates from regressions that include only eligible veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 8: Impact of the PGIB on Education Outcomes (Already Enrolled)

	(1) Years Enrolled	(2) Months Used	(3) BA Degree	(4) AA Degree
Panel A: All Sectors				
Eligible * Post	0.195***	3.796***	0.031***	0.027***
	(0.038)	(0.282)	(0.009)	(0.010)
Observations	59,208	59,208	59,208	59,208
R-squared	0.123	0.269	0.157	0.103
Panel B: Four-Year Public				
Eligible * Post	0.094	4.424***	0.070***	0.010
	(0.092)	(0.555)	(0.022)	(0.022)
Observations	16,002	16,002	16,002	16,002
R-squared	0.095	0.258	0.141	0.106
Panel C: Four-Year Private				
Eligible * Post	0.142	5.066***	-0.011	0.014
	(0.158)	(0.987)	(0.040)	(0.035)
Observations	3,332	3,332	3,332	3,332
R-squared	0.182	0.316	0.225	0.175
Panel D: Community College				
Eligible * Post	0.244***	4.134***	0.031***	0.024*
	(0.055)	(0.306)	(0.012)	(0.013)
Observations	29,802	29,802	29,802	29,802
R-squared	0.116	0.263	0.083	0.081
Panel E: For-Profit				
Eligible * Post	0.220***	2.206***	0.004	0.068***
	(0.080)	(0.649)	(0.012)	(0.021)
Observations	9,982	9,982	9,982	9,982
R-squared	0.150	0.282	0.131	0.122

Notes: The table presents estimates from difference-in-difference regressions that include eligible and ineligible veterans who first enrolled in college in 2003/2004 (pre-period) and 2008/2009 (post-period). The sample is restricted to veterans who enrolled within two years of their Army separation. All regressions include institution fixed effects along with the following controls, defined at the time of enlistment: gender, race, age, educational attainment, and marital status. Standard errors are clustered at the institution level.

Table 9: Impact of the PGIB Expansion on Earnings Outcomes (Already Enrolled)

	(1)	(2)	(3)	(4)	(5)
Years Since Enrollment	6	7	8	9	Mean(7,8,9)
			Panel A: All Sectors		
Post * Eligible	-675.961	502.342	1,030.538*	1,235.120**	922.667*
	(510.221)	(549.525)	(541.571)	(597.682)	(521.102)
Observations	59,208	59,208	59,208	59,208	59,208
R-squared	0.129	0.131	0.136	0.139	0.149
			Panel B: Four-Year Public		
Post * Eligible	-146.072	1,959.420	1,230.128	843.090	1,344.213
	(1,215.941)	(1,224.443)	(1,266.498)	(1,460.635)	(1,216.018)
Observations	16,002	16,002	16,002	16,002	16,002
R-squared	0.116	0.125	0.129	0.136	0.144
			Panel C: Four-Year Private		
Post * Eligible	-349.195	568.209	-2,759.824	-3,284.430	-1,825.349
	(3,146.163)	(3,391.169)	(3,510.453)	(3,121.059)	(3,130.020)
Observations	3,332	3,332	3,332	3,332	3,332
R-squared	0.214	0.208	0.218	0.206	0.225
			Panel D: Community College		
Post * Eligible	-795.044	195.308	1,181.707	2,236.907***	1,204.641*
	(685.284)	(710.429)	(791.168)	(831.092)	(727.945)
Observations	29,802	29,802	29,802	29,802	29,802
R-squared	0.113	0.113	0.120	0.121	0.129
			Panel E: For-Profit		
Post * Eligible	-2,738.933***	-936.534	244.831	-1,091.870	-594.524
	(978.063)	(1,213.524)	(833.663)	(825.606)	(865.724)
Observations	9,982	9,982	9,982	9,982	9,982
R-squared	0.163	0.153	0.149	0.148	0.164

Notes: The table presents estimates from difference-in-difference regressions that include eligible and ineligible veterans who first enrolled in college in 2003/2004 (pre-period) and 2008/2009 (post-period). The sample is restricted to veterans who enrolled within two years of their Army separation. All regressions include institution fixed effects along with the following controls, defined at the time of enlistment: gender, race, age, educational attainment, and marital status. Standard errors are clustered at the institution level.

