Fighting for Education: Financial Aid and Degree Attainment

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Abstract

The Post-9/11 GI Bill brought about the largest expansion in veteran education benefits since the end of World War II, increasing annual benefit expenditures from $3 billion to more than $13 billion dollars; in 2014, this comprised nearly one-third of college grant and scholarship aid provided by the federal government and exceeded the amount of college grant and scholarship aid provided by all states combined. Leveraging variation over time and across types of recipients, as well as geographic variation in the size of the benefit expansion, I explore the effect of financial aid on degree attainment. I find that this financial aid program increased degree attainment by five to six percentage points (25 percent), roughly 0.4 percentage points per $1,000 of additional maximum aid. These findings indicate that financial aid can increase degree attainment, even for individuals with high levels of initial support.

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1 Introduction

Each year billions of dollars are spent on financial aid in the hope of helping millions of students enroll in college and obtain a degree. While there is abundant evidence of the effects of aid on increasing college access, little evidence exists on the effects of aid on degree attainment. Furthermore, the existing evidence as to whether financial aid improves educational attainment is mixed, limited to very local treatment effects or small experimental groups, and focuses almost entirely on traditional students (Dynarski 2008, Scott-Clayton 2011, Sjoquist and Winters 2012, Castleman and Long 2016, and Goldrick-Rab et al 2012). This study brings new evidence to understanding the effects of financial aid on degree attainment and, unlike most existing research, how financial aid affects the attainment of older, non-traditional students.

I leverage variation provided by the Post-9/11 GI Bill, one of the largest financial aid expansions in decades, to identify the effects of financial aid on the degree attainment of an important group of non-traditional students, recent military veterans. Veterans are similar to other non-traditional students along a number of demographic dimensions, including family background, race, age, and marital status, as well as patterns of college enrollment in terms of motivations for attending, institution type, and major.[1] Each year roughly 200,000 military veterans transition out of the military. As the nation's largest employer of young adults and an organization that markets itself as providing educational opportunities to youth who might not otherwise have the means to pay for college, the military provides an interesting context in which to study how financial aid affects the decision to invest in education during a period of transition between occupations.[2] Easing this transition was one of the stated goals of the benefit expansion.[3]

In addition to addressing a fundamental question about the effect of financial aid on degree attainment, the economic magnitude of the ongoing investment, now more than $13 billion annually, makes it worthy of investigation. To put this in perspective relative to other well-known financial aid programs, annual expenditures on the GI Bill now exceed the annual amount of grants and scholarships provided by all states combined; are roughly half the size of federal expenditures on the Pell grant; and are more than three-quarters the size of federal education tax benefits.

[1] Source: Military Service Members and Veterans: A Profile of those Enrolled in Undergraduate and Graduate Education in 2007-2008 (NCES 2011-163).

[2] Military veterans are substantially more likely to enroll in college during this transition than several years after joining the civilian labor force. In some ways this is similar to civilian workers who are substantially more likely to enroll between occupations.

[3] "[T]he majority of the people who go into the military get out on or before the end of their first enlistment [a]nd they deserve the same opportunity to readjust into civilian life and a first-class shot at the future" (Senator Webb 2008).

One of the primary roadblocks to research in this area is the lack of data that track enrollment and degree attainment over time. While prior work examines the effect of the Post-9/11 GI Bill on enrollment, data limitations made it impossible to determine the extent to which the observed increases in enrollment were true increases in educational attainment or merely shifts in enrollment timing (Barr 2015). As no existing dataset contained a sufficient number of veterans to provide even a cursory analysis of veteran degree attainment, I created a new panel combining data on the choices of military service members from the Defense Manpower Data Center with postsecondary data from the National Student Clearinghouse (NSC). This unique panel dataset was constructed specifically for the purpose of examining veteran educational outcomes, particularly the effects of the additional aid allocated by the Post-9/11 GI Bill.

I focus on individuals separating prior to the announcement of the benefit expansion, circumventing potential concerns related to endogenous accession to or separation from the military.[4] To estimate effects of the benefit expansion, I use a difference-in-differences strategy that leverages over time variation in the post-separation generosity of available benefits that differs between eligible and ineligible veterans. As eligibility depends on an individual having an honorable characterization of service, I spend a considerable amount of time demonstrating the relative similarity of eligible and ineligible veterans in both observables and pre-period enrollment and attainment trends. I also address concerns related to the possibility of heterogeneous enrollment and attainment responses of these two groups to the Great Recession, which overlapped substantially with the early period of the benefit expansion. While it is impossible to conclusively rule out the contribution of the Great Recession to the break in the enrollment and attainment trends of the two groups, a variety of empirical evidence suggests that it is unlikely to have played a major role. This conclusion is supported by my second empirical strategy, which leverages variation within states over time that is unrelated to the differences across states in the severity of the recession.

Unique among the GI bills and other federal financial aid programs, the Post-9/11 GI Bill provides different benefit *levels* depending upon the state and zip code of enrollment. This feature creates variation in the size of the benefit *expansion* experienced in different states and zip codes; individuals in some areas received almost no change in maximum benefit levels, while those in others received tens of thousands of dollars in additional maximum benefits per year. I leverage this state-level variation in covered tuition and housing allowance levels as an additional strategy to identify the effect of aid.

I find that the benefit expansion increased the likelihood that a veteran would obtain a degree within six years of separation by five to six percentage points (25 percent), roughly 0.4

[4]This is because benefit levels were based on active-duty service durations after September 11, 2001, but the benefit was not passed into law until the summer of 2008.

percentage points per $1,000 of additional aid. Most of the increase in degree attainment occurred via an increase in bachelor's degree attainment. These results indicate that additional financial aid can increase degree attainment, even at high initial levels of support. This suggests that similar aid expansions for other older non-traditional students, particularly those transitioning between jobs, could increase degree attainment rates substantially.

2 Financial Aid, College Success, and Veterans

2.1 Financial Aid and Degree Attainment

While research on the effects of financial aid on the enrollment of traditional students is abundant, relatively little is known about its effects on degree attainment, particularly for older, non-traditional students. Previous studies focusing on persistence and degree attainment are largely limited to the evaluations of state merit-aid programs, and the evidence is mixed (Dynarski 2008, Scott-Clayton 2011, Sjoquist and Winters 2012).[5] Work on the longer-term effects of need-based aid is sparse and limited to very local treatment effects or small experimental groups (Castleman and Long 2016; Goldrick-Rab et. al. 2012).[6] While Castleman and Long (2016) suggest an important role for financial aid in increasing degree attainment in Florida, Goldrick-Rab et. al. (2012) found no positive effect of financial aid on persistence after the second year in college. So, the question remains open as to whether, how much, or in what respect various kinds and levels of financial aid impact degree attainment for different types of students.

Individuals who delay enrollment, enroll part-time or at community colleges, or face additional life responsibilities such as raising a family, may respond quite differently to changes in the cost of college. The greater non-college financial demands faced by such students may mean that financial aid plays an even more important role in college-going for veterans and other non-traditional students, particularly if this aid is targeted to the period of transition between occupations.[7] The current paper answers this question.

[5]There is also some recent evidence demonstrating positive effects of increased aid for individuals who are already enrolled in college (for example, Murphy and Wyness (2016) and Denning (2016))

[6]In a somewhat related study, Lovenheim and Owens (2014) demonstrate that cutting off federal financial aid access (loans, grants, or work study) to drug offenders may have an effect on college attendance and degree attainment. However, the confidence intervals are too large to draw strong conclusions.

[7]Two recent papers explore the effects on enrollment of unemployment benefit availability (Barr and Turner 2015) and information about financial aid (Barr and Turner 2016) during a similar transition period for civilian UI recipients, but do not explicitly measure how financial aid affects educational outcomes. Seftor and Turner (2002) present some of the only other evidence for non-traditional students, estimating the effect of a change in aid using a change in the definition of independent students in 1986. They find that access to aid increases enrollment of the affected students by several percentage points.

2.2 Effects of GI Bills on Educational Outcomes

Noted historian Sidney Burrell argued that the original GI Bill, passed after World War II, brought about "what may have been the most important educational and social transformation in American history" (Burrell 1967). The Bill contributed to a near-doubling of college enrollment in less than a decade, and it is widely credited as a driving force behind the creation of the middle class. A number of studies have provided support for Burrell's claim, finding that the World War II and Korean War GI bills had positive effects on educational attainment and earnings (Bound and Turner 2002, Stanley 2003, Card and Lemieux 2001). These studies identify the combined effect of military service and higher aid levels primarily by relying on variation in pre-existing enlistment rates or the likelihood of being drafted.

Like studies of the World War II and Korean GI Bills, studies of the Vietnam-era GI Bill also estimate the combined effects of compulsory military service and higher education benefit levels. Through a clever use of the Vietnam-era draft lotteries, Angrist (1990) demonstrated that the combined effects of compulsory service and higher benefit levels resulted in decreased earnings for young veterans. Twenty years later, Angrist and Chen (2011) followed up on the same cohorts of individuals and found large increases in schooling, consistent with those reported for earlier GI Bills. Surprisingly, though, there was no effect on earnings. The authors reconciled these results with the earlier earnings estimate by arguing that the earnings gap closed due to a flattening of the age-earnings profile at later ages, as well as modest returns to schooling that were induced by the GI Bill.

However, the combined effect of compulsory military service and higher aid levels is less relevant in the current era of the volunteer military. The research on post-Vietnam-era GI Bill benefits remains fairly sparse, but the studies that do exist are distinct from earlier GI Bill analyses in that they focus on the effects of additional benefits conditional on military service, instead of on the joint effect of both service and benefits. Angrist (1993) studied a small group of veterans who participated in the 1987 Survey of Veterans, finding suggestive evidence of increased educational attainment levels and earnings in response to higher benefit levels. Similarly, Simon, Negrusa, and Warner (2010) use plausibly unanticipated changes in the level of education benefits provided by the Montgomery GI Bill during the 1990s to explore the impact of those changes on veteran benefit usage. They find a half-percentage-point increase in benefit usage per $1,000 of additional benefits, substantially more modest responses than those found when evaluating traditional student financial aid programs (Dynarski and Scott-Clayton 2013). However, limited amounts of variation and the absence of a control group in Simon, Negrusa, and Warners' study force them to rely on the assumption that an extensive set of explanatory variables adequately controls for other

changes occurring over this period. In short, although interesting work has been done, it has been limited by the lack of variation in benefit levels over this period.

Even less is known about the Post-9/11 GI Bill's effects on educational outcomes; in fact, there is only one paper of which I am aware that explores the short-term causal effects of the benefit expansion (Barr 2015). Using publicly available survey data and a difference-in-differences strategy, I found that the benefit expansion increased by 20 percent the probability that young veterans would be enrolled at a particular age. However, data limitations made it impossible to determine the extent to which the observed increases in enrollment were true increases in educational attainment or merely shifts in enrollment timing. As with much of the literature on the effects of financial aid, data constraints prevented an exploration of the effects of the benefit expansion on degree attainment.

3 Expansion of Benefits from MGIB to Post-9/11 GI Bill

A brief background on the previous GI Bill helps to clarify how benefits changed when the Post-9/11 GI Bill was implemented. Prior to the Post-9/11 GI Bill, the primary education benefit for military veterans was the Montgomery GI Bill (MGIB). Implemented in 1985, the MGIB remains an option for enlisting active-duty personnel as of the writing of this paper. Eligibility for the MGIB has always depended largely on three factors. First, because the MGIB was not an entitlement, individuals who chose to participate in the program had to commit to a pay reduction of $100 for each of the first 12 months on active duty. Second, in order to be eligible, an individual had to complete the minimum active duty contract agreed to upon enlistment (generally at least three years) with an honorable discharge. Third, individuals who had been commissioned through military academies or who had received ROTC scholarship funds were not eligible for the MGIB benefit. Under the MGIB, the benefits were sent directly from the Department of Veterans Affairs (VA) to the veterans who had chosen to enroll. Benefit levels were raised periodically to adjust for inflation, and the flat amounts paid to individuals monthly were prorated according to half-time, three-quarters time, and full-time enrollment.[8,9] In 2007, a full-time student who had completed three or more years of active-duty service received approximately $1,100 per month under the MGIB. Veterans were eligible for 36 months of benefits; in other words, an individual enrolled full-time for four years on a traditional two-semester system would receive benefits throughout the degree program. The MGIB was not tied to a particular school or location,

[8]MGIB benefit levels were essentially flat in real terms between 2003 and 2007, around $1,140 per month in 2009 dollars, before rising to around $1,300 per month in 2008 and 2009.

[9]Individuals enrolling less than half time receive an amount equivalent to their tuition and fees. However, the partial benefit amounts only subtract from total benefit eligibility proportionally; i.e., a quarter-time student receiving approximately $300 per month only uses approximately one-fourth ($300/$1,100) of a month of benefits.

and there was little restriction on the types of programs approved. Individuals could use the benefit for vocational training, apprenticeships, flight classes, and test fees as well as formal degree-seeking coursework.

Education benefits for veterans were dramatically expanded in summer 2008 when Congress approved the Veterans Educational Assistance Act of 2008, more commonly known as the Post-9/11 GI Bill.[10] The actual benefit expansion began in August 2009, as additional benefits were provided to individuals who had served on active-duty after September 11, 2001. These benefits could only be applied to enrollment occurring after August 2009. Unlike the previous GI Bill, the Post-9/11 GI Bill does not require individuals to opt in to the program at enlistment or prepay for the benefit. The vast majority of individuals serving on active duty after September 11, 2001, and receiving an honorable discharge, are eligible.[11] Benefit levels are tiered based on active-duty service durations; veterans with at least 90 days of active-duty service after September 11, 2001, are eligible for 40% of maximum benefit levels, and those with six months are eligible for 50%. Each additional six months of post-9/11 service results in an additional 10% of eligibility; veterans with greater than three years are eligible for 100% of maximum benefit levels. In practice, because the Bill is retroactive, nearly all veterans who separated honorably after 2005 are eligible for the maximum benefit.[12]

The Post-9/11 GI Bill benefit has two major components: (1) tuition and fee coverage and (2) a monthly basic allowance for housing (BAH). For the tuition and fee benefit, maximum benefit eligibility is based on the highest tuition level and fee level of any public college in an individual's state of residence; the VA determines the amounts for each based on information provided by each state.[13] Unlike coverage under the MGIB, the VA pays the tuition and fee benefit directly to schools, reimbursing the level of tuition and fees up to the in-state maximum of each.[14] For an individual attending a community college or as an in-state student at a public four-year college, the school attended would receive reimbursement

[10]The delay between announcement and implementation of the benefit expansion suggests that individuals considering enrollment between these two points might have had an incentive to delay enrollment in order to capture the higher level of benefits available in the fall of 2009. However, Barr (2015) demonstrates that this is not occurring in practice.

[11]Officers commissioned at military academies and individuals previously receiving substantial ROTC scholarships are eligible only after completing an additional period of service above and beyond their initial requirements.

[12]This is because nearly all veterans who separate honorably will have served at least three years.

[13]In 2011, coverage for private colleges changed to a maximum of $20,000 across the country. As very few veterans are likely to be affected by this change (very few veterans are enrolled in colleges that would have been affected), it is unlikely to have a substantial effect on the estimates.

