Fighting for Education: Non-traditional Students and Financial Aid

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Abstract

Older students now account for roughly half of college enrollees, yet little attention is given to the factors that influence these individuals' decisions to enroll in college or complete a degree. I explore how various forms of government aid affect post-secondary decisions. In particular, I estimate the effect of financial aid and unemployment insurance generosity and program parameters on individuals' decisions to enroll and persist in college. Each year, the federal government alone spends tens of billions of dollars on financial aid, yet little is known about how this aid affects older students. Unemployment benefits may provide a similar subsidy to the enrollment of displaced workers, particularly when these individuals are able to maintain benefit receipt while enrolled in college. Both programs reduce the cost of education and have the potential to ease credit constraints that may inhibit human capital investment. I focus on exploring the effects of these programs on two groups of non-traditional students: military veterans and displaced workers.

The first chapter of my dissertation, coauthored with Sarah Turner and forthcoming in the *Journal of Public Economics*, focuses on the college enrollment of displaced workers. We examine how changing state labor market conditions and state-specific variation in Unemployment Insurance (UI) interact to affect enrollment outcomes during the Great Recession. We identify a substantial role of the UI program in affecting post-secondary enrollment choices. We provide some of the first evidence that the duration of UI affects a displaced individuals propensity to enroll, and suggestive evidence that these effects are larger in states with more inclusive approved training laws. These findings identify a substantial overlap between UI policy and post-secondary enrollment decisions, indicating the potential importance of UI in not only providing income but also facilitating investments in skills.

In the second chapter of my dissertation, I explore the hypothesis that many academically prepared individuals enter the military as a way to overcome credit constraints that prevent immediate enrollment in college. I develop a model of human capital investment that

generates an empirical prediction to test for the presence of credit constraints among individuals considering military enlistment. I explore this question by examining the enlistment response of individuals to financial aid shocks. I find that the introduction of a merit-aid program decreases the probability that a male enlists in the military by .6 percentage points (a six percent reduction), and that these effects are concentrated among applicants that are more likely to qualify for merit scholarships. These effects are located mainly in low-income areas, supporting the argument that the effects on enlistment are a result of easing financial constraints. These findings potentially rationalize the relatively large number of high-achieving, but frequently low-income, individuals who enter the military for only a few years before separating and enrolling in college. The results also contribute to a broader literature focused on whether credit constraints play an important role in the human capital investment decision.

In the third chapter in my dissertation, I leverage large changes in financial aid generated by the Post-9/11 GI Bill to provide the first rigorous evidence of the effect of aid on the degree attainment of non-traditional students as well as the first evaluation of the longer run effects of the Post-9/11 GI Bill. One of the primary roadblocks to research in this area has been the lack of data that have sufficient numbers of veterans to track enrollment and degree attainment. I have created a new panel combining rich data on the choices of military service members from the Defense Manpower Data Center with postsecondary data from the National Student Clearinghouse (NSC). This dataset contains a large amount of information on the choices of over four million soldiers between 1998 and 2014 and will be a fruitful source for future research. Unique among the GI bills and other federal financial aid programs, the Post-9/11 GI Bill provides different benefit levels depending on both the state and ZIP code of enrollment. Individuals in some areas received almost no change in benefit levels, while those in others received tens of thousands of dollars in additional maximum benefit levels per year. Using multiple quasi-experimental strategies, I find that the higher level of financial aid provided by the Post-9/11 GI Bill increases the likelihood that a veteran obtains a degree within six years of separation by six percentage points, a 30 percent increase. This translates to an increase in degree attainment of between .5 and 1 percentage point for each $1,000 of additional financial aid. I use school specific variation in the size of the benefit increase to demonstrate that the persistence of infra-marginal students contributed to this effect. Finally, I show that a large fraction of veterans did not switch to the new GI Bill when it became available, leaving thousands of dollars in additional benefits on the table. This result bolsters concerns about informational complexity associated with accessing government programs, and motivates one element of an ongoing project that will randomly assign different information treatments to thousands of separating soldiers in the armed forces (with Kelli Bird, Ben Castleman, and Bill Skimmyhorn).

Together, these essays add to the limited evidence on the financial factors that affect the college enrollment and degree attainment of older non-traditional students. Future work will examine (1) whether the additional education gained by these non-traditional students translates into improved labor market or other outcomes, and (2) the extent to which personalized information can help individuals make optimal choices.

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Chapter 1

Out of Work and Into School: Labor Market Policies and College Enrollment During the Great Recession (co-authored with Sarah Turner)

1.1 Introduction

During the Great Recession, unemployment spiked to a nearly three-decade high, unemployment insurance (UI) was expanded to the highest maximum number of weeks ever, and post-secondary participation increased substantially. Notably, many of the additional participants in post-secondary education were somewhat older than recent high school graduates, as 87% of the increase in enrollment of 1.9 million students between 2008 and 2010 was among students 20 years of age and older (Digest of Education Statistics, 2012, Table 224). The sharp cyclical increase in enrollment is consistent with large increases in enrollment among those individuals who lost jobs, as well as those who were unable to find work. In turn, the availability of UI may allow unemployed workers to avail themselves of post-secondary educational opportunities. That there has been little attention to the postsecondary participation of the young unemployed in the economic analyses of student or social insurance programs is somewhat surprising given that roughly 15 to 20% were enrolled during the Great Recession.[1]

Variation in state-specific program parameters of UI can provide unemployed workers

[1] Statistics from the CPS indicate that roughly 13% of unemployed individuals aged 20-30 were enrolled in college between 2008 and 2011, while calculations from the SIPP suggest that between 15 and 20% of UI recipients aged 20-30 enrolled within 6 months of initial UI receipt over a similar period.

with markedly different incentives to enroll in post-secondary programs. Using the timing and magnitude of expansions to UI during the Great Recession, we examine whether variation in the number of weeks of UI offered to unemployed workers affected rates of college enrollment. This variation in UI policies was driven by state Extended Benefit (EB) program parameters, federal changes in the provision of Extended Unemployment Compensation (EUC) and the triggering on and off of UI benefit tiers (see Section 2 for details). We find that an additional 10 weeks of UI benefits increases enrollment likelihoods by around 1.8 percentage points, or by about 20 percent. These effects are driven primarily by enrollment in two-year institutions. This is the first evidence of which we are aware that expected benefit durations affect unemployed workers' propensity to enroll in school.

There is considerable variation across states in the type of post-secondary programs that are approved for individuals to pursue while maintaining UI eligibility. Some states limit approved training to explicitly vocational programs tied to specific occupations, while other states allow for the inclusion of broad academic courses of study in the definition. We are interested in understanding whether the impact of the length of unemployment insurance duration on the propensity to enroll in college depends on the range of courses that UI recipients can enroll in without losing benefits. We find suggestive evidence that the enrollment effects of UI benefit durations were larger in states with less strict approved training rules.

In the next section, we outline the overall variation in unemployment and UI policies, specifically considering the state-level variation of UI benefit durations and the types of post-secondary programs included in approved training for UI eligibility. In the third section, we describe the individual-level microdata from the October CPS and the information on UI approved training and UI benefit durations that we link with these data. In section four, we outline our empirical strategy. The fifth section presents results, and the final section concludes.

1.2 Unemployment, Active Labor Market Policies and Policy Variation

There is little question in the research literature that a variety of negative consequences follow from job loss (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010). In addition to losses in income, there is evidence that individuals are more likely to suffer health issues, end up on the disability rolls, or die following job loss (Sullivan and Von Wachter 2009; Autor and Duggan 2003). The losses in income themselves have been shown to be large and long lasting, with one study suggesting a 20 percent reduction up to two decades after job loss (Von Wachter, Song, and Manchester 2009).

The purpose of UI, which dates to state and federal programs introduced in the 1930s, is to relieve near term credit constraints and facilitate job search. Given the potential moral hazard associated with UI receipt, much research has focused on the effect of UI benefit extensions on the search effort and unemployment duration of UI recipients. While evidence from the 1970s and 1980s suggests a sizable positive effect of UI extensions on unemployment duration, most recent evidence finds, at most, small effects (Katz and Meyer 1990, Card and Levine 2000; Rothstein 2011). Furthermore, some researchers have argued that effects on job search reflect the relaxation of credit constraints rather than disincentives to search (Chetty 2008).

While UI compensation may have a role in extending unemployment durations, the sharp and persistent rise in unemployment associated with the Great Recession suggests several deeper issues may be at play. In particular, trends in the distribution of jobs since the 1990s have resulted in weak demand for workers and managers in blue collar occupations. One avenue for these individuals to improve their job prospects is through retraining and skill acquisition. Evaluations of a number of job-training and education programs indicate that they can help displaced workers successfully reenter the labor force (Meyer 1995; Jacobson, LaLonde, and Sullivan 2005, 2011). In particular, the returns to high-quality community college training have been shown to be high (Jacobson, LaLonde, and Sullivan 2005).

Despite the large bodies of research that explore the effects of UI and training on job search, reemployment, and later labor market outcomes, there is little research on how the state-level parameters of the UI system affect the decision to pursue post-secondary enrollment. While there is a substantial focus in the economics of education on how student aid policies affect college enrollment (Dynarski and Scott-Clayton 2013), much of this analysis focuses on recent high school graduates rather than individuals with some labor market experience.[2] There has been very little work that addresses how UI program parameters affect post-secondary enrollment.[3] We explore the effects of two types of variation in UI programs: (1) variation in UI benefit durations, and (2) variation in approved training provisions.

1.2.1 UI Benefit Duration

The expected length of UI coverage likely impacts decisions to pursue post-secondary training. With an extended UI benefit duration, an individual can plan a training investment with reduced concerns about credit constraints impeding his or her capacity to finish the program. While one would generally be concerned that the extension of benefits is correlated with other state economic conditions, there is also a substantial "haphazard" component to the rollout of additional weeks of benefits. Laws predating the Great Recession generally provided 26 weeks of benefits with an additional 13 or 20 weeks of Extended Benefits (EB) in high unemployment circumstances. Beginning in June 2008, a relatively ad hoc set of Congressional authorizations eventually raised statutory maximum benefit durations as high as 99 weeks in some states. Emergency Unemployment Compensation (EUC), which provided these additional benefits at the federal level, added a series of benefit "tiers". In November 2008, EUC was extended to 20 weeks for all states with an additional tier of 13 weeks of

[2] Notable exceptions are Barr (2013) and Barr (2014), which focus on veterans, and Seftor and Turner (2002), which focuses on older independent students.

[3] We note that Stafford loans and Pell grants may also play an important role in easing credit constraints and subsidizing the college enrollment of those displaced from their jobs. Increases in the generosity of these programs and a presidential initiative to promote the use of Pell grants among workers displaced from their jobs likely also contributed to the rise in enrollment of the unemployed over this time period (Barr and Turner 2013, Barr and Turner 2014).

benefits for individuals in states with unemployment rates over 6 percent. In November 2009, the second tier was extended from 13 to 14 weeks and made available in all states, regardless of unemployment rate. In addition, a third and fourth tier were added to the EUC program. The third tier provided an additional 13 weeks of benefits in states with unemployment rates over 6 percent, while the fourth tier provided an additional 6 weeks in states with unemployment rates over 8.5 percent. So, for example, by December 2009 individuals in states with unemployment rates over 8.5 percent were available for Tiers I-IV for a total of 53 weeks of EUC benefits (in addition to the 26 weeks of regular benefits and up to 20 weeks of EB benefits). Table 2 summarizes how the number of tiers and weeks available evolved over time.[4]

In addition, the American Recovery and Reinvestment Act (ARRA) provided full funding for EB. As states had initially shared the cost of this program, this led to a number of states altering their participation and trigger decisions. Different decisions on trigger conditions resulted in states with similar labor market conditions having different durations of UI benefits.[5] For example, both Alabama and Mississippi had unemployment rates over ten percent and insured unemployment rates under four percent during January of 2010. However, because Alabama chose a more generous trigger option, individuals in Alabama were eligible for an additional 20 weeks of benefits. Combining EUC (up to 53 weeks) and EB (up to 20 weeks) with regular benefits (usually 26 weeks), statutory benefit durations were extended to as long as 99 weeks in a number of states. Figure 1 illustrates the variation in benefit duration generated by changes in the EUC and EB programs and state unemployment rates crossing program trigger thresholds.[6]

[4] At several points, the EUC program expired and was subsequently reauthorized. As the EUC was always reauthorized and benefits paid retroactively, it is likely that individuals anticipated their reauthorization. The periods of EUC expiration only lasted 2, 10, 50, and 18 days. In the main analyses, we assume that individuals anticipated EUC reauthorization, but we also explore the robustness of the results to the assumption that individuals expected the EUC program to disappear permanently at the expiration date.

[5] Triggers are rules that determine what labor market thresholds (e.g., unemployment rate) must be crossed before extended benefits are provided.

[6] While Congress also expanded funds available through the Workforce Investment Program during this time period, we largely ignore the program as only a small share of WIA recipients receive formal college training.

1.2.2 Approved Training

Because UI program parameters are determined mainly at the state level, different states not only have different benefit levels but also employ varying criteria for the determination of approved training. Approved training rules determine whether a beneficiary would be allowed to enroll in college or job skills training while also receiving benefits. UI benefit recipients who choose to enroll in non-approved programs will forfeit benefit receipt regardless of whether or not they meet other search and work availability requirements. While virtually any undergraduate program would qualify in some states (e.g., California), other states limit qualified training to a much narrower set of explicitly vocational programs (e.g., Alabama). The second and third columns of Table A1 indicate that nearly all states approve of job skills programs or programs to improve long-term employability at institutions of higher learning. There is far more variation in the first column, as many states do not approve of academic coursework that is unrelated to a specific occupation.[7]

Figure 2 presents a map distinguishing the states in which academic courses not related to a particular occupation meet the definition of approved training for the UI program. There is variation both across and with regions. Furthermore, the approved training categorization does not seem to be correlated with state political factors, as demonstrated by the absence of a relationship between Democratic vote shares in the 2008 presidential election and the treatment of academic coursework for the UI program.[8]

1.3 Data

In order to investigate the effect of active labor market policies on college enrollment, we must observe both college enrollment and employment status. Furthermore, we need a

[7] As the approved training definitions are often vague, we also cross-check this information with student eligibility conditions contained in a separate government publication (USDOLETA 2004-2009). We describe this process in further detail in the data section.

[8] Regressing our binary variable for approval of academic on the state share voting for Obama during 2008 suggests a small and insignificant relationship between the two (point estimate of 0.003 (se 0.006) indicates that a 10 percentage point increase in the share voting for Obama results in a three percentage point increase in the likelihood that a state approves academic coursework).

measure of the date of unemployment in order to link individuals with the duration of UI benefits available in their state during the week that they became unemployed. While a large panel dataset that tracks both unemployment and enrollment would be ideal, the available panel datasets either have insufficient samples or fail to cover the relevant period.[9] Instead, we use the October Education Supplement to the CPS as it is the only large scale micro-level dataset to pose enrollment and employment questions to a broad range of ages on an annual basis over the time frame of our study.[10] We combine these data with information collected on UI benefit durations as well as state variation in approved training rules.

In addition to college enrollment information, the October CPS contains basic labor force information including employment status, the reason an individual is unemployed, and unemployment durations. The unemployment duration information allows us to infer the date on which an individual became unemployed and the relevant expected duration of UI benefits. However, the CPS does not indicate whether an individual is actually eligible for or receiving unemployment benefits. We proxy for eligibility using the self-reported reason for unemployment, classifying job losers and individuals on layoff as UI eligible, while job leavers, entrants, and re-entrants are considered non-eligible.[11]

We examine the period from 2004 through 2011 and restrict the sample to individuals aged 20 to 30. We present basic statistics for unemployed individuals in Table 1, separating the sample by enrollment status. On average, unemployed enrolled individuals are younger (23.0 vs 24.7), more likely to be female, and less likely to be Hispanic. They also have generally been unemployed for fewer weeks (20.3 vs 23.6). Among job losers, the disparity in age and unemployment duration is smaller, suggesting that much of the difference in mean age and unemployment duration between enrolled and non-enrolled individuals is driven by

[9]For example, the SIPP contains the relevant variables but has a small sample relative to the CPS, censoring issues, and a gap in coverage between the 2004 and 2008 panels that poses an issue for our identification strategy.

[10]The ACS did not survey individuals living in group quarters (e.g., dorms) until 2006. Furthermore, we are unable to determine when in the year an individual is enrolled without access to restricted data. In other CPS months, the enrollment question is limited to those 24 and younger.

[11]As discussed later, and in Rothstein (2011), there is likely misclassification here resulting in some individuals classified as non-UI eligible being eligible and vice-versa. Furthermore, our classification will likely overstate eligibility on average as we are unable to condition on employment and earnings eligibility criteria. This should bias our estimates of the effect of changing benefit durations towards zero.

"new entrants" and "re-entrants".

One limitation of the CPS data is that it is not feasible to measure persistence in enrollment or post-enrollment outcomes. Because the CPS limits questions about college enrollment to those ages 24 and younger outside the month of October, we are unable to use the rotation structure to track persistence or month-to-month enrollment. Similarly, with only October to October repeated observations per individual, we are limited in our capacity to observe long-term outcomes such as employment or wages.[12]

We match individuals in the October CPS to two measures of benefit durations provided to UI eligible individuals: (1) the number of weeks of benefits available to them upon becoming unemployed, and (2) the number of weeks available during August of the year in which they are interviewed. We derive the number of weeks of benefits available from detailed information on Extended Unemployment Compensation (EUC) and Extended Benefit (EB) benefits at a state-week level.[13] As illustrated in Figure 1, the EUC and EB triggers resulted in meaningful levels of variation across and within states over time.

We also collected data from four sources to categorize training eligibility for UI recipients by state: (1) a National Association of State Workforce Agencies survey (NASWA 2010), (2) correspondence with state employment commissions, (3) comparisons of state UI laws compiled by the Department of Labor, Employment, and Training Administration (DOLETA 2009), and (4) state websites and associated training documentation. The NASWA survey contains definitions of approved training for most states during 2009 as well as how those definitions changed from before to after the ARRA. We supplemented these definitions with information from state websites and associated training documentation. As the definitions are generally vague and open to interpretation, we instead use information captured in

[12] A related limitation is that we cannot track individuals who are laid off and then leave the labor force. If an individual leaves the labor force after being laid off, we do not observe that individual as being unemployed and cannot infer the date of displacement.

[13] Trigger notices obtained from `http://ows.doleta.gov/unemploy/claims_arch.asp`. The EUC program was allowed to expire several times. When this happened, individuals were allowed to finish any tier of benefits which they had started. As the EUC benefit expirations were very short and any missed benefits were provided retroactively, we ignore the expiration in most specifications. The results are robust to the alternative assumption that program expiration dates indicated the end of EUC benefits permanently (results available from authors upon request).

responses to “yes or no” questions posed to state workforce agencies regarding what types of programs count as approved training. The information from these combined efforts is contained in columns (1)-(3) of Table A1. The information in the second and third columns indicates that nearly all states approve of job skills programs or programs to improve long-term employability at institutions of higher learning.[14] As nearly all states approve both of these types of programs, we are unable to use the variation in the paper.

There is far more variation in response to the question “Are academic courses not leading to a specific occupation allowed as approved training?” in column (1). We view this question as a proxy for how open a state's approved training process is, setting $A_s = 1$ if a state answers “yes”.[15] While we do not use the information in columns (2) and (3), the information there is consistent with that in column (1) in that any state that does not approve of job skill programs or programs intended to improve long-term employability also does not approve of academic coursework unrelated to a specific occupation.

We view this as the best variable for estimation for four reasons. First, it captures some notion of whether a state approves of academic coursework for UI recipients. If a state does not allow individuals to receive UI benefits and pursue academic coursework, the benefit of additional weeks of UI for those choosing to go to college is essentially zero. Second, a review of the approved training definitions suggests that a key difference in approved training across states is whether or not the coursework is in pursuit of a specific, usually in-demand, occupation. States that restrict the set of approved programs to a subset of programs are reducing the benefit of additional weeks of UI benefits for those considering an academic program not in this subset. Third, the variable is objectively quantifiable. We did not create subjective rules to classify states based on definitions that make quantification difficult; instead, we are using a binary variable created by an external agency that appears to capture some notion of the openness of a state's approved training rules. Fourth, and

[14]The exact text of these questions is: (1) “Are job skills programs at community colleges or institutions of higher learning approved?”, and (2) “Are institutions of higher learning programs to improve long-term employability approved?”

[15]As several states were missing from the NASWA survey, we surveyed the missing states via e-mail and asked them the same questions.

of practical concern, there is variation across states that allows us to estimate effects. For example, while it would be of great interest to compare states that forbid all training to those that do not, there is insufficient variation to do so.

