Working Paper:

From the Battlefield to the Schoolyard: The Short-term Impact of the Post-9/11 GI Bill

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The Post-9/11 GI Bill brought about an enormous expansion in veteran education benefits, roughly doubling the average maximum benefit level and generating large variation in the magnitude of benefit expansion across states. Using this context, I explore how enrollment of older, non-traditional students responds to educational subsidies. The transition from a conditional cash transfer to a more in-kind benefit affects the relative prices of different types of education, pushing veterans to invest in more expensive (plausibly higher quality) schooling. Using a difference-in-differences strategy, combined with state level variation in the degree of benefit expansion, I find that the higher level of benefits increased college enrollment of separated veterans by between fifteen and twenty percent, while also shifting the composition of enrollment towards four-year schools.

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FROM THE BATTLEFIELD TO THE SCHOOLYARD: THE SHORT-TERM IMPACT OF THE POST-9/11 GI BILL

Andrew Barr

I. Introduction

The Post-9/11 Veterans Educational Assistance Act of 2008 (Post-9/11 GI Bill) brought about the largest expansion in veteran education benefits since the end of World War II, roughly doubling the average maximum benefit level. The size of the benefit expansion makes it one of the largest increases in financial aid in decades: benefits pay for in-state tuition, fees, a monthly housing allowance, and a generous stipend for books and supplies.[1] While the fraction of individuals that join the military has dropped substantially over the last 60 years, it remains an important component of the population with over 5 percent (and nearly 9 percent of males) having served in the military before turning thirty. Furthermore, the likelihood of college enrollment for returning veterans is much higher today than after WWII; while roughly 2.2 million of over 16 million WWII veterans attended college during the twelve year duration of the original GI Bill, more than 600,000 of roughly two million recent veterans were enrolled in college during the 2010-2011 school year alone.

Previous studies of federal financial aid programs find mixed evidence of the effectiveness of education subsidies in increasing enrollment and largely focus on students considering enrollment more than two decades ago (Hansen 1983, Kane 1995, Seftor and Turner 2002, Dynarski 2003). This study brings new evidence to understanding the effects of financial aid and, unlike most previous studies, how financial aid impacts the choices of older, non-traditional students.[2] These individuals, who are frequently from low-income backgrounds and among the first in their families to seriously consider college, are likely those most at risk to underinvest in human capital due to credit or information constraints (Barr 2013). I address two fundamental questions: (1) how financial aid impacts the enrollment and persistence of these individuals and, (2) whether or not individuals' school choices respond to a change in the benefit

[1]In a comprehensive overview of financial aid studies, not a single program provided benefits equal to or greater than those provided by the Post-9/11 GI Bill, and most provided substantially less (Deming and Dynarski 2010).

[2]Veterans and non-traditional students are similar across a number of characteristics including age, race, motivations for attending a particular institution, institution type, and major; veterans are somewhat more likely to attend BA granting institutions (42 vs. 32 percent) and more likely to major in business or engineering. Source: Military Service Members and Veterans: A Profile of those Enrolled in Undergraduate and Graduate Education in 2007-2008 (NCES 2011-163).

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structure from a pure conditional cash transfer to a more in-kind benefit.

In contrast to other research that addresses the effect of financial aid, I am able to use both over time and geographic variation in the level of financial aid to answer these questions. Unique among the GI Bills and other federal aid programs, the Post-9/11 GI Bill benefits account for geographic differences in the cost of living and college attendance. This feature created variation in the size of the benefit increase across states: veterans in some states received almost no change in benefit levels, while others received tens of thousands of dollars in additional benefits per year.

I use a difference-in-differences strategy to estimate the impact of the benefit change on veteran educational choices. Trends in non-veteran enrollment proxy for the trend that veteran enrollment would have experienced absent the benefit expansion. I confirm, through a number of robustness checks, event study analyses, and falsification exercises, that veteran enrollment trends tracked non-veteran trends leading up to the policy change. Concern remains, however, that veteran enrollment may respond differently to the Great Recession. While I find no evidence that veteran enrollment is more responsive to labor market contractions, I also leverage cross-state variation in the degree of benefit expansion to examine the effect of changing benefit levels, mitigating these types of concerns. This geographic variation in covered tuition and housing allowance levels allows for a triple-difference type strategy.

While I spend a considerable amount of time addressing concerns related to the potential endogeneity of veteran status later in the paper, I note two characteristics of the bill's passage that mitigate concerns with selection into the military here. First, the Post-9/11 GI Bill was passed and implemented in roughly 13 months; therefore, the specifics of the bill were not clear until the spring before the implementation. This left little time for selection into the military to adjust to higher benefit levels.[3] Second, the Post-9/11 GI Bill benefit eligibility criteria, standard contract lengths, and enlistment delays make it unlikely that individuals who enlisted after the bill was announced could have returned and enrolled prior to the end of my sample period.

I find that the expansion of benefits increased overall college enrollment of individuals with active-duty military service by just under two percentage points. The college enrollment of separated veterans increased by four to six percentage points (a fifteen to twenty percent increase). The effects appear to be somewhat larger for males. The enrollment impacts are larger in states that experienced larger benefit expansions. Overall, the results suggest an increase of at most one percentage point per $1,000 increase in aid, somewhat smaller than that found by prior

[3]Figure 2 illustrates that there was little awareness of changes to veterans' benefits until late 2008 at the earliest, and interest remained low until the bill's implementation.

researchers. I argue that substantially higher initial levels of aid explain this disparity.

Furthermore, as predicted by the model, the change in the relative prices of different types of education resulted in more veterans enrolling in, and transferring to, four-year institutions. Finally, I present suggestive evidence that the benefit expansion has increased the persistence of veterans.

In the next section, I provide a brief overview of the financial aid literature. In Section 3, I describe the Post-9/11 GI Bill and the GI Bill that it replaced. In Section 4, I develop a model of human capital investment to illustrate the changing incentives faced by veterans as the Post-9/11 GI Bill went into effect. Section 5 introduces the relevant data, while Section 6 lays out the estimation strategy used to test the implications of the model. Section 7 presents the results and Section 8 concludes.

II. Veterans and Education Subsidies

I build on previous research on financial aid in examining the understudied older population and focusing on a policy change that affects much more recent cohorts of individuals. Although early studies of federal need-based programs found small and mixed results of Pell grants on college attendance (Hansen 1983, Kane 1995), a growing consensus points to a three to six percentage point increase in enrollment in response to $1,000 in grant aid (Deming and Dynarski 2010). Dynarski (2003) utilizes the elimination of a program that paid the college costs of children of disabled, deceased, or retired Social Security beneficiaries to estimate the effect of aid on enrollment. While the estimates use a credible difference-in-differences strategy, they focus on traditional-aged students with a very particular set of circumstances. Seftor and Turner (2002) present some of the only evidence on nontraditional students, finding a four to five percentage point effect of Pell eligibility on the enrollment of older students. The authors use a difference-in-differences strategy to leverage a change in the definition of independent students in 1986. They identify the effect of a change in financial aid on individuals who would have been eligible as independent students had they been born a few years earlier.

Studies of earlier GI Bills, including the World War II and Vietnam GI Bills, focus on a more similar set of individuals to those in this study, but they combine the effect of compulsory military service and aid availability (Bound and Turner 2002, Stanley 2003, Card and Lemieux 2001, Angrist 1990, Angrist and Chen 2011). While the studies all demonstrate positive effects on college enrollment or attainment, they are not truly measures of the effect of financial aid as military service is part of the treatment. Furthermore, all focus on the behavior of individuals before 1990; given large changes in the higher education landscape, it is unclear if their results

are applicable to more recent cohorts of students.

There have been only a handful of analyses of the educational benefit use of veterans enlisting after 1973. Angrist (1993) presents suggestive evidence of increased benefit usage in response to higher benefit levels using a small group of veterans who participated in the 1987 Survey of Veterans. Simon, Negrusa, and Warner (2010) is the only study of which I am aware to focus on veterans separating from the armed forces after 1990. Using plausibly unanticipated changes in the level of education benefits provided by the Montgomery GI Bill during the 1990s, they explore the impact of those changes on veteran benefit usage, finding a half percentage point increase in benefit usage per $1,000 of additional benefits, substantially smaller than much of the earlier literature. However, the absence of a control group forces the authors to rely on the assumption that an extensive set of explanatory variables adequately controls for other changes occurring over this period.

While addressing a more dissimilar group, relevant recent evidence is provided by studies of state merit-aid programs. While the extent of the benefits provided by merit-aid programs is probably the closest to the generous benefits provided by the Post-9/11 GI Bill, the population of treated students is quite different. Overall, estimates of the effects of the merit-aid programs are mixed, but initial enrollment estimates suggest a four to six percentage point increase per $1,000 change in cost (Dynarski 2000, 2008; Cornwell, Mustard, and Sridhar 2006; Fitzpatrick and Jones 2012; Sjoquist and Winters 2012). Additional studies of a tuition assistance program in Washington DC find similar effects, but also focus on traditional students (Kane 2004, Abraham and Clark 2006).[4] In sum, there is little evidence on the effectiveness of financial aid for older students, and the information that is available is dated. This paper focuses on this understudied population, and does so using variation between groups, over time, and across states to identify the effects of these additional benefits. Furthermore, the policy variation allows for an examination of the effect of increasing aid starting from a relatively generous level and of the alternative effects of cash versus in-kind financial aid. In the next two sections, I outline the relevant features of the new GI Bill, how it differs from the Montgomery GI Bill that it replaced, and how we might expect these changes to affect veterans' educational choices.