Table 10: Heterogeneity in Impacts of PGIB on Enrollment

	(1) Enrolled	(2) 4-Yr. Public	(3) 4-Yr. Private	(4) Community College	(5) For-Profit
			Panel A: Above AFQT vs. Below AFQT		
High AFQT	-0.0019	-0.0270***	0.0093**	0.0061	0.0152**
	(0.0111)	(0.0076)	(0.0036)	(0.0092)	(0.0072)
Observations	59,829	59,829	59,829	59,829	59,829
R-squared	0.1071	0.1024	0.0390	0.0935	0.0353
Low AFQT	0.0063	-0.0109**	-0.0043	0.0043	0.0222***
	(0.0094)	(0.0049)	(0.0027)	(0.0070)	(0.0068)
Observations	60,903	60,903	60,903	60,903	60,903
R-squared	0.1159	0.0795	0.0393	0.0858	0.0556
			Panel B: High-, Medium-, and Low- Skilled Military Jobs		
PMOS Type 0	0.0052	-0.0274***	0.0045	0.0093	0.0261***
	(0.0102)	(0.0062)	(0.0031)	(0.0086)	(0.0073)
Observations	53,520	53,520	53,520	53,520	53,520
R-squared	0.1244	0.1040	0.0390	0.1002	0.0586
PMOS Type 1	-0.0428*	-0.0309**	-0.0039	-0.0114	-0.0093
	(0.0237)	(0.0140)	(0.0068)	(0.0174)	(0.0170)
Observations	17,147	17,147	17,147	17,147	17,147
R-squared	0.1218	0.1226	0.0741	0.1143	0.0716
PMOS Type 2	-0.0102	0.0013	-0.0092	0.0218	-0.0113
	(0.0257)	(0.0160)	(0.0082)	(0.0219)	(0.0164)
Observations	17,433	17,433	17,433	17,433	17,433
R-squared	0.1269	0.1269	0.0761	0.0990	0.0700

Notes: The table presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 11: Heterogeneity in Impacts of PGIB on Attainment and Earnings

	(1) Years Enrolled	(2) Months Used	(3) Bachelor's Degree	(4) Associate's Degree	(5) Earnings Year 9	(6) Earnings Mean(Years 7-9)
Panel A: Above AFQT vs. Below AFQT						
High AFQT	0.1294***	1.4631***	0.0098**	0.0053	-301.2578	-891.4890*
	(0.0393)	(0.2041)	(0.0049)	(0.0033)	(597.0762)	(537.1265)
Observations	59,829	59,829	59,829	59,829	59,829	59,829
R-squared	0.1280	0.2487	0.0827	0.0341	0.1335	0.1450
Low AFQT	0.2071***	1.3592***	0.0121***	0.0026	-1,573.2520***	-1,961.9019***
	(0.0307)	(0.1935)	(0.0027)	(0.0019)	(475.8830)	(428.9670)
Observations	60,903	60,903	60,903	60,903	60,903	60,903
R-squared	0.1614	0.2463	0.0532	0.0316	0.1343	0.1491
Panel B: High-, Medium-, and Low- Skilled Military Jobs						
PMOS Type 0	0.1924***	1.4573***	0.0108***	0.0044*	-1,657.6737***	-2,028.8645***
	(0.0345)	(0.1880)	(0.0038)	(0.0024)	(551.9426)	(488.3639)
Observations	53,520	53,520	53,520	53,520	53,520	53,520
R-squared	0.1472	0.2608	0.0768	0.0374	0.1379	0.1496
PMOS Type 1	0.0632	0.5889	-0.0012	-0.0028	-845.9022	-1,753.5993
	(0.0781)	(0.4040)	(0.0080)	(0.0054)	(1,307.4936)	(1,160.2719)
Observations	17,147	17,147	17,147	17,147	17,147	17,147
R-squared	0.1495	0.2465	0.0994	0.0687	0.1859	0.2020
PMOS Type 2	0.0186	1.1634**	0.0157	0.0100	1,259.0071	530.6571
	(0.0897)	(0.4841)	(0.0105)	(0.0080)	(1,305.4273)	(1,169.3605)
Observations	17,433	17,433	17,433	17,433	17,433	17,433
R-squared	0.1622	0.2876	0.1230	0.0677	0.1896	0.2018

Notes: The table presents the estimated coefficient on the interaction between eligibility status and the post-treatment period. The regression includes veterans who separated from the Army in 2003/2004 (pre-period) and 2008/2009 (post-period). All regressions include the following controls, defined at the time of enlistment: gender, race, age, educational attainment, marital status, and home of record 3-digit zip code fixed effects. Standard errors are clustered at the 3-digit zip code level.