We also collected information from the United States Department of Labor Employment and Training Administration's (USDOLETA) 2004 through 2011 comparison of state unemployment insurance laws to corroborate the information captured in the first column of Table A1. We used the tables on "Treatment of Students" to determine: (1) whether students were ineligible for UI compensation, and (2) whether it was mandatory that they have prior work history while enrolled to be eligible. We then compared the responses to these questions with those in column (1) of Table A1, expecting that those states that approved academic training unrelated to a specific occupation would not indicate that students are ineligible for UI or that prior work history while enrolled was mandatory for a student to receive UI benefits. While the evidence was largely consistent with these expectations, we did find several discrepancies (Alaska, Connecticut, Ohio, and North Dakota) between academic approval and mandatory work history while enrolled.[16] We conducted additional research on training eligibility for students in each of these states and determined that individuals in Connecticut, Ohio, and North Dakota could enroll in approved training and receive UI benefits.[17]

1.4 Estimation Strategy

An individual's response to longer UI benefit durations depends on how the additional weeks of benefits affect the value of enrolling versus the value of searching for a new job. Most individuals looking for a job anticipate finding a new one within a few weeks or months; thus, absent credit constraints, the value of searching will increase by some fraction of the total

[16] The results are qualitatively robust to excluding the states whose classification is uncertain.

[17] Information available at: https://unemployment.ohio.gov/PDF/Workers_Guide_to_UC.pdf, http://www.ctdol.state.ct.us/progsupt/unemplt/claimant-guide/uc-288.pdf, http://www.jobsnd.com/sites/default/files/Request-for-Benefits-While-in-Training.pdf.

dollar amount of the additional weeks of compensation.[18] Most post-secondary programs are defined in academic units of quarters or semesters, which are often several months in duration. Certificate or degree programs generally require a number of courses, implying minimum duration of study of 6 months to a year to receive a certificate. Even after an individual finishes retraining, they must then search for a job, suggesting a longer period of UI receipt for those enrolled. Thus, the increase in the value of enrolling is likely to be substantially larger than to searching, especially when UI recipients can enroll in a wide range of courses and continue to receive UI benefits.

Using this framework, the institutional information on UI program variation at the state level leads to the following predictions about enrollment behavior. First, longer expected UI durations, ceteris paribus, will increase the likelihood of enrollment. Second, there will be stronger responses to increased expected UI durations in states with more open definitions of "approved" post-secondary programs. Training approval effectively lowers the cost of a particular program by allowing a UI recipient to enroll without losing benefits. Thus, as UI benefit durations increase, the value to enrolling increases more in states with a wider array of training programs that allow continued benefit receipt.

1.4.1 Unemployment Insurance Durations and Enrollment

We begin by exploring the effects of UI benefit durations on college enrollment. We examine how enrollment probabilities of unemployed individuals change as the duration of benefits available to those eligible for UI increases. As changes in state EB parameters, rollout of the EUC program, and unemployment triggers generate most within-state variation in benefit durations, we argue that after appropriately controlling for state labor market conditions, the changes in benefit durations are plausibly exogenous. We consider enrollment among the unemployed as a function of benefit availability, D_{ist}:

[18] The presence of credit constraints will cause the value of searching to increase further with extensions of UI as it allows efficiency gains by making a longer search feasible (Chetty 2008).

$$E_{ijt} = \beta_1 X_{ijt} + \alpha_s + \lambda_t + P_z(UR_{st}; \delta) + \beta_2 D_{ist} + \epsilon_{itj} \tag{1.1}$$

We condition on individual covariates X_{ijt} including age, sex, and race indicator variables. Regressions also control for the UI replacement rate, flexible controls for unemployment duration, state fixed effects, and year fixed effects. We control for variation in state labor market conditions using a flexible function $P_z(UR_{st}; \delta)$ of the unemployment rate.[19] Here, δ is a vector of coefficients on various polynomials of the unemployment rate. The coefficient on the total weeks of benefits available β_2 indicates how further weeks of benefits influence an individual's propensity to enroll. We use two measures of D_{ist}: (1) the number of weeks available to an individual during the week that they became unemployed, (2) the number of weeks available to individuals during August of the year in which they were interviewed in October.

While we prefer tying individuals to the weeks of expected benefits available to them at the time of layoff, we pursue the second strategy for three reasons. First, it allows us to address concerns related to the possibility of endogenous layoff. If individuals are somehow able to time their layoffs to take advantage of greater UI durations, this is a potential threat to our identification strategy. Assigning UI benefit durations based on a particular time of year addresses this concern. Second, using a UI duration that is not connected to date of layoff allows us to conduct falsification exercises for individuals whose layoff dates are endogenous or nonexistent. Finally, as more college enrollment (even for individuals displaced from their jobs) occurs in the fall, the available weeks of benefits just prior to that point may be the most salient.

As discussed previously, it is crucial to control for state labor market conditions as the level of benefits covaries with the unemployment rate. We control for these conditions using a flexible function of the state-year unemployment rate $P_z(UR_{st}; \delta)$. In our main specification,

[19] In our primary analyses in the paper, we focus on the BLS unemployment rate.

we include UR_{st} as a quadratic.[20] Remaining variation in D_{ist} comes from the staggered rollout of the EUC, the triggering on and off of benefit tiers, and state decisions about participation in the optional EB program.[21]

There are some threats to the validity of our general strategy. First, as a weaker state labor market (higher UR_{st}) alters the relative value of enrolling, a failure to fully capture this effect might be picked up in the estimates of the impact of longer benefit durations, which are correlated with state labor market conditions.[22] In order to mitigate these concerns, we estimate a number of specifications which flexibly control for state labor market conditions. We also conduct a specification check by examining the benefit duration response among (1) unemployed non job-losers and, (2) the full sample excluding job losers, two groups that are unlikely to be affected by benefit extensions.[23]

An additional concern is that individuals who are enrolled in college may be more likely to report being unemployed if they are receiving UI benefits. As potential selection into reported unemployment would bias in favor of finding a positive effect of UI benefit extensions on enrollment, we explore this concern further in the results section.

1.4.2 Approved Training Policies and Enrollment

For those considering enrolling in college, additional weeks of benefits are more valuable in states with more flexible approved training enrollment policies. To examine the potential complementarity between UI training and post-secondary participation, we estimate the interaction of UI approved training receptivity and the college enrollment-unemployment relationship. We expect that the relationship between labor market contractions and college enrollment will be stronger in states that are more accepting of college enrollment for UI

[20] For the two specifications we use: (1) the measure of labor market conditions in August of the year y in which an individual is interviewed in October, and (2) the measure of labor market conditions in the month and year in which an individual became unemployed.

[21] For further details on the nature of the rollouts of these program, see Rothstein (2011).

[22] While decreased opportunity costs make enrolling more appealing, negative income shocks could bring credit constraints into play for many individuals, making the overall effect of state labor market conditions on enrollment ambiguous.

[23] As discussed above, there is some misclassification of non job-losers so smaller positive effects of benefit extensions in this population cannot be ruled out.

recipients. Figure 3 plots the change in unemployment rate against the change in the fraction of individuals aged 20 to 30 that are enrolled between 2006 and 2011. Each dot represents a state.

The plots in Figure 3 suggest an important role for approved training policies in determining enrollment as the correlation between changes in unemployment and changes in college enrollment is larger in states with more flexible approved training laws. Similarly, estimates from regressions of enrollment on state and year fixed effects and the unemployment rate and its interaction with approved training status indicate that the relationship between state labor market conditions and enrollment is stronger in states with more flexible training rules (Table A2). This relationship is in line with our expectations that a greater fraction of those displaced from their jobs will enroll in states with more lenient approved training policies. These correlations motivate our analysis of the effects of UI benefit extensions on enrollment and the role that approved training policies play in magnifying or attenuating this effect. In order to capture this, we estimate specifications including interaction terms of maximum benefit durations and our approved training variable A_s:

$$E_{ijt} = \beta_1 X_{ijt} + \alpha_s + \lambda_t + P_z(UR_{st}; \delta) + \beta_2 D_{ist} + \beta_3 D_{ist} * A_s + \epsilon_{itj} \tag{1.2}$$

We are interested in the coefficient β_3 on the interaction term of benefit duration and our proxy of a state's approved training openness. Our hypothesis is that individuals in states with more lenient approved training rules will be more responsive to UI benefit duration extensions.

1.5 Empirical Results

1.5.1 Unemployment Insurance and Enrollment

The substantial variation in UI Benefit extensions during the Great Recession allows for identification of how benefit duration affects post-secondary participation. We consider two specifications of benefit durations; in the first approach we measure the benefit at the time an individual becomes unemployed. Because our data measure enrollment during the fall, we also estimate specifications using the level of benefits available during the summer prior to an individual's October interview.[24] The latter methodology allows us to conduct a set of falsification tests for individuals without layoff dates.

Panel A of Table 3 contains estimates of the effect of the benefit duration available in August on whether or not an individual is enrolled in October. Here, the first three columns are restricted to job losers, the portion of the unemployed more likely to have been eligible for UI benefits. The estimate in column 1 indicates that an increase in the duration of UI benefits by ten weeks increases the probability of enrollment by a little over 2 percentage points – over 20 percent. Table A3 demonstrates the robustness of the results to different sets of controls, including a semi-parametric control that includes indicator variables for each point of the unemployment rate in addition to controlling for the unemployment rate linearly.

Panel B presents equivalent estimates from our preferred methodology, using the benefit duration available to individuals at the point when they became unemployed. We combine employment status and unemployment duration information to generate the week in which an individual became unemployed; we then link these data with the number of expected weeks available and labor market conditions at that point. Estimates are similar to, but somewhat smaller than, those presented in Panel A, with a ten-week benefit extension increasing the probability of enrollment by around 1.8 percentage points.[25]

[24] Specifically, we present estimates using the benefit levels available during August of the summer prior to the individual's October interview.

[25] Both sets of results are robust to varying the sample start or end date by a year as well including the unemployment

A remaining question of interest is at what types of schools is this marginal enrollment accruing. While individuals displaced from their jobs are somewhat more likely than traditional students to attend two-year schools, they are roughly equally likely to be attending a two or four-year school in the CPS data. Examining the UI duration effects separately by two and four-year enrollment, we find that the effects appear to be driven primarily by changes in two-year enrollment. In a regression where enrollment in a two-year college is the dependent variable, we find effects of about 1.2 percentage points for each ten weeks of additional benefits. The parallel regressions for enrollment at a four-year institution suggest effects of between .4 and .9 percentage points , though this parameter is not precisely estimated.

Falsification Exercises and Addressing Concerns

In the right two columns, we present similar estimates for groups that are less likely to be affected by benefit extensions. In column (4), we present results for non job losers, including job leavers, entrants, and reentrants. These unemployed individuals are substantially less likely to be eligible for UI benefits and thus should be minimally affected by benefit extensions. Our results are consistent with this expectation with a small point estimate that is insignificantly different from zero. In the last column, we present similar estimates for all individuals in the sample, excluding job losers.[26] We again find no effect of benefit durations on the likelihood of enrollment.[27]

A potential threat to the validity of our results concerns the degree to which individuals who are enrolled are more likely to indicate that they are unemployed if they are receiving UI benefits. If this selection effect is significant, it would bias the estimates in the direction we observe. We take two approaches to address this concern. First, we present results that

rate as a cubic or using the insured unemployment rate in place of the unemployment rate.

[26]This includes all individuals aged 20-30 in the October CPS sample regardless of employment status (with the exception of job losers).

[27]We do not present analogous results for Panel B of Table 3 as individuals that are not unemployed do not have an unemployment duration. Similarly, for unemployed non-job losers, the start of an unemployment spell for the sample of job leavers, entrants, and reentrants is endogenous; however, we have run this specification and the estimate is small, negative, and statistically indistinguishable from zero.

interact UI benefit durations with an indicator variable for whether an individual has been unemployed for longer than 26 weeks. As all states provided at least 26 weeks of benefits during this time period, there should be no selection effect for individuals unemployed fewer than 26 weeks. If selection is driving the results in Table 3, we would expect to see the main effect disappear and be picked up by the interaction term. Results in Appendix Table A4 suggest that this is not the case with similar coefficients on the main terms and very small coefficients on the interaction term that are not statistically different from zero.[28]

Our second approach to estimating the degree to which this selection is operating is to examine the distribution of unemployment durations for individuals enrolled in college. If enrolled individuals are likely to change their unemployment status based on benefit receipt, we should see a cliff in the distribution around benefit exhaustion. Figure A1 presents these distributions and demonstrates that there is no evidence of a discontinuity at the point of benefit exhaustion. More formally, we implement the McCrary Density test, finding no significant differences in the density of unemployment durations around the point of likely benefit exhaustion (McCrary 2008).[29] These results suggest that selection of enrolled individuals into reported unemployment is not driving the results.

Another common concern when using within state variation is the presence of preexisting trends. If there was already an upward trend in the enrollment of job losers in states that experienced larger UI benefit durations during the Great Recession, our results may be only detecting this differential trend. In Table A5 we test for differential trends in enrollment during the pre-recession period by interacting a trend with the mean duration of UI benefits available in a state between 2008 and 2011. We find relatively small, negative, and insignificant point estimates, suggesting that differential trends are not a concern.

[28]Column (2) is equivalent to the main specification in the paper.

[29]In contrast, Panel B demonstrates a spike and then drop in unemployment duration at the point of benefit exhaustion for job losers not enrolled in college.

Interaction of Approved Training Policies and UI Benefit Durations

Finally, we estimate whether the UI benefit duration effects are magnified by a state's openness to broad based academic programs in its definition of approved training. Table 4 contains the coefficients on the interaction of UI benefit duration and our proxy for approved training openness. The coefficients in column (1) suggest that most of effect is occurring in states that have more lenient approved training policies. The estimate from our preferred specification indicates that an extension of ten weeks increases the likelihood of enrollment by 1.5 percentage points more in states that meet our definition of approved training openness. We estimate the interaction effect separately for two-year and four-year colleges. While the main effects in Table 3 are primarily driven by the two-year schools, we view our measure of approved training leniency as one that may have more of an effect on whether four-year schools are approved; in particular because most vocational type programs are at two-year schools.[30] Empirically, the interaction effect sizes for two-year schools and four-year schools are statistically indistinguishable.

As a specification check, we test for the presence of differential preexisting trends by training classification during the pre-Great Recession period. We find very small point estimates that are insignificantly different from zero, suggesting that preexisting trends are not responsible for the difference in response to increases in UI benefit durations across states (Table A6).

Combined, these results support the argument that UI parameters may play an important role for those displaced from their jobs in reducing the cost of investing in additional human capital. While we are unable to distinguish this effect empirically, it is likely that a portion of the enrollment effect is driven by an easing of credit constraints that would otherwise make enrollment infeasible.

[30] See `www.nces.ed.gov/pubs2010/2010167.pdf` (Tables 2-4).

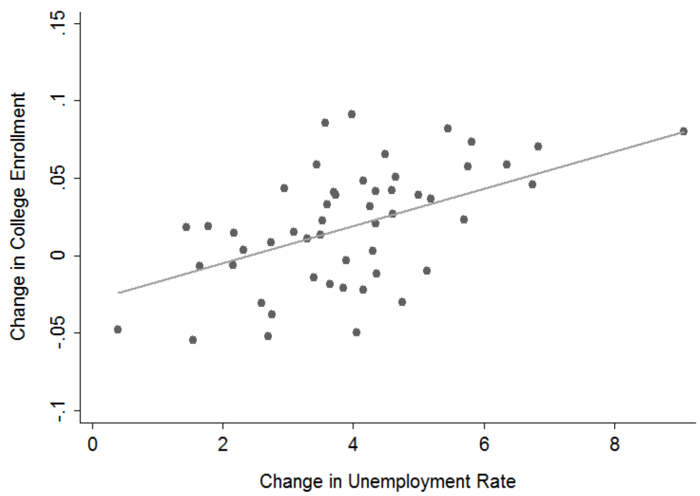
1.6 Conclusion

Economic theory and empirical evidence point to post-secondary enrollment as a potential transitional activity for workers who have lost jobs and are receiving UI. Overall, we observe that between 15 and 20% of young UI recipients enrolled in a post-secondary program during the Great Recession. The decisions to enroll for this population are likely affected by UI program parameters, including the duration of benefits and the extent to which general, non-vocational training programs are permitted with UI receipt.

We find that UI benefit durations play an important role in affecting the cost of enrollment. The duration of unemployment benefits available has a substantial impact on enrollment propensities, with an additional 10 weeks of benefits increasing enrollment likelihoods by around 1.8 percentage points, implying a relative adjustment of around 20%. Much of the marginal enrollment appears to accrue at two-year schools. When we allow the effect of additional weeks of benefits to vary by our proxy for flexibility in approved training, we find suggestive evidence that the enrollment effects of UI benefit durations were larger in states with less strict approved training rules.

These findings identify a substantial overlap between UI policy and post-secondary enrollment decisions, indicating the potential importance of UI in not only providing income but also facilitating investments in skills. Whether the demonstrated responsiveness of enrollment to benefit generosity complements the objective of helping workers invest in skills that improve long-term labor market outcomes is an important question for future work.

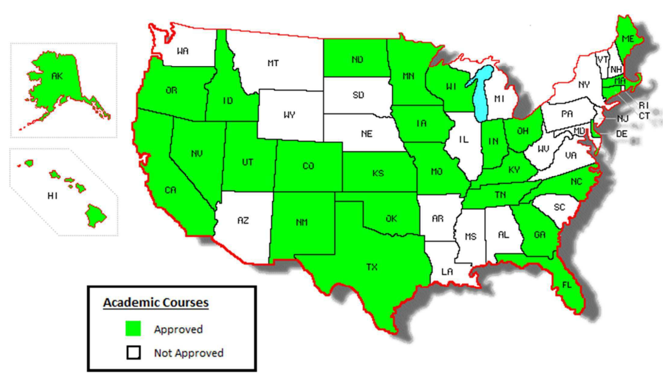
Figure 1: College Enrollment and Unemployment Rate Changes During Great Recession



Panel B: Great Recession

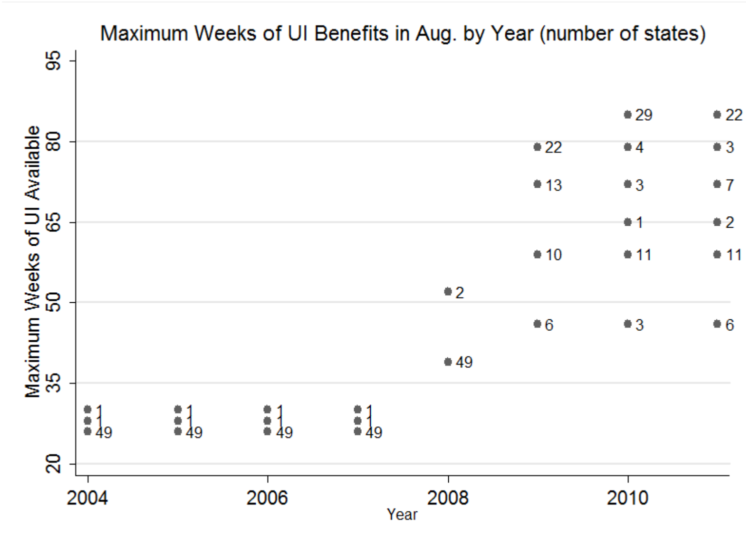
Note: Figure plots the unemployment rate change against the weighted college enrollment rate change. The slope of the regression line is .0132 (s.e. 0.0026). We use the October CPS surveys from 2006 and 2011. Results are robust to using official NBER recession dates or other small perturbations to chosen years.

Figure 2: State Approved Training for UI recipients



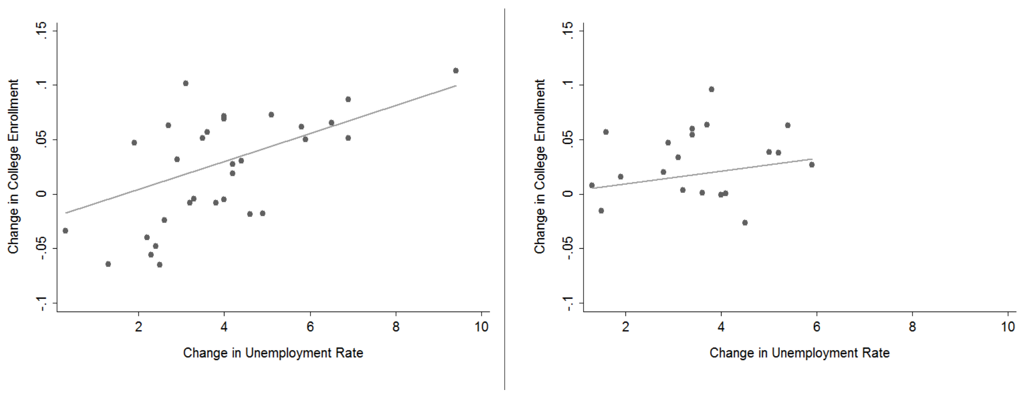
Note: Indicates state response to the question "Academic courses not leading to a specific occupation allowed as approved training?" Data obtained from correspondence with state employment commissions, state websites, and a National Association of State Workforce Agencies survey (NASWA 2010). See Appendix B for additional information.