III. Montgomery to the Post-9/11 GI Bill

The Montgomery G.I. Bill (MGIB) went into effect in 1985 and remained an option for entering active duty personnel until August of 2009. Eligibility for the MGIB depended largely

[4]See Deming and Dynarski (2010) or Dynarski and Scott-Clayton (2013) for more complete reviews of the financial aid literature.

on three factors. First, an individual had to elect to participate in the program or not by committing to a reduction of pay of $100 for each of the first twelve months on active duty.[5] Second, an individual had to complete the minimum active duty contract agreed to upon enlistment - which was generally at least three years. Third, an individual could not have been commissioned through a military academy or have received above a certain level of ROTC scholarship funds.

The benefits, which are paid directly to an individual, have been raised periodically since 1985 and pay a different flat amount per month to individuals enrolled half-time, three-quarters time, or full-time.[6] In 2009, a full-time student who had completed three or more years of active-duty service received $1,368 per month. Veterans are eligible for up to 36 months of benefits and can use the benefits on a wide variety of training programs including vocational training, apprenticeships, flight classes, and test fees as well as formal training leading to a degree.

Following September 11, 2001 and the ensuing wars in Afghanistan and Iraq, discussions began about expanding education benefits for returning veterans. During the summer of 2008, Congress approved the Veterans Educational Assistance Act of 2008, more commonly known as the Post-9/11 GI Bill.[7] The bill went into effect in August 2009, retroactively providing additional benefits to individuals with active-duty service after September 11, 2001.[8]

Unlike the previous MGIB, the Post-9/11 GI Bill does not require individuals to elect into the program upon enlistment or forego the $100 a month for the first year of active-duty service. The vast majority of individuals serving on active duty after September 11, 2001 are

[5]Conversations with Department of Defense officials suggest that recent Montgomery GI Bill participation (agreeing to a pay reduction) during the first year of enlistment was almost universal.

[6]Individuals attending less than half-time receive an amount equivalent to their tuition and fees. However, these partial benefit amounts only subtract from total benefit eligibility proportionally (in other words, a quarter-time student receiving $466 per month only uses approximately a third ($466/$1,400) of a month of benefits.

[7]The delay between announcement and implementation of the benefit expansion suggests that individuals considering enrollment between these two points may have had an incentive to delay enrollment in order to capture the additional potential benefits provided by the Post-9/11 GI Bill. However, this increase is (at most) the discounted difference between one year of Post-9/11 GI Bill benefits and one year of MGIB benefits. This gain must be balanced against the loss implied by delaying enrollment a year. Furthermore, the time between announcement and fall 2008 enrollment gave individuals very little time to adjust enrollment decisions. Empirically, there is no evidence that veteran enrollment fell relative to non-veteran enrollment during the fall of 2008.

[8]These benefits could only be applied to enrollment occurring after implementation.

eligible, with benefit levels tiered based on active-duty service lengths.[9]

Maximum benefit eligibility is based on the highest tuition level and fee level of any public college in an individual's state of residence. Unlike under the MGIB, the Department of Veterans Affairs (VA) pays benefits directly to schools, reimbursing the level of tuition and fees up to the in-state maximum of each. In addition, the VA provides veteran students enrolled half-time or more with a monthly housing allowance based on the zip code of the institution that the student attends. This monthly basic allowance for housing (BAH) ranges from around $800 in many rural areas to over $2700 in New York City. Finally, each full-time student receives an annual book stipend of roughly $1,000.

The most visible difference between the bills is the level of benefits provided. In 2008, the MGIB provided roughly $1,400 of benefits per month for up to 36 months, resulting in a maximum total benefit level of roughly $50,000. The maximum per-credit benefit provided under the Post-9/11 GI Bill is over $1,000 in several states, implying a reimbursement of up to $15,000 for a single semester of tuition.[10] In addition to this, the new GI Bill provides coverage for thousands of dollars of fees per term in nearly all states. Adding in the over $2,000 monthly housing allowance in New York or another high cost of living area and the $1,000 annual book stipend, an individual could receive more than $50,000 in benefits in a single year.

The second major difference between the two types of veterans' benefits is the degree to which benefits vary geographically. To quantify this variation, I estimate the combined annual maximum tuition and housing allowance by state. I exclude the benefits for fees since the maximum fee levels are generally orders of magnitudes larger than the level of fees charged by a state's flagship institution. For the housing allowance, I use the population-weighted average monthly housing allowance across zip codes in the state, multiplied by 9. Across all states, the average increase in a year's worth of maximum available benefits was roughly $13,000.[11] Table

[9]Officers commissioned at military academies and individuals previously receiving substantial ROTC scholarships are only eligible after completing an additional period of service above and beyond the initial requirement. Both bills require individuals to have a non-dishonorable discharge. Eligibility for all veterans is based on active duty tiers of 90 days (40 percent), 6 months (50 percent), 12 months (60 percent), 18 months (70 percent), 24 months (80 percent), 30 months (90 percent), 36 months or more (100 percent). I am unable to explore this level of heterogeneity due to data limitations. However, it is likely minor as very few enlisted individuals serve for less than two years on active duty and receive an honorable discharge.

[10]The Post-9/11 GI Bill similarly allows 36 months of benefits at full-time enrollment. This translates into four academic years if an individual is enrolled full-time.

[11]Realized increases were likely substantially smaller, since these depend on the costs and locations of the colleges selected by veterans.

A1 shows that changes in annual maximum benefit levels range from close to zero in some states to tens of thousands of dollars in others.[12] This variation in nominal benefit levels across states under the new GI Bill adds another source of variation as it generates fifty-one micro experiments.

IV. Theoretical Predictions

A. General Framework

The salient features of the Post-9/11 GI Bill are the in-kind nature of benefits and the explicit geographic variation in benefit generosity. The bill changed the relative prices of different educational options, and changed these differentially by state. I develop a stylized model to generate predictions about impacts on enrollment, school sector choice, and persistence.[13] The model takes working and leisure hours as fixed and abstracts from heterogeneity in preferences and in psychic or opportunity costs of attending school. While these aspects help to rationalize why individuals choose different types of schooling, incorporating them would not alter the qualitative predictions.

I assume that a veteran faces the following two-period optimization problem:

$$max\ V(c_1, E) = u(c_1) + \beta u(c_2)$$

subject to

$$c_1 P_c + \sum_{j=1}^{J} Max\left[\tau_j - \tau_{GI}, 0\right] \cdot 1\{E = j\} \leq I + T_{GI} \cdot 1\{E > 0\} + b \quad (1)$$

$$c_2 P_c \leq wf(E) - b(1 + R)$$

Veterans choose a type of education E, which varies from 1 to J, to maximize the stream of utility from consumption. The set J is constrained to the set of schools to which a veteran is able to gain admission. Institutions can be thought of more practically as varying from vocational training and two-year colleges to four-year schools. For expositional purposes, assume education types can be ordered by price such that $\tau_1 < \tau_2 \ldots < \tau_J$, where tuition increases monotonically

[12]While individuals can move freely between states to obtain this in-state public tuition maximum, residency conditions for in-state tuition eligibility are generally more stringent. The vast majority of states require an individual to have maintained residency in the state or to meet civilian residency requirements (12 months and demonstration of intent to live in the state) in order to obtain in-state tuition rates.

[13]The model shares some similarities with that developed in Peltzman (1973), although Peltzman focuses on the degree to which in-kind support for higher education crowds out private expenditures.

with some measure of the benefits provided by each institution.[14] $E = 0$ represents the choice to not enroll.

Veterans choose which type of institution to attend, balancing the marginal future return to investment in a higher type education with the higher cost of tuition. The term $\sum_{j=1}^{J} Max[\tau_j - \tau_{GI}, 0] \cdot 1\{E = j\}$ indicates that an individual pays the tuition of the particular type of institution chosen, net of any tuition reimbursement provided through the GI Bill. Expenditures on consumption and education are limited by income I, cash transfers T_{GI} received as part of the GI Bill if enrolled, and the amount an individual is willing to borrow from future income b.[15]

A subsidy to education, coming either through the price of schooling or the size of the conditional cash transfer, has two straightforward implications: (1) Higher levels of initial enrollment, and (2) Higher persistence conditional on already being enrolled. As discussed by Lochner and Monge-Naranjo (2011), a substantial amount of work addresses the extent to which credit constraints $(b \leq \overline{b})$ magnify this effect. Given sizable estimates of the return to college, the presence of credit constraints perhaps explains why small (relative to the return) changes in the cost of college might result in large changes in participation. This efficiency argument for subsidizing education is important to keep in mind in interpreting the magnitude of the results as well as thinking about the costs and benefits of the program.

B. Moving to the Post-9/11 GI Bill

Under the MGIB, individuals receive flat transfer amounts regardless of the level of tuition and fees. This increases income contingent on any enrollment, which allows individuals to choose among any type of schooling while keeping any remaining money. Although the MGIB was designed to promote investment in schooling, it does very little to alter the price of education contingent on enrollment (since $\tau_{GI} = 0$).

How this program affects the trade-off between current consumption and expenditures on education is shown in Figure 1. For ease of exposition, I depict E as a continuous ordered measure of education types in the figure. Relative to no intervention, the budget constraint is shifted outward under the MGIB. The transfer, denoted MGIB, primarily operates via an income

[14]Here, I have modeled this as representing different labor market returns ($f(E)$ is increasing in j) to different types of institutions, but it is also possible that more expensive institutions have different consumption values or other qualities which warrant their greater expense.
[15]The model and empirical work abstract from the decision to attend part-time. In practice, the level of cash benefits varies discretely by intensity of enrollment and not just at the extensive enrollment margin.

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effect. The open circle at $E = 0$ indicates the transfer is contingent on enrollment, so unavailable unless $E > 0$. This contingency can be thought of as a large price distortion for low education types.