[14]That schools are reimbursed directly may be important for at least two reasons. First, it raises interesting questions about the incidence of the benefits and whether or not it is easier for schools to reduce institutional aid to offset veteran aid if they receive the benefits directly (and thus observe the benefits). These questions are being investigated in a complementary project. Second, benefits sent to the veteran on a regular basis may be more salient to the veteran.

of, at most, the level of tuition and fees charged to the student. The benefit component paid on behalf of an out-of-state student at a public four-year college or any student at a private college would be capped at the maximum public in-state tuition and fee levels set by the VA.[15] Veteran students enrolled half time or more were eligible to receive a second benefit, a monthly housing allowance. Determined by the zip code of the institution that the student attends, the 2009 housing allowance ranged from under $800 in many rural areas to more than $2,700 in New York City. The housing allowance, like the MGIB, is paid directly to veterans as a conditional cash transfer.[16]

The greatest difference between the MGIB and the Post-9/11 GI Bill is the level of benefits provided. In 2008, the MGIB provided roughly $1,300 of benefits per month for up to 36 months, a maximum of roughly $50,000 in total benefits. By contrast, the designers of the new GI Bill wanted the benefit to cover the full cost of attendance at a four-year school, and thus set benefit levels to correspond to each component of this cost. The maximum per-credit benefit provided by the Post-9/11 GI Bill is more than $1,000 in several states, implying a reimbursement of up to $15,000 for a single semester of tuition. In addition to this, the new GI Bill provides coverage for thousands of dollars in fees per term in nearly all states. Adding in the over $2,000 monthly housing allowance in high cost-of-living areas, it is possible for an individual to receive more than $50,000 in benefits in a single school year. While the actual level of benefits received by most veterans is much smaller than this, a back-of-the-envelope calculation suggests that the Post-9/11 GI Bill roughly doubled average maximum benefit levels.[17]

Although the Post-9/11 GI Bill greatly expanded education benefits for veterans, the degree of benefit expansion varies dramatically by geographic region. To quantify this variation, I estimate the combined annual maximum tuition and housing allowance for each state, $Benefit_s = 24 * CreditMaximum + 9 * BAH$.[18] Table A1 and Figure 1 show that the annual maximum benefit levels under the Post-9/11 GI Bill range from close to MGIB levels in some states to tens of thousands of dollars more than the MGIB in others; on average, the increase in a year's worth of maximum available benefits was roughly $13,000. I also produce a simulated state benefit under the assumption that the distribution of enrollment across

[15]The VA also initiated a separate yellow ribbon program to cover the tuition gap for veterans attending out-of-state or private schools. Schools participating in the yellow ribbon program received matching dollars from the VA for every dollar that they contributed to a 100% eligible veteran's tuition.

[16]Students also receive an annual book allowance; the amount is $1,000 for full-time students.

[17]This is based on comparing the veteran population weighted average of state maximum benefit levels under the Post-9/11 GI Bill with the maximum benefit available under the MGIB.

[18]As an estimate of the annual tuition benefit, I multiply the credit maximum within a state by 24. As an estimate of a year's worth of housing allowance benefits available in a given state, I use the weighted average monthly housing allowance across zip codes in the state, multiplied by 9. This corresponds to nine months of enrollment in a traditional two-semester system. I exclude the benefits for fees because the maximum fee levels are generally orders of magnitude larger than the true fee levels charged by all public institutions in a state.

school sectors within a state will mirror that observed nationally after availability of the Post-9/11 GI Bill benefits. The realized increase in average benefit levels and the simulated increase (holding school choice constant) are closer to $5,000 to $6,000. This variation in nominal benefit levels under the new GI Bill in essence generates fifty-one micro-experiments, in which the level of benefits that veterans received depended upon the states to which they likely returned, and whether they returned before or after the Post-9/11 GI was available.[19]

4 Data

Data constraints are one of the primary factors that have limited rigorous research on the effect of financial aid on degree attainment. These constraints are heightened in assessing the effects on veterans of the expansion in GI Bill benefits as no existing dataset contains data on enough veterans to provide even a basic idea of veteran collegiate outcomes, much less a rigorous evaluation of the benefit expansion's effects on educational attainment. As U.S. Senator Harkin, Chairman of the Committee on Health, Education, Labor, and Pensions, noted in 2014, "It remains impossible for anyone other than the companies and colleges to determine how veterans are performing" (Harkin 2014). In order to overcome these data constraints, I assembled a unique panel of administrative data that tracks military service members while in the military as well as their educational choices after separation.

My primary source of data on those with military service is the active duty files of the Defense Manpower Data Center (DMDC). These are administrative data collected by the various branches and maintained by the Department of Defense. I received data on the universe of individuals on active duty from 1998 through the end of 2010. The datasets include all individuals on active duty in the Army, Navy, Marine Corps, and Air Force, for a total of over four million service members.[20] In addition to the standard demographics available in most large survey datasets, I also observed information on birth, home, and residence locations; occupation codes; and aptitude (AFQT) scores. I used yearly records for individuals, combined with information on military separations from the DMDC loss file, to construct measures of service duration, dates of separation, and likely eligibility for the Post-9/11 GI Bill (see the Data Appendix for further details). Likely eligibility for the Post-9/11 GI Bill is determined by both an individual's separation date as well as his or her service characterization.

Having collected data on military service members, I link this data with information on educational outcomes. Due to the financial cost associated with the data linking process, I

[19] As discussed in the estimation strategy section I assign veterans to the state listed as their home of record prior to separation, avoiding concerns related to endogenous location.

[20] Most analyses exclude Air Force veterans because codes indicating home of record are missing.

restricted the population in several ways before drawing a random sample to be matched. First, I restricted the military dataset to enlisted individuals between ages 22 and 39 who had separated within ten years of initial entry into the military.[21] I excluded those who had separated with more than ten years of service and those aged forty and over because (1) they were more likely to view the military as a permanent career and (2) they were substantially less likely to enroll in college. I then selected a random sample of individuals who had separated with an honorable discharge between January 1, 2000 and December 31, 2009, as well as a smaller random sample of those who had separated with less than honorable discharges. The latter would be used as a control group as they are ineligible for GI Bill benefits. I linked the sample of approximately 73,000 veterans with post-secondary data obtained from the National Student Clearinghouse (NSC).

Colleges submit enrollment data to the NSC that indicate the beginning and end date of student enrollment by term. Participating institutions also indicate whether a student has earned a degree, the title of the degree, and the associated college major.[22] The linkage between datasets occurred in August 2014, so the sample contains enrollment and degree attainment information up to that point. The NSC data provide a picture of veteran enrollment and degree attainment not previously available, but they contain very little information about the characteristics of schools themselves. Using school-specific identifiers in the NSC data, I link all institutions to information on the universe of Title IV aid-receiving colleges contained in the Integrated Postsecondary Education Data System (IPEDS). These data contain information about the control of the institution (i.e., public, private non-profit, or for-profit), whether the college is a two-year or four-year school, measures of selectivity, tuition and fee levels, and graduation rates.

Table 1 provides summary statistics for the sample of veterans linked with NSC data. Veterans are substantially more likely to be male and have been married than the general student population. Nearly half of the veterans in my sample enroll in college within three years of separating from the military. Most veteran enrollment occurs at public two-year (45%) or four-year (32%) institutions. Enrollment at for-profit institutions (13%) is likely underestimated given the coverage of the NSC data (see Dynarski et al. 2013).

Overall, the summary statistics indicate that nearly half of the eligible veterans in the sample enrolled in college, and around 20 percent obtained an associate's degree or higher

[21] Over 80% of enlisted individuals will separate within ten years of initial entry. Of those who remain in service after ten years, over three-quarters will remain in service at least 20 years, the vesting point for retirement benefits.

[22] As Dynarski et al. (2013) note, the NSC's expansion in enrollment coverage (caused by the addition of new colleges) over time can create a challenge for researchers who are examining the effect of policies on different cohorts of students. I follow their suggestion for how to account for the change in enrollment coverage by restricting my sample to institutions reporting at the beginning of my sample; in most specifications, this was 2002. Enrollment results are robust to varying the cutoff year for inclusion. This has no effect on the degree outcomes. See Data Appendix for further details of the NSC data and the data linking process.

within six years. Despite the negative press surrounding veteran educational outcomes, these statistics imply that around 40 percent of veterans who go to school within three years of separation obtain a degree of some kind within six; this is comparable to the six-year degree receipt of individuals starting college between ages 19 and 24 (Baum et al. 2013) and substantially higher than statistics about veterans that have been cited in the media.[23]

5 The Timing of Benefit Availability Across Cohorts

Table 2 illustrates the differential treatment of separating cohorts, with the earliest cohorts (2000 and 2001) having no eligibility for the Post-9/11 GI Bill, the middle cohorts (2002 through 2004) having partial eligibility, and the most recent cohorts (2005 through 2009) having full eligibility.[24] As most veterans begin to use their benefits within a few years of separation, the fourth and fifth columns indicate the number of years that the Post-9/11 GI Bill was actually available to individuals in each cohort during the 5 to 6 years after separation. In other words, although individuals in the 2003 separating cohort were eventually given partial Post-9/11 GI Bill eligibility in 2009, their education decisions during the period 5 to 6 years after separation were made when only the MGIB was available.[25] In contrast, individuals in the 2008 separating cohort were able to use the Post-9/11 GI Bill during nearly all of the 5 to 6 years after separation.

The main idea of the paper is to leverage this variation in post-separation benefit availability to identify the effect of the generosity of benefits available during the years immediately following separation on degree attainment. I focus on this particular margin (1) because of the policy import of understanding how increased benefit generosity facilitates investments in human capital during the transition into the civilian labor force (a stated motivation of the benefit expansion), (2) because most veterans begin using their benefits immediately after separation, making it the natural period to study[26], and (3) to ease comparison with the existing literature, which tends to focus on degree attainment within 5 to 6 years of high-school graduation.

Figure 2 illustrates how the main estimation strategy will leverage this over-time vari-

[23] See, for example, `http://www.huffingtonpost.com/2012/10/25/veterans-college-drop-out_n_2016926.html` which cites a dropout figure of 88 percent.

[24] Here, a cohort is defined to include individuals separating prior to August 1 of the year indicated and after July 31 of the prior year. Thus, individuals in the 2008 separation cohort have separation dates between August 1, 2007, and July 31, 2008. I use this definition for two reasons: (1) it follows the traditional college calendar, and (2) it allows me to focus on veterans who separated at least one year prior to the implementation of the Post-9/11 GI Bill, whose separation was thus unlikely to have been influenced by the benefit expansion.

[25] Recall that the extent of eligibility depends on the duration of active-duty service after September 11, 2001.

[26] 50 percent of those eligible begin using their benefits within 3 years after separation. By 9 years after separation, only 55 to 60 percent (i.e., an additional 5 to 10 percent) have begun using their benefits.

ation in post-separation benefit availability to identify the effect of additional aid. Figure 2 plots the proportion of veterans in a cohort who have obtained an associate's degree or higher in the years after separation. For consistency with my main estimation strategy, I focus on the 2003, 2004, and 2008 cohorts; a figure with all cohorts is included in the appendix (Figure A1).[27] Degree attainment rates for the 2003 and 2004 cohorts (blue and red circles), which had highly delayed access to Post-9/11 GI Bill benefits, are nearly identical to each other.[28] Only about 17 percent of these individuals received a college degree within six years of separation. By contrast, the 2008 cohort (green triangles), which had access to Post-9/11 GI Bill benefits soon after separation, were between five and seven percentage points more likely to obtain a degree over the six years following separation. Figure 2 thus indicates that individuals who were able to access the higher levels of aid from the Post-9/11 GI Bill (those separating in 2008) were more likely to obtain a degree than those who were not (those separating several years earlier).

Figure 3 combines the over-time variation in post-separation benefit availability with cross-sectional variation in eligibility to illustrate the difference-in-differences strategy. The figure plots the percentage of likely GI Bill-eligible and -ineligible veterans who enrolled within three years of separation, by year of separation.[29] Here, likely eligibility is determined by having an honorable service characterization at separation.[30,31]As expected, the enrollment rate of veterans unlikely to be eligible for the MGIB or the Post-9/11 GI Bill is substantially lower than that of eligible veterans. However, the trend in enrollment of both groups is relatively flat between 2002 and 2006. As benefits for the Post-9/11 GI Bill first became available in August of 2009, many veterans in the cohort separating in 2007 would have had access to the higher benefits during their third year after separation; this can explain the small uptick in the enrollment of eligible veterans for the 2007 cohort. All eligible veterans separating in 2008 had access to the higher benefit levels within one to two years of separation, which explains the substantially higher enrollment levels for these individuals. The three-year enrollment rate of eligible veterans separating in 2008 is roughly eight percentage points higher than those of cohorts separating between 2002 and 2006. The

[27]Figure A1 shows the attainment trajectories for all separating cohorts, illustrating (as expected) that while the partially treated cohorts had somewhat higher attainment the levels, the cohorts separating around the time of the actual benefit expansion experienced the greatest increases in degree attainment.

[28]The 2003 cohort would not receive Post-9/11 GI Bill funds until the seventh year after separation, while the 2004 cohort would be eligible for benefits in their sixth year after separation.

[29]The set of schools is restricted to those that began reporting to the NSC by the beginning of 2002.

[30]Recall that having an honorable service characterization is required for benefit eligibility. As I use the ineligible group as part of my estimation strategy, I discuss its suitability as a control group at length in that section.

[31]Although veterans can have their service characterization altered after separation, the characterization at separation serves as a good proxy for benefit eligibility because only 10% of enrolled veterans with a non-honorable service characterization are observed using MGIB benefits. The degree that individuals classified as ineligible are actually eligible biases my estimates towards zero.

enrollment of veterans unlikely to be eligible exhibits no such pattern. For completeness, I include the 2009 separating cohort. This cohort was potentially affected by endogenous separation from the military in response to higher benefit levels and/or endogenous changes in service characterization. While I do not include this cohort in the formal analyses, there is evidence of even higher enrollment rates for these individuals. Combined, the evidence suggests that a portion of the increase in degree attainment came via increased enrollment.

6 Estimating the Effects of Aid Expansion

In order to identify the effects of financial aid on veterans' degree attainment, I begin with a simple difference-in-differences approach, identifying the effect of the benefit expansion as the difference between degree attainment rates of eligible veterans separating in the pre-period (2003 or 2004) and those separating in the post-period (2008), using ineligible veterans as a control group.[32] I then turn to a strategy that leverages the geographic variation in the size of the benefit expansion.

6.1 DD: Estimation Strategy

The DD specification takes advantage of the difference in the over-time availability of benefits across veteran types. Eligible veterans, those with an honorable service characterization at separation, form the treatment group, and ineligible veterans, those with a less than honorable characterization, are the controls. The basic specification is:

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \lambda_t + \gamma_0 Eligible_i + \gamma_1 Post911_t * Eligible_i + \epsilon_{ist} \qquad (1)$$

Here, E_{ist} indicates whether individual i who lists home of record of state s and separated in year t has obtained a degree within five or six years of separation. The vector X_{ist} refers to characteristics at the time of separation including age, sex, AFQT (aptitude score), marital status, whether a veteran is black, separation month, and education level at separation. In Z_{st}, I control for changing state-level characteristics, including the average unemployment rate faced by an individual in state s during the year following separation. Fixed effects α_s for each home of record state control for time invariant differences in the likelihood of degree attainment for individuals returning to different states, and fixed effects λ_t for each year of separation control for changes over time that affect the likelihood of degree attainment for all veterans within a cohort. The sample is restricted to individuals separating in the

[32] Recall that while both of these cohorts separated prior to the implementation of the Post-9/11 GI Bill, there is substantial variation in the post-separation benefit availability between those separating in the pre- and post-period (Table 2).