Figure 3: Variation in Maximum UI Benefit Duration (Aug.)



Note: The figure presents the distribution of states by maximum weeks of UI benefits available in August of each year, by year (see Data Appendix and Rothstein (2011) for additional information).

Figure 4: College Enrollment and Unemployment Rate Changes During Great Recession

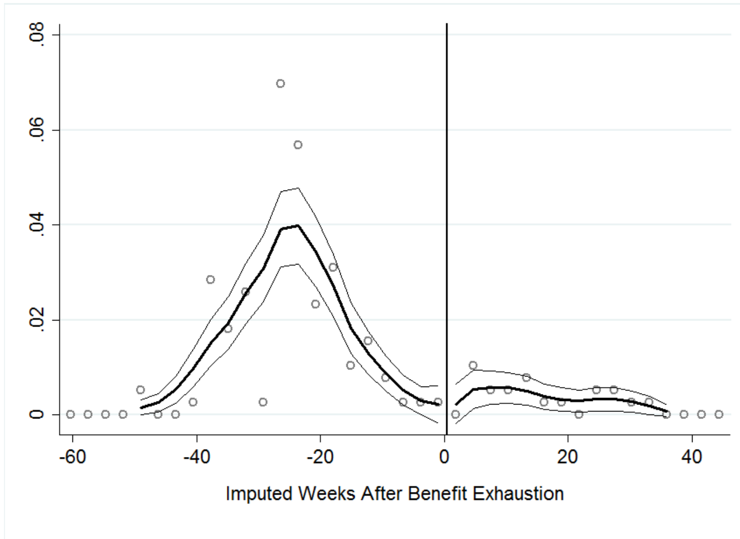


Panel A: Approved

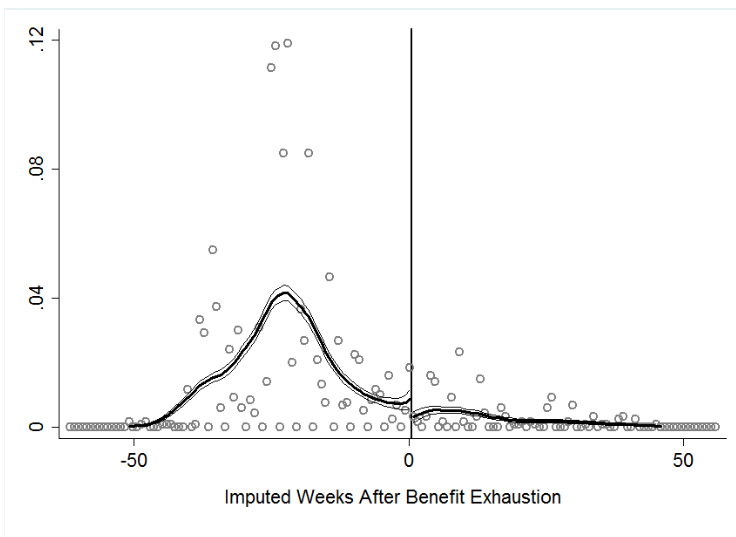
Panel B: Not Approved

Note: Each data point plots the unemployment rate change and the weighted college enrollment change for individuals 20-30 between October 2006 and 2011 as described in the note to Figure 1 (October CPS). Panel A contains the states that approve coursework not related to a specific occupation. Panel B contains the states that do not approve this type of coursework (see Data Appendix for additional details). The slopes of the superimposed regression lines are .0128 (se .0035) and .0058 (se .0056) respectively.

Figure A1: Distribution of Imputed Time Until Benefit Exhaustion



Panel A: Job Losers Enrolled in College



Panel B: Job Losers Not Enrolled in College

Note: The figure presents the imputed weeks after benefit exhaustion (calculated as an individual's unemployment duration minus the weeks of benefits available at the beginning of unemployment). Distributions are restricted to 60 weeks on either side of benefit exhaustion. Dark solid lines are fitted using local linear regression (95% confidence intervals are in grey) on each side of benefit exhaustion. The discontinuity estimates (log difference) are .154 (se 1.340) and -1.13 (se .37) for the top and bottom panels respectively.

Table 1.1

Average Characteristics of Unemployed by Enrollment Status: 2004-2011

	All		Job Losers		Non-Job Losers	
Characteristic	Enrolled	Not Enrolled	Enrolled	Not Enrolled	Enrolled	Not Enrolled
Age	23.00	24.70	24.08	25.29	22.67	24.26
Male	0.49	0.56	0.49	0.65	0.49	0.49
Black	0.69	0.68	0.72	0.70	0.68	0.66
Hispanic	0.14	0.20	0.11	0.22	0.14	0.19
Unemp. Duration	20.33	23.55	22.62	21.39	19.64	25.17
N	1,182	7,486	282	3,139	900	4,347

Note: Unemployment duration is measured in weeks. Includes unemployed individuals aged 20-30 from October CPS years 2004-2011. Averages calculated using CPS weights. Job Losers defined as those who lost their job or were laid off. Non-Job Losers defined as those who are job leavers, re-entrants, or new entrants.

Table 1.2

Changes in Emergency Unemployment Compensation (EUC)

	Weeks available under EUC Tier			
Start Date	I	II	III	IV
June 30, 2008	13			
November 21, 2008	20	13 C		
February 17, 2009	20	13 C		
November 6, 2009	20	14	13 C	6 H

Note: Each cell indicates the number of weeks of UI benefits available under each tier of EUC beginning on a particular date. For example, by June 30, 2008 individuals in all states were eligible for an additional 13 weeks of EUC UI benefits. In contrast, by February 17, 2009 individuals in all states were eligible for an additional 20 weeks of EUC UI benefits in all states and in states meeting certain provisions individuals were eligible for an additional 13 weeks for a total of 33 weeks of EUC UI benefits. "C" indicates that the tier of benefits is only available in states with unemployment rates over 6 percent. "H" indicates that the tier of benefits is only available in states with unemployment rates over 8.5 percent. The table ignores expirations of EUC program occurring in February, April, June, and November of 2010. The periods of EUC expiration only lasted 2, 10, 50, and 18 days respectively.

Table 1.3

Impact of Weeks of UI Benefits: 2004-2011

	Outcomes for Job Losers			Unaffected Populations	
				(Non Job Loser)	(All But Job Losers)
	Enrolled	Two-Year	Four-Year	Enrolled	Enrolled
Panel A: August Benefit					
Benefits Weeks / 10	0.0208**	0.0123**	0.0085	0.0026	0.0033
	(0.0086)	(0.0052)	(0.0073)	(0.0115)	(0.0086)
N	3,421	3,421	3,421	5,247	159,705
Panel B: Date of Displacement					
Benefits Weeks / 10	0.0176**	0.0139**	0.0037	-	-
	(0.0074)	(0.0053)	(0.0062)	-	-
N	3,385	3,385	3,385	-	-

Note: Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October. Panel B uses the number of weeks of benefits available at the point they became unemployed. All specifications include year and state fixed effects. Individual covariates include age, sex, and race indicator variables, and following Rothstein (2011), unemployment duration controls include a quadratic in unemployment duration, the log of unemployment duration, and an indicator variable for individuals unemployed for a week or less. Regressions also control for the UI replacement rate. First three columns restricted to job losers (i.e., those that lost their job or were laid off). Last two columns focus on populations less likely to be affected by UI benefit durations and serve as specification checks. Unemployed individuals that are not job losers include job leavers, entrants, and new entrants. All individuals excluding job losers contains all individuals between 20 and 30 in the CPS sample that are not classified as job losers. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.4

Impact of Weeks of UI Benefits on Enrollment of Job Losers: 2004-2011

	Enrolled	Two-Year	Four-Year
Panel A: August Benefit			
*Academic Approved * Benefits Weeks / 10*	0.0129	0.0008	0.0121
	(0.0176)	(0.0115)	(0.0134)
N	3,421	3,421	3,421
Panel B: Date of Displacement			
*Academic Approved * Benefits Weeks / 10*	0.0152**	0.0070	0.0082
	(0.0074)	(0.0061)	(0.0053)
N	3,385	3,385	3,385

Note: "Approved" or "Not Approved" indicates whether "academic courses not leading to a specific occupation [are] allowed as approved training." Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October. Panel B uses the number of weeks of benefits available at the point they became unemployed. All specifications include year and state fixed effects as well as year by academic approved fixed effects. Individual covariates include age, sex, and race indicator variables, and following Rothstein (2011), unemployment duration controls include a quadratic in unemployment duration, the log of unemployment duration, and an indicator variable for individuals unemployed for a week or less. Regressions also control for the UI replacement rate. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2010). Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.A1

Variation in State Policies

State	Academic Courses Approved?	Job Skill Programs at CC or IHL Approved?	IHL Programs to Improve Long-Term Employability Approved	Work History While Enrolled for Eligibility?	Students Never Eligible for UI?
Alabama	No	No	No	No	No
Alaska	Yes	Yes	Yes	No	No
Arizona	No	Yes	Yes	Yes	No
Arkansas	No	Yes	Yes	No	No
California	Yes	Yes	Yes	No	No
Colorado	Yes	Yes	Yes	No	No
Connecticut	Yes	Yes	Yes	No	No
Delaware	Yes	Yes	Yes	No	No
DC	Yes	Yes	Yes	No	No
Florida	Yes	Yes	Yes	No	No
Georgia	Yes	Yes	Yes	No	No
Hawaii	Yes	Yes	Yes	No	No
Idaho	Yes	Yes	Yes	No	No
Illinois	No	Yes	Yes	No	No
Indiana	Yes	Yes	Yes	No	No
Iowa	Yes	Yes	Yes	No	No
Kansas	Yes	Yes	Yes	No	No
Kentucky	Yes	Yes	Yes	No	No
Louisiana	No	Yes	No	Yes	No
Maine	Yes	Yes	Yes	No	No
Maryland	No	Yes	No	No	No
Massachusetts	Yes	Yes	Yes	No	No
Michigan	No[1]	Yes	Yes	No	No
Minnesota	Yes	Yes	Yes	No	No
Mississippi	No	Yes	Yes	No	No
Missouri	Yes	Yes	Yes	No	No
Montana	No	Yes	Yes	No	Yes
Nebraska	No	Yes	No	Yes	No
Nevada	Yes	Yes	Yes	No	No
New Hampshire	No	Yes	Yes	No	No
New Jersey	No	Yes	Yes	Yes	No
New Mexico	Yes	Yes	Yes	No	No
New York	No	Yes	Yes	No	Yes
North Carolina	Yes	Yes	Yes	No	No
North Dakota	Yes	Yes	Yes	No	No
Ohio	Yes	Yes	Yes	No	No

Oklahoma	Yes	Yes	Yes	No	No
Oregon	Yes	Yes	Yes	No	No
Pennsylvania	No	Yes	Yes	No	No
Rhode Island	No[1]	Yes	Yes	No	No
South Carolina	No	No	No	No	No
South Dakota	No	Yes	No	No	Yes
Tennessee	Yes	Yes	Yes	No	No
Texas	Yes	Yes	Yes	No	No
Utah	Yes	Yes	Yes	No	No
Vermont	No	Yes	Yes	No	No
Virginia	No	Yes	Yes	No	No
Washington	No	Yes	Yes	No	No
West Virginia	No	Yes	Yes	No	No
Wisconsin	Yes	Yes	Yes	No	No
Wyoming	No	Yes	Yes	Yes	No

[1] Policy changed to allow "some" academic training following ARRA.

Note: "Academic Courses Approved?" indicates whether "academic courses not leading to a specific occupation [are] allowed as approved training." "Work History While Enrolled for Eligibility" indicates whether an individual must have been enrolled in school and/or had earnings while in school in order to be eligible for UI benefits while enrolled. See Section 3 of the text for additional information on data sources and definitions. Policies in place before and after the American Reinvestment and Recovery Act unless otherwise noted.

Table 1.A2

Impact of Weeks of UI Benefits on Enrollment of Job Losers: 2004-2011

	(1)	(2)	(3)
Panel A: August Benefit			
Benefits Weeks / 10	0.0241***	0.0208**	0.0220**
	(0.0084)	(0.0086)	(0.0090)
Panel B: Date of Displacement			
Benefits Weeks / 10	0.0141***	0.0176**	0.0163***
	(0.0049)	(0.0074)	(0.0058)
Unemployment Rate	Linear	Quad	Linear
UI Replacement Rate		Y	
Individual Covariates	Y	Y	Y
Unemp. Duration		Y	
Semiparametric UR Control			Y

Note: Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October. Panel B uses the number of weeks of benefits available at the point they became unemployed. All specifications include year and state fixed effects. Individual covariates include age, sex, and race indicator variables, and following Rothstein (2011), unemployment duration controls include a quadratic in unemployment duration, the log of unemployment duration, and an indicator variable for individuals unemployed for a week or less. The semiparametric UR control includes indicator variables for each point of the rounded unemployment rate. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2010). Sample sizes are 3,421 and 3,385 for Panels A and B respectively. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.A3

Impact of Weeks of UI Benefits on Enrollment of Job Losers: 2004-2011
Specification Check for Selection Effect

	(1)	(2)	(3)
Panel A: August Benefits			
Benefits Weeks / 10	0.0230***	0.0197**	0.0210**
	(0.0085)	(0.0088)	(0.0092)
*(Duration > 26)*BW / 10*	0.0023	0.0025	0.0023
	(0.0028)	(0.0034)	(0.0028)
Panel B: Date of Displacement			
Benefits Weeks / 10	0.0176***	0.0186**	0.0209**
	(0.0063)	(0.0076)	(0.0078)
*(Duration > 26)*BW / 10*	-0.0002	-0.0013	-0.0012
	(0.0035)	(0.0036)	(0.0037)
Unemployment Rate	Linear	Quad	Linear
UI Replacement Rate		Y	
Individual Covariates	Y	Y	Y
Unemp. Duration		Y	
Semiparametric UR Control			Y

Note: Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October. Panel B uses the number of weeks of benefits available at the point they became unemployed. All specifications include year and state fixed effects. Individual covariates include age, sex, and race indicator variables and, following Rothstein (2011), unemployment duration controls include a quadratic in unemployment duration, the log of unemployment duration, and an indicator variable for individuals unemployed for a week or less. The semiparametric UR control includes indicator variables for each point of the rounded unemployment rate. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2010). Sample sizes are 3,421 and 3,385 for Panels A and B respectively. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.A4

Pretrends in Enrollment by 2008-2011 Mean UI Benefit Duration

	(1)	(2)
Panel A: August Benefit		
*(2008-2011 Avg. Ben. Weeks / 10) * Trend*	-0.0092	-0.0085
	(0.0085)	(0.0086)
Panel B: Date of Displacement		
*(2008-2011 Avg. Ben. Weeks / 10) * Trend*	-0.0060	-0.0064
	(0.0103)	(0.0105)
Unemployment Rate		Linear

Note: Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October to compute an average for 2008-2011. Panel B uses the number of weeks of benefits available at the point individuals became unemployed to compute an average for 2008-2011. All specifications include year and state fixed effects. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2007). Sample sizes are 1,099 and 1,135 for Panels A and B respectively. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.A5

Pretrends in Enrollment by Academic Approved Designation

	(1)	(2)
Panel A: August Benefit		
*Academic Approved * Trend*	0.0035	0.0032
	(0.0143)	(0.0146)
Panel B: Date of Displacement		
*Academic Approved * Trend*	0.0041	0.0038
	(0.0116)	(0.0109)
Unemployment Rate	Linear	

Note: All specifications include year and state fixed effects. Panel A includes individuals in the October CPS samples from 2004-2007 while Panel B includes individuals whose unemployment spells began between 2004 and 2007. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2007). Sample size are 1,099 and 1,135 for Panels A and B respectively. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 1.A6

Impact of Weeks of UI Benefits on Enrollment of Job Losers: 2004-2011
Displaced Workers Anticipate That Expirations are Permanent

	(1)	(2)	(3)
Panel A: August Benefit			
Benefits Weeks / 10	0.0252**	0.0209*	0.0196
	(0.0120)	(0.0124)	(0.0124)
Panel B: Date of Displacement			
Benefits Weeks / 10	0.0123***	0.0118**	0.0149**
	(0.0046)	(0.0051)	(0.0057)
Unemployment Rate	Linear	Quad	Linear
UI Replacement Rate		Y	
Individual Covariates	Y	Y	Y
Unemp. Duration		Y	
Semiparametric UR Control			Y

Note: Panel A uses the weeks of benefits available to an individual in August of the year in which they are interviewed in October. Panel B uses the number of weeks of benefits available at the point they became unemployed. All specifications include year and state fixed effects. Individual covariates include age, sex, and race indicator variables, and following Rothstein (2011), unemployment duration controls include a quadratic in unemployment duration, the log of unemployment duration, and an indicator variable for individuals unemployed for a week or less. The semiparametric UR control includes indicator variables for each point of the rounded unemployment rate. Sample restricted to displaced unemployed individuals aged 20-30 from October CPS (2004-2010). Sample sizes are 4,138 and 3,385 for Panels A and B respectively. Robust standard errors clustered at the state level are in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Chapter 2

Enlist or Enroll: Credit Constraints, College Aid, and the Military Enlistment Margin

2.1 Introduction

Each year, millions of high school graduates choose between entering the labor market and investing in higher education. Some youth from low-income families underinvest in education due to the costs of college; thismay be especially true if these individuals lack access to credit or have high levels of debt aversion. As one avenue to overcome these constraints, individuals may join the military to access military education benefits. The military provides a variety of generous education benefits in return for an individual's service. These benefits rank among the top two reasons provided when individuals explain their motivations for joining the military.[1] In this paper, I explore the enlistment versus enrollment decision to estimate the importance of borrowing constraints in the college enrollment.

Recruits disproportionally come from the second and third lowest income quintiles, areas of the income distribution that are barely ineligible for most need-based financial aid.[2] These individuals may then enlist in the military because they are incapable of financing college

[1]The other motivation is job training (Youth Attitude Tracking Study (YATS) 1991-1994). The YATS is a survey conducted annually by the Department of Defense to collect information from youth on topics such as military enlistment expectations, military recruitment advertising, and future plans. See the Appendix for additional information.

[2]During the 1999-2000 school year, only 19% of dependents with family incomes in this range were Pell recipients. Nearly 60% of dependents with lower family incomes received a Pell grant (author's calculations using the 2000 National Postsecondary Student Aid Study (NPSAS)).

without access to military education benefits. One respondent to a 1998 survey on military enlistment expectations noted, "I'm sorry ... I can't afford [college for my children]... So I'm going to have at least two children gone to the Army" (Lehnus 2000). Another stated "I would have liked [my son] to go straight to college... He wants to have some sort of medical career... [But] we're not even a middle-class family... Money is an issue... the biggest idea of going in the Navy... was college money." These anecdotes are supported by Table 1, which separates individuals' enlistment motivations by their ability to pay yearly schooling and living expenses out of family earnings and savings. Among those able to pay 50-100% of college expenses, only 25% list education benefits as a motivation for joining the military. In contrast, among those able to pay 1-49% or none of college expenses, over 40% and 86%, respectively, list education benefits as a motivation.

Research on the existence of credit constraints in the college enrollment process is inconclusive. However, recent work suggests an increased role of credit constraints, furthering the argument that some individuals are enlisting in the military because they simply cannot afford to enroll in college otherwise (see Lochner and Monge-Naranjo (2011) for an overview of this work). Much work has been done to test for the presence of credit constraints in human capital investment decisions, yet most of it focuses on the relationship between family income and college enrollment and thus suffers from endogeneity concerns. Building on intuition developed by Cameron and Taber (2004), I present a new test relying on the differential effect of changes in immediate and delayed benefits for constrained and unconstrained individuals.

I leverage a series of financial aid shocks, provided by the introduction of 14 merit aid programs over 12 years, to identify the effect of changing financial aid conditions on the decision to enlist. Because nearly all statutes allow individuals to delay receipt of merit aid if they enter the active-duty military, these programs increase (1) the value of enrolling directly after high school and (2) the value of enlisting in the military and then enrolling.[3] The

[3]The ability to delay merit-aid receipt in Arkansas is unclear; all results are robust to the exclusion of Arkansas from the analysis, which is done implicitly for regressions for the 1995-2004 period as Arkansas had a merit-aid program for this entire period.

increase in the value of enlisting and then enrolling may be slightly smaller due to discounting, but the returns to both options increase by a similar amount.[4] Absent meaningful credit constraints, theory and prior empirical evidence on the effect of education subsidies predicts a minor response to the small subsidy generated by discounting. In contrast, theory predicts an observable decrease in enlistment if credit constraints are meaningful.