When the new GI Bill went into effect in August 2009, it increased the level of benefits available to nearly all post-9/11 veterans and also changed the nature of benefits. The cash transfer available to individuals conditional on enrolling at all is now the housing allowance (BAH). Also, tuition and fees are now sent directly to schools. The in-kind nature of this benefit means that Post-9/11 GI Bill veterans are unable to obtain the difference between their tuition and fees and the maximum benefit level. This drops the monetary cost of investing in higher return types of education to zero over a segment of the budget constraint, as show in Figure 1.

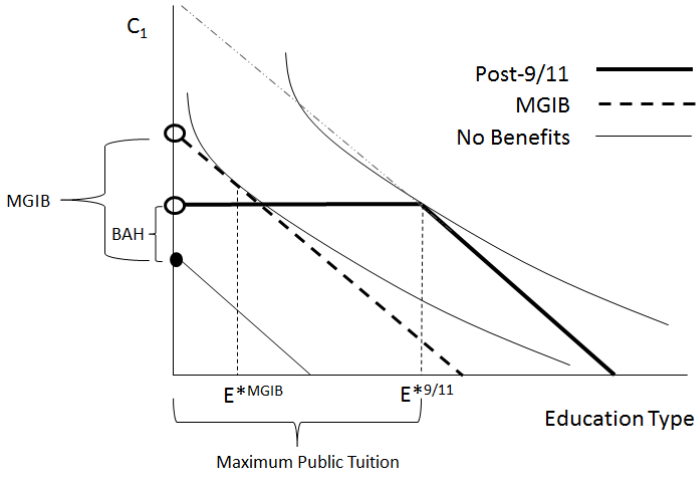

Figure 1

Impact of Post-9/11 GI Bill

As compared to the prior bill, the new bill encourages enrollment and persistence for low education types to the extent that BAH exceeds MGIB. For others, standard assumptions on preferences lead to the third prediction: (3) Composition of human capital investments should shift towards more expensive (plausibly higher quality) four-year schools and away from community colleges, certificate programs, and other less expensive forms of education.

One potential concern that has received both media and government attention is the degree to which for-profit institutions might target veterans as a source of revenue following the benefit expansion (Harkin 2010).[16] Data from the 2009-2010 school year indicate that while roughly 23 percent of veterans using the Post-9/11GI Bill did so at for-profit institutions, almost half of the funds have been used at these types of institutions. If for-profit schools are effectively

[16]For-profit institutions may have this incentive since this aid is not counted in the 90 percent cap on the proportion of revenue these institutions may receive from Federal Title IV sources.

targeting veterans, a shift towards more expensive institutions may not reflect an increase in quality. Understanding the composition of any observed shift to four-year schools will be important in interpreting welfare effects.

C. Cross-State Variation in Benefit Expansion

Since the $MGIB$ provided a nominally flat amount nationally, the real value of the benefit varied substantially across states depending on local prices (P_c and τ_j). Thus, individuals in low cost of living and education states received a much larger real benefit than those in high cost states. The Post-9/11 GI Bill, in contrast, attempts to implement a relatively equal real benefit across states by providing housing allowances based on the prices of local housing and pegging the maximum benefit to the highest in-state tuition and fee levels. This results in a larger increase in benefits for individuals living in high cost of living states than for those in low cost states, implying larger behavioral responses.

In order to exploit cross-state variation, I use an estimate of the combined annual benefit under the Post-9/11 GI Bill in each state (as described in the prior section). This abstracts from the differential role the BAH and tuition benefits play at the margin of enrolling in any education. However, if both the BAH and maximum tuition benefits are higher in one state than another, then it is unambiguous that individuals in the state have greater incentives to enroll and to enroll in higher quality institutions. In practice, the cash and in-kind benefits are in fact positively correlated (r=0.18). I also present estimates where the two types of benefits are entered separately to test for differential impacts.

V. Data

The data for the main analyses of enrollment, school choice, and persistence come from two sources: the American Community Survey (ACS) and the October CPS (Education) supplement. I utilize additional data from the 2010 National Survey of Veterans, the CPS Veteran supplement, administrative military recruiting data, summary benefit usage data provided by the Veteran Affairs administration, and data collected on and maximum in-state covered tuition and fee levels from the Veteran Affairs website.

The relatively large size of the ACS and CPS October samples makes them well-suited for a study of impacts on the subpopulation of veterans. I use the 2006 through 2011 ACS and CPS October surveys, restricting the sample to individuals born and currently residing in the United States.[17] I limit the sample to individuals with at least a high-school degree (or

[17]I start the sample in 2006 to balance the pre and post-treatment years for the CPS.

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equivalent) but less than a bachelor's degree. As many non-veterans have acquired a bachelor's degree by 23 and nearly all veterans have a high-school degree (or equivalent), these restrictions serve to make the control and treatment groups more similar.[18] I further restrict the data to individuals age 23-28. This restriction serves three purposes. First, it excludes the traditional non-veteran college-aged population; these individuals likely respond differently than veterans to changing economic and financial aid conditions and thus serve as a poor control group. Second, college enrollment rates are very low among individuals in their thirties and beyond; therefore, I am focusing on the population most likely to be impacted by the benefits. Third, the age restriction limits the veteran population during the Post-9/11 GI Bill eligible sample years to individuals who are very likely to have served on active duty after September 11, 2001.[19] Including the few veterans aged 23-28 during the Post-9/11 GI Bill period who did not serve after September 11, 2001 should only bias my results down, as it includes untreated individuals in the treated group.

Table 1 provides summary statistics for the ACS and CPS October samples, illustrating characteristics of the veteran and civilian control group population. Veterans between 23 and 28 are more likely to be enrolled in college, somewhat more likely to be in public schools contingent upon enrollment, and much more likely to be male and have been married. The large sample size of the ACS allows for an examination of demographic heterogeneity and the leveraging of cross-state variation in the benefit levels. Furthermore, the ACS asks basic enrollment questions to both current and former active-duty individuals, while the CPS asks a more detailed set of questions to individuals with active-duty service who are no longer in the military. The CPS does not ask enrollment questions of individuals currently on active-duty service. Throughout the paper, I refer to veterans as individuals with active-duty military service, whether or not they are still in the military. I refer to the more traditional veteran group, those no longer in the military,

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Furthermore, the 2006 ACS was the first to contain group quarters (including college dormitories). The results are robust to the inclusion of additional pre-treatment years (Table A2).

[18] As this restriction has the potential to introduce selection issues, I also present results without this restriction.

[19] About 6.6 percent of the veterans in the Post-9/11 GI Bill period in the CPS did not serve on active-duty after September 11, 2001. Removing these veterans from the sample has little impact on the results, but introduces some selection issues as it differentially removes a particular type of veteran from the pre and post periods. Similarly, restricting the entire veteran sample to individuals with active-duty service after September 11, 2001 would eliminate far more veterans from the pre period than the post period. In order to avoid these concerns, I have taken the conservative approach of including a small number of veterans that are not treated in the treatment group.

as separated veterans; all analyses using CPS data focus on this group of individuals. In order to ease comparison across data sets, in all ACS results I include a column in which the veteran sample is restricted to separated veterans, rendering it roughly equivalent to the CPS sample.

VI. Estimation Strategy

My baseline econometric enrollment specification takes the following difference-in-differences linear probability form:[20]

$$(2) \qquad E_{it} = X_{it}\beta_1 + \lambda_t + \alpha_b + \beta_2 After_t + \beta_3 Vet_i + \gamma After_t * Vet_i + \epsilon_{it},$$

where E_{it} is a binary variable representing enrollment of individual i in year t, X_{it} is a vector of variables for age, age*veteran, race, and sex characteristics; α_b and λ_t are state of birth and year fixed effects[21]; Vet_i indicates whether an individual is a veteran; and $After_t$ is an indicator variable for whether or not an observation occurs after August 1, 2009. Regressions using the ACS include 2009 observations in the pre-treatment period as it is impossible to separate observations into months without restricted access data. Since veterans were only eligible for Post-9/11 benefits during the last four months of 2009, any positive enrollment effects during those months will be captured in the $Before_t * Vet_t$ period and thus should bias the estimate of an enrollment impact of the Post-9/11 GI Bill towards zero.

In estimation, $After_t$ is dropped as it is collinear with year fixed effects. The coefficient γ provides a measure of the marginal increase in the probability that a veteran is enrolled in school once the Post-9/11 GI benefits became available. The key identifying assumption is that the shift in enrollment patterns of non-veterans from before August 2009 to after effectively proxies for the shift in enrollment patterns that would have occurred among veterans absent the new GI benefits. In addition to concerns regarding non-parallel trends and differential responsiveness to time-varying variables (such as the economy), changes in the composition of veterans over time could introduce bias. In Section 7, I provide evidence that these threats are not a major concern in practice.

In addition to the general increase in benefit levels, I exploit heterogeneity in the change in benefits across states. As discussed in the prior section, the cross-sectional variation in

[20] All marginal effects are estimated using probit and logit specifications as robustness checks.
[21] In most specifications, I also include state of residence fixed effects that control for heterogeneity in state specific drivers of enrollment including college costs and labor market opportunities. I address the potential endogeneity of state of residence in Section 7.

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benefits provided by the Post-9/11 GI Bill is directly related to variation in the change in incentives. Thus, I alter the basic equation to allow the impact to vary with the level of benefits:

$$E_{izt} = X_{it}\beta_1 + \lambda_t + \alpha_b + \alpha_z + \alpha_z * Vet_i + \beta_2 After_t + \beta_3 Benefit_z + \beta_4 Vet_i + \beta_5 After_t * Benefit_z + \beta_6 Vet_i * Benefit_z + \gamma_1 After_t * Vet_ + \gamma_2 After_t * Vet_i * Benefit_z + \epsilon_{izt}. \quad (3)$$

Here, E_{izt} indicates whether individual i is enrolled in state z during year t. $Benefit_z$ takes on an estimate of a maximum year's worth of benefits varying at the state level. This estimate is equal to 9 times the basic allowance for housing plus 24 times the maximum per credit tuition benefit. The addition of state of residence and state by veteran status fixed effects control for cross-state variation in the pre-existing enrollment patterns of veterans and non-veterans. State, year, and state by veteran status fixed effects are collinear with $Benefit_z$, $After_t$, and $Vet_i * Benefit_z$ respectively, thus the latter terms are dropped in estimation. The coefficient γ_2 on the triple interaction term indicates how the enrollment impact of the new benefits varies with the benefit level in a certain state.