2003, 2004, and 2008 cohorts. As mentioned above, I use the 2003 and 2004 cohorts as the pre-period because their five to six-year degree receipt was unlikely to have been affected by a benefit expansion implemented in the fall of 2009. Those separating in 2008 are in the post-period; $Post911_t$ is an indicator variable set to one if an individual separated in 2008. As mentioned previously, my approach is to compare cohorts that lack post-separation availability of Post-9/11 GI Bill benefits (2003 and 2004) with a cohort that has the benefits available soon after separation (2008). The treatment group is the group of eligible veterans, given by $Eligible_i$. The parameter of interest, γ_1, is the difference-in-differences estimate of the effect of additional benefits. If additional aid has an effect on degree receipt, the difference in attainment rates of eligible and ineligible veterans should be greater for those separating in 2008 than those separating in 2003 or 2004.

The key identifying assumption is that, conditional on a large and detailed set of controls, the change in the degree attainment of ineligible veterans serves as a reasonable proxy for the counterfactual change in eligible veteran degree attainment if the benefit expansion did not occur – the standard parallel trends assumption. I devote considerable attention to discussing whether ineligible veterans are a reasonable control group. Here, I merely note two pieces of evidence that support the notion: first, the two groups are observationally similar; and second, degree attainment (and enrollment) levels trend together prior to the benefit expansion, suggesting that the two groups respond similarly to changing conditions. To further address this concern over the control group selection, I also implement a matching strategy to select a control group that is observationally equivalent to the treated group (Ferraro and Miranda 2014; Smith and Todd 2005).[33]

6.1.1 DD: Estimates

The main DD estimates in column (2) of Table 3, which condition on soldier characteristics, indicate that eligible veterans separating a year prior to implementation of the Post-9/11 GI Bill are 4.5 percentage points more likely to obtain a degree within five years than those separating five or six years prior to implementation, an increase of over 25 percent. Dividing by an estimate of the average increase in maximum benefit levels of $13,000 suggests that every $1,000 of aid increases degree attainment within five years by 0.35 percentage points.[34]

[33] See Appendix A for more detail on how this approach is implemented in the current paper. Recent work by Ferraro and Miranda (2014) suggests that estimates from observational studies may be improved if researchers "pre-process" the data to carefully select an appropriate control group. The basic idea is to use propensity-score matching based on fixed observables to generate a better control group. Once this control group is determined, standard difference-in-differences estimation is implemented implemented.

[34] The effect of actual aid levels is likely larger because most veterans do not access maximum benefit levels. Dividing by an estimate of the realized average increase in benefit levels, $5,000 suggests a larger 0.9 percentage point effect for each $1,000 of aid.

Most of this increase in attainment occurs through an increase in the receipt of degrees at the bachelor's level or higher. Extending the horizon to six years, the estimate grows to 6 percentage points, or 0.45 percentage points for every $1,000 of aid, for any degree attainment, and 4.5 percentage points for bachelor's degree attainment, over 25 percent higher than the base.[35]

6.1.2 DD: Addressing Threats to Internal Validity

The primary assumption underlying the DD analyses is that the degree attainment of ineligible veterans is a good proxy for the change that would have occurred for eligible veterans, absent the benefit expansion. Figure 4 addresses this parallel trends assumption. The event study plots the coefficients and confidence intervals from a similar specification as in equation (1) but with the full set of $SepYear_i$, $Eligible_i$, and $SepYear_i * Eligible_i$ indicator variables. The coefficients for each separation year interacted with benefit eligibility are plotted. The larger black circles represent the years used in estimation. The plot suggests that the attainment levels of eligible and ineligible veterans roughly move together for veterans separating between 2001 and 2006, but diverge sharply in 2007, 2008, and 2009. The lack of divergence for the 2005 through 2006 cohorts may be somewhat surprising because these individuals were eligible for the higher level of benefits within four or five years of separation, and suggests that aid is substantially more effective if it comes during the first few years after separation. In contrast, a similar event study for bachelor's degree receipt shows an upward trend in the attainment rate beginning for the 2005 and 2006 separating cohorts (Figure 5). This is, perhaps, expected because BA degree recipients generally enroll for at least four years and thus individuals who separated in 2005 and 2006 and attempted a BA degree were likely still enrolled when the benefit expansion was implemented. It also suggests that some individuals may have foregone attainment of an Associate's degree in favor of BA degree attainment.

I include the 2001, 2002, 2005 through 2007, and 2009 separation cohorts in Figure 4 for illustrative purposes, although they are not in the sample underlying Table 3.[36] My approach is to compare cohorts (the dark black circles in Figures 4 and 5) who lack post-

[35]Standard errors are clustered at the state level. These standard errors may be underestimated if the residuals are auto-correlated within cross-sections (eligibility groups). For completeness, I note here that p-values are quite similar, and even smaller when clustering on eligibility (which would account for this correlation) as well as eligibility by state, separation year, or separation year by eligibility. While in some cases this results in few clusters, p-values remain well below 0.05 even after making the appropriate adjustment to the sample test statistics (i.e., using T(G-1) p-values). The standard errors and p-values presented are the most conservative produced across any of these strategies.

[36]The 2001 and 2002 cohorts are excluded because MGIB benefit levels were increased between 2001 and 2003. The 2005 through 2007 cohorts are excluded because they are partially treated (i.e., eligible for the higher level of benefits within a few years of separation) and the 2009 cohort is excluded because of concerns related to endogenous separation from the military in response to the benefit expansion.

separation of Post-9/11 GI Bill benefits (2003, 2004) with a cohort (2008) that has the higher level of benefits available soon after separation. While I think it is natural to focus on the stark variation in post-separation availability of benefits between these cohorts, I also present estimates that leverage the intervening cohorts. This is addressed more formally in Table A2, but the event studies and associated discussion suggest the answers I will find; the attainment of ineligible and eligible veterans appears to trend together until we get to cohorts that experienced exposure to the higher level of benefits. The partially treated cohorts (2005, 2006, 2007) appear to be affected to some extent (particularly in terms of bachelor's degree attainment), but less so than fully treated cohorts (2008 and 2009). Interpretation of the effect on the 2009 separating cohort is muddied by concerns related to the endogenous separation and service characterization of individuals in response to the benefit expansion.

The similar pre-treatment trends suggest that ineligible veterans are an appropriate control group for eligible veterans, but the groups differ in other ways. In Table A3, I provide information on the covariate balance of the two groups. While the eligible veterans are slightly older, less likely to be black, slightly more educated, and have higher aptitude scores, the differences are fairly small. Furthermore, there is very little change in the observables of the two groups from the pre- to post-period (Table A3) or over the sample period more generally (Figure A2). A comparison of columns (1) and (2) in Table 3 indicates how the inclusion of covariates affects the estimates. While the inclusion of covariates reduces the magnitudes slightly, the estimates are statistically distinguishable. I also use a matching approach to select ineligible veterans who are similar to eligible veterans.[37] In column (3) of Table 3, I present the DD estimates using this matched control group. The point estimates are somewhat larger but statistically indistinguishable from those using the other DD approaches.

In addition to general concerns about the appropriateness of the control group, another potential concern with the DD strategy is that the Great Recession may be responsible for the relative increase in the attainment levels of eligible veterans. For this to be the case, eligible veteran educational attainment would have to be more responsive to downturns than non-eligible veteran educational attainment. This may be reasonable if non-eligible enrollees have reduced resources to buffer economic downturns. However, the inclusion of state-year unemployment rates $Unemp_{st}$ and their interaction with eligibility status helps control for this concern; this strategy leaves the results unchanged.[38] This point is buttressed by recent

[37] I implement this approach separately in the pre- and post-periods (see Appendix A for details of the process). Table A3 illustrates how the pre-processing improved the covariate balance between the two groups, making them observationally equivalent across a range of covariates.

[38] The unemployment interaction term is insignificant and the point estimates for my preferred specifications in the top panel of Table 3 (columns (2) and (5)) are .048 (se 0.016) vs. 045 (se 0.016) for Associates + and 0.035 (se 0.011) vs. 0.033 (se 0.012) for BA+. The analogous comparisons in the bottom panel are 0.064 (se 0.020) vs. 0.060

evidence that the college enrollment of veterans is not affected by state unemployment rates (Borgschulte and Martorell 2016). Furthermore, Figures 4 and 5 suggest that the two groups were trending together in the pre-period, including the period of the 2001 recession. While a shallower and shorter recession, this suggests that there is likely not a strong difference in how the two groups respond to business cycles. Although it is unlikely that the Great Recession is driving the results, in the next section I turn to estimates that use geographic variation in the size of benefit expansion over time; this variation is unrelated to differences across states in the severity of the recession (as measured by the unemployment rate).

A related concern is that changes to the generosity of the Pell grant, which occurred at roughly the same time as the benefit expansion and Great Recession, may have affected GI Bill eligible and ineligible veterans differently. This is unlikely to be the case because both groups of veterans face similar requirements to access the Pell grant. Furthermore, the Post-9/11 GI Bill benefits have no effect on Pell eligibility. Pell eligibility is based on an individual's expected family contribution (EFC). By statute, veteran education benefits are excluded from income for the purposes of this calculation. Furthermore, eligibility for the Pell grant does not take into account other scholarships or awards so there was no crowd out from changes in policy.

Another related concern with the DD strategy is that the composition of separating veterans has changed over time in ways that are not controlled for or observed, perhaps as a result of the Great Recession. However, given the administrative process of separations and the fact that labor market conditions didn't begin to deteriorate until the middle of 2008, it is unlikely that endogenous separation as a result of the Great Recession is responsible for the observed results. Furthermore, there is no evidence of a change in reenlistment rates of soldiers over this period. While my regressions all control for a rich set of individual characteristics, Figure A2 further illustrates relatively small changes in a variety of demographic characteristics over the time period of the analysis.[39] We might still be concerned if veterans with unobservable preferences for education were more likely to separate in 2008. However, the sample is restricted to veterans separating prior to the end of the summer during which the Post-9/11 GI Bill was passed. Given the administrative process of separation, it is extremely unlikely that individuals in the sample separated in response to a benefit expansion that had not yet been passed.

(se 0.021) for Associates + and 0.047 (se 0.017) vs. 0.045 (se. 0.018) for BA+.

[39]Barr (2015) also explores a variety of factors that may have affected the composition of military veterans, including changes in the composition of recruits, suicide, and mortality. Overall, the evidence suggests that, if anything, changes in composition likely would have led to lower enrollment and attainment levels as the military lowered standards slightly for enlistment during the mid-2000s.

6.2 Geographic Variation: Estimation Strategy

I now turn to another strategy for estimating the effects of veteran financial aid expansion, one that does not make use of ineligible veterans as a control group. This complementary strategy instead leverages geographic variation in the size of the benefit expansion generated by the transition from the MGIB to the Post-9/11 GI Bill (Figure 1 or Table A1). Assigning eligible individuals to states based on their home of record at time of separation, I link in average maximum expected benefit levels:

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \lambda_t + \gamma Post911_t * Benefit_s + \epsilon_{ist} \quad (2)$$

The effect of additional aid is identified by the covariation between degree attainment levels and the size of the benefit expansion experienced by veterans returning to a particular state ($Benefit_s$). For example, in 2005, an MGIB-eligible veteran returning to any state was eligible for roughly $11,000 (2009 dollars) in one year of benefits. In the fall of 2009, a veteran returning to Oklahoma was eligible for about $3,500 in tuition benefits and $8,280 in housing allowance benefits, for a total of just under $12,000. In contrast, a veteran returning to New Jersey was eligible for about $10,800 in tuition benefits and $17,000 in housing allowance benefits, almost $28,000 in total. If additional aid causes higher educational attainment, we would expect to see a larger increase in the educational attainment levels of veterans returning to New Jersey versus those returning to Oklahoma, for example, when comparing cohorts separating several years before Post-9/11 GI Bill implementation with those separating within a year of the benefit expansion.[40]

My key assumption is that, conditional on the set of observables, the unobserved factors that affect attainment are uncorrelated with the size of the benefit expansion in a state. For example, if veterans interested in college enrollment migrate to states with higher benefit levels following the benefit expansion, and I used their eventual state of residence to assign benefit levels, this assumption would be violated. In other words, the actual residence of the veteran once the details of the benefit expansion are available is endogenous. It is for this reason specifically that I limit the sample to individuals separating prior to the fall of 2008 and use the veteran's home of record state at separation to assign benefit levels.[41] The benefit maximum in a veteran's home of record state at separation will be strongly correlated with the actual benefit maximum faced by the veteran, but it will be uncorrelated with preferences for education after conditioning on state-fixed effects. To the degree that

[40] As the new GI Bill itself illustrates, a dollar may not be worth the same amount everywhere in the country. In the online appendix, I discuss this point and also present estimates that adjust the differences in the benefit expansion for cost-of-living differences.

[41] Results are nearly identical using the home of record state from a year or two prior to separation (which varies little from that listed at separation).

veterans do not reside in their home of record at separation or move to take advantage of the higher benefit levels in another state, my estimates will be biased towards zero. In this sense, the geographic estimates provide an intent-to-treat estimate, and should be scaled up when they are being compared with the average treatment effect estimates provided by the other estimation approaches.

6.2.1 Geographic Variation: Estimates

In Table 4, I present estimates of the effect of an additional $1,000 in maximum annual combined tuition and housing allowance benefit levels, instead of the effect of the Post-9/11 GI Bill as a whole. The coefficient on $Post911 * CombinedMax.$ indicates how six-year degree attainment levels vary with the size of the benefit expansion in a state. The estimate in column (1) indicates that $1,000 of additional annual benefits increases the probability that a veteran obtains any degree within six years by 0.2 percentage points; the estimate is 0.4 percentage points when using the simulated benefit size.[42] Both sets of estimates are highly statistically significant.[43] As discussed in the previous section, not all veterans actually return to or remain in their "home of record" state; this "crossover" of veterans from one state to another is similar to the crossover of participants from one treatment arm to another in a randomized control trial. As such, the results in Table 4 are intent-to-treat estimates that should be scaled up before being compared to the DD estimates. Roughly 65 percent of enrolled veterans are enrolled in their home of record state. Under the assumption that veterans will be more likely to migrate towards higher-benefit states, dividing the point estimate by .65 provides a lower bound estimate for the treatment effect.[44] This calculation indicates that each additional $1,000 of aid increases the likelihood of degree attainment by 0.31 percentage points, slightly smaller than, but statistically indistinguishable from, the effect sizes implied by the DD estimates.[45] The size of the benefit may not have been salient to all veterans, explaining the somewhat smaller estimates using geographic variation in the size of the benefit expansion. First, a portion of the higher benefit level is only accessible if one attends a more expensive school. Second, it is unclear to what degree all veterans were aware of the exact nature of the provided benefit (see the Online Appendix for further discussion). Finally, if veterans moved to take advantage of benefit levels, estimates will be

[42] Recall that the simulated benefit size is generated using the nationwide post-benefit expansion enrollment distribution across sectors to generate state-level simulated average benefit levels.

[43] In Figures A3 and A4, I demonstrate the robustness of the p-values to a conservative form of randomization inference. I randomly reassign benefit levels to states and estimate the coefficients in column (1) of Table 4. I repeat this process 1,000 times and plot the distribution of estimates. For both specifications, just over one percent of estimates are larger than the estimate obtained using the true assignment of benefit levels.