I use the American Community Survey (ACS) to illustrate that merit-aid introduction has an effect on the decision to enlist, and that for males this effect is stronger for those most likely to be eligible for merit aid (non-Hispanic white individuals). Using a dataset of all military applications and contracts between 1990 and 2004, I show that the effects are concentrated among those most likely to be affected by the introduction of merit-aid: higher ability recruits and those enlisting while in high school. I find smaller or null effects for groups less likely to be eligible for aid: dropouts and low aptitude individuals. Finally, I show that the effects are concentrated in low-income areas, supporting the argument that credit constraints motivated some individuals to enlist.

In the next section, I provide an overview of the prior work on military enlistment, post-secondary investment, and credit constraints. In Section 3, I provide background information on the recruits and merit aid programs. Section 4 provides the theoretical motivation for the test, Section 5 outlines the data, and Section 6 sets up the relevant tests. Section 7 presents the results, and Section 8 provides further discussion and concludes.

2.2 The Intersection of Enlistment, Enrollment, and Aid

There are established strands of research literature on the transition to college and the decision to enlist, yet researchers have largely ignored the intersection of the two. With the exception of a handful of papers that examine changing veteran education benefits (Angrist 1993; Angrist and Chen 2011; Barr 2015a; Barr 2015b; Bound and Turner 2002; Stanley 2003; Lemieux and Card 2001), research on the enrollment process largely ignores the mili-

[4] Furthermore, several programs cover "full tuition"; therefore, increases in the size of the merit award may outpace this discounting.

tary. Similarly, research on military enlistment largely ignores changes in the cost of many potential recruits' most likely outside option: college.[5]

This is surprising because 33% of those with plans to join the military mention education benefits as a motivation for enlisting.[6] During the 1990s and early 2000s, the Montgomery GI Bill (MGIB) was the primary education benefit provided by the military. Roughly half of military veterans used the benefit. Because military education benefits are set at the national level, the few studies that attempt to identify their effects use variation over time in benefit levels.[7] However, there is no published work of which I am aware that considers the effect of changes in non-military forms of financial aid on enlistment. Understanding this effect is particularly important if individuals join the military primarily because they cannot finance college.

The role of credit constraints in determining college enrollment is uncertain. Early work examining the relationship between family income and college enrollment in the 1980s found little evidence of credit constraints (Cameron and Heckman 1998, 1999; Carneiro and Heckman 2002). In a clever study, Cameron and Taber (2004) compared changes in the direct (ease of access) and delayed (earnings) costs and benefits of education, arguing that the effects should be equivalent in the absence of credit constraints. They found no evidence to suggest that credit constraints play an important role. However, work focusing on more recent cohorts demonstrates a stronger relationship between family income and enrollment (Belley and Lochner 2007). The relationship between income and college enrollment is suggestive, but researchers have noted that there may be other explanations for this correlation (Lochner and Monge-Naranjo 2011). In one of the only attempts to leverage an exogenous change in family resources, Lovenheim (2011) finds significant effects of changes in housing equity on college enrollment; these effects are particularly pronounced for low-income

[5] Instead the literature tends to focus on changes in military compensation, recruiting practices, and local labor market conditions (Brown 1985; Orvis and Asch 2001; Warner, Simon, and Payne 2003; Asch, Heaton, and Savych 2009).

[6] Author's calculations using the 1991-1994 versions of Youth Attitude Tracking Study (YATS).

[7] For example, a study by Warner, Payne, and Simon (1999) suggests the importance of the changing generosity of military education benefits (including the GI Bill) in driving changes in recruiting, finding that an increase in the level of education benefits of one percent results in a .2 percent increase in high quality accessions.

families.

Studies using data from single institutions are also mixed. Stinebrickner and Stinebrickner (2008) draw on a direct survey at a tuition-free institution in Kentucky to argue that credit constraints play a limited role in the dropout decisions of individuals already in college. In contrast, Rothstein and Rouse (2008) use the adoption of a no-loan policy to argue that credit constraints play an important role in student occupational choice and alumni donations at an elite private university. In sum, the evidence on the importance of credit constraints in higher education settings is inconclusive.[8]

2.3 Recruits and Merit Aid

Many recruits are qualified for college. Roughly 90% of active duty recruits have at least a high-school diploma and the average and median AFQT scores are both around 60, better than 60% of the population.[9] The bottom panel of Table 2 presents these statistics for all states, southern states, and only southern states that introduced a merit-aid program between 1987 and 2004. On average, recruits are more educated and higher scoring than the median young adult. Furthermore, nearly fifty percent of those with active-duty service obtained a high-school GPA over 2.5 and 24 percent over 3.0.

Although many potential recruits are academically prepared for college, they may be vulnerable to credit constraints due to their often low-income backgrounds. The poorest and richest quintiles of neighborhoods are underrepresented among military recruits, whereas the middle three are overrepresented; nearly 65% of late 1990s recruits lived in neighborhoods with median incomes between $29,382 and $52,068, ranges with limited eligibility for federal need-based financial aid (Kane 2006).[10] Thus, although many recruits appear academically

[8] Results from more recent structural models of lifecycle behavior are also mixed (see Lochner and Monge-Naranjo 2011 for an overview).

[9] An individual's Armed Forces Qualification Test (AFQT) score is determined from four components of the Armed Services Vocational Aptitude Battery. The AFQT is used in the National Longitudinal Survey of Youth as an aptitude measure.

[10] For the 1999-2000 school year, only 30% of dependent students from households with incomes between $30,000 and $39,999, 12% of those from households with incomes between $40,000 and $49,999, and 3% of those in the

prepared for college, they may not have the finances to enroll.

Merit aid availability may ease financial constraints by reducing the immediate cost of college.[11] State merit-aid programs have become increasingly prevalent over the last two decades as 14 states introduced a broad-based merit-aid program between 1991 and 2004.[12] In many states, individuals a with high-school GPAs of a 3.0 or, less commonly, a 2.5 are eligible for merit-aid. During the sample window, merit-aid programs provided between \$1,000 and \$4,000 per year, generally somewhat less than the approximately \$4,000 per year provided by the primary military education benefit, the MGIB.[13]

Receiving MGIB benefits or merit aid has no direct effect on eligibility for other types of aid. Merit aid receipt does not affect eligibility for MGIB benefits. Similarly, all but one state merit aid statute enacted during this time period have provisions explicitly allowing those who enlist upon graduation to delay receipt of merit aid for three or more years.[14][15] Thus, merit aid raises the value of both immediate and delayed (post enlistment) college enrollment; the following sections use this feature to develop a test for the presence of financial constraints.

\$50,000-\$59,999 received any level of Pell grant (author's calculations using 2000 NPSAS).

[11] Whereas much of the previous merit aid literature has found positive effects of state merit aid programs on enrollment, the evidence on attainment is mixed and sometimes even negative (Cornwell et al 2006; Dynarski 2000, 2003, 2008; Fitzpatrick and Jones 2012; Sjoquist and Winters 2012).

[12] Arkansas (1991), Florida (1997), Georgia (1993), Kentucky (1999), Louisiana (1998), Maryland (2003), Michigan (2000), Mississippi (1996), Nevada (2000), New Mexico (1997), South Carolina (1998), South Dakota (2004), Tennessee (2004), and West Virginia (2002). With the exception of Maryland, which only had a program for one year, all of the programs studied remained implemented throughout the remaining sample period. See the data appendix, Table A1, and Fitzpatrick and Jones (2012) or Sjoquist and Winters (2012) for more information on the merit aid programs and the distribution of pre and post merit aid cohorts.

[13] The Army College Fund and similar programs across services played an important role in increasing the size of MGIB payments somewhat during this time period. These education benefit bonuses were provided using objective measures of occupation, term of enlistment, and recruit quality set at the national level (Warner, Simon, and Payne 2003).

[14] Most states statutes indicate that those enlisting within a year after graduation may delay initial merit aid receipt until one year after separation as long as this occurs within five to six years after high school graduation. Other states have no requirement on using the benefits within a certain time period after graduation. The rules outlining the Arkansas program are unclear (full details from author's review of all relevant merit aid statutes available upon request).

[15] Furthermore, neither the MGIB nor merit aid are counted as income on the FAFSA; therefore, they do not affect Pell grant eligibility.

2.4 Setting up the Test

The paper's estimation strategy relies on the hypothesis that the availability of merit aid should have little effect on the military enlistment decisions of unconstrained individuals, but a measurable effect on the decisions of those constrained. This is because the introduction of merit aid results in a relatively tiny subsidy to immediate enrollment for unconstrained individuals, but a potentially large one for those facing constraints. This argument relies on the notion that unconstrained individuals are affected similarly by changes in the direct and delayed costs of college, whereas constrained individuals are affected more by changes in direct costs, an idea first developed by Cameron and Taber (2004). They compare the effects of changes in college availability (direct) and wage premiums (delayed), whereas I compare the effect of an immediate (direct) and post-enlistment (delayed) college subsidy.

Consider two individuals. The first (C) enrolls in college immediately, and the second (EC) enlists in the military and then enrolls in college.[16] Under free borrowing and saving, the introduction of merit aid provides a small relative increase in the lifetime income of the individual who chooses to enroll in college immediately, compared with the individual who delays enrollment to enlist in the military. Both receive the merit-aid subsidy, but the individual who delays receipt receives slightly less as a result of discounting. Because individuals are able to borrow and save freely, both types of individuals will smooth the increase in consumption across periods, resulting in similar increases in lifetime utility.

Now, consider the same two individuals when borrowing is limited. When borrowing is limited, individuals are not capable of using future earnings to increase consumption during earlier periods. These constraints drive a wedge between marginal utility across periods. I assume that the individual who enrolls in college immediately has less wealth at the point of enrollment than the individual who enlists and then enrolls. Given this assumption, the increase in lifetime utility provided by the merit aid will be substantially larger for the individual enrolling immediately.

[16]Empirically, nearly all (approximately 95%) individuals with a 3.0 GPA or higher who enlist in the military attend college within 6 years of initial enlistment, thus I ignore the option to enlist and never enroll.

In other words, the presence of credit constraints or high levels of debt aversion may distort agent optimization. Faced with lower levels of period one consumption (and utility) under the immediate enrollment option (C), individuals may choose to enlist and then enroll (EC) because the military affords individuals an opportunity to save money and provides them access to a number of education benefit programs. As period one marginal utility is higher under C, merit aid increases the relative value of immediate enrollment resulting in a reduction in enlistment. Therefore, a reduction in enlistment can be interpreted as evidence of credit constraints.

Most prior models of human capital investment predict that unconstrained investment is independent of wealth W, whereas constrained investment is increasing in wealth; this prediction is central to a large body of literature that tests for the presence of credit constraints by regressing enrollment on family income (Lochner and Monge-Naranjo 2011). Extending this logic to the current test, the effect of merit-aid introduction on enlistment should be decreasing in wealth only if individuals are constrained.[17]

2.5 Data

To implement the tests, I use the American Community Survey (ACS) and administrative military recruiting data. The ACS is a nationally representative one percent sample of the population conducted each year. I use data from the 2006 through 2011 samples of the ACS, which include active-duty military living in military housing.[18] The ACS contains information on military veteran and active-duty status in addition to state of birth, state of residence, and an array of demographic variables. I use the veteran and active-duty status variables to construct my binary outcome variable indicating whether an individual was ever

[17]The stylized model abstracts from several important considerations in the college-going decision related to child and parent preferences and the nature of higher education costs and benefits. For additional discussion of these issues, see the working paper version posted at `www.people.virginia.edu/~acb3u/research.htm`

[18]Prior to 2006, the ACS did not survey group quarters and thus are missing some active-duty military individuals. I also present a robustness check including the 2000 Census. Results are further robust to inclusion of the 2001-2005 ACS samples.

in the military. I restrict the sample to individuals aged 24 to 35.[19]

I examine effects on the composition of recruits using individual level data on military recruit applications and contracts from 1990-2004. I obtained these data via a number of FOIA requests to the Defense Manpower Data Center. These data indicate both the date of the application (or contract) and the five or three-digit zip code of the home of the individual applying. I use this information to construct the year and state in which the individual applied and contracted into the military. The data also contain the educational background of the potential recruit as well as his or her AFQT score, a piece of information that I do not observe in the ACS. I use these attainment and ability measures to help proxy for the likelihood of eligibility for merit aid. A further advantage of these data is the presence of detailed geographic information on the location of each recruit. I link zip codes to information on average resources in an area to proxy for the likelihood of constraints and test whether enlistment effects are concentrated in lower income areas.[20]

2.6 Estimation Strategy

To investigate the effect of changing financial aid conditions on the decision to enlist, I estimate the following equation:

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \lambda_t + \gamma_1 Aid_{st} + \epsilon_{ist} \tag{2.1}$$

Here, E_{ist} indicates whether individual i born in state s and aged 17 at time t has ever served in the military, and X_{ist} refers to observed traits of that individual, including age, sex, and race.[21] In Z_{st}, I control for changing state-level characteristics including the cohort

[19] Most individuals enlisting during the 1990s enlisted by the age of 24. The maximum age is equal to the maximum treated age in the sample.

[20] I link the data (1990-2000), at the 5 digit zip code level, to median household income levels. Further details of the construction of each dataset, the dates of merit-aid implementation, and the determination of delayed aid eligibility for enlistees are contained in the data appendix.

[21] I use age 17 because this is the age at which an individual can sign an enlistment contract. Furthermore, this places the marginal cohort in the control group and thus should bias me against finding an effect. Results are similar using the presence of a merit aid program at age 18 or excluding the marginal cohorts.

size and economic conditions (state unemployment rate) faced by an individual in state s at the time t that that individual was 17. The terms α_s and λ_t are fixed effects for each state of birth and year, respectively. The Aid_{st} variable is an indicator equal to one if an individual was born in a state which had a merit aid program when that individual was age 17. The parameter of interest, γ_1, identifies the effect of the presence of a merit aid program on an individual's propensity to enlist.[22]

The key identifying assumption is that there were no other changes taking place at the same time as merit-aid introduction that may have affected military enlistment. Although I cannot test this assumption directly, I include a large set of demographic and state level time-varying controls that may be important in the enlistment process. In particular, indicators for age, sex, and race control for changes in the composition of individuals within a state; cohort size controls for changes in the likelihood of being accepted into college or the military; and the state unemployment rate controls for local economic conditions that may lower the opportunity cost of enlistment or proxy for the resources available for college enrollment.[23] I also explore the presence of pre-existing trends in military enlistment that would suggest that something else may be driving any observed effect. Finally, I use proxies for eligibility to: (1) compare the effects among groups with different likelihoods of being treated, and (2) to set up a number of falsification tests for similar groups that are ineligible. I address additional threats to internal validity below.[24]

Given the stronger military presence in the South, the different demographic composition, and the fact that most broad merit aid programs were introduced in the South, I present additional estimates focusing on this subset of programs and using only other Southern states as control states.[25] As Table 2 illustrates, the Southern states and the Southern

[22] Ideally, I could leverage variation in eligibility across states. Unfortunately, as far as I know there are no data that provide sufficient samples of likely recruits and the associated necessary variables to produce meaningful predictions of eligibility at the state level.

[23] In the appendix I also present results from a triple-difference specification that flexibly controls for state-year changes using state-year fixed effects (Table A2).

[24] To account for intra-group correlation, I cluster the standard errors at the state of birth (Bertrand, Duflo, Mullainathan 2004). The standard errors are robust to using a clustered bootstrap approach.

[25] There is suggestive evidence that access and use of credit may have been substantially lower in the South during this time period (Hogarth and O'Donnell 2000).

merit aid states have much higher proportions of black individuals and lower median household incomes than the rest of the country. Whereas using the full sample of states provides an estimate of the average effect on an individual of this type of program, the southern merit-aid programs and associated control states are likely more similar. As the identification strategy relies on the control states contributing to the identification of the counterfactual enlistment patterns, I view these estimates as the most credible. I present a final set of estimates using only the differential timing of program introduction within Southern merit aid states. I estimate these basic specifications with individual-level data from the ACS.

Merit-aid eligibility, particularly for likely recruits, varies substantially by race.[26] As illustrated in Figure 1, the GPAs of Southern black males with military experience are substantially lower than those of Southern white males during the late 1990s. Whereas only 10% of Southern black male veterans obtained GPAs of 3.0 or greater, nearly 28% of Southern white male veterans did.[27] Based on this, I split the sample by race with the expectation of larger effects for white males.

I also use administrative recruiting data on the log of state-level counts of military recruit applications and contracts.[28] The data do not contain information on the state in which an individual attended high school, but most individuals enter the military before the age of 24 and thus are likely to enlist in the state in which they attended high school. However, just as there will be some degree of measurement error in assuming an individual's state of birth is the same as that individual's state of high school attendance, there will be some

[26]Prior merit aid work (discussed above) finds mixed evidence of differential effects by race with some evidence that minorities were more likely to be affected by the implementation of later merit aid programs. However, the effects in the general population may differ dramatically from the effects of those on the enlistment-enrollment margin for several reasons, including the fact that the eligibility of likely black male recruits is substantially lower than the eligibility of blacks in the general population. This is not the case for white males. Furthermore, there is evidence that minorities drawn into college by the merit aid programs were substantially more likely to drop out. This may have resulted in delayed rather than avoided enlistment.

[27]These figures are biased upwards somewhat as they are for those that completed high school; this bias is likely larger for blacks as their high school completion rates are lower. Table A3 confirms these statistics for likely recruits using the YATS data.

[28]I construct these counts by linking three-digit zip codes with states. Five of the nearly 1,000 three-digit zip codes cross state lines. In this case, I assign the three-digit zip code to the state that contains the majority of the population in the zip code.

measurement error here.[29]

Because only high-achieving individuals are eligible, I expect higher AFQT individuals to be more responsive. To test this, I compare the response among individuals more and less likely to be affected by changing aid eligibility. Although AFQT scores do not translate directly into GPA, they are positively correlated (Figure 2). I use this positive correlation to separate the application and contract data into groups more or less likely to be affected by the changing availability of merit aid. Panel B of Figure 2 illustrates that only 20-30% of those with AFQT scores between 31 and 50 have a GPA at or above 3.0, but over 40% of those with an AFQT score between 71-99 meet the requirement. Using log recruits as the dependent variable, I estimate the basic specification in equation (1) by AFQT category (31-50, 51-70, and 71-99), with the expectation of larger effects among the two categories containing individuals with scores above the median.

2.7 Effect of Changing Financial Aid Conditions on Enlistment

Estimates from my basic specification indicate that changing merit-aid availability affects the likelihood that an individual ever joins the military (Table 3), suggesting that credit constraints are meaningful for this group. I present results for two time periods: 1987-2004 and 1995-2004. Each time period includes all individuals aged 17 in those years.[30] The first period, 1987-2004, includes the introduction of fourteen merit aid programs.[31] The military drawdown of the early 1990s may have reduced enlistment propensities of individuals in some states more than others (Kleykamp 2010). Because of this, I also estimate specifications using the shorter window from 1995-2004, excluding the years in which force reductions dramatically affected recruiting.

[29] A reduction in military labor supply will not necessarily result in fewer total recruits if military labor demand is relatively inelastic. Recruiters may substitute older recruits or transplants from other states for those affected by the introduction of merit aid; either strategy would work against finding an effect using yearly recruit totals. This could also occur through a reduction in recruiting standards or an increase in recruiting resources (advertising, bonuses, etc.).

[30] The individuals are actually observed in the 2006 through 2011 samples of the ACS when they are older.

[31] The last age-17 cohort year in the sample is 2004 as this is the last cohort year in the sample based on the lower bound age restriction of 24.

As there is variation in program features, I follow Sjoquist and Winters (2012) in presenting a separate set of results for the "strong" merit aid programs that provide generous aid packages and broad eligibility.[32] For both the strong and inclusive measures of merit aid, I find statistically significant and substantial effects of changing financial aid conditions on an individual's propensity to serve in the military. Results for the full set of states are robust across sample periods. Restricting to Southern states, where most merit aid programs are located, the presence of a merit aid program reduces an individual's propensity to have enlisted by .003, or five percent of the mean enlistment rate of .06. When I identify the effect using only the differential timing of the introduction of Southern merit-aid programs, the estimates are somewhat smaller and less precise but still suggest that changing financial aid conditions affect an individual's decision to enlist.[33]

Dividing the reduction in enlistment (5 percent) by the percentage of enlisted individuals who are likely to be eligible for merit-aid (20-30 percent) indicates that between 15 and 25 percent of merit aid-eligible potential recruits were constrained. This suggests that a sizeable fraction of high aptitude individuals enlist in the military because borrowing constraints make immediate college enrollment too costly. There are very few attempts to quantify the role of credit constraints, but my estimate is similar to the fraction (20%) of low-income students that indicated a desire for additional loan availability at tuition-free Berea college (Stinebrickner and Stinebrickner 2008).