The key assumption is that, conditional on the set of other observables, the shift in the enrollment of non-veterans in a state with a high or low benefit level effectively proxies for the counterfactual shift that would have occurred among veterans absent the benefit expansion. Although concerns related to migration are mitigated by residency requirements associated with in-state tuition, I also present evidence that the benefit expansion did not affect veterans' decisions of where to live. I also provide a standard triple-difference estimate comparing the change in the difference between the college enrollment of veterans and non-veterans, from before to after the implementation of the bill, between high and low benefit states. Finally, I separate the sample by quartile of this combined benefit level and estimate the standard DD effect for each subsample. This specification more flexibly controls for trends in low and high benefit states.

As discussed in the theory section, the housing and tuition components of the benefit expansion may have different effects on enrollment. While the combined benefit proxies for the overall increase in incentive to enroll, I also estimate specifications including each benefit type and the associated interactions. The estimates from these specifications allow for a comparison of the effects of the two benefit components on enrollment.

VII. Results

Overall, I find that the higher benefit levels resulted in roughly a two percentage point

increase in college enrollment for all veterans, and a larger four percentage point increase for veterans who have separated from the military. This effect is concentrated in white and black males.[22] The impact on enrollment, particularly for males, is stronger in regions that experienced larger increases in benefit levels, underlining the point that nominally flat national benefit levels, which existed under the MGIB, can generate different real benefits based on variation in pricing across markets. While enrollment has increased at both public and private schools, evidence from the October CPS indicates that the Post-9/11 GI Bill has made enrolled veterans more likely to attend costlier four-year institutions. Finally, I present evidence that the higher benefit levels resulted in an increase in the persistence of veterans enrolled in college as well as encouraged transfer from two to four-year schools.

A. Enrollment

Table 2 presents the key results from regressions that estimate the impact of the Post-9/11 GI Bill on enrollment using the ACS. Each cell contains the simple difference-in-differences estimate; the coefficient on the interaction of $After_t$ and $Veteran_i$ provides the percentage point impact of the new benefits on veteran enrollment (robust standard errors are clustered at the state level throughout). The coefficient for college enrollment in column (1), 0.0165, indicates that the benefit expansion increased college enrollment of all veterans by slightly under two percentage points, a roughly six percent increase.

I estimate several additional specifications of the basic enrollment equation as robustness checks. The inclusion of state of residence fixed effects in column (2) has no effect on the point estimate. As the inclusion of 2009 individuals in the pre-period should bias results downward, I exclude them from the sample in column (3). This results in a slightly larger point estimate, roughly 0.0174, which is statistically indistinguishable from the base specifications. In column (4), I allow for a veteran specific trend. Veteran enrollment has a slight downward trend relative to non-veteran enrollment during the 2006-2008 period; thus, inclusion of trends results in slightly higher, although less precisely estimated, point estimates.

Finally, in column (6), I estimate specifications that restrict the veteran sample to separated veterans, those no longer employed by the military. These estimates are more directly comparable to those produced using the CPS data.[23] This group exhibits an even larger response

[22]I interpret the smaller and insignificant effect for females as inconclusive due to the smaller sample sizes of female veterans.

[23]While this potentially introduces selection (out of the military) into the sample, the degree to which the military enforced contracts and the use of stop-loss measures implemented throughout this time period make it unlikely that this is much of a concern. I discuss this concern further below.

to the benefits, a 4.5 percentage point or roughly seventeen percent increase in enrollment. Throughout, the increase in public college enrollment is more than twice the increase in private college enrollment.

1. Robustness Checks and Falsification Exercises

In the appendix, I probe the robustness of the results to a number of alternative sample restrictions. First, I examine the effect of limiting the sample based on educational attainment. While restricting the sample to individuals with at least a high-school degree but less than a bachelor's degree renders the samples more similar and eliminates the higher proportion of non-veterans with at least a bachelor's degree, it also has the potential to introduce sample selection concerns. I present results analogous to those in columns (2) and (6) of Table 2 without a restriction on educational attainment and restricting the sample to those with less than a bachelor's degree; they are nearly identical (Table A2). Similarly, I examine the robustness of the results to a longer sample period (2004-2011). The results are robust to the inclusion of additional pre-treatment years with statistically indistinguishable point estimates (Table A2).

I also present event study plots to explore the degree to which pre-existing trends in enrollment of veterans and non-veterans may be driving the results. I employ a similar specification as in equation (1), but with the full set of $Year_t$, Vet_i, and $Year_t * Vet_i$ indicator variables. Figure A1 plots the coefficient estimates for each $Year_t * Vet_i$ indicator variable for the outcomes in Table 2. These plots provide strong evidence of parallel trends among these two groups during the pre-treatment period (2006-2008), with near zero coefficients for all outcomes. In 2009, treated for only 4 of the 12 months, there is a slight uptick in veteran enrollment followed by a larger increase in 2010 and 2011, consistent with the results in Table 2.

While the pre-treatment trends suggest that the enrollment of veterans and non-veterans move together, it is possible that the Great Recession affected veterans' enrollment behavior differently than non-veterans' behavior. However, it is not clear in which direction this bias would go. In particular, the generosity of financial aid programs increased markedly over this period; to the degree that veterans' own aid crowded this aid out (as much aid is earmarked for tuition and fee coverage), the estimates presented here may be biased downward.[24] Or, perhaps,

[24]Some affected aid programs, including the Pell grant, are not earmarked for tuition and fee coverage. However, statistics from the 2008 and 2012 National Postsecondary Student Aid Study indicate that veterans are less likely to receive Pell grants (26.9 vs. 27.8 percent during 2008 and 38.1 vs. 41.4 during 2012) and receive lower average Pell grant amounts conditional on receipt (author's calculations).

veterans are more responsive to labor market contractions and so would shift toward investing in education more than the general population. I present three pieces of evidence to argue that the latter possibility is not driving the results. First, I leverage cross-state variation in the severity of the recession to examine the differential enrollment response of veterans to changing labor market conditions using an identical specification as in Table 2, but with the inclusion of the state-year unemployment rate $Unemp_{st}$ and the unemployment rate interacted with veteran status $Unemp_{st} * Vet_i$. While the unemployment rate is positively associated with enrollment, there is no differential effect (Table 3). Furthermore, the estimates of the effect of the Post-9/11 GI Bill are statistically indistinguishable from those in Table 2. Second, I have also estimated these effects using individuals with training for the reserves or National Guard only as the control group. These individuals are ineligible for Post-9/11 GI Bill benefits as they have no active-duty service. I find statistically significant effects similar to those presented in Table 2 and 3 (Table A3). Third, in the next section, I present estimates of the enrollment response using geographic variation in benefit levels that is unrelated to the state-specific variation in the labor market contraction.[25]

Another concern is that the estimates in Table 2 may merely reflect a change in the composition of veterans or separated veterans captured in the ACS. Changes in the residency decisions or death rates of veterans over this time period may have affected the composition of veterans in the sample.[26] In the online appendix I demonstrate that there is little evidence of changing characteristics of veterans relative to non-veterans. I also examine trends in active-duty and veteran totals and deaths over time; there are two major takeaways. First, the separated veteran population (estimated from my sample) and the total active-duty population have remained fairly stable between 2004 and 2010, suggesting that there have not been major shifts in the number of veterans choosing to return to the United States. Second, the number of active-duty deaths and the number of military suicides have not changed dramatically over time and their magnitudes are too small to meaningfully affect the estimates.[27]

While the benefit expansion appears not to have coincided with changes in mortality or

[25]More specifically, the state-specific expansion in benefits is unrelated to the degree of labor market contraction that occurred between 2007 and 2011 (full results available from author upon request).

[26]For example, if those individuals that return to the United States after implementation of the Post-9/11 GI Bill have stronger preferences for education, this change in the composition of veterans in the sample may bias the estimates upwards.

[27]Even if we assume that every marginal suicide (.0003 of the young veteran population) was certain not to enroll in college, this would increase veteran enrollment by .00007, a tiny fraction of the observed estimate.

residency, it is also possible that the benefit expansion affected the decision to separate from the military. I present several pieces of evidence that suggest that this is not likely to be a problem. First, the leftmost columns of Table 2, 3, and 4 include both separated veterans and those currently on active duty. This means that the pool of individuals who appear to be affected by the benefit expansion is not experiencing a change in composition as a result of separation. The estimated enrollment effect may then incorporate the decision to separate from the military, but it is not driven by compositional changes in the types of individuals who are veterans in the sample.

However, there are several reasons to believe that separation itself was not greatly affected and should not bias the estimates. First, earlier work using veterans separating between 1988 and 2003 finds small effects of benefit increases on separation and finds somewhat larger effects of benefit size on benefit usage once the effect on separation is taken into account (Simon, Negrusa, and Warner 2010). Second, while I do not have detailed information on the separation of veterans over this time period, I can examine the duration of active duty service of individuals separating before and after the Post-9/11 GI Bill's announcement using the 2007, 2009, 2010, and 2011 Veteran Supplements to the CPS. If enlisted individuals are changing their decisions to separate in order to enroll in school, we would expect the distribution of active duty service durations to shift to the left following the announcement of the benefit expansion (in other words, for those separating in between 2009 and 2011). Examinations of the distributions suggest that this is not the case (see online appendix). Finally, I look for changes in the characteristics of separating veterans relative to those on active duty. Most characteristics appear stable with some suggestive evidence that veterans separating in the post period are less likely to have families (see online appendix).[28] In sum, there is little reason to suspect that changing separation is a major concern.