[44] This also assumes that the migration decisions of all veterans are roughly similar to those of enrolled veterans.

[45] The effect of the simulated benefit size is scaled to 0.62 pp.

biased towards zero.

An alternative approach to scaling the point estimates is to calculate the combined benefit level for veterans with a particular home of record under the assumption that veterans separating in 2008 will return to geographic locations in the same way that individuals separating earlier did.[46] In other words, if 50% of veterans with a Kansas home of record end up in Colorado and 50% end up in Kansas, the maximum benefit for an individual with a Kansas home of record will be set to equal the average of the Colorado and Kansas benefit levels.[47] Estimates from this strategy indicate that each additional $1,000 of maximum aid increases the likelihood of degree attainment by 0.30 percentage points, statistically indistinguishable from the effect sizes implied by the scaled geographic estimates and the DD estimates.

6.2.2 Geographic Variation: Addressing Threats to Internal Validity

A major concern associated with these geography-based estimates is that the size of the Post-9/11 GI Bill benefit in a state, and thus the magnitude of the benefit increase, is somehow related to other factors that influence educational choices. For example, the Post-9/11 benefit maximum in a state may be correlated with trends in resources available for higher education or labor market opportunities. If this is the case, it may be that these factors, and not the changing benefit availability, are causing veteran attainment rates to change differentially across states. Table A6 indicates that there is no relationship between trends in appropriations or labor market conditions and the size of the benefit increase; in fact, the point estimate for log appropriations to higher education is negative, suggesting that states with larger veteran benefit increases have actually been spending less on higher education over the relevant period. The estimate for the unemployment rate is a tightly estimated zero.

A separate approach to addressing this threat to validity is to test for trends in attainment or enrollment among the civilian population that correlate with the Post-9/11 benefit maximum. Table A6 presents these estimates for a number of age groups and education

[46] I follow the literature in estimating the effect of additional *available* benefits, but another interesting suggestion that has been made is to instrument for the actual benefit level received by each veteran. While data constraints prevent a full exploration of this approach, there are also concerns related to the interpretation of the effect of actual benefits received in a structural equation where positive benefit receipt is conditional on enrollment (i.e., enrollment must equal one if the level of actual benefits received is positive). For example, if the benefit expansion has no effect on the level of benefits conditional on enrollment, but does increase enrollment, the average benefit level received will go up. However, interpreting this as an effect of additional benefits is misleading. A related approach, instrumenting for the maximum benefits in the state an individual ends up in, would do a better job addressing the scaling issues discussed in this section, but I do not observe where non-enrolled veterans reside.

[47] In practice, I use data on veterans separating in 2003 and 2004 to calculate the share of individuals with a particular home of record who are enrolled in each state. I use these shares to create a weighted average benefit level for each home of record.

restrictions using the 2006 through 2012 samples of the American Community Survey. Individuals in higher benefit states become less (not more) likely to have a degree or be enrolled in college over this time period.

Finally, I present an event study which illustrates that the relative increase in the degree attainment of veterans returning to states with higher benefit levels under the Post-9/11 GI Bill only began to occur for the cohorts separating in 2008 and 2009 (Figure 6). Prior to those separating cohorts, there was a flat to slightly downward trend.[48]

7 Heterogeneity and Mechanisms

The above evidence strongly indicates that higher levels of financial aid cause individuals to be more likely to obtain a degree, but it does not explain why. Degree attainment is a product of the decision to enroll and the decision to persist to degree. In this context, the Post-9/11 GI Bill has primarily three effects. First, it reduces the cost of enrolling at most institutions. Second, the Post-9/11 GI Bill reduces the relative cost of institutions with high tuitions, or in areas with high costs of living, making those types of institutions more appealing. This may affect degree attainment if veterans shift their enrollment towards higher-quality institutions. Third, the Post-9/11 GI Bill reduces the direct cost of persisting in college.[49]

7.1 Enrollment and Heterogeneity

Having laid out the channels through which the benefit expansion may increase degree attainment, I first estimate specifications similar to equation (**??**) for enrollment. The bottom panel of Table 5 presents the standard difference-in-differences estimate of whether an individual has enrolled in any program within three years of separation. Post-9/11 GI Bill eligibility results in a 7.7 percentage point increase in the likelihood that a veteran enrolls within three years of separation, an increase of over 15 percent.[50] The DD estimate implies a substantially larger effect than the enrollment estimates using geographic variation in the size of the benefit expansion (0.001*, presented in Table 6), suggesting that veterans may have been unaware of the variation across geography. Reconciling this with the effects on degree attainment suggests that veteran enrollment was responsive to awareness of the general

[48] Combining this result with the small negative coefficients for the civilian population and the appropriation results, suggests another reason that the geographic variation approach may produce underestimates for the effect on veterans.

[49] See the Online Appendix for a discussion of how changes in the cost of persisting may have contributed directly to the increase in degree attainment rates.

[50] While Barr (2015) focuses on a somewhat different population and set of outcomes, the finding of a 15 to 20 percent increase in the likelihood of veterans being enrolled at a particular age is consistent with the estimates in Table 5.

increase in benefit levels, while degree attainment was responsive to the size of the increase. This implies that veterans were potentially unaware of the size of the benefit expansion when making their enrollment decisions, but were affected by the additional dollars available once enrolled. This lack of awareness of the net cost of attending different institutions is consistent with recent work by Hoxby and Turner (2015) that explores the effect of information among low-income high achieving students.

Table 5 also illustrates how the enrollment and degree attainment estimates vary across individuals. While all of the point estimates are statistically indistinguishable from each other, there is suggestive evidence that the benefit expansion increased the college attainment of high-aptitude individuals more than that of low-aptitude individuals, with the degree attainment of high-aptitude individuals increasing by nearly nine percentage points, almost double the point estimate for those with AFQT scores below the median. Interestingly, the enrollment estimates for the low and high aptitude are opposite in magnitude from the degree attainment estimates, suggesting that the higher benefit levels may have pulled some lower-aptitude students into school, who then failed to graduate.

Does the overall increase in enrollment account for the higher degree attainment rates? Assuming that the rate of degree attainment among marginal enrollees is similar to other veterans, we can get a back of the envelope estimate of the effect on degree attainment. Multiplying the estimate of marginal enrollment (.077) by the share of enrolled veterans obtaining a degree within six years of separation suggests that marginal enrollees are responsible for at most 3 percentage points, or one-half, of the observed increase in attainment.[51] This finding suggests that the other half of the degree attainment effect is coming through the channel of increased persistence, which may result from increased school quality or the increased level of resources available to veterans (holding school choice constant). While I cannot pin down these elements completely, I can provide some evidence as to the contributions of each.

7.2 School Choice and Persistence

Recent evidence points to the importance of school quality in influencing completion rates and adult outcomes (Bound et al. 2010, Cohodes and Goodman 2013; Cellini and Turner 2016). Because the Post-9/11 GI Bill reduces the relative cost of more expensive institutions, we might expect individuals to gravitate toward these types of schools. Following this logic, if college quality is positively correlated with cost, then the expansion in aid may result in quality upgrading that translates into higher degree attainment. For several reasons, though, it is difficult to determine whether this hypothesis is borne out in

[51] While it is possible that the marginal enrollees were more likely to finish a degree, this runs counter to most human capital investment models and is inconsistent with the evidence that the marginal enrollees had lower aptitudes on average.

the data. First, college quality is very difficult to measure, particularly outside of the most selective institutions. Second, disentangling the effect of the benefit expansion on school quality is complicated by the fact that the expansion also affected veteran enrollment. So if, for example, the marginal group of veteran enrollees has an unobservable preference for inexpensive schools, it may appear that the average school cost has decreased, when in fact inframarginal enrollees are enrolling in more expensive schools. The third reason why it is difficult to assess whether the aid expansion results in higher school quality is that, due to limitations in the coverage of the NSC data, I am likely to underestimate effects on for-profit enrollment, which may affect measured school quality. With these caveats in mind, I present estimates of how school choice has changed since the passage of the Post-9/11 GI Bill (Table 6).

The first row presents the simple change in enrollment for eligible veterans from the pre- to the post-period. Most of the enrollment increase occurs at four-year schools. The increase in for-profit enrollment, while small in percentage terms (2.5 pp), is over 50%. Columns (7) indicates that individuals are enrolling at schools with higher Post-9/11 benefit levels; veterans separating in the post-period are enrolling in colleges with an additional $433 per year in benefits under the Post-9/11 GI Bill (column (7)). While it appears that veterans are enrolling in more expensive institutions, columns (8)-(9) indicate that they are not necessarily enrolling in institutions with higher graduation rates. I interpret these results cautiously, as the types of veterans enrolled have changed at the same time as the incentives to enroll in different school types.

The second set of results uses the geographic variation in the benefit expansion. These results are largely consistent with the overall changes in the first row. A $1,000 increase in maximum benefits in the state results in enrollment at a school with, on average, roughly $20 more in annual benefits under the Post-9/11 GI Bill (column (7)). The last three columns contain some suggestive evidence that individuals experiencing increases in maximum benefit levels are more likely to enroll in schools with higher graduation rates and less likely to enroll in non-selective institutions. In summary, while it appears that veterans are shifting towards schools with higher Post-9/11 GI Bill benefit levels, there is only limited evidence that this has resulted in an upgrade in school quality. This indicates that something else is likely responsible for the increase in veteran persistence. Additional analyses suggest that the direct effect of additional aid may play an important role.[52]

[52]See the Online Appendix for further discussion.

8 Discussion and Conclusion

Overall, my results suggest that the Post-9/11 GI bill increased degree attainment by more than 25%, roughly 0.4 percentage points per $1,000 of additional maximum aid. Scaling these by an estimate of the average realized increase in benefits ($5,000) suggests an effect of roughly one percentage point per $1,000 of additional aid. This effect is the result of getting additional veterans into school as well as making inframarginal students (in terms of enrollment) more likely to obtain a degree. Although the data show that veterans are attending more expensive schools, there is little evidence that increased school quality has contributed to higher persistence levels. Instead, it appears that the direct effect of the aid has contributed to increased rates of year-to-year persistence.

Veterans belong to a larger category of older non-traditional students who frequently enroll as a way to transition between occupations. I have shown here that even when these types of individuals have high initial levels of support (like under the MGIB), offering additional financial aid makes them more likely to obtain a college degree. This initially high level of aid may explain why the effects estimated in this paper are substantially smaller than some estimates on the effects of financial aid on the degree attainment of traditional students (e.g., Castleman and Long (2016) who find a 4 pp effect per $1,000) as well as the more substantial set of estimates of the effect of aid on enrollment (Dynarski and Scott-Clayton (2013) who summarize a 3-4 pp effect per $1,000). However, it is worth noting that prior estimates of the effect of financial aid on the enrollment of older, non-traditional students indicate a smaller effect (1 pp per $1,000) for this group (Seftor and Turner 2002). The effects of additional aid on the college success of the broader group of non-traditional civilian students may be larger than that observed among veterans as the latter group already received relatively generous levels of financial aid prior to the benefit expansion.

Given the U.S government's enormous expenditures on the Post-9/11 GI Bill, a natural question is whether this significant aid intervention on behalf of veterans is a sound investment. Prior estimates of the effect of degree attainment on earnings and employment suggest that the increase in education levels among veterans will help hundreds of thousands of them transition into the workforce. Whether or not the benefit expansion is a beneficial investment for society, however, is another question. We can produce a back-of-the-envelope estimate of the direct cost of each additional degree by estimating the increase in benefits paid out and dividing by the number of additional degrees obtained. These calculations suggest costs between $75,000 and $150,000.[53] Higher than analogous estimates from some financial aid interventions, this cost per degree is substantially smaller than net present value

[53] See Appendix B for the details of these calculations. These calculations ignore any benefits or costs that might accrue to society through the use of the GI Bill as a recruiting tool.

estimates of the return to a college degree.[54] Future work will explore the degree to which prior research on the return to education generalizes to Post-9/11 veterans.

[54]See Oreopoulos and Petronijevic (2013) and Avery and Turner (2012) for overviews of these net present value calculations.

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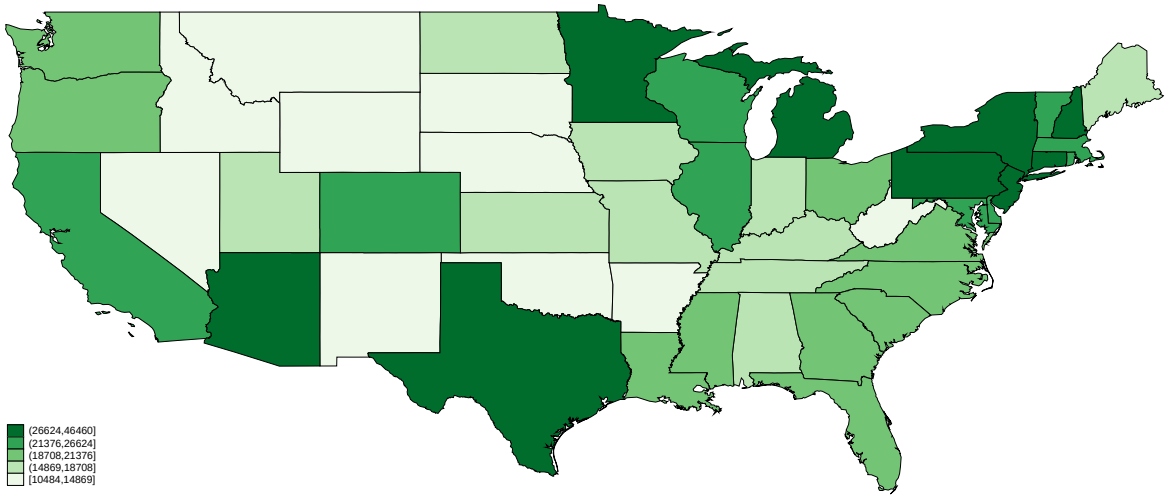
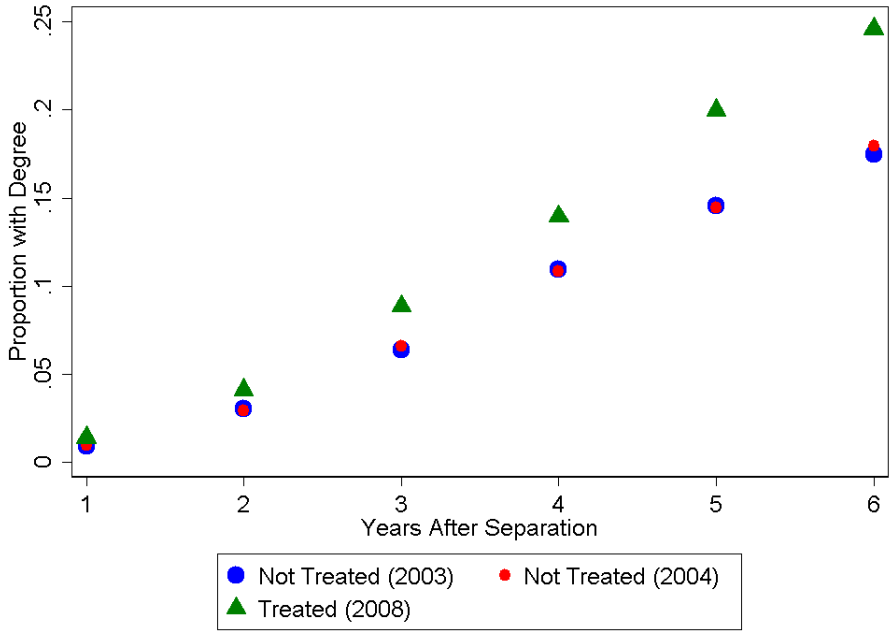
Figure 1: Estimate of Annual Combined Maximum Benefits



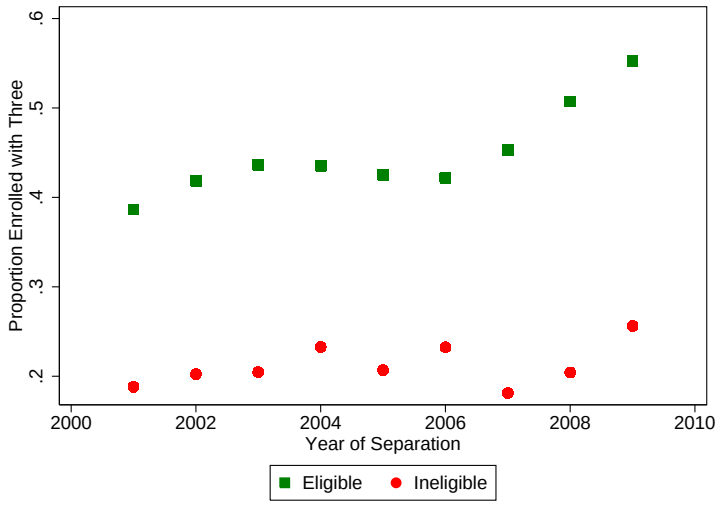
Note: Estimate of annual combined maximum is equivalent to 9*BAH + 24*Credit Maximum. Tuition credit maximums for 2009-2010 obtained from `http://www.gibill.va.gov/gi_bill_info/ch33/tuition_and_fees_2009.htm`. Basic allowances for housing are weighted state averages of zip code BAH levels for 2010.