If these effects are driven by availability of merit aid, they should not affect the enlistment probabilities of those with less than a high school degree. The estimates from this falsification exercise are all small and statistically indistinguishable from zero (Table A5). I present additional robustness checks below after focusing the analysis on those most likely to be

[32] These programs are broad in terms of eligibility (at least 30% of high school students) and provide relatively large awards and include the programs introduced in Florida (1997), Georgia (1993), Kentucky (1999), Louisiana (1998), Nevada (2000), New Mexico (1997), South Carolina (1998), Tennessee (2004), and West Virginia (2002). See Sjoquist and Winters (2012). "Inclusive" includes all broad merit-aid programs.

[33] Appendix Table A4 demonstrates that the average marginal effects from probit estimation are quite similar. All linear probability estimates in the paper are robust to using Logit or Probit estimation; these estimates are excluded from the paper due to space constraints.

affected by merit-aid introduction.[34]

2.7.1 Demographic Heterogeneity

Because males are far more likely to enter the military, I present additional estimates focusing on them.[35] In Table 4, the estimated impact of merit aid eligibility is .5 to .6 percentage points for males, more than six percent of baseline enlistment. This result is robust across samples, time frames, and measures of merit aid. As discussed above, existing patterns observed in the GPAs of Southern potential recruits make it substantially more likely that white males will be affected by the introduction of merit aid. Indeed, estimates from the Southern merit aid only states indicate that the introduction of merit aid decreased the probability of military service by nearly a percentage point for whilte males. In contrast, the estimated effect on black males is frequently positive.[36] This is initially surprising, but recall that enlistment is determined by both supply and demand factors. Military labor demand is relatively inelastic (even within states); thus, it is not surprising that the composition of recruits may change when a determinant of labor supply affects one group more than another. As demonstrated in Figure 1 and Table A3 and discussed above, black male potential recruits are less likely to be eligible for merit aid.

2.7.2 Additional Robustness Checks and Event Study Analysis

Focusing on the group most affected by the financial aid changes, I conduct a number of falsification exercises to test the validity of the results. In addition to presenting results over two time periods and two definitions of broad-based merit aid, I conduct a leave-one-out

[34] Estimates of the effect of the merit aid programs on the proportion of individuals' with any military service that remain in the military (i.e., equation (1) with $E_{ist} = Active_{ist}$ and the sample restricted to those who ever served in the military) suggest that the programs have resulted in individuals more likely to remain in the military enlisting. This is consistent with the rest of the evidence that those interested in joining the military for a short duration to obtain GI Bill funds are less likely to join following the introduction of merit aid.

[35] Estimates for females are small and consistently indistinguishable from zero, but very low military participation rates makes it difficult to rule out sizeable percentage effects.

[36] There is a statistically significant negative coefficient in column (1). One potential explanation for this disparity is that blacks in non-Southern states are arguably a worse control group for those in the predominantly Southern merit aid states. Although inconclusive, a comparison of black characteristics across states suggests this may be the case.

procedure. I estimate the basic merit aid effect holding each merit aid program out of the estimation in turn. These results are presented in Table A6; estimates for white males are consistently between .7 and 1 percentage points indicating that a single state is not driving the results.

I also conduct an event study analysis to examine the degree to which pre-existing trends are driving the results. Panels A and B of Figure 3 suggest no pre-existing trend in military enlistment prior to the adoption of merit aid. Following the introduction of aid, we observe a drop in the propensity that an individual has been in the military. Because there are very few state-year cohorts further than eight years from the adoption of merit aid in the sample, I limit the figures to this range.[37]

2.7.3 Recruiting Results and Heterogeneity by Ability

Using the administrative military recruiting data, I begin with an attempt to confirm the overall results in Tables 3 and 4. Recall that the detailed recruit data includes only enlisted individuals and that I cannot restrict the sample to recruits who attended high school in the state in which they are recruited because this information is not in the data. Furthermore, I do not observe the age of recruits, so if recruiters are able to replace younger merit-aid eligible recruits with older ineligible individuals then I will be less likely to observe an effect. Despite these differences from the ACS sample, I find similar results (Table 5). The introduction of a merit aid program results in a 2-5% reduction in total contracts. This result is similar, but slightly smaller than the effect found in the ACS data.[38] As I cannot identify state of birth, state of high school attendance, or age in the military data, there are several potential explanations for this difference. The magnitude of the effects on total applications

[37]The event studies for the full window are available in Figures A1 and A2. As might be expected if the military responds to recruiting shortfalls by increasing effort, these effects appear to fade out after five to six years. This fadeout will bias my estimates toward zero.

[38]The effects appear somewhat weaker outside of the South. As we do not observe this in the ACS data, this is potentially explained by higher enlistment probabilities (30% higher) in the South which make it more difficult for ineligible recruits to fully substitute for eligible ones. As I demonstrate below, the pattern of larger effects on higher ability individuals (Table 6) and the analyses that restrict to current high-school seniors (Table 7) do not exhibit this type of heterogeneity.

is somewhat larger, consistent with (1) the military responding to a more difficult recruiting environment by increasing effort, or (2) an excess supply of applicants.[39]

Table 6 disaggregates the application results by AFQT category. As expected, the point estimates are significantly larger for the 51-70 and 71-99 AFQT ranges, those with higher likelihoods of merit-aid eligibility. The largest and most significant effects are for the 71-99 category, containing the individuals most likely to be eligible for merit aid.[40]

In Figure 4, I present event study figures for each of these AFQT ranges for the introduction of "strong" merit aid programs. The pictures indicate a reduction in the number of high ability contracts (AFQT: 51-99) following the introduction of a merit aid program (Panel A and B). The number of lower ability recruits (AFQT: 31-50) appears unaffected by the introduction of a merit aid program.

2.7.4 Focusing on Current High-School Recruits

I use additional education information contained in the application data to focus the analysis on those most likely to be affected by the introduction of aid. I restrict the sample to high-quality (AFQT 51-99) applicants that are currently high-school seniors. This restriction *overcomes concerns related to the potential substitution of older or out-of-state individuals* that may dampen the observed effect. Because older students (that graduated prior to merit aid introduction) and individuals from out-of-state (that did not attend high-school in the state) are ineligible for merit aid, they bias against finding an effect of merit-aid introduction.

The estimates using this sample are larger, a 5 to 8 percent reduction in applications (Table 7). This estimate is fairly consistent across samples and classifications of merit aid. Dividing the estimates (5 to 8 percent) by the percentage of high-quality applicants that are

[39] As before, those with less than a high-school diploma (and not currently in school) are ineligible for merit aid, and thus we would expect to find no negative application effect of changing merit aid conditions on this group. The estimates from this falsification exercise are consistent with this expectation with most estimates positive or close to zero and no significant negative results (Table A7). We might also expect the composition of recruits to shift to those less interested in joining the military as a route to college. Estimates from the ACS (using a similar difference-in-differences specification) indicate that veterans from states that had merit aid programs when they were 17 were less likely to have gone to college (full results available from author upon request).

[40] Appendix Table A2 contains estimates from a triple-difference approach. These estimates similarly suggest that the number of high-ability applicants falls relative to the number of low-ability applicants.

likely to be eligible for merit aid (40%) suggests that 13 to 20 percent are constrained. This is similar, but slightly lower, than the fraction estimated using the ACS data.

2.7.5 Effect Sizes and Family Resources

I draw on the secondary prediction regarding the interaction of resources and effect sizes as a supplementary test. Ideally, I could use detailed information on availability of credit and household income and assets to design more refined tests for the presence of constraints. Unfortunately, this information is not available in surveys with large enough samples of potential recruits. A second-best approach is to use finer geographic data to proxy for the likelihood of financial constraints. I estimate similar specifications to equation (1) but split the sample at the 5-digit level by median household income (in 2000 dollars) into four groups: (1) between \$9,521 and \$28,726, (2) between \$28,726 and \$32,735, (3) between \$32,735 and \$35,563, and (4) between \$35,563 and \$65,865.[41] For each quartile, I estimate specification (1) using the log of applications with an AFQT score of 51 or greater (Table 8). The effects are largest in the lowest income areas, consistent with the merit aid programs easing constraints.[42] These results suggest that more attention should be paid to individuals in these low-income areas who potentially lack the means to make optimal schooling decisions.

2.7.6 Competing Explanations

The estimates in Tables 4-8 show that the introduction of merit aid has sizeable effects on whether an individual joins the military and that these estimates are robust to multiple periods and classifications of merit aid. Here, I consider a variety of additional threats to

[41]The zip codes are split by taking the quartile median household income levels in the states that implemented merit aid between 1990 and 1999 after weighting by number of recruits such that the bottom quartile of zip codes contains roughly one quarter of the total number of recruits. I only have data at this level from 1990-2000, so I limit the analysis to these years.

[42]There is also some evidence of sizeable effects in the highest median income quartile. This is still potentially consistent with greater responsiveness among low-income individuals as a large portion of Southern high median income counties lie in urban areas with high income inequality.

the validity and interpretation of the results including peer effects, reallocation of recruiting effort, and the role of information.

Changing peer quality may change the relative value of immediate enrollment. If the value of college is dependent on peer quality and this characteristic is more important to students of traditional age, the introduction of merit-aid programs may be responsible for the observed effects. There are at least two reasons why this is unlikely to drive the results. First, the effects on college enrollment are likely too small (5-7%) to have a meaningful effect on peer quality. Second, it is not clear what the direction of the effect on peer quality would be because many of the marginal students are lower ability than the inframarginal students.

Another possibility is that the military may actively reallocate effort towards recruiting individuals who are unlikely to be eligible for merit aid. First, following the basic logic of the test in this paper, reallocation of effort should only be necessary if individuals are constrained. Second, my review of several Department of Defense commissioned reports on military recruiting uncovered no evidence of this type of targeting. Similarly, recruiting goals at the state level suggest that this type of reallocation did not take place across states.[43] A reallocation within state would require a relatively high level of sophistication on the military's behalf; this is possible, but it is not clear why this would result in overall reductions in state recruit totals.

A final alternative is that many individuals may lack information about the true cost of enrollment. Evidence has begun to emerge about the importance of information constraints in the college-going process (for example Hoxby and Avery 2012, Bettinger et al 2012). The introduction of highly visible merit aid programs may have made the net price of enrollment for low-income individuals more salient, overcoming a lack of information available for those in poor rural areas. Or, it may have led guidance counselors to encourage students to forego enlistment and enroll immediately. I view these interpretations of the effects as plausible; however, if information or encouragement are driving the results, I would expect the effects on enlistment to show up for cohorts just prior to the introduction of merit aid. Presumably

[43] Results available from author upon request.

the new program would be discussed prior to implementation and would influence enlistment just prior to implementation; this is not the case. Despite this, I acknowledge ths inability of the test to conclusively rule out this alternative hypothesis.

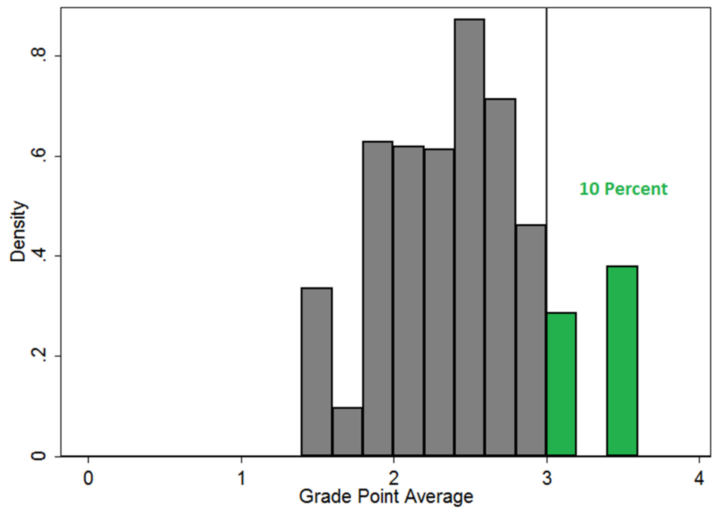
2.8 Discussion and Conclusion

Changing non-military financial aid conditions play an important role in the decision to enlist. A back of the envelope calculation indicates that between 15 and 25 percent of merit-aid eligible likely recruits face constraints, suggesting the presence of meaningful credit constraints in the college enrollment process. Furthermore, these effects are concentrated in areas with lower median household incomes, buttressing the argument that credit constraints may motivate some individuals to enlist. Evidence that those individuals who are unable to pay for much of college are more likely to mention education benefits as a motivation for enlisting supports this conclusion.

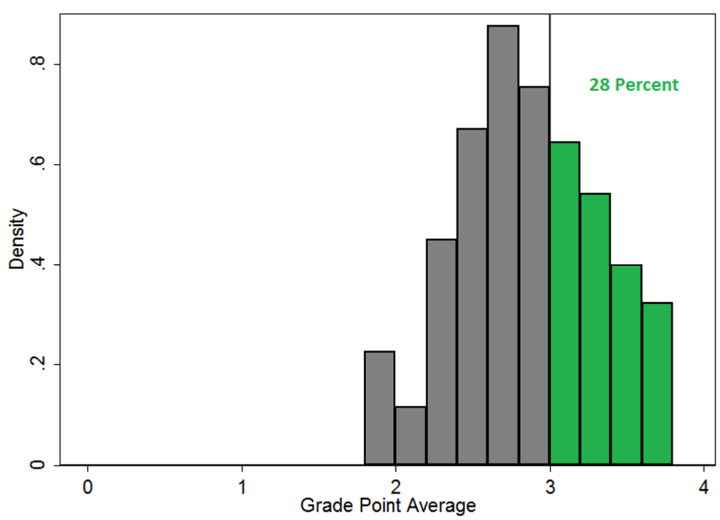
Whether the provision of military education benefits is the most efficient method to encourage college enrollment is unclear. In some sense, the military is providing a great service to these individuals who otherwise may not have been able to attend college at all. But, it is an open question whether it is the military that should be providing this service. If instead these individuals were provided other financial aid opportunities (which has increasingly been the case) or additional information about college costs and aid availability, they might immediately enroll upon graduating from high school and contribute additional years in a labor market that is more suited to their interests and skill sets. To better understand the degree to which veteran subsidies are optimal, further work is needed to understand the relative values of traditional versus delayed enrollment for different types of individuals.

Figure 2.1: GPA Distribution of Southern Males with Military Experience

Panel A: Black



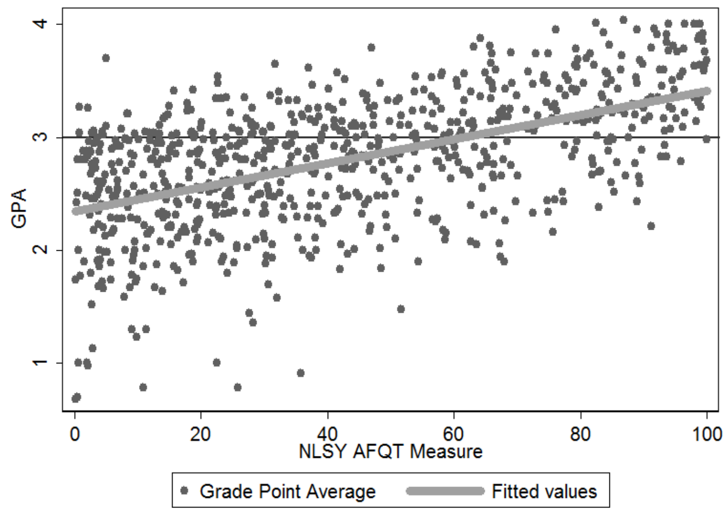
Panel B: White



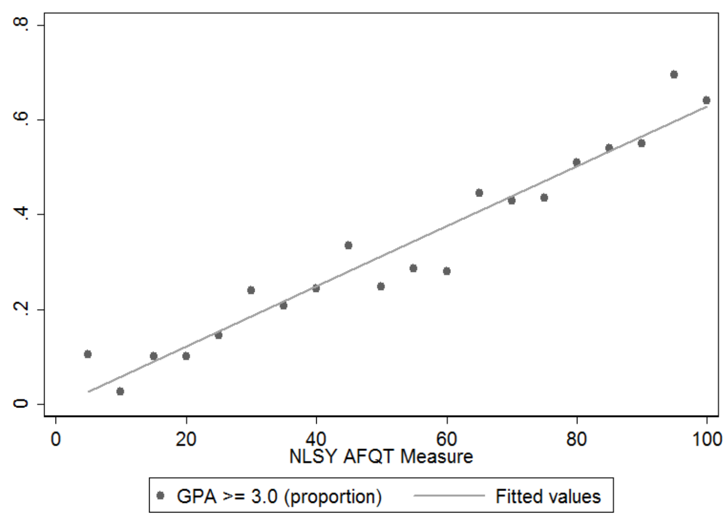
Note: Histogram of Grade Point Averages produced using NLSY 1997 and cross-sectional survey weights. Distributions for non-Southern states are quite similar, with a slight shift to the right in Panel A.

Figure 2.2: NLSY AFQT and GPA: Southern Males

Panel A: GPA by AFQT

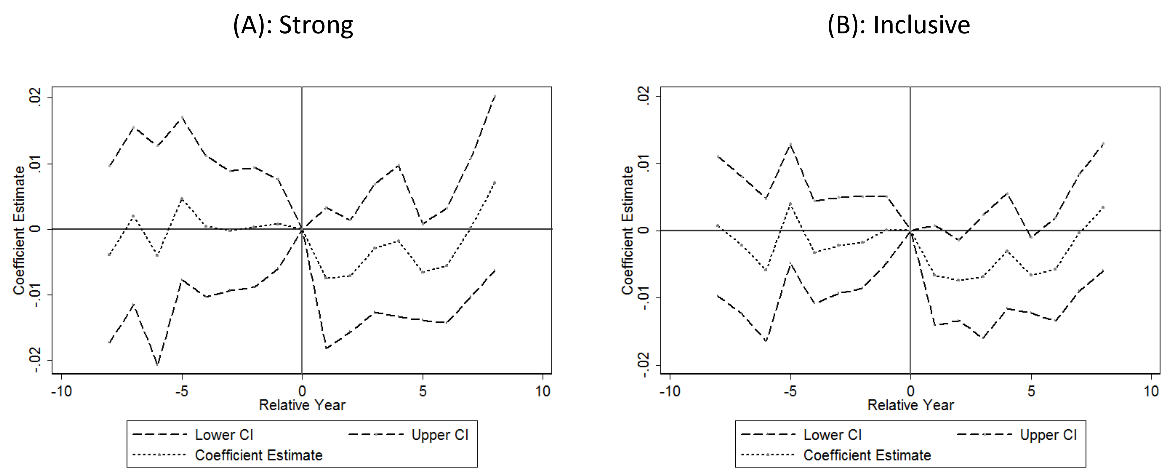


Panel B: Proportion with GPA of 3.0 or Greater



Note: AFQT and GPA measures obtained from the NLSY 1997. Proportions are calculated for AFQT bins of size five (i.e., 0-5, 5-10, etc.) using cross-sectional survey weights.

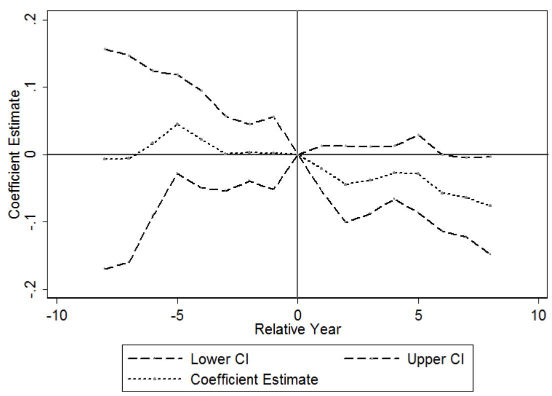
Figure 2.3: Event Study Figures(-8 ≤ k ≤ 8)



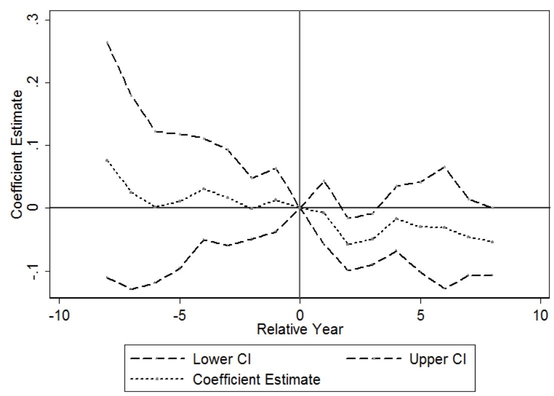
Note: Dotted lines plot coefficients on dummy variables for each cohort relative to the last cohort prior to the introduction of merit aid. Dashed lines indicate upper and lower 95 percent confidence intervals. Sample includes white males in all states and age-17 years (ie, 1987-2004) using the 2006-2011 samples of the ACS. Regressions include cohort year and state of birth fixed effects as well as age indicators and time-varying state characteristics. Standard errors are clustered at the state of birth level. Event studies are presented with shortened windows (-8 ≤ k ≤ 8) to improve readability (full window figures are in Figure A2). See Data Appendix or notes to Table 4 for more details of "Strong" and "Inclusive" definitions.