2. Demographic and Geographic Heterogeneity

The substantial sample size of the ACS makes it possible to explore heterogeneity in treatment effects across subpopulations. I present these subgroup results in Table 4. Specifically, I divide the analytical sample by gender and race. These subgroup results indicate that the college enrollment impact is larger for males with the point estimate reaching .023. While I estimate a smaller effect for females, the confidence intervals are large enough that I cannot rule out equivalence of the effect across gender. I further divide the sample by race, finding similar effects for white and black males.

[28]The estimates in Table 2 are robust to the inclusion of these characteristics.

In Table 5, I explore the impact of the geographic heterogeneity in benefit levels generated by the Post-9/11 GI Bill. I focus on separated veterans as those still on active duty are not eligible to receive the housing allowance benefit. The coefficient on "Post-9/11 Impact*Combined Benefit" demonstrates how the enrollment effect varies with the maximum yearly combined tuition and housing benefit levels available in a state. The second row shows that $1,000 of available benefits increases enrollment by .07 percentage points for the full sample and .12 percentage points for males. I also present results from a standard triple-difference setup. While this strategy ignores some information, it is quite straightforward to interpret. The estimates indicate that the change in college enrollment of veterans relative to non-veterans from before to after the implementation of the bill is 2.35 percentage points larger in states with combined benefit levels above the median relative to those below. Dividing this estimate by the difference in maximum combined benefit levels in states with below and above median benefit levels indicates that college enrollment increases by roughly .18 percentage points per $1,000 of aid.

I also split the sample by quartile of combined benefit level, estimating the basic DD effect in each quartile. The estimates of the GI Bill effect are higher in areas with higher combined benefit levels. The online appendix contains event studies for each quartile; there is no evidence of a pre-trend in any of the four quartiles.

While these results suggest that veterans are responding to higher benefit levels, concern remains that the estimates are perhaps driven by migration of those interested in enrolling to the states experiencing larger benefit expansions. I address this concern by indexing state of residence by the combined maximum yearly benefit level and then estimating the effect of the Post-9/11 GI Bill on this index. If there is sorting to high benefit states, we would expect a positive effect on this index. In Table A4, I present the estimates on the index of estimated combined annual benefits (in thousands). There is no significant effect on migration. Most point estimates are small and slightly negative, indicating, if anything, a move away from high-benefit states.[29] This buttresses the interpretation of the estimates in Table 5 as the causal effects of higher benefit levels on enrollment.

Finally, I present estimates from a specification including the benefit amounts separately (Table 6). These estimates are consistent with those in Table 5. As suggested in the theory

[29]While perhaps somewhat surprising, veterans consistently list location among the most important attributes when choosing a school. In the 2004 BPS, 79 percent of veteran (both current and separated) students listed location as a reason they are attending their institution (author's calculations using BPS: 04/09).

section, the effect of an expansion in the housing allowance benefit has a larger effect, with a $1,000 increase in annual housing allowance benefits resulting in a .3 percentage point increase in enrollment across samples. However, the estimates are not precise enough to rule out the equivalence of effect sizes across samples.

B. In-Kind Transfers and College Choice

While the previous section demonstrated a strong and relatively large impact of the bill on enrollment, here I examine the degree to which the Post-9/11 GI Bill impacts the types of schools that veterans choose to attend. I focus on the effect on separated veterans because the CPS does not ask schooling questions to individuals still in the military. The estimated effect on college enrollment of .06 in Table 7 is slightly larger than the comparable estimate presented in column (6) of Table 2, but statistically indistinguishable.[30] As discussed in Section 4, a shift in the structure of the benefits towards a more in-kind provision generates changes in the budget constraint that should push more individuals towards four-year schooling. Table 7 presents the impacts on enrollment in different types of institutions; full-time and four-year enrollment (particularly at public institutions) appear to rise dramatically as a result of the Post-9/11 GI Bill.

Table 8 presents the average marginal effect (for the cross derivative) among veterans from multinomial logit estimation of the college choice problem (standard errors are bootstrapped). Overall enrollment increases roughly six percentage points overall and for males, similar to the corresponding point estimates for separated veterans in Tables 2, 3, and 7. Perhaps more interesting are the marginal effects on two-year and four-year college enrollment presented in Table 8. While two-year enrollment appears to be relatively unaffected by the benefit increase, the probability of four-year enrollment increases substantially, accounting for the majority of the increase in enrollment. The results in Table 7 and 8 suggest that the change in the manner of benefit provision has pushed veterans to invest in more expensive (plausibly higher-quality) schooling.

Some concern exists that this enrollment shift may be towards for-profit institutions which have high costs and low graduation rates. While the overall labor market effects of for-profit enrollment are unclear, there is a growing consensus that wage gains are at most equivalent to similar, but substantially cheaper, public options (Cellini and Chaudhary 2012; Deming, Goldin, and Katz 2012). In contrast to the picture painted by the media, data from the CPS suggest that most of the shift (75 percent) in enrollment is into public institutions.

[30]Recall that the Table 2 estimate may be slightly downward biased due to the inclusion of all of 2009 in the pre-period.

C. Persistence

In Table 9, I examine how the benefit expansion has affected the choices of veterans already enrolled in school. Regressions are restricted to individuals enrolled as freshmen, sophomores, or juniors in the prior year; estimates in the first two columns suggest that higher benefit levels make an individual more likely to persist in college. This increase in persistence appears to contribute to higher enrollment levels at four-year public institutions, as individuals initially observed enrolled in 2008, 2009, or 2010 are between ten and fifteen percentage points more likely to be enrolled in a four-year school during the following year. As this may be an artifact of the inclusion of the 2010 and 2011 data, and thus including individuals already pushed to four-year schools initially by the higher benefit levels, I estimate similar specifications dropping the 2010 and 2011 data. Presented in the final two columns, the same pattern of results remains, indicating that the higher level and change in structure of the benefits potentially drove veterans to transfer to four-year institutions.[31]

In the middle two columns, I focus on the increase in persistence of those individuals who actually received benefits at time $t-1$. If there are meaningful liquidity constraints for these individuals (as there likely were during late 2008 and 2009), there should be a larger effect on persistence for those that received funds at both time $t-1$ and time t. Although estimates in columns (3) and (4) are in line with this expectation, the point estimates are statistically indistinguishable from those in the first two columns. I interpret these results with caution as this introduces selection of individuals drawn into college by the benefits. However, standard economic theory suggests that a benefit expansion should shift the composition of students towards individuals less likely to persist, biasing against finding an increase in persistence.

8. Discussion and Conclusion

As college costs and the percentage of older students enrolled continue to rise, understanding the effects of financial aid on non-traditional students becomes increasingly relevant. The GI Bill provides exogenous variation in education benefits that offers insight into the effects of financial aid on this group. Older students may be of particular interest as they are frequently first-generation students from low-income backgrounds, making them more susceptible to credit or information constraints. Most previous studies of federal financial aid focus on cohorts of students who considered enrollment more than two decades ago. More

[31]Veterans changing their initial enrollment patterns towards four-year schools in 2008 is also consistent with this pattern, although there is no evidence that enrollment of veterans was affected at this time.

recent estimates from studies of state financial aid programs focus on the behavior of relatively high-achieving high-school graduates. Current estimates of the effects of financial aid on older students are completely absent, but this paper offers a starting point.

I use variation generated by the Post-9/11 GI Bill to add to the evidence on the impact of financial aid on enrollment. In particular, I add to the sparse literature on the impact of financial aid on older, non-traditional students, demonstrating sizable effects of the new GI Bill on college enrollment.

Assuming an average maximum benefit increase of $13,000, estimates from the basic specification suggest an enrollment increase of at most .35 to .45 percentage points per $1,000 of additional yearly maximum benefits, substantially smaller than the consensus of three to six percentage points presented by Deming and Dynarski (2010).[32] Focusing on a subset of the literature that examines a more similar population, Seftor and Turner (2002) find effects closer to one percentage point per $1,000 in annual aid, much closer to the estimates presented here. Simon, Negrusa, and Warner (2010) find even smaller effects, roughly a half percentage point per $1,000 increase in total aid. Two differences between the settings may drive the remaining disparity. First, the estimates presented here assume variation in maximum potential benefit levels which may be somewhat larger than the salient level of variation. If we instead use a very conservative estimate of the average realized increase in benefit receipt, $5,000, the estimates suggest an enrollment increase of .85 to 1.1 percentage points per $1,000 of additional yearly benefits.[33] This leads to the second, and more likely, reason for the decreased effect size; potential recipients of the Post-9/11 GI Bill were eligible for relatively high levels of financial aid before the benefit expansion. As a number of researchers have argued, the sizable enrollment responses to changing financial aid suggest that credit constraints may play a large role in the college enrollment decision. If this is the case, we would expect enrollment responses to be smaller at higher initial levels of financial aid as credit constraints become less of a concern.

This leads to efficiency questions related to the decision to provide aid to this group. Many individuals that enlist in the military are first-generation students from relatively low-income backgrounds that are likely to have faced credit or information constraints (Barr 2013). To the degree to which this is true, aid to this group is likely an efficient choice relative to

[32]Results from the triple-difference specification suggest an even smaller enrollment increase of roughly .18 percentage points per $1,000 of additional yearly maximum benefits.