Figure 2: Attainment of any Degree by Separation Year



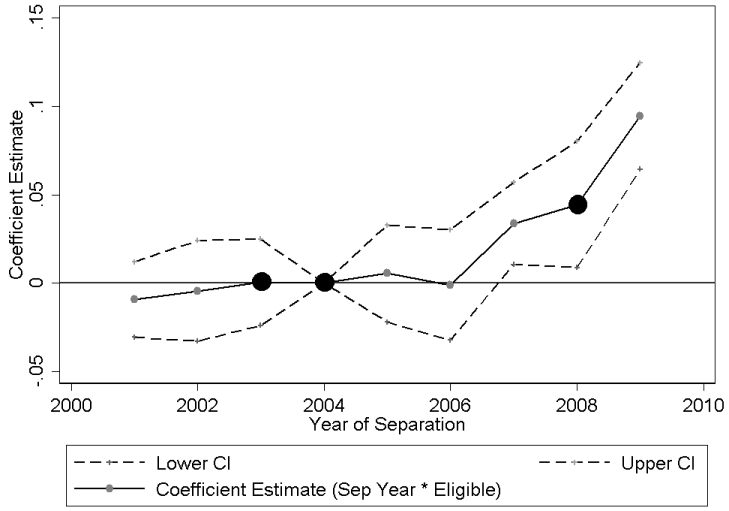
Note: Each marker represents the share of eligible veterans that obtained any degree by years after separation. Statistics are presented separately for the 2003, 2004, and 2008 cohorts. See Table 3 or the text for details of the sample restrictions.

Figure 3: Three Year Enrollment Rates by Year of Separation



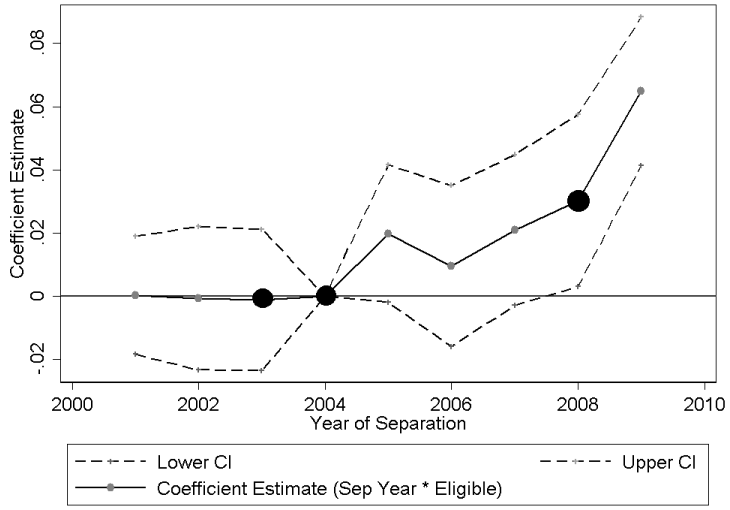
Note: The figure plots the share of veterans enrolling within three years of separation by year of separation. The dark circles represent the enrollment rates of eligible veterans and the light squares represent the enrollment rates of ineligible veterans. See the notes to Table 3 or the text for details of the sample restrictions.

Figure 4: Event Study: Effect on Any Degree Five Years After Separation



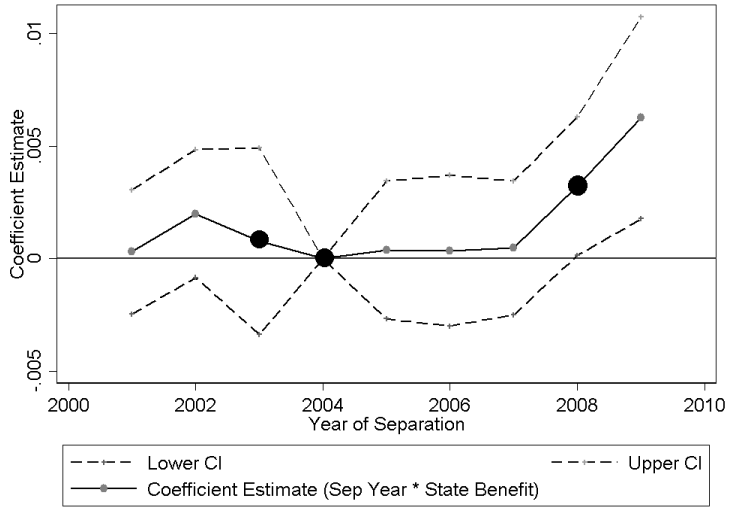
Note: The dependent variable is receipt of any degree within five years of separation (results similar using six years). Circles represent the coefficient estimates for each year of separation interacted with eligibility. Large black circles represent years used in main estimates presented in tables. Smaller and lighter plus signs indicate the 95 percent confidence interval for each point estimate. The 2004 coefficient is set to zero as it is the omitted year. See the notes to Table 3 for details of the sample restrictions.

Figure 5: Event Study: Effect on BA Degree Five Years After Separation



Note: The dependent variable is receipt of a bachelor's degree or higher within five years of separation (results similar using six years). Circles represent the coefficient estimates for each year of separation interacted with eligibility. Large black circles represent years used in main estimates presented in tables. Smaller and lighter plus signs indicate the 95 percent confidence interval for each point estimate. The 2004 coefficient is set to zero as it is the omitted year. See the notes to Table 3 for details of the sample restrictions.

Figure 6: Event Study (geographic variation): Effect of State Benefit Level on Any Degree



Note: The dependent variable is receipt of any degree within five years of separation (results similar using six years). Circles represent the coefficient estimates for each year of separation interacted with the simulated state benefit size. Large black circles represent years used in main estimates presented in tables. Smaller and lighter plus signs indicate the 95 percent confidence interval for each point estimate. The 2004 coefficient is set to zero as it is the omitted year. See the notes to Table 3 for details of the sample restrictions.

Table 1: Descriptive Statistics

VARIABLES	(1) Mean
Age at Separation	25.05
Black	0.155
Male	0.840
Married	0.419
AFQT Score	60.12
HS Degree at Separation	0.902
Enroll within 3 Years	0.447
Any Degree within 5 Years	0.181
Any Degree within 6 Years	0.222
BA within 5 Years	0.097
BA within 6 Years	0.127
Obs.	29,361
Enrolled Fall 2006 through Fall 2013	
Share Two-year Public	0.45
Share Four-year Public	0.32
Share For-profit	0.13
Share Private Non-Profit	0.10

Note: Table presents descriptive statistics for individuals in the NSC sample who separated between 2003 and 2008. Enrollment statistics present the breakdown of school types for those observed enrolled between Fall 2006 and Fall 2013.

Table 2: Timeline of Eligibility and Availability of Benefits

Separating Cohort	MGIB Eligibility	Post-9/11 Eligibility	Years of Post-9/11 Availability	
			5 Years After Separation	6 Years After Separation
2000	Yes	No	0	0
2001	Yes	No	0	0
2002	Yes	Partial	0	0
2003	**Yes**	**Partial**	**0**	**0**
2004	**Yes**	**Partial**	**0**	**1**
2005	Yes	Full	1	2
2006	Yes	Full	2	3
2007	Yes	Full	3	4
2008	**Yes**	**Full**	**4**	**5**
2009	Yes	Full	5	6

Note: Separating cohorts defined as indicated in the text. All cohorts eligible for MGIB (column 2). Column 3 indicates eligibility for Post-9/11 GI Bill benefits, which is based on the duration of active-duty service after September 11, 2001 (see the text for details). While all cohorts after 2001 had at least partial eligibility for Post-9/11 GI Bill benefits, these benefits were not available until the Fall of 2009. Columns 4 and 5 indicate the number of years that Post-9/11 benefits were available during the 5 or 6 years after separation. Column 4 indicates the number of years out of the 5 years after separation that Post-9/11 GI Bill benefits were available to a cohort. Similarly, column 5 indicates the number of years out of the 6 years after separation that Post-9/11 GI Bill benefits were available to a cohort. Cohorts used to produce main estimates are in bold.

Table 3: Degree Effects Five and Six Years After Separation: DD Regressions

VARIABLES	(1) Associates +	(2) Associates +	(3) Associates+	(4) BA+	(5) BA +	(6) BA +
Five Years after Separation	0.056***	0.045***	0.051**	0.041***	0.033***	0.047***
	(0.015)	(0.016)	(0.024)	(0.011)	(0.012)	(0.016)
Covariates		X	X		X	X
Pre-processed			X			X
Observations	15,457	15,457	15,136	15,457	15,457	15,136
Mean	0.164	0.164	0.164	0.0994	0.0994	0.0994
Six Years after Separation	0.072***	0.060***	0.064**	0.054***	0.045**	0.062***
	(0.018)	(0.021)	(0.027)	(0.017)	(0.018)	(0.020)
Covariates		X	X		X	X
Pre-processed			X			X
Observations	15,457	15,457	15,136	15,457	15,457	15,136
Mean	0.201	0.201	0.201	0.131	0.131	0.131

Note: Table shows coefficients from difference-in-differences regressions that compare degree attainment levels for eligible and ineligible veterans from before to after the implementation of the Post-9/11 GI Bill. Each column presents the estimate from a single regression for a different dependent variable. All specifications also includes year and state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. Specifications with covariates also include indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, and baseline degree level. Pre-processed data use matching procedure to form control groups as described in the text and appendix. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans aged 22 to 39 with less than a bachelor's degree who separated between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period) and listed a U.S. state as a home of record. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 4: Degree Effects Using Geographic Variation (6 Years)

VARIABLES	(1) Associates +	(2) BA +
Post 9/11 * Combined Max.	0.0020***	0.0016***
	(0.0005)	(0.0005)
Post 9/11 * Combined Simulated	0.0040**	0.0037**
	(0.0016)	(0.0015)
Observations	14,222	14,222
Mean	0.201	0.131

Note: Table shows coefficients from regressions of degree attainment levels on the size of the annual estimate of the combined Post-9/11 GI Bill maximum or the simulated size of the annual benefit (in thousands) interacted with whether the individual separated between August 1, 2007 and July 31, 2008 (the post-period). Each cell presents the estimate from a single regression. Specification also includes indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, baseline degree level, year and state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans aged 22 to 39 with less than a bachelor's degree who separated between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period) and listed a U.S. state as a home of record. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$). ***($p<0.01$).

Table 5: DD Demographic Heterogeneity

VARIABLES	(0) Full Sample	(1) Black	(2) Not Black	(3) Male	(4) Female	(5) Young	(6) Old	(7) Low Aptitude	(8) High Aptitude
Associates+ (6 Years)	0.060***	0.058	0.064**	0.062**	0.019	0.061**	0.001	0.046	0.087*
	(0.018)	(0.050)	(0.029)	(0.026)	(0.103)	(0.026)	(0.112)	(0.028)	(0.045)
Group Difference (p-value):			0.868		0.395		0.437		0.119
Mean	0.201	0.169	0.207	0.185	0.279	0.201	0.191	0.136	0.268
BA+ (6 Years)	0.045**	0.038	0.050**	0.045**	0.006	0.046**	-0.015	0.022	0.083**
	(0.018)	(0.043)	(0.024)	(0.022)	(0.088)	(0.022)	(0.098)	(0.022)	(0.040)
Group Difference (p-value):			0.724		0.304		0.330		0.220
Mean	0.131	0.115	0.134	0.121	0.179	0.131	0.137	0.0771	0.187
Enrollment (3 Years)	0.077**	-0.014	0.109***	0.090***	-0.084	0.074**	0.02	0.097**	0.055
	(0.029)	(0.069)	(0.036)	(0.033)	(0.117)	(0.033)	(0.142)	(0.041)	(0.052)
Group Difference (p-value):			0.014		0.153		0.470		0.345
Mean	0.460	0.450	0.462	0.446	0.534	0.465	0.377	0.406	0.517
Observations	15,456	2,529	12,927	13,025	2,431	14,652	804	8,147	7,309

Note: Table shows coefficients from difference-in-differences regressions that compare degree attainment and enrollment levels for eligible and ineligible veterans from before to after the implementation of the Post-9/11 GI Bill. Each cell presents the estimate from a single regression. Young is defined as being under the age of 30. Specification also includes indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, baseline degree level, year and state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans aged 22 to 39 with less than a bachelor's degree who separated between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period) and listed a U.S. state as a home of record. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 6: Enrollment within Three Years: School Choice

VARIABLES	(1) Enroll	(2) 4-Year	(3) 2-Year	(4) Public	(5) For-profit	(6) Private Non-profit	(7) School Cost	(8) Grad Rate ≥ 25	(9) Grad Rate ≥ 40	(10) Open Admission
Post 9/11	0.065***	0.048***	0.013	0.027***	0.025***	0.010***	0.433***	0.015**	0.008	0.008
	(0.009)	(0.007)	(0.008)	(0.009)	(0.004)	(0.003)	(0.114)	(0.007)	(0.005)	(0.012)
Post 9/11 * Combined Max.	0.0013*	0.0008	0.0003	0.0008	-0.0001	0.0004**	0.0195***	0.0001	0.0004	-0.0021
	(0.0007)	(0.0009)	(0.0008)	(0.0005)	(0.0003)	(0.0002)	(0.0072)	(0.0005)	(0.0005)	(0.0013)
Observations	14,219	14,219	14,219	14,219	14,219	6,324	14,219	14,219	14,219	6,562
Mean	0.460	0.186	0.261	0.377	0.0451	0.0247	15.46	0.196	0.091	0.684

Table shows coefficients from regressions that compare the enrollment of veterans from before to after the implementation of the Post-9/11 GI Bill. Each cell presents the estimate from a single regression. Specification also includes indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, baseline degree level, year and state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans aged 22 to 39 with less than a bachelor's degree who separated between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period) and listed a U.S. state as a home of record. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Appendix A: Data and Methods

A. Active-duty Military Data

The Defense Manpower Data Center provided data on all individuals on active-duty between January 1, 1998 and early 2011. The data are drawn from two sources: (1) the active-duty personnel files, and (2) the loss files. The first source tracks individuals on active-duty as they transition through different enlistment periods in the military. An individual by year panel on over four million individuals was extracted by individuals at the Defense Manpower Data Center. Among other things, these data contain a large set of demographic variables, location information, military rank and occupation, dates of entry and reenlistment, and AFQT scores. Information on dates of separation and service characterization were linked to these data from a separate loss file.