Table 12: Heterogeneity in Impacts of PGIB on Education and Earnings (Already Enrolled)

	(1) Years Enrolled	(2) Months Used	(3) Bachelors Degree	(4) Associates Degree	(5) Earnings Year 9	(6) Earnings Mean(7,8,9)
Panel A: High vs. Low AFQT						
High AFQT	0.243***	4.498***	0.041***	0.023*	1,902.777**	1,391.834*
	(0.051)	(0.352)	(0.012)	(0.013)	(910.125)	(824.813)
Observations	33,567	33,567	33,567	33,567	33,567	33,567
R-squared	0.128	0.293	0.171	0.127	0.144	0.152
Low AFQT	0.140***	2.978***	0.017	0.033***	550.941	293.836
	(0.054)	(0.388)	(0.011)	(0.013)	(859.846)	(733.090)
Observations	24,921	24,921	24,921	24,921	24,921	24,921
R-squared	0.158	0.275	0.150	0.115	0.152	0.165
Panel B: High-, Medium-, and Low-Skilled Military Jobs						
PMOS Type 0	0.178***	3.249***	0.023*	0.018	-31.734	75.658
	(0.055)	(0.412)	(0.013)	(0.014)	(944.456)	(815.353)
Observations	24,763	24,763	24,763	24,763	24,763	24,763
R-squared	0.148	0.304	0.172	0.125	0.161	0.168
PMOS Type 1	0.072	3.583***	0.053**	0.010	1,379.969	460.488
	(0.119)	(0.812)	(0.025)	(0.024)	(1,640.183)	(1,422.202)
Observations	8,792	8,792	8,792	8,792	8,792	8,792
R-squared	0.190	0.316	0.208	0.165	0.196	0.212
PMOS Type 2	0.036	4.806***	0.057*	0.001	1,752.203	916.922
	(0.109)	(0.775)	(0.030)	(0.029)	(1,780.676)	(1,709.838)
Observations	8,250	8,250	8,250	8,250	8,250	8,250
R-squared	0.178	0.338	0.219	0.163	0.222	0.233

Notes: The table presents estimates from difference-in-difference regressions that include eligible and ineligible veterans who first enrolled in college in 2003/2004 (pre-period) and 2008/2009 (post-period). The sample is restricted to veterans who enrolled within two years of their Army separation. All regressions include institution fixed effects along with the following controls, defined at the time of enlistment: gender, race, age, educational attainment, and marital status. Standard errors are clustered at the institution level.

Table 13: Oaxaca Decomposition of the PGIB Effect on Earnings Nine Years Since Separation

	Diff-in-Diff Effect	Beta, Pre-Period	Portion Explained
Earnings	($907.92)		
Enroll Years	0.16	241.26	38.12
Work Experience Years	-0.19	3445.25	-658.04
Work Experience Squared	-2.72	271.71	-740.13
Four-Year Public Within 2 Years	-0.02	5167.82	-82.69
Four-Year Private Within 2 Years	0.002	3416.74	6.83
Two-Year Public Within 2 Years	0.007	2327.17	16.29
For-Profit Within 2 Years	0.015	684.25	10.26
Online Institution Within 2 Years	-0.005	-675.57	3.38
Institution Grad Rate Less than 30%	-0.001	446.564	-0.45
Bachelors Degree within 6 Years	0.012	11054.2	132.65
Associates Degree Within 3 Years	0.004	1120.18	4.48
			($1,269.29)

Notes: Column (1) shows the estimated effect of PGIB on earnings and each of the other outcomes: years of enrollment, work experience (level and square) and institution choice within the first two years since separating. Column (2) contains the OLS coefficients from earnings regressed on those other outcomes (explanators) during the pre-period. Column (3) multiplies the OLS effect of the explanator (column 2) by the PGIB effect on the explanator (column 1).

Table 14: Forecast Earnings Effect Differential as a Result of PGIB Expansion

Parameters		**Scenario**	**NPV Earnings Difference**
Discount Rate	3%	Forecast years 8-30 w/ returns to experience	$ (38,773)
Return to Experience	7.30%	Experience plus additional 9% return to Yrs Education	$ (31,228)
Return to Experience Squared	-0.23%	Experience plus additional 25% return to Yrs Education	$ (17,547)
Difference Months Education	1.90	Experience plus additional 20% return to B.A. Completion	$ (37,507)
Difference BA Completion	0.01		
Difference Experience Year 7	-0.19		
Fraction in Labor Force	0.84		
Years since Sep in Forecast	30		

Notes: The table illustrates the estimated effect of the PGIB on net present value (NPV) of earnings under different assumptions on the returns to additional educational attainment. We parameterize the return to additional experience using the estimated coefficient on experience in the American Community Survey (ACS) for veterans age 25-55 (7.3 percent pear year of experience). Because we estimate minimal effect of work experience squared among veterans in the ACS, we set the coefficient equal to the civilian equivalent (-0.23 percent). Effects on educational attainment and experience are estimated using the basic difference-in-differences specification 7 years post-separation. To be conservative, returns to additional education are assumed to occur after year 7, despite these returns potentially having already shown up by this point.