[33]The $5,000 statistic is approximately equivalent to the average annual tuition and fee benefit received by Post-9/11 GI Bill recipients in 2009. The average housing allowance level under the Post-9/11 GI Bill is roughly equivalent to the Montgomery GI Bill monthly benefit level and thus nets out when comparing benefit levels between the two periods.

programs that award aid without regard to resources. More work is needed to understand whether these individuals were prepared to enroll at the time of their enlistment or if they use their time in the military to prepare themselves for college. This hints at an important potential limitation of this study: the degree to which military enlistees and their behavior may differ from other non-traditional students. While veterans and non-veterans are similar across a number of individual and schooling-related characteristics, other types of non-traditional students may respond somewhat differently to a financial aid expansion.

Despite this limitation, this paper provides the only current evidence on the effects of financial aid on older students and the first causal estimates of the effect of the Post-9/11 GI Bill on enrollment and school choice. As more time passes, Post-9/11GI Bill users will begin to graduate from college and enter the workforce, permitting more comprehensive estimates of the impacts of the large scale aid expansion.

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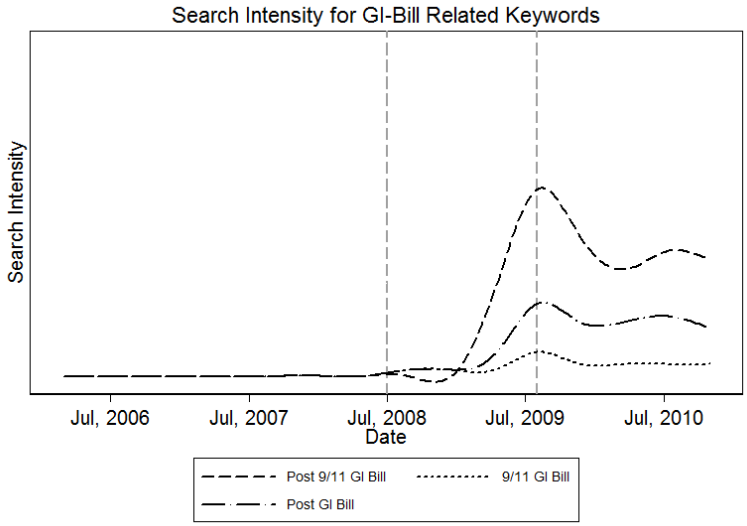

Figure 2
Interest in the Post-9/11 GI Bill

Note: The vertical lines represent the passage (June 30, 2008) and the implementation (August 1, 2009) of the Post-9/11 GI Bill. Search intensity for each set of keywords is normalized as a ratio relative to the value in July 2008 (the first month with positive search intensities for all queries). Weekly data on search intensity are smoothed using a median spline. Search intensities collected from Google Insight. See Google Insight documentation for explicit documentation.

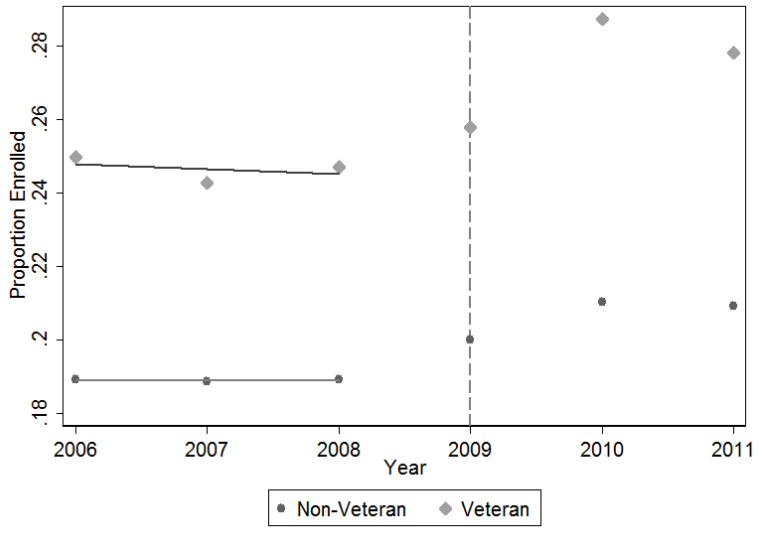

Figure 3
Veteran vs. Non-Veteran Enrollment

Note: Dashed line represents the implementation (August 1, 2009) of the Post-9/11 GI Bill. Markers represent the (weighted) proportion of veterans and non-veterans aged 23-28 enrolled in college using the American Community Survey 2006-2011.

Table 1
Descriptive Statistics

	ACS		CPS	
Variable	Veteran	Non Veteran	Veteran	Non Veteran
Male	0.84	0.50	0.77	0.51
White	0.70	0.66	0.70	0.66
Black	0.15	0.16	0.16	0.17
Age	25.58	25.41	25.75	25.39
Ever Married	0.56	0.34	0.53	0.35
Number of Children	0.51	0.55	0.62	0.61
College	0.26	0.20	0.28	0.19
Public	0.21	0.16	0.23	0.17
Private	0.04	0.04	0.06	0.03
Two-Year			0.11	0.07
Four-Year Public			0.14	0.10
Four-Year Private			0.03	0.02
Part-Time			0.06	0.06
Full-Time			0.23	0.13
N	39,937	587,345	1,285	29,684

Note: Statistics calculated for individuals aged 23-28 from ACS and CPS years 2006-2011. Sample restricted to individuals with at least a high-school degree (or equivalent) but less than a college degree. College indicates whether an individual is enrolled as a college undergraduate.

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Table 2
Impact of Post-9/11 GI Bill on Enrollment: ACS

Dependent Variable	Vet Mean	(1)	(2)	(3)	(4)	(5)	(6)
College	0.26	0.0165***	0.0170***	0.0174***	0.0263	0.0309***	0.0445***
		(0.00470)	(0.00467)	(0.00482)	(0.0178)	(0.0112)	(0.00496)
Public	0.22	0.0121**	0.0124**	0.0122**	0.0148	0.0204**	0.0345***
		(0.00482)	(0.00487)	(0.00500)	(0.0179)	(0.00906)	(0.00547)
Private	0.04	0.00444	0.00458*	0.00522*	0.0115*	0.0105*	0.0100***
		(0.00270)	(0.00271)	(0.00284)	(0.00603)	(0.00553)	(0.00331)
N		627,282	627,282	522,208	522,208	593,798	614,592
State of Residence Fixed Effects		No	Yes	Yes	Yes	Yes	Yes
Exclude ACS 2009		No	No	Yes	Yes	No	No
Veteran Trend		No	No	No	Yes	No	No
Vet Restriction: Active Last Year		No	No	No	No	Yes	No
Vet Restriction: Not in Military		No	No	No	No	No	Yes

Note: Each cell is a separate regression. Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All specifications include age, age*veteran, race, and sex indicators as well as year and state of birth fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 3
Impact of Post-9/11 GI Bill on Enrollment: ACS
Unemployment Controls Included

	(1)	(2)	(3)	(4)	(5)	(6)
Post-9/11 Impact	0.0207***	0.0225***	0.0400**	0.0426*	0.0420**	0.0394***
	(0.00742)	(0.00781)	(0.0190)	(0.0242)	(0.0179)	(0.00853)
UR	0.00502***	0.00130	0.00153	0.00152	0.00134	0.00137
	(0.00162)	(0.00123)	(0.00144)	(0.00144)	(0.00126)	(0.00128)
UR*Veteran	-0.00134	-0.00171	-0.00526	-0.00519	-0.00309	0.00156
	(0.00157)	(0.00166)	(0.00381)	(0.00381)	(0.00387)	(0.00176)
N	627,282	627,282	522,208	522,208	593,798	614,592
State of Residence Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Exclude ACS 2009	No	No	Yes	Yes	No	No
Veteran Trend	No	No	No	Yes	No	No
Vet Restriction: Active Last Year	No	No	No	No	Yes	No
Vet Restriction: Not in Military	No	No	No	No	No	Yes

Note: Each column is a separate regression. Robust standard errors clustered at the state level in parentheses. All specifications include age, age*veteran, race, and sex indicators as well as year and state of birth fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 4
Impact of Post-9/11 GI Bill on College Enrollment: ACS
Demographic Heterogeneity

Subgroup	N	Vet Mean	(1)	(2)	(3)	(4)	(5)	(6)
Males	326,751	0.24	0.0213***	0.0219***	0.0230***	0.0339*	0.0367***	0.0528***
			(0.00502)	(0.00501)	(0.00518)	(0.0190)	(0.0131)	(0.00562)
Public		0.20	0.0147***	0.0151***	0.0149**	0.0180	0.0196*	0.0406***
			(0.00524)	(0.00530)	(0.00562)	(0.0198)	(0.0114)	(0.00629)
Private		0.04	0.00653**	0.00677**	0.00807***	0.0159***	0.0171***	0.0122***
			(0.00279)	(0.00279)	(0.00291)	(0.00545)	(0.00491)	(0.00343)
White	231,593	0.23	0.0253***	0.0253***	0.0251***	0.0429*	0.0515***	0.0570***
			(0.00595)	(0.00595)	(0.00606)	(0.0227)	(0.0148)	(0.00716)
Public		0.20	0.0175***	0.0175***	0.0165**	0.0246	0.0290**	0.0418***
			(0.00589)	(0.00595)	(0.00620)	(0.0214)	(0.0123)	(0.00676)
Private		0.04	0.00782***	0.00783***	0.00854***	0.0183***	0.0224***	0.0152***
			(0.00272)	(0.00270)	(0.00279)	(0.00678)	(0.00608)	(0.00392)
Black	40,975	0.22	0.0312*	0.0320*	0.0331*	0.0660	0.0639	0.0514**
			(0.0173)	(0.0170)	(0.0171)	(0.0405)	(0.0402)	(0.0193)
Public		0.18	0.0161	0.0165	0.0163	0.0433	0.0641	0.0422**
			(0.0163)	(0.0163)	(0.0163)	(0.0380)	(0.0467)	(0.0209)
Private		0.04	0.0151	0.0155	0.0168	0.0227	-0.000152	0.00916
			(0.0121)	(0.0120)	(0.0116)	(0.0166)	(0.0174)	(0.0123)
Females	300,531	0.37	0.0115	0.0120	0.0125	0.00418	0.0211	0.0217
			(0.00909)	(0.00905)	(0.0112)	(0.0365)	(0.0268)	(0.0135)