B. Selecting a Sample to Match with National Student Clearinghouse Data

As described in the text, I limited my sample of active-duty individuals to those separating from active-duty between January 1, 2000 and December 31, 2009. I further restricted the sample to individuals separating between the ages of 22 and 39. Finally, I restricted the sample to individuals separating within ten years of initial entry into the military. Because the majority of enlisted individuals separate after their first term, and many more after their second, I eliminated those who appeared to stay in the military until retirement. This allowed me to focus my sample on those individuals most likely to take advantage of their GI Bill benefits. I took a stratified random sample of approximately 65,000 honorably discharged veterans in the remaining sample, stratifying on home of record, separation year, sex, and race (black). Finally, I took an analogous random sample of approximately 8,000 individuals separating with less than an honorable discharge. Using a SSN scrambling procedure, these individuals were linked to postsecondary data obtained from the National Student Clearinghouse without releasing personally identifiable information.

The National Student Clearinghouse is a non-profit organization that collects information on college enrollment and degree attainment at the national level. Initially created in 1993 as a provider of enrollment and degree attainment verification services, the NSC data is increasingly being used as a resource for post-secondary researchers. The NSC collects administrative enrollment and degree attainment information from participating colleges. As Dynarski et al. (2013) note, while the

enrollment coverage of the NSC data is currently over 90%, coverage has grown dramatically over the past 15 years. As I am investigating enrollment of veterans at different points in time, it is important to account for this change in coverage; to accomplish this, I am careful to restrict the set of schools in the NSC data to those reporting by a particular year. For example, if I am looking at enrollment of veterans separating between 2004 and 2009, I restrict the NSC data to colleges that begin reporting by 2004. This affects the interpretation of the results somewhat, but overcomes the larger issue that enrollment of individuals separating later will likely appear higher partially due to the larger set of schools covered by the NSC data. In contrast, degree information is nearly always provided with multiple decades of historical data; thus, institutions that have joined by the period of my sample match (August 2014) provide accurate degree information for all cohorts.[55]

C. "Pre-Processing" Methods

Ferraro and Miranda (2014) suggests that "pre-processing" the data to select a non-experimental comparison group results in superior (i.e., less biased) estimates. [56] A similar approach is used by Smith and Todd (2005). As a robustness check, I use a similar approach to select ineligible veterans that are similar to eligible veterans. In practice, this amounts to estimating the probability that each individual is eligible as a function of individual characteristics X_i. I then use nearest-neighbor matching (with replacement) to select ineligible veterans with similar estimated probabilities of being eligible as those that actually are eligible.[57] I implement this process separately in the pre- and post-period and then run the DD on the pre-processed data.[58]

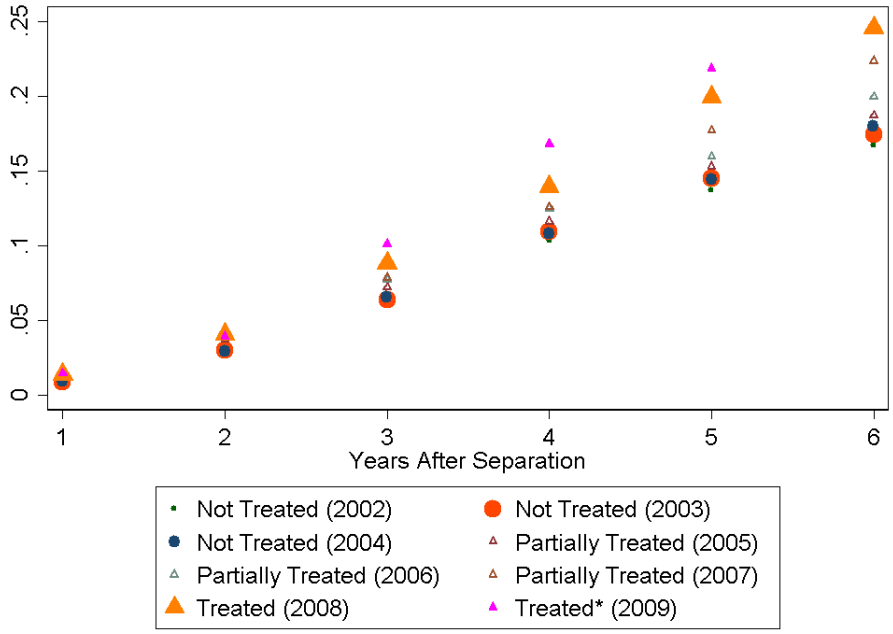
[55] An additional weakness of the NSC data is that the coverage rate differs by sector; while nearly all public school enrollment is tracked, only 50 percent of for-profit enrollment is covered. It is important to keep this in mind as one thinks about the possibility that some of the marginal veteran enrollment will accrue at schools that are not covered.

[56] The authors use a design-replication study, comparing estimates from a number of observational designs with those obtained from a randomized control trial. They find that approaches that pre-process the data to make the control group more similar to the treated group result in more accurate estimates of treatment effect sizes.

[57] There is substantial common support.

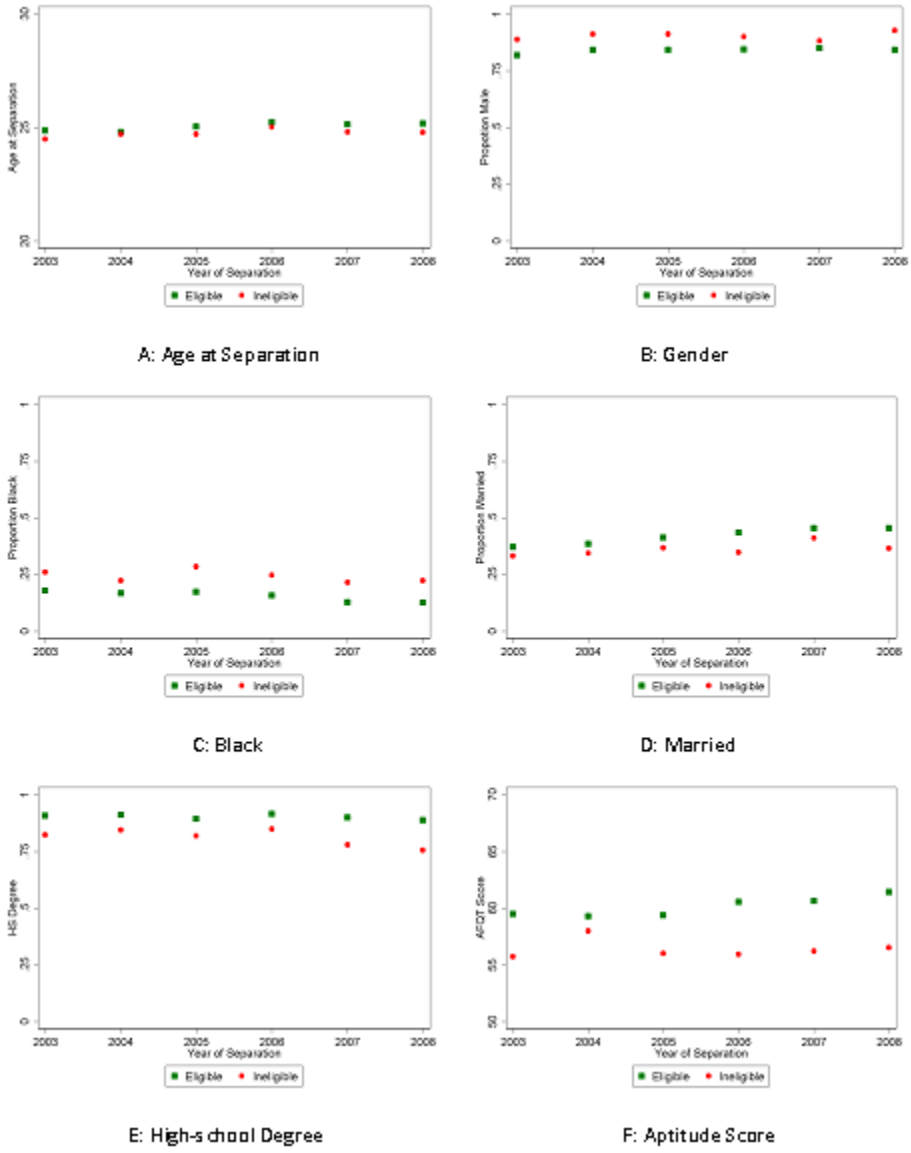
[58] Results are similar using multiple neighbors, a caliper, various restrictions to the support, or kernel density matching.

Figure A1: Attainment of any Degree by Separation Year (all cohorts)



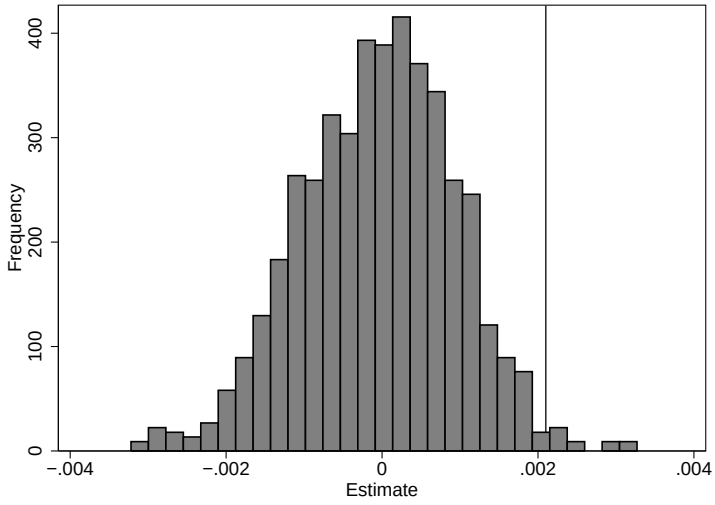
Note: Each marker represents the share of eligible veterans that obtained any degree by years after separation. * The 2009 separating cohort is treated, but endogenous separation from the military may have affected the composition of separating soldiers. See Table 3 or the text for details of the sample restrictions.

Figure A2: Group Composition over Time



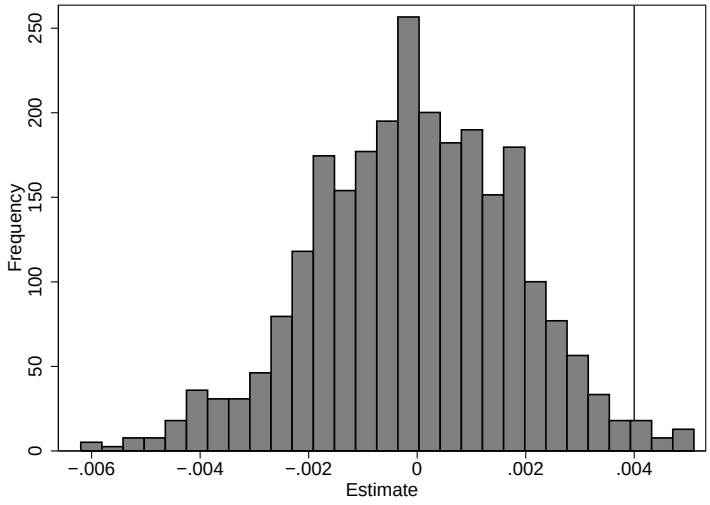
Note: Each panel plots average characteristics for eligible and ineligible veterans for the period between 2003 and 2008. See the text for further details.

Figure A3: Randomization Inference for Combined Maximum Estimates (Table 4)



Note: The figure plots the distribution of estimates when state maximum benefit levels are randomly reassigned. The vertical line indicates the estimate obtained under the true assignment of benefit levels to states. The figure plots estimates from 1,000 random reassignments. The specification follows that of column (1) in Table 4. See the text for further details

Figure A4: Randomization Inference for Simulated Benefit Estimates (Table 4)



Note: The figure plots the distribution of estimates when state simulated benefit levels are randomly reassigned. The vertical line indicates the estimate obtained under the true assignment of benefit levels to states. The figure plots estimates from 1,000 random reassignments. The specification follows that of column (1) in Table 4. See the text for further details.

Table A1: Post-9/11 GI Bill Benefit Maximums

STATE	Tuition per Credit	Housing Allowance (per month)	Estimate of Annual Combined
Alabama	$292	$1,010	$16,092
Alaska	$159	$1,900	$20,915
Arizona	$657	$1,243	$26,959
Arkansas	$200	$880	$12,734
California	$336	$1,877	$24,955
Colorado	$497	$1,335	$23,941
Connecticut	$516	$1,875	$29,261
Delaware	$356	$1,572	$22,688
District of Columbia	$198	$1,917	$22,003
Florida	$295	$1,471	$20,322
Georgia	$434	$1,103	$20,335
Hawaii	$282	$1,972	$24,517
Idaho	$259	$957	$14,828
Illinois	$575	$1,425	$26,624
Indiana	$322	$1,039	$17,075
Iowa	$324	$921	$16,073
Kansas	$394	$992	$18,380
Kentucky	$430	$931	$18,708
Louisiana	$430	$1,090	$20,131
Maine	$329	$1,170	$18,430
Maryland	$458	$1,721	$26,483
Massachusetts	$330	$1,838	$24,464
Michigan	$990	$1,129	$33,918
Minnesota	$750	$1,242	$29,176
Mississippi	$449	$995	$19,727
Missouri	$269	$1,030	$15,722
Montana	$205	$980	$13,746
Nebraska	$237	$982	$14,523
Nevada	$136	$1,289	$14,869
New Hampshire	$933	$1,507	$35,944
New Jersey	$451	$1,879	$27,723
New Mexico	$213	$1,044	$14,497
New York	$1,010	$2,076	$42,925
North Carolina	$494	$1,058	$21,376
North Dakota	$410	$933	$18,224
Ohio	$477	$1,023	$20,656
Oklahoma	$151	$918	$11,889
Oregon	$438	$1,164	$20,985
Pennsylvania	$886	$1,364	$33,540
Rhode Island	$343	$1,674	$23,296
South Carolina	$484	$1,076	$21,297
South Dakota	$93	$916	$10,484
Tennessee	$248	$1,041	$15,318
Texas	$1,471	$1,240	$46,460
Utah	$209	$1,119	$15,083
Vermont	$488	$1,416	$24,453
Virginia	$326	$1,358	$20,050
Washington	$380	$1,303	$20,845
West Virginia	$267	$924	$14,718
Wisconsin	$663	$1,082	$25,648
Wyoming	$94	$1,022	$11,450

Note: Tuition credit maximums and fee level maximums are for 2009-2010 obtained from `http://www.gibill.va.gov/gi_bill_info/ch33/tuition_and_fees_2009.htm`. Basic allowances for housing are population weighted state averages of zip code BAH levels for 2009. Estimate of annual combined maximum is equivalent to 9*BAH + 24*Credit Maximum.