Figure 2.4: Event Study Figures (Strong: -8 ≤ k ≤ 8)

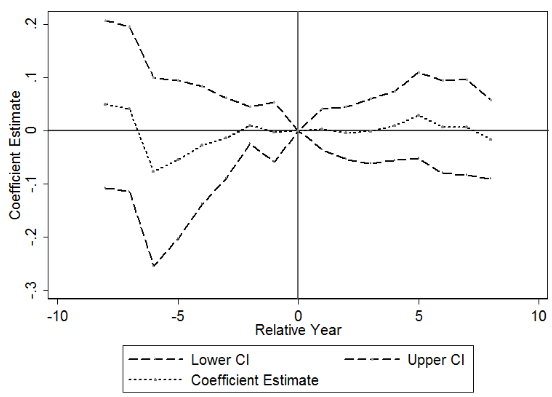
(A): AFQT: 71-99



(B): AFQT: 51-70



(C): AFQT: 31-50



Note: Dotted lines plot coefficients on dummy variables for each cohort relative to the last cohort prior to the introduction of a “strong” merit aid program (figures for introduction of “inclusive” merit aid programs are qualitatively similar and available from the author upon request). See Data Appendix or notes to Table 4 for more details of "Strong" and "Inclusive" definitions. Dashed lines indicate upper and lower 95 percent confidence intervals. Sample includes the log of active-duty contracts in all states between 1990-2004. Regressions include year and state fixed effects and are weighted by state cohort size. Standard errors are clustered at the state level. Panels are presented with a shortened window to improve readability.

Table 2. 1

Reasons to Join by Ability to Pay for School
(Percentage Indicating "Yes")

Ability to Pay for School	Education	Job Training	Duty to Country
75-100%	25%	50%	32%
51-74%	26%	21%	37%
50%	27%	41%	15%
25-49%	46%	41%	8%
1-24%	41%	29%	18%
0%	86%	57%	0%
N = 229			

Note: Ability to pay is derived from the answer to: "Taking into account help from your family and your own savings and earnings, what percent of your yearly school and living expenses could you cover if you go to school?". Percentages given indicate the proportion of individuals who indicated a particular reason as a motivation for joining the military conditional on mentioning "military service" in their plans for the future. Source: 1991 Youth Attitude Tracking Study (YATS). See data appendix for additional information.

Table 2.2

Descriptive Statistics

	All	South	South Merit Only
ACS Sample			
Female	0.50	0.50	0.50
Black	0.14	0.23	0.26
Hispanic	0.11	0.09	0.03
Ever in Military	0.059	0.065	0.064
Unemployment Rate	5.48	5.40	5.57
Median HH Income ($1990)	31,782	28,597	27,856
Contracted Recruits			
AFQT (Percentile Score)	60.45	59.40	58.96
HS Diploma or More	0.93	0.93	0.93

Note: Statistics derived from 2006-2011 ACS samples using survey weights. ACS sample limited to age-17 years 1987-2004. For example, inclusion of age-17 year 1987 indictes that individuals who were 17 during 1987 are included in the sample. "South" limits the sample to states in the southern region and "South Merit Only" limits the sample to southern states that introduced a merit-aid program between 1987 and 2004. Contracted recruit statistics generated from 1990-2004 active-duty contract data. AFQT is a percentile aptitude score derived from the Armed Services Vocational Aptitude Battery (ASVAB), an entrance exam taken by all applicants for military enlistment. It is normed using the U.S. population.

Table 2.3

Impact of Merit Aid Availability on Veteran Status: ACS

		1987-2004			1995-2004		
Merit-Aid Measure	Ever Military	All	South	South Merit Only	All	South	South Merit Only
STRONG	0.059	-0.00400***	-0.00337**	-0.00228	-0.00254***	-0.00220**	-0.00402***
		(0.000824)	(0.00126)	(0.00157)	(0.000836)	(0.000899)	(0.000952)
INCLUSIVE		-0.00366***	-0.00376***	-0.00156	-0.00180*	-0.00231**	-0.00289**
		(0.000817)	(0.00111)	(0.00132)	(0.000961)	(0.000966)	(0.00118)
N		2,044,699	672,341	286,499 / 361,505	1,300,422	430,795	183,637 / 230,664

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include age, race, and sex indicator variables and state of birth and year of birth fixed effects. "Strong" merit aid programs include those in Florida, Georgia, Kentucky, Lousiana, Nevada, New Mexico, South Carolina, Tennessee, and West Virginia. They are a subset of all merit-aid programs with broad eligibility (see Data Appendix for more details of "Strong" and "Inclusive" definitions). "South" limits the sample to states in the southern region and "South Merit Only" limits the sample to southern states that introduced a merit-aid program. Regressions are limited to age-17 years indicated using data from the 2006-2011 ACS surveys. For example, inclusion of age-17 year 1987 indicates that individuals that were 17 during 1987 are included in the sample. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.4

Impact of Merit Aid Availability on Veteran Status: Demographic Heterogeneity

Merit-Aid Measure	Ever Military	1987-2004 All	1987-2004 South	1987-2004 South Merit Only	1995-2004 All	1995-2004 South	1995-2004 South Merit Only
Male							
STRONG	0.096	-0.00609***	-0.00532**	-0.00474	-0.00484**	-0.00465**	-0.00534**
		(0.00146)	(0.00202)	(0.00284)	(0.00206)	(0.00191)	(0.00161)
INCLUSIVE		-0.00555***	-0.00596***	-0.00424**	-0.00387**	-0.00467***	-0.00524**
		(0.00141)	(0.00154)	(0.00164)	(0.00164)	(0.00153)	(0.00179)
White							
STRONG	0.098	-0.00560***	-0.00686***	-0.00911***	-0.00771***	-0.00925***	-0.0108***
		(0.00180)	(0.00212)	(0.00266)	(0.00196)	(0.00171)	(0.00198)
INCLUSIVE		-0.00513***	-0.00763***	-0.00799***	-0.00603***	-0.00849***	-0.0111***
		(0.00162)	(0.00184)	(0.00199)	(0.00171)	(0.00181)	(0.00176)
Black							
STRONG	0.092	-0.00826**	-0.00112	0.0102**	0.00215	0.00613	0.00408
		(0.00377)	(0.00446)	(0.00377)	(0.00646)	(0.00541)	(0.00450)
INCLUSIVE		-0.00549	0.000319	0.00895**	0.00303	0.00565	0.00891
		(0.00350)	(0.00421)	(0.00387)	(0.00531)	(0.00511)	(0.00612)

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include age, race and sex indicator variables, state of birth and year of birth fixed effects, and state by year unemployment rates and and cohort size controls. Regressions are limited to age-17 years indicated using data from the 2006-2011 ACS surveys. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" and "South" and "South Merit Only" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.5

Impact of Merit Aid Availability on Log Recruits

	1990-2004			1995-2004		
Merit-Aid Measure	All	South	South Merit Only	All	South	South Merit Only
Applications						
STRONG	0.00574	-0.0487**	-0.00538	-0.0330	-0.0559**	-0.0443
	(0.0283)	(0.0224)	(0.0245)	(0.0258)	(0.0211)	(0.0323)
INCLUSIVE	-0.0198	-0.0537**	-0.0342	-0.0430**	-0.0530**	-0.0415
	(0.0255)	(0.0195)	(0.0284)	(0.0185)	(0.0181)	(0.0249)
Contracts						
STRONG	-0.00224	-0.0364	0.00582	-0.0335	-0.0422*	-0.0254
	(0.0253)	(0.0213)	(0.0300)	(0.0269)	(0.0228)	(0.0251)
INCLUSIVE	-0.0202	-0.0322*	-0.00964	-0.0364*	-0.0341*	-0.0249
	(0.0219)	(0.0182)	(0.0280)	(0.0192)	(0.0190)	(0.0218)
N	765	240	105/150	510	160	100/70

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state and year fixed effects and state by year unemployment rate and cohort size controls. Regressions are limited to years indicated using administrative application and contract data aggregated to the state-year level. Applications are restricted to those currently in high school or with a high school degree or greater level of educational attainment. Contracts limited to individuals with a high school degree or more. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" and "South" and "South Merit Only" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.6

Impact of Merit Aid Availability on Log Applications by AFQT

	1990-2004			1995-2004		
	All	South	South Merit Only	All	South	South Merit Only
STRONG						
AFQT: 11-30	0.0182	-0.0379	0.0493	-0.0152	-0.0407	-0.0131
	(0.0386)	(0.0360)	(0.0357)	(0.0342)	(0.0250)	(0.0225)
AFQT: 31-50	0.0396	-0.0300	0.00902	-0.0125	-0.0401*	-0.0422
	(0.0307)	(0.0225)	(0.0259)	(0.0293)	(0.0221)	(0.0358)
AFQT: 51-71	0.00765	-0.0493***	-0.0237	-0.0182	-0.0456**	-0.0490
	(0.0274)	(0.0159)	(0.0169)	(0.0258)	(0.0173)	(0.0256)
AFQT: 71-99	-0.00838	-0.0850***	-0.0536**	-0.0239	-0.0777***	-0.0723
	(0.0261)	(0.0206)	(0.0146)	(0.0299)	(0.0261)	(0.0417)
INCLUSIVE						
AFQT: 11-30	-0.0129	-0.0491	-0.00556	-0.0239	-0.0492*	-0.0257
	(0.0352)	(0.0361)	(0.0472)	(0.0255)	(0.0242)	(0.0239)
AFQT: 31-50	0.00957	-0.0351	-0.0257	-0.0199	-0.0421*	-0.0462
	(0.0290)	(0.0213)	(0.0316)	(0.0199)	(0.0201)	(0.0278)
AFQT: 51-71	-0.0109	-0.0525***	-0.0418*	-0.0263	-0.0460***	-0.0455**
	(0.0228)	(0.0154)	(0.0199)	(0.0179)	(0.0145)	(0.0187)
AFQT: 71-99	-0.0202	-0.0759***	-0.0496***	-0.0312	-0.0627**	-0.0506
	(0.0233)	(0.0189)	(0.0151)	(0.0246)	(0.0253)	(0.0335)

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state and year fixed effects and state by year unemployment rate and cohort size controls. Regressions are limited to years indicated using administrative application data aggregated the state-year-AFQT band level. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" and "South" and "South Merit Only" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.7

Impact of Merit Aid Availability on Log Applications
High School Seniors with AFQT > 50

Merit-Aid Measure	1990-2004 All	1990-2004 South	1990-2004 South Merit Only	1995-2004 All	1995-2004 South	1995-2004 South Merit Only
Applications						
STRONG	-0.0480	-0.0934***	-0.0322	-0.0520*	-0.0806***	-0.0461
	(0.0300)	(0.0283)	(0.0219)	(0.0271)	(0.0242)	(0.0312)
INCLUSIVE	-0.0825***	-0.105***	-0.0674*	-0.0808***	-0.0754***	-0.0507*
	(0.0307)	(0.0263)	(0.0308)	(0.0283)	(0.0203)	(0.0226)
N	765	240	105/150	510	160	100/70

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state and year fixed effects as well as state by year unemployment rate and cohort size controls. Regressions are limited to years indicated using administrative application and contract data aggregated to the state-year level. Applications are restricted to those currently seniors in high school and with AFQT scores between 51 and 99. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" and "South" and "South Merit Only" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.8

Impact of Merit Aid Availability on High Quality Log Applications
By County Income Quartile (1990-2000)

	Strong		Inclusive	
	All	South	All	South
HH Income: Q1	-0.117**	-0.103*	-0.117**	-0.0976*
	(0.0512)	(0.0524)	(0.0452)	(0.0469)
HH Income: Q2	0.00222	-0.0432	-0.0217	-0.0561
	(0.0251)	(0.0307)	(0.0267)	(0.0323)
HH Income: Q3	-0.0198	-0.0255	-0.0608*	-0.0651
	(0.0344)	(0.0451)	(0.0360)	(0.0464)
HH Income: Q4	-0.0127	-0.0713***	-0.0385*	-0.0839***
	(0.0266)	(0.0236)	(0.0224)	(0.0218)

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state and year fixed effects. Regressions are limited to applicants with AFQT scores between 51 and 99 and years indicated using administrative application data aggregated to the state-year level. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" and "South" and "South Merit Only" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

2.9 Appendix: Data and Methods

A. Merit-Aid Programs

The merit-aid programs vary in terms of their eligibility conditions and generosity (Fitzpatrick and Jones 2012; Sjoquist and Winters 2012). Sjoquist and Winters place some focus on the "strong" merit aid programs that provide generous aid packages and broad eligibility.These programs are broad in terms of eligibility (at least 30% of high school students) and provide relatively large awards and include the programs introduced in Florida (1997), Georgia (1993), Kentucky (1999), Louisiana (1998), Nevada (2000), New Mexico (1997), South Carolina (1998), Tennessee (2004), and West Virginia (2002). These are a subset of the programs focused on by Fitzpatrick and Jones (2012). The additional programs included are Arkansas (1991), Mississippi (1996), Michigan (2000), Maryland (2003), and South Dakota (2004). Maryland's program only lasted for one year, whereas all other programs remained implemented throughout the remaining sample period.

I reviewed the statute and associated documentation for each state in order to determine whether it allowed individuals entering active-duty to delay merit-aid receipt. In Table A1, I present the year of implementation of each program as well as representative quotes and information from my review supporting the determinations discussed in the main body of the paper (sources for each are available on request).

B. Youth Attitude Tracking Study

I use the 1991-1994 samples of the Youth Attitude Tracking Study (YATS) as they are the only publicly available data from the YATS. Table 1 relies only on the 1991 data as the question related to ability to pay for schooling was only asked that year. The goal of the study was to collect information on topics such as military enlistment expectations, military recruitment advertising, and future plans. The study surveyed persons aged 16 to 24 living in the United States in households or noninstitutionalized group homes with telephones. Individuals in the military, with prior military service, or currently accepted for service in the military (active or reserve component) were excluded. Individuals were selected using random digit dialing.

C. American Community Survey

I use the American Community Survey (ACS) public use micro samples from 2006 through 2011.[44] The 2006 ACS is the first to survey group quarters and thus includes active-duty military as well as individuals in dorms. I restrict the sample to individuals age 24 to 35. I construct a binary variable indicating whether an individual has ever been in the military: $E_{ist} = max\{Active_{ist}, Veteran_{ist}, Reserve_{ist}\}$.[45] All samples are restricted to individuals born in the United States. Where noted, the sample is restricted to Southern states and Southern merit aid states.

D. Applications and Contracts Data and Linking

My data on applications and contracts were produced by the Defense Manpower Data Center through a number of FOIA requests and contain a record for each application and contract.[46] I produce aggregate counts by state, year, and AFQT category after restricting the sample to all active-duty service components. I exclude reserve and national guard data as these series appear to be missing data. In order to aggregate counts at the state level, I link three digit zip-codes (provided in the recruit data for all years) to states. Five of the nearly 1,000 three-digit zip codes cross state lines. In this case, I assign the three-digit zip code to the state that contains the majority of the population in the zip code.[47]

In some specifications, I aggregate the data at the five-digit zip code level before splitting the sample and aggregating further. I use the same procedure to assign each zip code to a state. I also link median household income information to the five-digit zip codes. I use data obtained from the Census Small Area Income and Poverty Estimates (SAIPE) on the 1993 median household incomes at the county level.[48] I generate a population weighted median

[44] I use the 2000 Census and 2001-2005 ACS samples in robustness checks. See Table A8 for robustness of the results including the 2000 Census.

[45] The results are not sensitive to the exclusion of $Reserve_{ist}$ from this measure. Individuals with reserve service are included as military call ups directly affect the proportion of individuals with active duty service in a particular state, but do not directly affect the proportion with reserve or active-duty service.

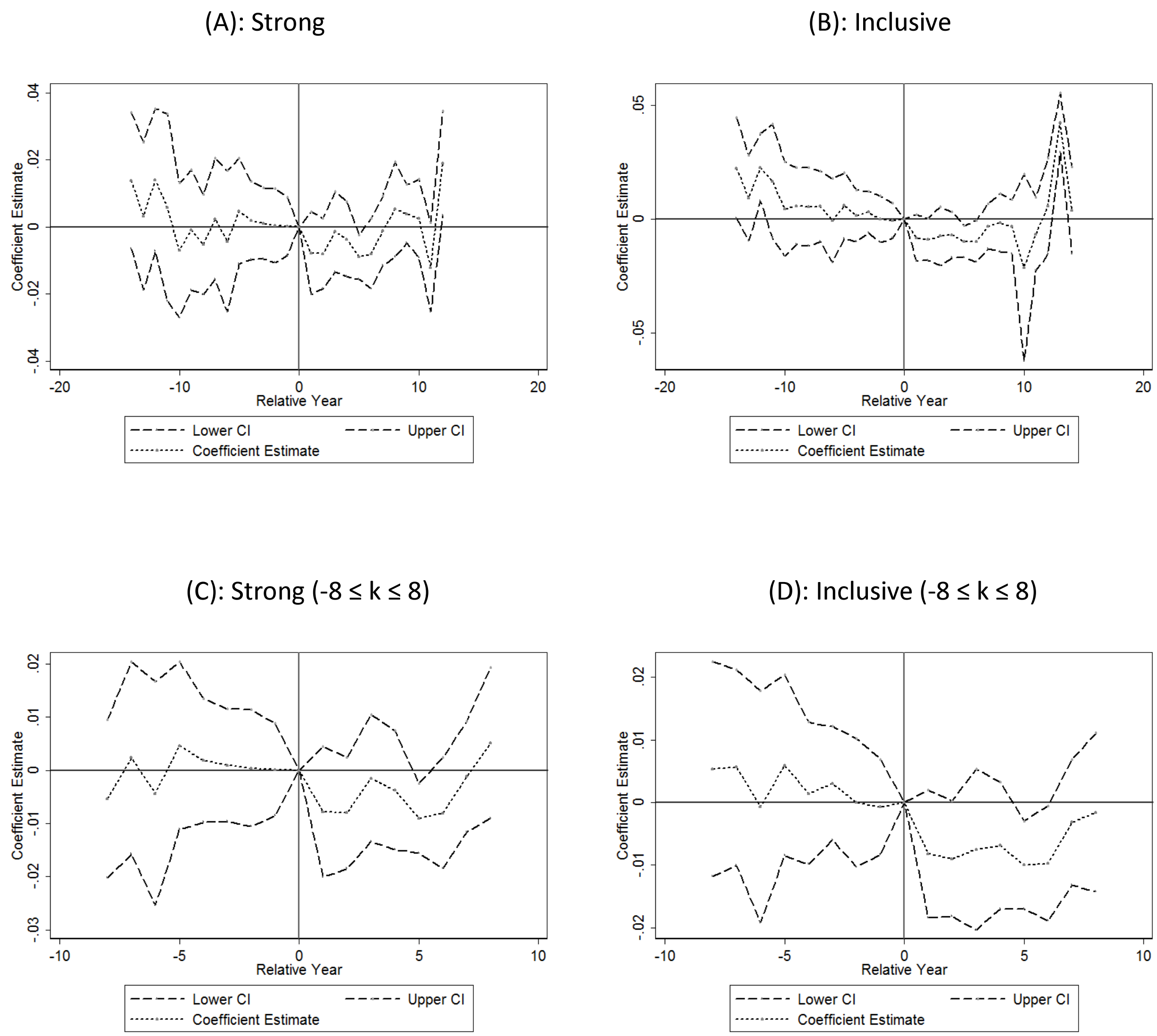
[46] I thank Garrett Christensen (Swarthmore College) for providing me with data from 1990-2000.

[47] Usually, the vast majority of the population resides in one state.