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Public	0.30	0.0111	0.0119	0.0132	0.00355	0.0346	0.0157
		(0.00752)	(0.00746)	(0.00865)	(0.0387)	(0.0230)	(0.0112)
Private	0.06	0.0004	0.0002	-0.0006	0.0006	-0.0135	0.0061
		(0.00655)	(0.00656)	(0.00695)	(0.0193)	(0.0199)	(0.00809)
State of Residence Fixed Effects		No	Yes	Yes	Yes	Yes	Yes
Exclude ACS 2009		No	No	Yes	Yes	No	No
Veteran Trend		No	No	No	Yes	No	No
Vet Restriction: Active Last Year		No	No	No	No	Yes	No
Vet Restriction: Not in Military		No	No	No	No	No	Yes

Note: Each cell is a separate regression. Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All specifications include age and age*veteran indicators as well as year and state of birth fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. Sample sizes provided for columns (1) and (2). Sample sizes somewhat smaller for columns (3)-(6). ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 5
Impact of Post-9/11 GI Bill on College Enrollment of Separated Veterans: ACS Geographic Heterogeneity in Combined Tuition and Housing Benefit

	Full Sample	Males
Post-9/11 Impact	0.0268*	0.0217
	(0.0137)	(0.0133)
Post-9/11 Impact*Combined Tuition and Housing	0.0007	0.0012***
	(0.0005)	(0.0004)
Post-9/11 Impact	0.0329***	0.0392***
	(0.00644)	(0.00745)
Post-9/11 Impact*(Combined Benefit > Median)	0.0235**	0.0268**
	(0.0104)	(0.0117)
Effect by Quartile of Combined Tuition and Housing		
Post-9/11 GI Bill Impact: Quartile 1	0.0356***	0.0404***
	(0.00943)	(0.0103)
Post-9/11 GI Bill Impact: Quartile 2	0.0382***	0.0438***
	(0.00787)	(0.00742)
Post-9/11 GI Bill Impact: Quartile 3	0.0491***	0.0528***
	(0.0131)	(0.0146)
Post-9/11 GI Bill Impact: Quartile 4	0.0580***	0.0709***
	(0.0147)	(0.00964)

Note: Coefficients in the top panel are for the interaction of post-2009 indicator variable with veteran status and triple interaction with combined benefit maximum (robust standard errors clustered at the state level in parentheses). Each cell in the bottom panel represents a separate regression. Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). "Combined Tuition and Housing" is a generated variable equal to 9*BAH plus 24*Tuition Maximum where BAH is the monthly basic allowance for housing. Benefits are in thousands of dollars. All specifications include age, age*veteran, race, and sex indicators as well as year, state of birth, state of residence, and state of residence*veteran fixed effects. The top panel includes the full set of interactions between Post-2009, veteran status, and the state benefit level. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 6
Impact of Post-9/11 GI Bill on College Enrollment of Separated Veterans: ACS Geographic Heterogeneity in Tuition and Housing Benefits

	Full Sample	Males
Post-9/11 Impact	-0.0008	0.0051
	(0.0231)	(0.0250)
Post-9/11 Impact*BAH	0.0034*	0.0028
	(0.0019)	(0.0020)
Post-9/11 Impact*Tuition	0.00033	0.0010**
	(0.00037)	(0.0004)

Note: Coefficients are for the interaction of post-2009 indicator variable with veteran status and triple interaction with each benefit maximum (robust standard errors clustered at the state level in parentheses). "BAH" and "Tuition" are a generated variables equal to 9*BAH and 24*Tuition Maximum respectively, where BAH is the monthly basic allowance for housing. Benefits are in thousands of dollars. All specifications include age, age*veteran, race, and sex indicators as well as year, state of birth, state of residence, and state of residence*veteran fixed effects and the full set of interactions between Post-2009, veteran status, and the state benefit levels. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

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Table 7
Impact of Post-9/11 GI Bill on Enrollment of Separated Veterans: CPS
Type of Schooling: Linear Probability Estimates

Dependent Variable	Vet Mean	Full Sample	Males
College	0.28	0.0607**	0.0598**
		(0.0259)	(0.0286)
Full-Time	0.23	0.0608***	0.0711***
		(0.0221)	(0.0249)
Part-Time	0.06	0.00259	-0.00826
		(0.0173)	(0.0183)
Four-Year	0.17	0.0507**	0.0533*
		(0.0249)	(0.0298)
Public	0.14	0.0347	0.0452*
		(0.0221)	(0.0244)
Private	0.03	0.0160	0.00811
		(0.0104)	(0.0113)
Two-Year	0.11	0.0126	0.00952
		(0.0215)	(0.0199)
Two-Year Public	0.10	0.0111	0.00438
		(0.0208)	(0.0201)
N		30,969	15,630

Note: Each cell is a separate regression. Coefficients are for interaction of post-2008 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All regressions include age, age*vet, and race indicators as well as state of residence fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a college degree from October CPS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 8
Impact of Post-9/11 GI Bill on Enrollment of Separated Veterans: CPS
Average Marginal Effects from Multinomial Logit Estimation

Choice	Full Sample	Male
Not Enrolled	-0.0647**	-0.0621**
	(0.0296)	(0.0275)
Two-Year	0.0141	0.0085
	(0.0211)	(0.0195)
Four-Year	0.0507***	0.0536**
	(0.0190)	(0.0214)
N	30,969	15,630

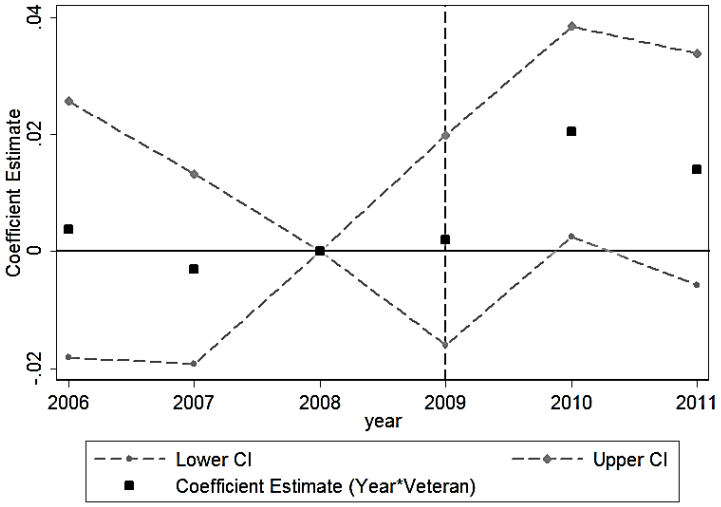
Note: Average marginal effects for the veteran group are calculated for the cross derivative with respect to Post and Vet Status. Bootstrapped standard errors are in parentheses. All regressions include age, age*veteran, sex, and race indicators as well as state of residence and year fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a college degree from October CPS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 9
Impact of Post-9/11 GI Bill on Persistence and Transfer: CPS

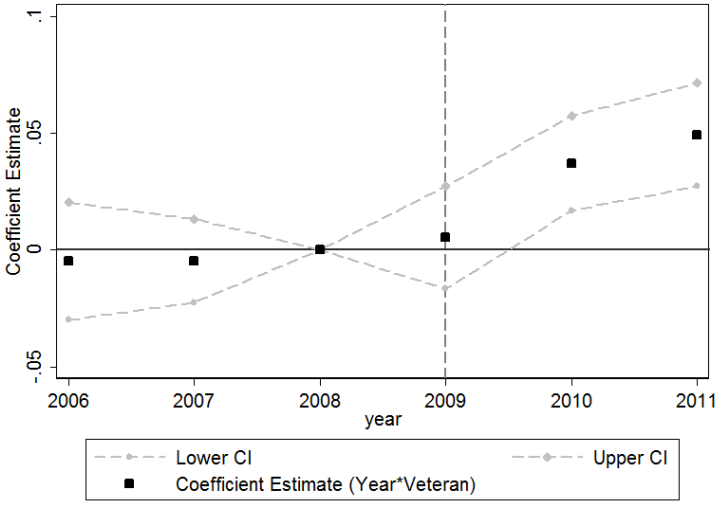
	ALL YEARS				EXCLUDE 2010 AND 2011	
	Post-2008 Effect		Post-2009 Effect		Post-2008 Effect	
Dependent Variable	Full Sample	Males	Full Sample	Males	Full Sample	Males
College	0.0864	0.0882	0.108	0.133**	0.0439	0.00670
	(0.0529)	(0.0578)	(0.0652)	(0.0636)	(0.0735)	(0.0951)
Four-Year	0.131*	0.142	0.124	0.127	0.163*	0.201*
	(0.0683)	(0.0846)	(0.0816)	(0.0935)	(0.0888)	(0.106)
Two-Year	-0.0469	-0.0507	-0.0176	0.00984	-0.117	-0.189**
	(0.0646)	(0.0684)	(0.0735)	(0.0741)	(0.0717)	(0.0735)
N	5,309	2,426	4,447	2,036	3,349	1,549

Note: Each cell is a separate regression. Coefficients are for interaction of Post-2008 or Post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). For example, Post-2008 is a dummy variable equal to one when an individual is observed in sample years 2009, 2010, or 2011. Regressions using Post-2009 indicator and interaction drop 2009 observations. All regressions include age, age*veteran, race, and sex indicators as well as state of residence and year fixed effects. Regressions are restricted to individuals enrolled at the freshman, sophomore, or junior level during the prior year. Coefficients are for interaction of post-2008 or post-2009 indicator variable with veteran status. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a college degree from CPS October years 2006-2009/2011.***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