Table A2: Degree Effects by Separation Cohort

VARIABLES	(1) Associates +	(2) BA +
Year=2001 * Eligible	-0.009	-0.000
	(0.011)	(0.010)
Year=2002 * Eligible	-0.004	0.000
	(0.015)	(0.012)
Year=2003 * Eligible	0.000	-0.001
	(0.012)	(0.011)
Year=2005 * Eligible	0.006	0.020*
	(0.014)	(0.011)
Year=2006 * Eligible	-0.000	0.010
	(0.016)	(0.013)
Year=2007 * Eligible	0.035***	0.022*
	(0.012)	(0.012)
Year=2008 * Eligible	0.046**	0.031**
	(0.018)	(0.014)
Year=2009 * Eligible	0.094***	0.064***
	(0.015)	(0.012)
Observations	46,013	46,013

Note: Table shows coefficients that compare degree attainment levels (wthin 5 years of separation) for eligible and ineligible veterans (omitted year is 2004). Each column presents the estimate from a single regression for a different dependent variable. Specification also includes indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, baseline degree level, state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. See Table 3 for additional sample restrictions. Robust standard errors clustered at the state level are in parentheses. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table A3: Preprocessing Data: Balance Comparison

VARIABLES	(1) Ineligible	(2) Eligible	(3) P-Value	(4) Matched Ineligible	(5) Matched Eligible	(6) P-Value
Pre						
Age at Separation	24.58	24.831	0.00	24.63	24.83	0.121
Black	0.245	0.173	0.00	0.165	0.174	0.620
Male	0.897	0.831	0.00	0.857	0.834	0.241
Married	0.337	0.379	0.00	0.342	0.382	0.115
AFQT Score	56.80	59.38	0.00	60.32	59.38	0.375
HS Degree at Sep.	0.831	0.909	0.00	0.915	0.912	0.783
Post						
Age at Separation	24.78	25.18	0.026	24.99	24.80	0.387
Black	0.223	0.126	0.00	0.199	0.125	0.778
Male	0.927	0.842	0.00	0.843	0.842	0.982
Married	0.366	0.454	0.00	0.413	0.453	0.380
AFQT Score	56.52	61.41	0.00	61.44	61.41	0.983
HS Degree at Sep.	0.756	0.887	0.00	0.907	0.887	0.249
Post - Pre						
Age at Separation	0.19	0.349	0.459	0.36	-0.03	0.961
Black	-0.022	-0.047	0.353	0.034	-0.049	0.926
Male	0.03	0.011	0.299	-0.014	0.008	0.657
Married	0.029	0.075	0.151	0.071	0.071	0.998
AFQT Score	0.28	2.03	0.062	1.12	2.03	0.625
HS Degree at Sep.	-0.075	-0.022	0.048	-0.008	-0.025	0.424

Note: Table compares means for eligible and ineligible veterans for the pre-period, the post-period, and the full period. Additionally, it shows the covariate balance after pre-processing the data. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table A4: Degree Effects Using Geographic Variation (6 Years) Benefit Heterogeneity

VARIABLES	(2) Associates +	(3) BA +
Post 9/11 * Tuition Max.	0.0021***	0.0015**
	(0.0007)	(0.0005)
Post 9/11 * Housing Allow.	0.0013	0.0022
	(0.0021)	(0.0020)
Observations	14,222	14,222
Mean	0.201	0.131

Note: Table shows coefficients from regressions of degree attainment levels on the size of each Post-9/11 GI Bill benefit component interacted with whether the individual separated between August 1, 2007 and July 31, 2008 (the post-period). Each column presents the estimate from a single regression. Specification also includes indicator variables for sex, black, marital status, high-school degree at separation, and month of year of separation as well as AFQT scores, controls for age of separation, baseline degree level, year and state fixed effects, and the average unemployment calculated for an individual's home of record during the twelve months after separation. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans aged 22 to 39 with less than a bachelor's degree who separated between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period) and listed a U.S. state as a home of record. Significance levels indicated by: * ($p<0.10$) ** ($p<0.05$), *** ($p<0.01$)..

Table A5: Falsification Exercise: Relationship Between Benefit Maximum and Enrollment Factors

VARIABLES	(1) Log Appropriations	(2) Unemployment Rate
Trend * Combined Max.	-0.00082	0.00232
	(0.00050)	(0.00339)
Observations	528	561

Note: Table shows coefficients from regressions of indicated outcomes on the interaction of the size of the combined Post-9/11 GI Bill benefit and a year trend. Each column presents the estimate from a single regression with state by year observations. Appropriations regressions are missing Hawaii, Alaska, and the District of Columbia due to data limitations (Appropriations obtained from Grapevine reports produced by Illinois State University's Center for Education Policy and include ARRA stimulus funds). Sample restricted to years 2003-2013 for unemployment rates and school-years 2003-04 through 2013-14 for state appropriations to higher education. Robust standard errors clustered at the state level are in parentheses. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table A6: Falsification Exercise: Non-Veteran Effects Using Geographic Variation

	(1)	(2)	(3)	(4)
Any Degree				
Trend * Combined Max.	-0.00009	-0.00006	-0.00009	-0.00006
	(0.00006)	(0.00007)	(0.00007)	(0.00008)
Observations	3,763,474	3,303,427	2,610,674	2,291,510
Mean	0.421	0.421	0.429	0.429
Enroll				
Trend * Combined Max.	-0.00006***	-0.00005**	-0.00005***	-0.00005**
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Observations	3,763,474	3,303,427	2,610,674	2,291,510
Mean	0.113	0.113	0.0816	0.0816
Age: 24-39	X	X		
Age: 29-39			X	X
Ed Rest: $\geq$ HS		X		X

Note: Table shows coefficients from regressions of indicated outcomes on the interaction of the size of the combined Post-9/11 GI Bill benefit and a year trend. Each column presents the estimate from a single regression. Sample restricted to individuals without military service in ACS sample years 2006-2012. Robust standard errors clustered at the state level are in parentheses. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Appendix B: Estimating Cost of a Marginal Degree

This appendix supports the bounds for the cost of each marginal degree presented in the Conclusion. The cost estimates presented here refer only to the direct costs (i.e., additional financial aid paid to veterans) and ignore any indirect costs such as state subsidies to education, foregone wages, or deadweight loss to taxation.

The goal is to estimate the increase in benefits paid out and divide this figure by the increase in the number of degrees obtained. To fix ideas, I think about this problem in terms of 100 individuals separating from the military in the pre and post periods. In the pre period, roughly 44 of these individuals chose to enroll in college. From the DD enrollment estimates, roughly 7.7 additional individuals enrolled due to the higher level of benefits. From the DD attainment estimates, roughly 6 additional individuals obtained a degree due to the higher benefit levels. As discussed in the text, this increased degree attainment is generated by an increase in enrollment and an increase in persistence of inframarginal enrollees. In estimating additional costs, I choose an upper (70%) and lower (30%) estimate for the share of marginal enrollees who complete a degree. Under the higher assumption, I am assuming that more of the overall degree attainment effect is driven by new (marginal) enrollees. As these individuals were previously receiving no benefits, this will imply a larger cost increase.

The increase in costs is a function of more individuals enrolling and higher benefit levels for those that would have enrolled anyway. I assume that the pre-period benefits were roughly $11,000 per year and present estimates for three choices of the change in benefit levels ($2,500, $5,000, and $7,500), which imply post-period annual benefit averages of $13,500, $16,000, and $18,500, respectively.

Table B1 provides estimates of the cost per additional degree by decomposing the change in costs into three components: (1) the higher cost generated by marginal enrollees, (2) the higher cost generated by inframarginal enrollees whose educational attainment changes, and (3) the higher cost generated by inframarginal enrollees whose educational attainment levels are unaffected.

For marginal enrollees, the increase in costs is a function of how long they enroll multiplied by the average post-period benefit level. From the DD attainment estimates, I assume that 25 percent of the marginal degree attainment is in the form of associate's degrees and the other 75 percent is in the form of bachelor's degrees. I assume that obtaining an associate's degree takes two years of benefits, while a BA degree takes four. Finally, I assume that those who enroll and do not obtain a degree do so for an average of one year. Thus, assuming a $2,500 benefit increase and 70% degree completion, the increase in cost through this channel is $5.4 * 0.75 * 4 * \$13,500 + 5.4 * .25 * 2 * \$13,500 + (7.7 - 5.4) * 1 * \$13,500 \approx 286,000$.

I assume that the remaining increase in degree completion (6 - amount coming through marginal enrollment channel) is accounted for by increased degree attainment of inframarginal enrollees. I assume the same split in degree attainment. Here, I assume that those now attaining an associate's degree are receiving the higher level of benefits (Δ Benefit) for one year and the average post-period benefit levels for a second year. Similarly, those now attaining bachelor's degrees are receiving the higher level of benefits for two years and the average post-period benefit levels for an additional two years.[59]

Finally, there is an increase in costs for inframarginal enrollees who do not respond to the

[59] I am essentially assuming that, on average, marginal associate's recipients moved from one to two years of benefit usage and marginal bachelor's recipients moved from two to four.

higher level of benefits. I assume that these individuals are attending for 1.5 years on average and multiply this by the post-period average benefit levels to get a measure of the change in costs.

I sum the increase in costs coming through each of these channels and divide by the additional 6 degrees per 100 generated by the benefit increase in order to produce a back of the envelope calculation of the cost per degree. In the final row of Table B1, I present a range of cost per degree estimates under various assumptions on the size of the change in benefits and the share of marginal enrollees who will complete a degree.

These estimates abstract away from at least two important considerations. First, inefficient switching to the new benefit implies that the cost per degree may be substantially overestimated because the assumptions underlying Table B1 assume that all individuals switch to the new benefit. Second, I am ignoring the possibility that veteran aid crowds out other forms of financial aid (e.g. institutional or state aid) which is very likely. This omission will also lead to overestimates of the cost per degree.

Table B1: Cost Estimates

	Δ Benefit =$2,500		Δ Benefit=$5,000		Δ Benefit=$7,500	
Marginal Enrollees						
Share Completes Degree (assumption)	70%	30%	70%	30%	70%	30%
Implied $\Delta Degrees$	5.4	2.3	5.4	2.3	5.4	2.3
Implied $\Delta Cost$	$285,862	$181,912	$338,800	$215,600	$391,738	$249,288
Inframarginal Enrollees (Δ Attainment)						
Implied $\Delta Degrees$	0.6	3.7	0.6	3.7	0.6	3.7
Implied $\Delta Cost$	$17,080	$103,320	$22,417	$135,608	$27,755	$167,895
Inframarginal Enrollees (No Δ Attainment)						
Implied $\Delta Cost$	$161,213	$149,662	$322,425	$299,325	$483,638	$448,988
Total Implied Degrees	6	6	6	6	6	
Total Implied Cost	$464,155	$434,895	$683,643	$650,533	$903,130	$866,170
Implied Cost per Degree	$77,359	$72,483	$113,940	$108,422	$150,522	$144,362

Appendix C: Cost of Living Adjustment, Persistence, and Benefit Uptake

8.1 Cost of living

As the new GI Bill itself illustrates, a dollar may not be worth the same amount everywhere in the country. While this variation in the real value of financial aid exists for all financial aid programs available countrywide, it is an issue that few people think much about *unless* the variation being leveraged is explicitly based on variation in cost (as it is in this case). The Post-9/11 GI Bill attempts to provide a roughly equivalent real benefit level across geography. Therefore, if cost of living were perfectly adjusted for, the size of the real Post-9/11 GI Bill benefit would be the same across geography. Under this set of assumptions, while the nominal benefit was the same under the MGIB, the real benefit was lower in high-cost states. Under either set of assumptions the key intuition is the same: the relative increase in the size of the real benefit (or put another way, the reduction in the cost of college) was larger in states with higher nominal Post-9/11 GI Bill benefits. The purchasing power parity (PPP) estimates thus present a slightly different parameter.

Table C1 presents estimates using combined benefit levels adjusted for the local cost of living. Recall that an equivalent-dollars benefit increase in a high cost-of-living state may not be as large as in a low cost-of-living state. The estimates presented here are scaled to present the effect of an additional $1,000 provided in a median cost-of-living area.[60] The effect sizes are slightly smaller than, but statistically indistinguishable from, the unadjusted estimates in 4. Tables A4 and C2 allow the effect on degree attainment to vary by benefit type. Table A4 presents the effects of the unadjusted benefit levels, suggesting that the benefits generate statistically indistinguishable effects on attainment, yet only the tuition benefit is statistically distinguishable from zero. Table C2 adjusts each benefit by cost of living; while the effects remain statistically indistinguishable, the estimates suggest that increases in the cash portion of the benefit may have larger effects on attainment.

8.2 Persistence and Benefit Uptake

As part of an overall effort to understand the effects of aid on degree attainment, I also examine the direct effect on persistence; that is, I attempt to isolate the effect of aid on an individual's decision to stay in college conditional on that individual already being enrolled. I define persistence as enrollment at time $t + 2$ given enrollment at time t, where t is measured in semesters. In examining persistence, I can assign a much more precise guess of the actual increase in benefit levels experienced by individuals over time because I observe the actual schools attended. Unlike the MGIB, the benefit levels for the Post-9/11 GI Bill are directly related to the cost of attendance at a school. Thus, those enrolled during the fall of 2008 would experience very different benefit increases when Post-9/11 GI Bill benefits became available for the fall of 2009. To better illustrate the identification strategy, I will focus briefly on the variation generated by the housing allowance level provided at each school.

Consider two community colleges from the same state with roughly the same $5,000 yearly tuition level. College A is located in a rural area and has an associated housing allowance of $981 per month. College B is located outside of a major metropolitan area and has an associated housing

[60] To operationalize this, the size of the benefit expansion in each state is divided by the average housing cost (BAH) in each state and then multiplied by the median cost of housing in the country.

allowance of $2,085 per month. Under the MGIB, a veteran received roughly $12,000 for each nine-month school year at either college. At college A, this amount has grown to $14,000 under the new GI Bill. At college B, the benefit is now $24,000. Merely by choosing to enroll in a more expensive area, the veteran in college B receives an additional ten thousand dollars in cash under the new GI Bill.[61] I leverage this variation in aid by comparing the persistence rates of veterans enrolled at different institutions in the fall of 2008 with the persistence rates of veterans in prior years, controlling for student characteristics, school-fixed effects, school characteristics interacted with year fixed effects, and county-year unemployment rates.[62] The basic specification is as follows:

$$E_{ict} = \beta_1 X_i + \beta_2 Z_{ct} + \lambda_t + \beta_3 C_c * \lambda_t + \alpha_c + \gamma Post911_t * Benefit_c + \epsilon_{ict} \quad (3)$$

Here, I restrict the sample to individuals enrolled during the fall of 2007 or 2008. The enrollment of these individuals is unlikely to have been affected by the benefit expansion because the bill only became law on June 30, 2008, and was not implemented until August 1, 2009.[63] Individual characteristics X_i control for variation in the likelihood of persistence due to observable veteran differences, and county-year unemployment rates Z_{ct} control for changes in local labor market conditions near college c that may affect the decision to persist. Year fixed effects λ_t control for changes over time in the likelihood of persistence, and school-fixed effects α_c control for fixed characteristics of schools that affect persistence. Because the persistence of individuals at different types of schools may be affected differently by changing conditions over time, I interact school graduation rates C_c with the year fixed effects λ_t. I identify the effect of additional aid by interacting $Post911$ with the size of the benefit $Benefit_c$ inferred for an individual enrolled full-time at a particular school.[64,65]

I infer the school-specific benefit level under the Post-9/11 GI Bill using information on in-state tuition and fees at the school, as well as the housing allowance assigned to the zip code in which the school resides. For community colleges and public schools, this means adding the in-state tuition and fee levels to the annual housing allowance amount assigned to the school. For private schools, tuition benefits are capped at the state maximum.[66] Table C3 presents estimates of the effect of an additional $1,000 in combined benefits on the likelihood of being enrolled the following fall (i.e., persisting).[67] Individuals are 0.5 percentage points more likely to persist for every $1,000 of additional aid. Effect sizes are similar when including school-fixed effects. Table C4 allows each benefit type to independently affect veteran persistence. While statistically indistinguishable from

[61] Again, one can argue that these differences should be scaled by the cost-of-living differences in the two areas. Results using this approach are similar.