[48] These statistics can be obtained here: http://www.census.gov/did/www/saipe/downloads/estmod93/est93ALL.dat.

household income at the five-digit zip code level. As five-digit zip codes are relatively small, this generally consists of assigning the median household income level of a county to the zip code.

Figure 2.A1: Event Study Figures for Southern States

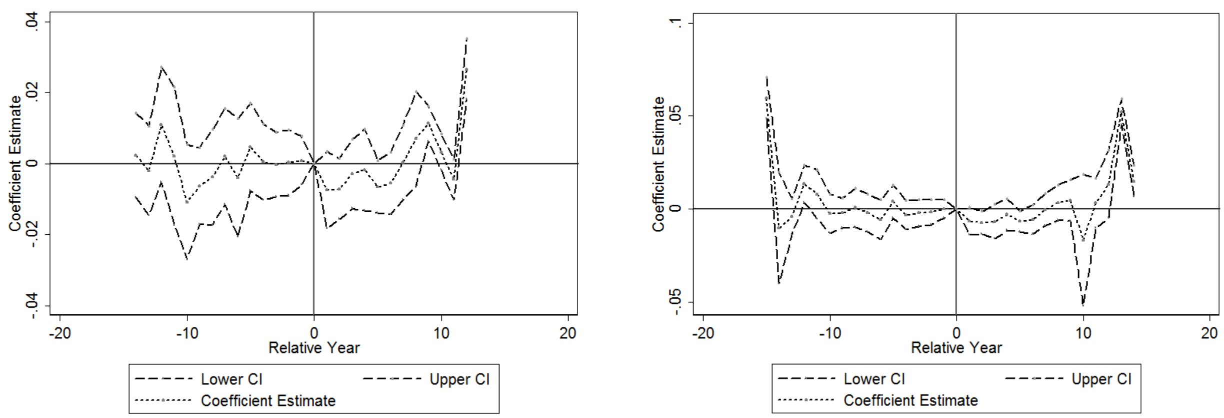


Note: Dotted lines plot coefficients on dummy variables for each cohort relative to the last cohort prior to the introduction of merit aid. Dashed lines indicate upper and lower 95 percent confidence intervals. Sample includes white males in southern states and all age-17 years (ie, 1987-2004). Regressions include cohort year and state fixed effects as well as age indicators and time-varying state characteristics. Standard errors are clustered at the state of birth level. Panel C (D) plots the same information as A (B) with a shortened window to improve readability. See Data Appendix or notes to Table 4 for more details of "Strong" and "Inclusive" definitions.

Figure 2.A2: Event Study Figures – Full Windows

(A): Strong

(B): Inclusive



Note: Dotted lines plot coefficients on dummy variables for each cohort relative to the last cohort prior to the introduction of merit aid. Dashed lines indicate upper and lower 95 percent confidence intervals. Sample includes white males in all states and age-17 years (ie, 1987-2004). Regressions include cohort year and state of birth fixed effects as well as age indicators and time-varying state characteristics. Standard errors are clustered at the state of birth level. See Data Appendix or notes to Table 4 for more details of "Strong" and "Inclusive" definitions.

Table 2.A1

Merit Aid Programs

State	Year	Military Delay Language
Arkansas	1991	See Note*
Georgia	1993	"The expiration of eligibility date will be June 30th of the seventh academic year following the student's date of high school graduation ... However, a student who served on active duty in the U.S. Military after his or her date of high school graduation ... and before the calculated expiration of eligibility date may request an extension of eligibility ..."
Mississippi	1996	No Requirement on Date of Initial Receipt Being Within a Certain Time Frame after Graduation
Florida	1997	"If a student enlists directly into the military after graduation, the 2-year or 3-year period begins on the date the student is separated from active duty."
New Mexico	1997	"According to state regulations, students who ... begin service in the United States armed forces can establish eligibility for the scholarship during their first college semester if within 120 days of completion of honorable service or medical discharge from the service are accepted for entrance to and attend of the state educational institutions."
Louisiana	1998	"The TOPS statute allows a high school graduate who enters on active duty with the U.S. Armed Forces within one year after graduation to delay first-time, full time enrollment to ... five years after the date of high school graduation or within one year of separation from active duty, whichever is earlier."
South Carolina	1998	No Requirement on Date of Initial Receipt Being Within a Certain Time Frame after Graduation
Kentucky	1999	"The expiriation of a student's ... five (5) year eligibility shall be extended [if] the student was unable to enroll ... due to any of the following circumstances: ... (d) 1. Active duty status for the student in the United States Armed Forces ... [the extension will be] for the total number of years during which the student was on active duty status."
Nevada	2000	"There are exemptions to the six year limitation for members of the military…"
Michigan	2000	"Enroll at an approved postsecondary institution in Michigan or a military service academy within two years of becoming a high school graduate (two-year period would be extended for service in armed forces or Peace Corps)."
West Virginia	2002	A person who entered full-time, active duty with the United States armed services within two years of his or her high school graduation and is discharged with a status other than dishonorable is eligible to apply or claim a PROMISE award within seven years of the time he or she has initially entered military service."
South Dakota	2004	"Enter into the program within 5 years of high school graduation, or within 1 year of the student's release from active duty military service (if that release is within 5 years of the date of the student's high school graduation)."
Tennessee	2004	"If, however, such student is separated from active duty under a fully honorable discharge before applying for a Tennessee HOPE scholarship, then the student shall apply for a scholarship within one (1) year of the date of separation from active duty under a fully honorable discharge or within seven (7) years of the student's date of entry..."

Note: Due to space constraints, I have excluded Maryland from the table as its program was only implemented during 2003 (results robust to exclusion). With the exception of Maryland, which had a program for 2003 only, all state merit-aid programs were in place from the year of implementation through the study period. * The text of the law regarding delay of merit-aid receipt in Arkansas is unclear. As stated in the text, the results are robust to the exclusion of Arkansas from all analyses.

Table 2.A2

Impact of Merit Aid Availability on Log Applications by AFQT
Triple Difference

	1990-2004			1995-2004		
	All	South	South Merit Only	All	South	South Merit Only
*STRONG*AFQT:51-70*	-0.0318**	-0.0247*	-0.0374	-0.00908	-0.00797	-0.00688
	(0.0152)	(0.0133)	(0.0194)	(0.0133)	(0.0157)	(0.0157)
*STRONG*AFQT:71-99*	-0.0472	-0.0604	-0.0738*	-0.0194	-0.0376	-0.0223
	(0.0350)	(0.0387)	(0.0363)	(0.0339)	(0.0357)	(0.0256)

Note: Each column represents a separate regression. Coefficients are for interaction of indicator variable indicating presence of merit aid program with particular AFQT category (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state, year, and state-year fixed effects as well as state by AFQT category and year by AFQT category indicator variables. Regressions are limited to applicants with AFQT scores greater than or equal to the minimum (31) and years indicated using administrative application data. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.A3

Grades of Individuals Planning on Military Service in the Future
(Restricted to Southern Region)

Type of Grades in High School	All	White	Black
90-100 Average	4%	6%	1%
85-89 Average	19%	24%	8%
80-84 Average	19%	19%	18%
75-79 Average	36%	33%	42%
70-74 Average	14%	11%	20%
65-69 Average	6%	5%	8%
65-69 Average	1%	1%	1%
N	377	252	89

Note: Percentages of individuals in each grade category conditional on mentioning "military service" in their plans for the future. Totals do not sum to 100% as a small number of individuals indicated that they did not know. Distributions for non-Southern states are quite similar, with a somewhat higher proportion (14%) of Black individuals earning grades in the top two categories. Source: 1991-1994 samples of the YATS. See appendix for additional information.

Table 2.A4

Impact of Merit Aid Availability on Veteran Status: ACS
Probit Marginal Effects

		1987-2004			1995-2004		
Merit-Aid Measure	DV Mean	All	South	South Merit Only	All	South	South Merit Only
STRONG	0.059	-0.00288***	-0.00333***	-0.00172	-0.00196**	-0.00218**	-0.00388***
		(0.000747)	(0.00117)	(0.00139)	(0.000798)	(0.000896)	(0.00111)
INCLUSIVE		-0.00286***	-0.00362***	-0.00107	-0.00168**	-0.00240**	-0.00283**
		(0.000660)	(0.00106)	(0.00116)	(0.000821)	(0.000988)	(0.00120)
N		2,044,699	672,341	286,499 / 361,505	1,300,422	430,795	183,637 / 230,664

Note: Each cell represents a separate regression. Average marginal effects are presented for the indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include state of birth and year of birth fixed effects. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. Regressions are limited to age-17 years indicated using data from the 2006-2011 ACS surveys. For example, inclusion of age-17 year 1987 indicates that individuals that were 17 during 1987 are included in the sample. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.A5

Impact of Merit Aid Availability on Veteran Status: ACS
Less than HS Degree

Merit-Aid Measure	1987-2004 All	1987-2004 South	1987-2004 South Merit Only	1995-2004 All	1995-2004 South	1995-2004 South Merit Only
STRONG	0.00159	0.000109	0.00120	-0.000296	-0.00215	-0.000985
	(0.00109)	(0.00151)	(0.00218)	(0.00163)	(0.00192)	(0.00296)
INCLUSIVE	0.00149	0.00000	0.00145	0.000500	-0.00170	-0.000493
	(0.000920)	(0.00138)	(0.00173)	(0.00143)	(0.00179)	(0.00250)
N	175,752	73,075	32,774 / 40,510	116,913	48,304	21,850 / 26,979

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include state of birth and year of birth fixed effects. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. Sample restricted to individuals with less than a high-school degree (or GED). Regressions are limited to age-17 years indicated using data from the 2006-2011 ACS surveys. For example, inclusion of age-17 year 1987 indicates that individuals that were 17 during 1987 are included in the sample. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.A6

Impact of Merit Aid Availability on Veteran Status of White Males
Sensitivity Analysis

Omitted State	Arkansas	Florida	Georgia	Kentucky	Louisiana	Maryland	Michigan
Merit-Aid Measure							
STRONG		-0.00696**	-0.0113***	-0.00717**	-0.00938***		
		(0.00200)	(0.00274)	(0.00291)	(0.00263)		
INCLUSIVE	-0.00661***	-0.00639**	-0.00919***	-0.00651**	-0.00769***	-0.00769***	-0.00825***
	(0.00209)	(0.00219)	(0.00184)	(0.00224)	(0.00234)	(0.00236)	(0.00244)
Omitted State	Mississippi	Nevada	New Mexico	South Carolina	South Dakota	Tennessee	West Virginia
Merit-Aid Measure							
STRONG		-0.00888**	-0.00843**	-0.00786**	-0.00835***	-0.00768**	-0.00831**
		(0.00287)	(0.00268)	(0.00261)	(0.00254)	(0.00274)	(0.00322)
INCLUSIVE	-0.00780***	-0.00733***	-0.00758***	-0.00683**	-0.00758***	-0.00712***	-0.00712***
	(0.00223)	(0.00225)	(0.00225)	(0.00225)	(0.00226)	(0.00223)	(0.00223)

Note: Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include state of birth and year of birth fixed effects. Regressions are limited to age-17 1987 through 2004 years using data from the 2006-2011 ACS surveys. Sample restricted to states (of birth) ever having a strong or inclusive merit-aid program, respectively. Missing estimates indicate that that particular state is not in the "Strong" category. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.A7

Impact of Merit Aid Availability on Log Applications
Less than HS Degree

Merit-Aid Measure	1990-2004 All	1990-2004 South & Merit	1990-2004 Merit Aid Only	1995-2004 All	1995-2004 South & Merit	1995-2004 Merit Aid Only
STRONG	0.0624	-0.0372	-0.0108	0.0926*	0.0438	-0.00755
	(0.0478)	(0.0600)	(0.0418)	(0.0519)	(0.0515)	(0.0335)
INCLUSIVE	0.0566	-0.00932	0.00313	0.0913**	0.0421	-0.000868
	(0.0393)	(0.0602)	(0.0451)	(0.0360)	(0.0458)	(0.0402)
N	1,020	320	200	510	160	100

Note: Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state level in parentheses). Regressions weighted by state cohort size. All regressions include state and year fixed effects as well as cohort size. Regressions are limited to individuals with less than a high school diploma (and not currently in high-school) and years indicated using administrative application data. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 2.A8

Impact of Merit Aid Availability on Veteran Status: 2000 Census and 2006-2011 ACS

		1987-2004			1995-2004		
Merit-Aid Measure	DV Mean	All	South	South Merit Only	All	South	South Merit Only
STRONG	0.059	-0.00400***	-0.00337**	-0.00228	-0.00254***	-0.00220**	-0.00402***
		(0.000824)	(0.00126)	(0.00157)	(0.000836)	(0.000899)	(0.000952)
INCLUSIVE		-0.00366***	-0.00376***	-0.00156	-0.00180*	-0.00231**	-0.00289**
		(0.000817)	(0.00111)	(0.00132)	(0.000961)	(0.000966)	(0.00118)
N		2,044,699	672,341	286,499 / 361,505	1,300,422	430,795	183,637 / 230,664

Note: Each cell represents a separate regression. Coefficients are for indicator variable indicating presence of merit aid program (robust standard errors clustered at the state of birth level in parentheses). All regressions include state of birth and year of birth fixed effects. See Data Appendix or notes to Table 3 for more details of "Strong" and "Inclusive" definitions. Regressions are limited to age-17 years indicated using data from the 2006-2011 ACS surveys. For example, inclusion of age-17 year 1987 indicates that individuals that were 17 during 1987 are included in the sample. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Chapter 3

Fighting for Education: Veterans and Financial Aid

3.1 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. Over $40 billion dollars has been spent during the first five years of the program, and more than 1 million individuals have used the benefits. In the last few years, more than 25 percent of all federal grant aid to college students went to veterans.

While the fraction of individuals that join the military has dropped substantially over the last 60 years, it remains an important career path for Americans with over 5 percent (and nearly 9 percent of males) having served in the military before turning thirty. Furthermore, veterans are a potentially vulnerable population. They are frequently from low-income backgrounds and among the first in their families to seriously consider college. Some enter the military as a route to overcome credit constraints that prevent them from enrolling in college immediately following high school graduation (Barr 2014). Enlisted individuals who separate from the military may face obstacles translating skills learned in the military into those valued by the civilian labor market. Young veterans frequently face higher unemployment rates and health issues. Recent evidence suggests that education may play an important role in improving these types of outcomes.[1] As nearly all first-term enlisted soldiers leave the military without a post-secondary credential, the Post-9/11 GI Bill may be particularly important in helping veterans invest in skills that facilitate their transition to the civilian labor market. As a first step in ongoing work to answer this question, I explore how this increase in financial aid affected veteran educational choices and outcomes.

Summary statistics from the United States Department of Veterans Affairs (VA) indicate that veterans use the new benefits, yet almost nothing is known about veteran success in college or how the Post-9/11 GI Bill has affected veteran educational outcomes. This study provides the first evaluation of the longer run effects of the Post-9/11 GI Bill.

I estimate the effects of additional aid using a unique panel dataset constructed specifically for examining veteran educational outcomes. One of the primary roadblocks to research in this area is the lack of data that track veteran enrollment over time and veteran degree attainment. As no existing dataset contains a sufficient number of veterans to provide even a cursory analysis of veteran degree attainment, I created a new panel combining rich data on the choices of military service members from the Defense Manpower Data Center with postsecondary data from the National Student Clearinghouse (NSC).

Because benefit levels were based on active-duty service durations after September 11, 2001, but the benefit was not established until the summer of 2008, 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.[2] Using ineligible veterans as a control group, I use a difference-in-differences strategy to estimate effects of the benefit expansion. Unique among the GI bills and other federal financial aid programs, the Post-9/11 GI Bill provides different benefit levels depending on 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 benefit levels, while those in others received tens of thousands of dollars in additional max-

[1]See Oreopoulos and Petronijevic (2013) for evidence on earnings and Hout (2011) and Lleras-Muney (2005) for evidence on health.

[2]Barr (2013) provides support for the argument that the benefit announcement was not anticipated.

imum benefits per year. I leverage this geographic variation in covered tuition and housing allowance levels as an additional strategy to identify the effect of aid.

I find that the higher level of financial aid increases the likelihood that a veteran obtains a degree within six years of separation by six percentage points, a 30 percent increase. Most of the increase in degree attainment occurs via an increase in bachelor's degree attainment. Analogous estimates indicate that college enrollment increased by over seven percentage points. Assuming that these marginal enrollees complete college at similar rates to those enrolling prior to the benefit expansion, it appears that the higher level of benefits also helped some individuals, who would likely have dropped out without the benefits, to finish their degrees. One possibility is that the GI Bill helped veterans enroll in better schools, resulting in higher attainment rates. Indeed, veterans responded to the in-kind nature of the benefit expansion by enrolling in more expensive schools. Yet, there is no clear evidence that this resulted in veterans attending "better" schools as distinguished by likely completion outcomes. Another possibility is that, holding school choice constant, the higher level of benefits helped individuals persist, perhaps acting as a buffer against negative financial shocks for those with limited resources. Additional resources do appear to have this effect, but the estimated size of the effect of aid on persistence does not fully account for the proportion of the increase in degree attainment that is not explained by higher enrollment rates. I reconcile these results with evidence that many individuals did not switch from the MGIB to the Post-9/11 GI Bill when it became available in the fall of 2009, leaving thousands of dollars on the table. This result bolsters recent evidence that suggests more attention should be paid to simplifying access to government programs (e.g., Bettinger et al 2012).

In the next section, I provide an overview of the existing literature on veterans, financial aid, and college success. In Section 3, I describe the benefit expansion generated by the Post-9/11 GI Bill. Section 4 introduces a new panel dataset that allows a first look at the enrollment and attainment patterns of recent veterans. Section 5 formalizes the research design used to estimate the effect of additional financial aid on education attainment. Section 6 presents the main results, Section 7 provides evidence on mechanisms, and Section 8 concludes.

3.2 Veterans, Financial Aid, and College Success

Noted historian Sidney Burrell argued that the original GI Bill brought about "what may have been the most important educational and social transformation in American history" (Burrell 1967). It 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. Consistent with this argument, a number of studies have found positive effects of the mid-twentieth century GI bills on educational attainment and earnings (Bound and Turner 2002, Stanley 2003, Card and Lemieux 2001). These studies primarily rely on variation in pre-existing enlistment rates or the likelihood of being drafted in identifying the combined effect of military service and higher aid levels.

Similarly, studies of the Vietnam-era GI Bill provide estimates of the combined effects of compulsory military service and higher education benefit levels. Through a clever use of the Vietnam-era draft lotteries, Angrist (1990) demonstrates that the combined effects result in increased earnings for young veterans. Using the same strategy 20 years later, Angrist and Chen (2011) find large increases in schooling, consistent with those reported for earlier GI Bills. Surprisingly, there was no positive effect on earnings. The authors reconcile these results with the earlier positive earnings estimate by arguing that the earnings gap closes due to a flattening of the age-earnings profile at later ages and modest returns to the schooling induced by the GI Bill.

There have been only a handful of analyses of the educational benefit use of veterans enlisting after 1973. These studies are distinct from earlier GI Bill analyses in that they focus on the effect of additional benefits conditional on military service, instead of the joint effect of both. Using a small group of veterans who participated in the 1987 Survey of

Veterans, Angrist (1993) presents 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, 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.

There has been very little work done thus far to explore the effects of the Post-9/11 GI Bill on educational outcomes; in fact, there is only one paper of which I am aware to explore the causal effects of the benefit expansion (Barr 2013). Using publicly available survey data and a difference-in-differences strategy, in Barr (2013) I find that the benefit expansion increased the probability that young veterans are enrolled by 20 percent. However, data limitations prevent any exploration of the effects on longer-term outcomes or a more detailed analysis of the effects on school choice.

This paper also contributes to a small but growing literature on the effects of financial aid on degree attainment. While research on the effects of financial aid on the *enrollment* of traditional students is abundant, there is relatively little known about its effects on degree attainment. Furthermore, the effects of aid on enrollment and attainment are open questions for the older non-traditional student population.[3] 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. Greater non-college financial demands may mean that financial aid plays an even more important role in college going for veterans and other non-traditional students.

Studies focusing on persistence and degree attainment are largely limited to the evalua-

[3] In addition to Barr (2013), 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.

tions of state merit-aid programs, and the evidence is mixed (Dynarski 2008, Scott-Clayton 2011, Sjoquist and Winters 2012). Work on the longer run effects of need-based aid is sparse. Recent papers by Castleman and Long (2012) and Goldrick-Rab et al (2012) provide conflicting evidence. Castleman and Long (2012) leverage a discontinuity in the need-based aid formula for the Florida Student Access Grant (FSAG), and find that an additional $1,300 in grant aid resulted in a 4.3 percentage point increase in the probability of staying continuously enrolled through the spring semester of a student's freshman year at public schools and similar effects on the probability of earning a bachelor's degree within six years, suggesting an important role for financial aid in increasing degree attainment.[4] In contrast, Goldrick-Rab et al (2012) demonstrate no positive effect of financial aid on persistence past the second year in college.