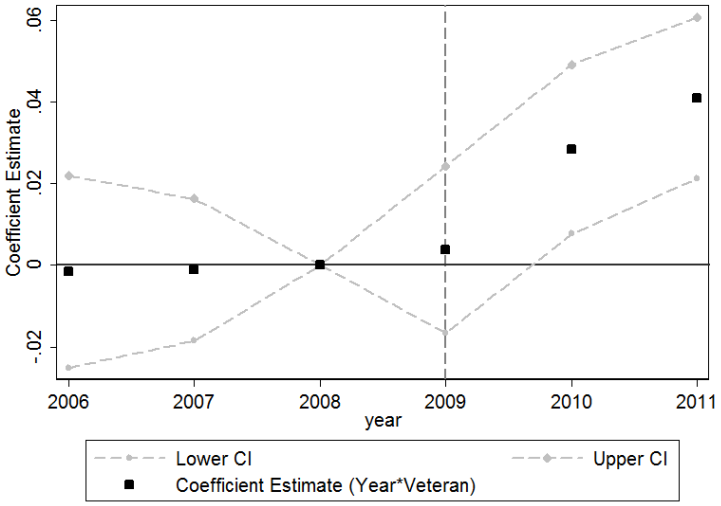
A: College



B: College (separated)



C: Public College (separated)



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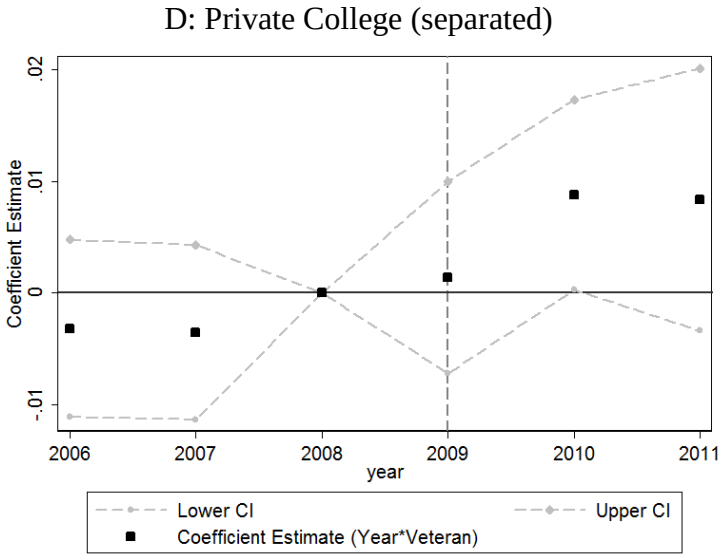

Figure A1
Event Study Figures

Note: Based on the author's calculations using the 2006-2011 ACS. Solid black squares plot coefficients on the interaction of veteran status and the respective year from a single regression. Sample restricted to individuals aged 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree. All regressions include sex, race, age, and age*vet indicators, and state of birth and residence fixed effects. Robust standard errors are clustered at the state level. Dotted lines represent upper and lower 95 percent confidence intervals. Each panel reports information from a separate regression with the dependent variable indicated by the title of the panel. "Separated" indicates that the event study includes only separated veterans in the veteran group.

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Table A1
Post-9/11 GI Bill Benefit Maximums

State	Tuition Per Credit Maximum	Fees Per Term Maximum	Basic Allowance for Housing	Annual Combined Maximum
Alabama	$292	$13,328	$1,010	$16,092
Alaska	$159	$13,429	$1,900	$20,915
Arizona	$657	$15,001	$1,243	$26,959
Arkansas	$200	$1,811	$880	$12,734
California	$336	$2,165	$1,877	$24,955
Colorado	$497	$45,388	$1,335	$23,941
Connecticut	$516	$2,509	$1,875	$29,261
Delaware	$356	$523	$1,572	$22,688
D.C.	$198	$620	$1,917	$22,003
Florida	$295	$62,000	$1,471	$20,322
Georgia	$434	$15,215	$1,103	$20,335
Hawaii	$282	$1,449	$1,972	$24,517
Idaho	$259	$2,821	$957	$14,828
Illinois	$575	$12,836	$1,425	$26,624
Indiana	$322	$12,438	$1,039	$17,075
Iowa	$324	$11,817	$921	$16,073
Kansas	$394	$3,804	$992	$18,380
Kentucky	$430	$11,235	$931	$18,708
Louisiana	$430	$2,623	$1,090	$20,131
Maine	$329	$2,805	$1,170	$18,430
Maryland	$458	$2,380	$1,721	$26,483
Massachusetts	$330	$17,787	$1,838	$24,464
Michigan	$990	$9,792	$1,129	$33,918
Minnesota	$750	$4,494	$1,242	$29,176
Mississippi	$449	$805	$995	$19,727
Missouri	$269	$9,863	$1,030	$15,722
Montana	$205	$4,250	$980	$13,746
Nebraska	$237	$1,574	$982	$14,523
Nevada	$136	$2,839	$1,289	$14,869
New Hampshire	$933	$4,977	$1,507	$35,944
New Jersey	$451	$5,736	$1,879	$27,723
New Mexico	$213	$11,342	$1,044	$14,497
New York	$1,010	$12,697	$2,076	$42,925
North Carolina	$494	$2,164	$1,058	$21,376
North Dakota	$410	$25,928	$933	$18,224
Ohio	$477	$15,134	$1,023	$20,656
Oklahoma	$151	$11,546	$918	$11,889

Oregon	$438	$22,190	$1,164	$20,985
Pennsylvania	$886	$6,391	$1,364	$33,540
Rhode Island	$343	$2,020	$1,674	$23,296
South Carolina	$484	$4,306	$1,076	$21,297
South Dakota	$93	$4,748	$916	$10,484
Tennessee	$248	$13,190	$1,041	$15,318
Texas	$1,471	$12,130	$1,240	$46,460
Utah	$209	$63,577	$1,119	$15,083
Vermont	$488	$2,511	$1,416	$24,453
Virginia	$326	$3,840	$1,358	$20,050
Washington	$380	$14,445	$1,303	$20,845
West Virginia	$267	$4,030	$924	$14,718
Wisconsin	$663	$30,979	$1,082	$25,648
Wyoming	$94	$4,335	$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 2010. Estimate of annual combined maximum is equivalent to 9*BAH + 24*Credit Maximum. Annual MGIB benefit level for a full-time student in 2008 was roughly $12,000.

Table A2
Impact of Post-9/11 GI Bill on Enrollment:
ACS
Robustness Checks

Dependent Variable	Vet Mean	All Veterans			Separated Veterans		
College	0.23	0.0185***	0.0182***	0.0149***	0.0447***	0.0446***	0.0455***
		(0.00456)	(0.00468)	(0.00470)	(0.00502)	(0.00499)	(0.00475)
Public	0.19	0.0143***	0.0136***	0.0122**	0.0355***	0.0349***	0.0371***
		(0.00451)	(0.00478)	(0.00475)	(0.00503)	(0.00533)	(0.00494)
Private	0.04	0.00420*	0.00460*	0.00265	0.00922***	0.00979***	0.00844**
		(0.00231)	(0.00268)	(0.00286)	(0.00301)	(0.00326)	(0.00350)
N		1,043,235	723,955	764,155	1,027,286	711,147	749,498
No Education Restriction		Yes	No	No	Yes	No	No
Educ < BA		No	Yes	No	No	Yes	No
HS < Educ < BA		No	No	Yes	No	No	Yes
Extended Sample: 2004-2011		No	No	Yes	No	No	Yes

Note: Each cell is a separate regression. Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All specifications include age, age*veteran, race, and sex indicators as well as year, state of birth, and state of residence fixed effects. Specifications equivalent to columns (2) and (6) of Table 2. Regressions are limited to individuals age 23-28 from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table A3
Impact of Post-9/11 GI Bill on College Enrollment: ACS
Control Group: National Guard and Reserve Training

	Full Sample	Males
Post-9/11 Impact	0.0286**	0.0290*
	(0.0111)	(0.0152)
N	38,387	29,958

Note: Each cell is a separate regression. Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All specifications include age, age*veteran, race, and sex indicators as well as year, state of birth, and state of residence fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. The sample is further restricted to include only individuals with national guard or reserve training (but not active-duty service) or those with active-duty service but not currently in the military. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table A4
Effects of Post-9/11 GI Bill on Migration: ACS
GI Bill Effect on Index of Combined Tuition and Housing (in thousands)

	(1)	(2)	(3)	(4)	(5)
Full Sample	-0.0712	-0.0354	-0.115	-0.0090	-0.0264
	(0.0784)	(0.0805)	(0.159)	(0.176)	(0.109)
Males	-0.0508	-0.0088	-0.115	-0.0557	0.0065
	(0.0934)	(0.0969)	(0.196)	(0.175)	(0.101)
Exclude ACS 2009	Yes	Yes	No	No	No
Veteran Trend	No	Yes	Yes	No	No
Vet Restriction: Active Last Year	No	No	No	Yes	No
Vet Restriction: Not in Military	No	No	No	No	Yes

Note: Each cell is a separate regression. "Combined Tuition and Housing" is a generated variable equal to 9*BAH plus 24*Tuition Maximum divided by a thousand (an estimated maximum benefits for a year). Coefficients are for interaction of post-2009 indicator variable with veteran status (robust standard errors clustered at the state level in parentheses). All specifications include age, age*veteran indicators, and year and state of birth fixed effects. Regressions are limited to individuals age 23-28 with at least a high-school degree (or equivalent) but less than a bachelor's degree from ACS years 2006-2011. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.