[62] The unemployment rate is the average unemployment rate during the year following initial fall enrollment. For example, the unemployment rate for individuals initially observed enrolled during the fall of 2008 is the 2009 unemployment rate.

[63] As discussed in Barr (2015), the details of the benefit expansion were not well defined or publicized until the beginning of 2009 at the earliest.

[64] If the distribution of part-time enrollment is uniform across schools, this will bias my estimates downward somewhat.

[65] For part-time students, the benefit increase will be somewhat smaller, implying that the estimates will be biased towards zero somewhat. Statistics from the NSC data, where enrollment intensity is available, suggest that just under 70% of eligible veterans were enrolled full-time during the fall of 2007 and 2008, and that over 92% were enrolled half-time or more.

[66] These assumptions ignore variation in benefit levels potentially generated by out-of-state student status.

[67] If individuals are observed obtaining a non-certificate degree during a semester after they are initially observed enrolled, it is counted as though they persisted to the following fall.

the tuition component, the housing allowance benefit appears to have a much stronger effect on persistence, nearly 0.6 percentage points per $1,000 of aid. This effect may be a result of the greater salience of a cash benefit or a result of the tuition benefit crowding out other forms of aid (e.g., institutional or state grants).

While the samples of veterans are somewhat different for the attainment and persistence analyses, these results can be used to obtain a rough estimate of the extent to which the overall attainment results are driven by increased persistence of already-enrolled individuals. If all of the year-to-year increases in persistence translated into degree attainment, we would expect to find an overall increase in degree attainment of close to (5*0.005*0.45), or 1.25 percentage points. This is admittedly unrealistic because many veterans who are drawn to enroll for another year will still drop out prior to obtaining a degree. However, assuming that 50 percent will eventually receive a degree suggests that degree attainment would still increase by close to 0.6 percentage points through the increased completion channel.[68] Combining this with the rough calculations on the contribution of additional enrollment to degree attainment (3 percentage points) reveals a gap between the sum of the effects of new enrollment and increased persistence, 4 percentage points, and the overall effect on degree attainment, 6 percentage points.

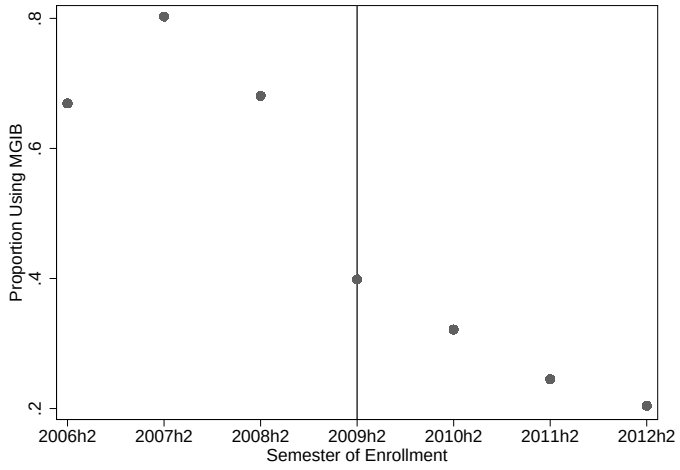
A potential explanation for the observed gap in persistence is that the persistence effects estimated using individuals initially enrolled during the fall of 2008 may be lower than the effect on persistence of individuals separating during 2008 but enrolling for the first time when the Post-9/11 GI Bill benefits were available in 2009. Another explanation is that many veterans who were already enrolled in 2008 simply did not switch to the new benefit program.[69] Figure C1, which plots the share of enrollees receiving MGIB benefits during the fall semester between 2006 and 2012, indicates that this was likely the case. Although nearly all veterans were likely to receive greater benefits under the Post-9/11 GI Bill, forty percent of enrolled veterans were still using the MGIB during the first semester of implementation (fall 2009). Further analyses show that most of the veterans who continued to use the MGIB benefit left thousands of dollars unclaimed.[70]

[68] Recall that slightly fewer than half of veterans enroll in college, so assuming a degree completion rate of 50 percent, an increase in persistence of .025 will translate into separating veterans being $0.45 * 0.5 * .025 \approx 0.006$ more likely to obtain a degree.

[69] While veterans who had already separated were not directly informed of the new benefit, there were marketing campaigns aimed at promoting Post-9/11 GI Bill usage. Switching to the new benefit required filling out a form estimated to take less than one hour.

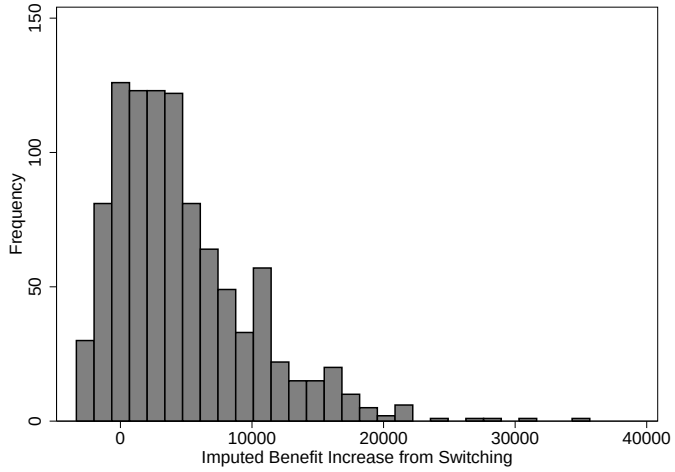
[70] By examining the change in MGIB usage of veterans enrolled and using the MGIB during the fall of 2008, I can estimate the distribution of foregone benefits, which may be interpreted as a lower bound on switching costs, that is implied by those who choose not to switch. Figure C2 plots the distribution of implied switching costs for veterans who were enrolled in and using their MGIB benefits during the fall of 2008, and who remained enrolled and using their MGIB benefits during the fall of 2009. While a small fraction of these veterans would have been worse off under the Post-9/11 GI Bill, many veterans who continued to use the MGIB benefit left thousands of dollars unclaimed. The question of why such a large share of eligible veterans did not switch remains unsettled. Conversations with VA officials suggests that the rapid rollout of the new benefit made it quite difficult for anyone, including veterans, to understand the change, calculate eligibility, or transition to the new benefit. For example, in 2009, individuals had to visit between 14 and 22 separate web pages to determine eligibility and benefit amounts under the Post-9/11 GI Bill.

Figure C1: MGIB Usage of Enrollees Over Time



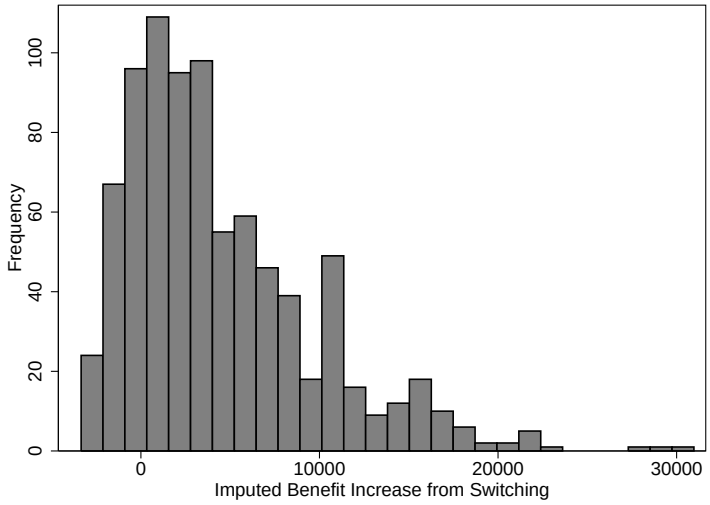
Note: The figure plots the share of enrolled veterans receiving MGIB benefits by semester of enrollment (e.g., 2009h2 is fall enrollment for 2009). The vertical line indicates the first semester that Post-9/11 GI Bill benefits were available.

Figure C2: Implied Cost of Switching for Those Not Switching to the New GI Bill



Note: The figure plots the distribution of implied foregone benefits for veterans enrolled and receiving MGIB benefits during the fall of 2008 who were still enrolled and receiving MGIB benefits during the fall of 2009. See the text for further details.

Figure C3: Implied Cost of Switching for Those Not Switching to the New GI Bill (same school)



Note: The figure plots the distribution of implied foregone benefits for veterans enrolled and receiving MGIB benefits during the fall of 2008 that were still enrolled and receiving MGIB benefits during the fall of 2009. Unlike Figure C2, this restricts the set of veterans to those enrolled at the same institution the following year. See the text for further details.

Table C1: Degree Effects Using PPP Geographic Variation (6 Years)

VARIABLES	(2) Associates +	(3) BA +
Post 9/11 * PPP Combined Max.	0.002**	0.001**
	(0.001)	(0.001)
Observations	14,222	14,222
Mean	0.201	0.131

Note: Table shows coefficients from regressions of degree attainment levels on the size of the annual estimate of the combined Post-9/11 GI Bill benefit adjusted to purchasing power parity (see text) interacted with whether the individual separated between August 1, 2007 and July 31, 2008 (the post-period). Each column presents the estimate from a single regression. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans separating between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period). Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table C2: Degree Effects Using PPP Geographic Variation (6 Years)Benefit Heterogeneity

VARIABLES	(2) Associates +	(3) BA +
Post 9/11 * PPP Tuition Max.	0.002**	0.001**
	(0.001)	(0.000)
Post 9/11 * PPP Housing Allow.	0.004	0.005
	(0.003)	(0.003)
Observations	14,222	14,222
Mean	0.201	0.131

Note: Table shows coefficients from regressions of degree attainment levels on the size of each Post-9/11 GI Bill benefit component adjusted to purchasing power parity (see text) interacted with whether the individual separated between August 1, 2007 and July 31, 2008 (the post-period). Each column presents the estimate from a single regression. Robust standard errors clustered at the state level are in parentheses. The sample is restricted to veterans separating between August 1, 2002 and July 31, 2004 (the pre-period) or between August 1, 2007 and July 31, 2008 (the post-period). Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table C3: Persistence Using School Specific Benefit Expansion

VARIABLES	(1) Enrolled $(t+2)$	(2) Enrolled $(t+2)$
School Combined	0.005**	0.004*
	(0.002)	(0.003)
School FE		X
Observations	10,321	10,321
Mean	0.665	0.665

Note: Table shows coefficients from regressions of enrollment during the following fall $(t + 2)$ given enrollment during a particular fall semester (t). Coefficients presented for combined estimate of benefits available during fall of 2009 (in thousands). Each column presents the estimate from a single regression. Specifications also include indicator variables for sex, black, marital status, and high-school degree at separation as well as AFQT scores, controls for age of separation, year and school fixed effects, year fixed effects interacted with a college's graduation rate, and county by year unemployment rates. Robust standard errors clustered at the school level are in parentheses. The sample is restricted to veterans enrolled in the fall of 2007 or 2008. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table C4: Persistence Using School Specific Benefit Expansion Benefit Heterogeneity

VARIABLES	(1) Enrolled $(t+2)$	(2) Enrolled $(t+2)$
School Housing Allow.	0.006**	0.007**
	(0.003)	(0.003)
School Tuition	0.003	0.000
	(0.004)	(0.004)
School FE		X
Observations	10,321	10,321
Mean	0.665	0.665

Note: Table shows coefficients from regressions of enrollment during the following fall $(t+2)$ given enrollment during a particular fall semester (t). Coefficients presented for the effect of tuition and housing allowance benefit components (in thousands) available in 2009. Each column presents the estimate from a single regression. Specifications also include indicator variables for sex, black, marital status, and high-school degree at separation as well as AFQT scores, controls for age of separation, year and school fixed effects, year fixed effects interacted with a college's graduation rate, and county by year unemployment rates. Robust standard errors clustered at the school level are in parentheses. The sample is restricted to veterans enrolled in the fall of 2007 or 2008. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table C5: Persistence Using School Specific Benefit Expansion by School Type

VARIABLES	(1) Enrolled $(t+2)$	(2) Enrolled $(t+2)$
Public	0.007***	0.007**
	(0.003)	(0.003)
Observations	8,321	8,321
Mean	0.669	0.669
Four-Year	0.007	0.006
	(0.004)	(0.005)
Observations	3,273	3,273
Mean	0.717	0.717
Two-Year	0.008**	0.008**
	(0.004)	(0.004)
Observations	5,048	5,048
Mean	0.638	0.638
Private NP	0.007	0.002
	(0.005)	(0.006)
Observations	753	753
Mean	0.649	0.649
For-profit	0.001	-0.000
	(0.007)	(0.007)
Observations	1,246	1,246
Mean	0.647	0.647
School FE		X

Note: Table shows coefficients from regressions of enrollment during the following fall $(t+2)$ given enrollment during a particular fall semester (t). Coefficients presented for the effect of tuition and housing allowance benefit components (in thousands) available in 2009. Each column presents the estimate from a single regression. Specifications also include indicator variables for sex, black, marital status, and high-school degree at separation as well as AFQT scores, controls for age of separation, year and school fixed effects, year fixed effects interacted with a college's graduation rate, and county by year unemployment rates. Robust standard errors clustered at the school level are in parentheses. The sample is restricted to veterans enrolled in the fall of 2007 or 2008. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table C6: Persistence Using School Specific Benefit Expansion by School and Benefit Type

VARIABLES	(1) Enrolled $(t+2)$	(2) Enrolled $(t+2)$
Public		
School Housing Allow.	0.008***	0.009***
	(0.003)	(0.003)
School Tuition	0.004	0.002
	(0.005)	(0.006)
Observations	8,321	8,321
Mean	0.669	0.669
Four-year		
School Housing Allow.	0.008*	0.007
	(0.004)	(0.005)
School Tuition	0.013*	0.012
	(0.007)	(0.008)
Observations	3,273	3,273
Mean	0.717	0.717
Two-year		
School Housing Allow.	0.008**	0.010**
	(0.004)	(0.004)
School Tuition	-0.005	-0.008
	(0.008)	(0.009)
Observations	5,048	5,048
Mean	0.638	0.638
School FE		X

Note: Table shows coefficients from regressions of enrollment during the following fall $(t+2)$ given enrollment during a particular fall semester (t). Coefficients presented for the effect of tuition and housing allowance benefit components (in thousands) available in 2009. Each column presents the estimate from a single regression. Robust standard errors clustered at the school level are in parentheses. The sample is restricted to veterans enrolled in the fall of 2007 or 2008. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).