Non-traditional students now comprise roughly half of college enrollment and they receive the majority of federal financial aid, yet there is little research on how aid affects their collegiate success. As a sizable group of non-traditional students, understanding the effects of financial aid on veterans may contribute to our understanding of how aid affects the degree attainment of older individuals more generally.

3.3 From the Montgomery GI Bill to the Post-9/11 GI Bill

Prior to the Post-9/11 GI Bill, the Montgomery GI Bill (MGIB) served as the primary education benefit for military veterans. Implemented in 1985, the MGIB remained an option for enlisting active-duty personnel until at least 2009. Eligibility for the MGIB depends largely on three factors. First, the MGIB was not an entitlement; individuals choose whether or not to participate in the program and commit to a reduction of pay of $100 for each of the first 12 months on active duty. Second, an individual has to complete the minimum active-duty contract agreed to upon enlistment with an honorable discharge; this was generally at

[4] The study makes a valuable contribution, but it is limited by data constraints that make it impossible to track attainment outside of Florida colleges; thus, while degree attainment at Florida colleges has increased, it is unclear to what extent this is a result of a shift in enrollment towards in-state schools.

least three years. Third, individuals commissioned through military academies or recipients of ROTC scholarship funds are not eligible for the benefit.

Under the MGIB, the VA sends the benefits directly to veterans who choose to enroll. Benefit levels have been raised periodically over time to adjust for inflation and are a different flat amount per month to individuals enrolled in school half-time, three quarters time, or full time.[5][6] 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 are 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 MGIB is not tied to a particular school or location and there is little restriction on the types of programs approved; individuals can use the benefit for vocational training, apprenticeships, flight classes, and test fees as well as formal training leading to a degree.

Congress approved the Veterans Educational Assistance Act of 2008, more commonly known as the Post-9/11 GI Bill, Webb GI Bill, or New GI Bill, during the summer of 2008.[7] The benefit expansion was implemented beginning in August 2009, providing additional benefits to individuals with active-duty service after September 11, 2001.[8]

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.[9] 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

[5] 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.

[6] 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 a fourth ($300/$1,100) 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 might have had an incentive to delay enrollment in order to capture the higher level of benefits available in the fall of 2009. Barr (2013) demonstrates that this is not occurring in practice.

[8] These benefits could only be applied to enrollment occurring after August 2009.

[9] 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.

post-9/11 service results in an additional 10% of eligibility; veterans with greater than three years are eligible for 100%. In practice, nearly all veterans separating honorably after 2005 are eligible for the maximum benefit.

There are two major components of the Post-9/11 GI Bill benefit: (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.[10] 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.[11] 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 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 was capped at the maximum public in-state tuition and fee levels set by the VA.[12]

Veteran students enrolled half-time or more received a second benefit component, a monthly housing allowance based on the zip code of the institution that the student attends. In 2009, this monthly housing allowance ranged from under $800 in many rural areas more than $2,700 in New York City. The housing allowance, like the MGIB, is paid directly to veterans as a conditional cash transfer.[13]

The biggest difference between the two GI Bills 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

[10] In 2011, coverage for private colleges changed to $20,000 across the country. As very few veterans are likely to be affected by this change, it is unlikely to have a substantial effect on the estimates.

[11] 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 them). These questions are being investigated in a complementary project. Second, benefits sent to the veteran on a regular basis may be more salient.

[12] 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.

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

roughly \$50,000 in total benefits. 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 of 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.

The degree of benefit expansion varies dramatically by geography. To quantify this variation, I estimate the combined annual maximum tuition and housing allowance for each state.[14] As an estimate of a year's worth of housing allowance benefits available in a state, I use the weighted average monthly housing allowance across zip codes in a state, multiplied by 9.[15] Table 3.A1 and Figure 3.1 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. This variation in nominal benefit levels under the new GI Bill generates fifty-one micro experiments in which, relative to earlier cohorts, veterans returning to some states received much larger benefit increases than those returning to others. Even greater variation occurred at the school level as tuition and housing allowance benefit amounts are determined primarily by the school attended. I use this school-level variation to estimate the effects on year-to-year persistence of veterans enrolled during the fall of 2008.

[14] I exclude the benefits for fees as the maximum fee levels are generally orders of magnitude larger than the true fee levels charged by all public institutions in a state.

[15] This corresponds to nine months of enrollment in a traditional two-semester system.

3.4 Data

No existing dataset contains enough veterans to obtain even a basic idea of veteran collegiate outcomes, much less an evaluation of the causal effects of the benefit expansion 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).[16] 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 services 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 data cover all individuals on active duty in the Army, Navy, Marine Corps, and Air Force, over four million in total.[17] In addition to the standard demographics available in most large survey datasets, I also observe information on birth, home, and residence locations; occupation codes; and ability scores. I use 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).

I link individuals from the active-duty files with data that capture information on educational outcomes. Due to cost considerations associated with the data linking, I restricted the population in several ways before drawing a random sample to be matched. First, I restricted the military dataset to enlisted individuals between age 22 and 39 who separated within ten years of initial entry into the military. I exclude those separating with more than ten years of service and those aged forty and over because (1) they are more likely to view

[16] "Companies" likely refers to the for-profit entities that receive GI Bill benefits.

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

the military as a permanent career, and (2) they are substantially less likely to enroll in college. I then selected a random sample of individuals separating with an honorable discharge between January 1, 2000 and December 31, 2009 as well as a smaller random sample of those separating with less than honorable discharges to use as a control group. I linked this sample of approximately 75,000 veterans with post-secondary data obtained from the National Student Clearinghouse (NSC).

The NSC, founded in 1993, initially gathered data used to ascertain the enrollment status of students with loans. Over time, the breadth of NSC's enrollment coverage has increased dramatically, from 40% in 1996 to 80% in 2001 to over 90% by 2014. As Dynarski et al (2013) note, this can create a challenge for researchers examining the effect of policies affecting different cohorts over time. I follow their suggestion and account for the change in *enrollment coverage* by restricting my sample to institutions reporting at the beginning of my sample; in most specifications, this is 2002.[18]

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. As of 2014, NSC's data cover roughly 90% of all degrees granted in the United States; as schools generally provide several decades of historical data when they join NSC's "DegreeVerify" service, changing *degree coverage* by cohort is not a significant concern. The linkage between datasets occurred during August 2014; therefore, the sample contains enrollment and degree attainment information up to that point.[19]

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. 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

[18]Where applicable, I note changing inclusion of institutions in the notes to each table. Enrollment results are robust to varying the cutoff year for inclusion.

[19]See Data Appendix for further details of the NSC data and the data linking process.

institution (i.e., public, private non-profit, or for-profit), whether the college is a two-year or four-year school, measures of selectivity, and graduation rates.

3.4.1 A Picture of Veteran Educational Choices and Outcomes

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).

Given the paucity of information on veteran enrollment and attainment, I begin by illustrating how the enrollment and attainment of young veterans has changed over time. Figure 3.2 presents the share of veterans earning a degree between one and six years after separation. The proportions are presented separately by year (cohort) of separation. 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 provides a natural breaking point for veterans considering enrolling on the traditional college calendar, and (2) it allows me to focus on veterans separating at least one year prior to benefit implementation whose separation is unlikely to be influenced by the benefit expansion. The figure plots the proportion of veterans in a cohort who have obtained an associate's degree or higher in the years after separation.[20] Degree attainment rates for the 2003 and 2004 cohorts (blue and red circles), those with highly delayed eligibility for Post-9/11 GI Bill funds, are nearly identical.[21] Roughly 17 percent of these individuals receive any college degree within six years of separation. The 2008 cohort (green triangles), those with access to Post-9/11 GI Bill benefits, are between five and seven percentage points

[20] The sample is limited to individuals with less than a bachelor's degree at the date of military separation.

[21] 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.

more likely to obtain a degree over this time period.

Figure 3.2 thus indicates that individuals eligible for the higher levels of aid provided by the Post-9/11 GI Bill (those separating in 2008) were more likely to obtain a degree, but it does not tell us whether this results from an increase in the probability of completion conditional on enrollment or an increase in enrollment. Figure 3.3 plots the percentage of likely GI Bill eligible and ineligible veterans enrolled within three years of separation, by year of separation.[22] Here, likely eligibility is determined by having an honorable service characterization at separation.[23][24] 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 in 2007. All eligible veterans separating in 2008 had access to the higher benefit levels within a year or two of separation, explaining the substantially higher enrollment levels for these individuals. The three-year enrollment rates of eligible veterans separating in 2008 and 2009 are roughly eight percentage points higher than those of cohorts separating between 2002 and 2006. The enrollment of veterans unlikely to be eligible exhibits no such pattern. Combined, the evidence suggests that a portion of the increase in degree attainment came via increased enrollment.

Overall, the figures indicate that nearly half of the eligible veterans in the sample enroll in college and around 20 percent obtain an associate's degree or higher 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

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

[23]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.

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

degree of some type within six; this is comparable to the six-year degree receipt of individuals starting college between 19 and 24 (Baum et al 2013) and substantially higher than statistics presented in the media. In the next section, I turn to more formal strategies to evaluate the effect of additional financial aid.

3.5 Estimation Strategy

I use multiple strategies to identify the effects of financial aid on degree attainment. I begin with OLS estimates, simply identifying the effect of the benefit expansion as the difference between degree attainment rates of veterans separating in the pre-period (2003 or 2004) and those separating in the post-period (2008), conditional on a large set of controls. To deal with concerns about non-GI Bill factors that change between the pre and post-period and that affect degree attainment, I complement this approach with a difference-in-differences (DD) strategy, using ineligible veterans as a control group. As ineligible veterans may respond differently to changing conditions, I further consider specifications that "pre-process" the data to generate a control group that is observationally quite similar to the treated group prior to implementing the DD. Finally, I leverage geographic variation in the size of the benefit expansion to identify the effects of additional aid.

3.5.1 Over Time and DD Estimation

My first specification formalizes the descriptive evidence on degree attainment in Figure 3.2, using over-time variation in the availability of benefits to identify the effect of the benefit expansion:

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \gamma Post911_t + \epsilon_{ist} \tag{3.1}$$

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. The sample is restricted to individuals separating in the 2003, 2004, and 2008 cohorts. As mentioned above, I use the 2003 and 2004 cohorts as the pre-period as their degree receipt is unlikely to be 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. The initial parameter of interest, γ, is the estimate of the effect of additional benefits, simply comparing the degree attainment of individuals separating in 2003 or 2004 with that of those separating in 2008. If I am appropriately controlling for all time-variant variables that affect degree attainment, then γ provides an unbiased estimate of the average treatment effect of the benefit expansion. As with all approaches leveraging variation over time, if, conditional on observables, there are other factors changing between the two periods that affect degree attainment, these estimates will be biased. For example, if state-year unemployment rates fail to appropriately control for the effect of the Great Recession on degree attainment, the estimate will be biased.

The DD specifications address this concern by including a control group that provides the counterfactual change in degree attainments over this period. For the DD specifications, veterans with an honorable service characterization at separation form the treatment group and veterans with a less than honorable characterization are the controls. Recall that only veterans with an honorable service characterization are eligible for veteran education benefits.[25] The DD specifications are similar to the first specification, adding ineligible veterans

[25]Empirically, roughly 10% of enrolled veterans with a less than honorable service characterization are observed receiving MGIB benefits. This will bias my estimates towards zero.

as a control group:

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \lambda_t + \gamma_0 Eligible_i + \gamma_1 Post911_t * Eligible_i + \epsilon_{ist} \tag{3.2}$$

The terms λ_t are fixed effects for each state and year of separation. All other variables and sample restrictions are equivalent to those in equation (1). 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 whether ineligible veterans are a reasonable control group in the results section. Here, I note two pieces of evidence that support the notion. First, the two groups are observationally similar. 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, I also implement a strategy to select a control group that is observationally equivalent to the treated group. 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 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.

3.5.2 Geographic Variation in the Size of Benefit Expansion

I turn to a final strategy that does not make use of ineligible veterans as a control group. This complementary strategy leverages geographic variation in the size of the benefit expansion (Figure 3.1 or Table 3.A1). Assigning eligible individuals to states based on their home state at time of separation, I link in average maximum expected benefit levels:[26]

$$E_{ist} = \beta_1 X_{ist} + \beta_2 Z_{st} + \alpha_s + \lambda_t + \gamma Post911_t * Benefit_s + \epsilon_{ist} \tag{3.3}$$

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 ($Benefit_s$).[27] 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.[28]

The key assumption is that, conditional on the set of observables, the unobserved factors that affect enrollment and 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 resi-

[26] I exclude ineligible veterans as there are not enough ineligible veterans in state-by-separation year cells to support estimation.

[27] All other variables are defined as above.

[28] As the new GI Bill itself illustrates, a dollar may not be worth the same amount everywhere in the country. As such, I also present estimates that adjust the differences in the benefit expansion for cost of living differences.

dence 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 at separation to assign benefit levels.[29] The benefit maximum in a veteran's home of record at separation will be strongly correlated with the actual benefit maximum faced by the veteran, but will be uncorrelated with preferences for education after conditioning on state fixed effects. To the degree that 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 comparing with the average treatment effect estimates provided by the other estimation approaches.[30]

3.5.3 Persistence of those Enrolled

As part of an overall effort to understand effects of the aid on degree attainment, I also examine the direct effect on persistence. I take a different approach as I now 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. As demonstrated in Table 3.A1, at the state level, there were both large increases in benefit levels and variation in the size of the benefit increase experienced. These averages mask even greater variation occurring at the school level. Unlike the MGIB, the benefit levels for the Post-9/11 GI Bill are directly related to the cost of attendance at that school. Thus, those enrolled during

[29] Although limiting the sample to individuals separating prior to the fall of 2008 mitigates these types of migration concerns, I address this and other threats to internal validity below.

[30] While I cannot observe veteran residence after separation, 65% of eligible veterans enrolled during the first three years following separation are enrolled in the state indicated as their home of record at separation. Assuming this proportion is similar for all veterans and that veterans that do not return to their home state move randomly to other states, provides a lower bound for the scaling factor of $1/.65 = 1.54$.

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 a month. College B is located outside of a major metropolitan area and has an associated housing allowance of $2,085 a 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.[31] I leverage this variation 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.[32] The basic specification is:

$$E_{ict} = \beta_1 X_i + \beta_2 Z_{ct} + \lambda_t + \beta_3 C_c * \lambda_t + \alpha_c + \gamma Post911_t * Benefit_c + \epsilon_{ict} \tag{3.4}$$

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 as the bill only became law on June 30, 2008 and was not implemented until August 1, 2009.[33] Individual characteristics X_i control for variation in the likelihood of persistence due to

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

[32] 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.

[33] As discussed in Barr (2013), the details of the benefit expansion were not well defined or publicized until the beginning of 2009 at the earliest.

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. I interact school graduation rates C_c with the year fixed effects λ_t as the persistence of individuals at different types of schools may be affected differently by changing conditions over time. 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.[34] The implied benefit $Benefit_c$ is equal to nine times the basic monthly allowance for housing plus tuition and fees up to the covered levels.[35] I also estimate specifications that allow the two components of the benefit, tuition coverage and housing allowance, to affect persistence differently:

$$E_{ict} = \beta_1 X_{ist} + \cdots + \gamma_1 Post911_t * TuitionBen_c + \gamma_2 Post911_t * BAH_c + \epsilon_{ict} \tag{3.5}$$

If, for example, the tuition benefit crowds out institutional aid or is less salient to the veteran it may have less of an effect on persistence. I estimate this specification separately by college type as tuition benefits may correlate with heterogeneity in the effect of additional benefits on persistence.

3.6 Effects of Additional Financial Aid

I begin with a presentation of the effects of aid on degree attainment using the three different strategies I outlined above. The results are similar across specifications, suggesting that higher levels of aid increase degree attainment, and further that the results are not

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

[35]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.

sensitive to potential confounding sources of endogeneity. I also devote considerable attention to addressing threats to the internal validity of the different strategies.

3.6.1 Difference and DD Estimates

Estimates from the basic difference approach in Table 3.2 confirm the descriptive evidence. Each cell contains an estimate of the difference in degree attainment rates of veterans separating in the pre and post-periods. The estimates in column (1) indicate that veterans separating in the post-period are 5.4 (6.5) percentage points more likely to have an associate's degree or higher within 5 (6) years. Most of this effect is driven by an increase in bachelor's degree attainment, as shown in (2).

The difference-in-differences estimates in Table 3.3 are only slightly smaller than the estimates produced using just the over-time variation. Each cell contains the coefficient on the interaction of $Eligible_i$ and $Post911_t$. The DD estimates indicate that eligible veterans separating a year prior to passage 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 passage, an increase of over 25 percent.[36] 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.[37] Most of this increase in attainment is occurring 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, more than 30 percent higher than the base.

[36] Recall that those separating 2003 and 2004 are eligible for zero and one year, respectively, of Post-9/11 GI Bill benefits during the first six years after separation.

[37] The effect of actual aid levels is likely larger as 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.

3.6.2 Addressing Threats to 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 3.4 addresses this parallel trends assumption. The event study plots the coefficients and confidence intervals from a similar specification as in equation (3.2) 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 plot suggests that the attainment levels of eligible and ineligible veterans roughly move together for veterans separating between 2002 and 2006, but diverge sharply in 2007 and 2008.[38] I include the 2002 and 2005 through 2007 separation cohorts in Figure 4 for illustrative purposes, although they are not in the sample underlying Table 2.[39]

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 3.A2, 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.

As described in the Estimation Strategy section, I use a pre-processing approach to select ineligible veterans that are similar to eligible veterans. I implement this approach separately in the pre and post-period (see Appendix A for details of the process). Table 3.A2 illustrates how the pre-processing improved the covariate balance between the two groups, making them observationally equivalent across a range of covariates. Table 3.A3 presents the DD estimates using this pre-processed control group. The point estimates are somewhat larger

[38]This is somewhat surprising for the 2005 through 2006 cohorts as 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 during the first few years after separation. In contrast, a similar event study for bachelor's degree receipt shows an upward trend in attainment rate beginning in the 2005 and 2006 cohorts. This makes sense as BA degree recipients generally enroll for at least four years.

[39]The 2002 cohort is excluded because MGIB benefit levels were increased in 2003. The 2005 and 2006 cohorts are excluded because they are partially treated (i.e., eligible for the higher level of benefits within a few years of separation).

but statistically indistinguishable from those using the unadjusted DD approach.

Another potential concern is that the Great Recession may be responsible for increased attainment levels. 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.[40] While 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.

Another concern is that the composition of separating veterans has changed over time in ways that are not controlled for or observed. Barr (2013) 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. A related threat to internal validity arises if veterans interested in using their education benefits were more likely to separate in 2008. But, the sample is restricted to veterans separating prior to the end of the summer during which the Post-9/11 GI Bill was passed. It is extremely unlikely that these individuals separated in response to a benefit expansion that had not yet been passed.

3.6.3 Estimates Exploiting Geographic Variation

Next, I turn to a complementary approach that circumvents many of the concerns that arise with the DD estimates. In Table 3.4, I present estimates of the effect of an additional thousand dollars 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

[40] Results are identical when adding these controls.

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 over 0.2 percentage points. As discussed in the prior section, not all veterans actually return to or remain in their "home of record" state, similar to crossover in an experiment. As such, the results in Table 3.4 are intent to treat estimates that should be scaled up before comparing to the DD estimates. Roughly 65 percent of enrolled veterans are enrolled in their home 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.[41] This calculation indicates that each additional $1,000 of aid increases the likelihood of dgree attainment by .31 percentage points, slightly smaller, but statistically indistinguishable, from the effect sizes implied by the DD estimates.[42] Again, most of the effect is driven by increased bachelor's degree attainment.

Table 3.A5 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.[43] The effect sizes are slightly smaller, but statistically indistinguishable, from the unadjusted estimates in Table 3.4.

Tables 3.5 and 3.A6 allow the effect on degree attainment to vary by benefit type. Table 3.5 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 3.A6 adjusts each benefit by cost of living; while the effects remain statistically indistinguishable, the estimates suggest that increases

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

[42]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 (estimates using simulated benefit size exhibit a stronger effect on behavior - see Table 3.A4.). Second, it is unclear to what degree all veterans were aware of the exact nature of the provided benefit (see below for further discussion). Finally, if veterans moved to take advantage of benefit levels, estimates will be biased towards zero.

[43]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.

in the cash portion of the benefit may have larger effects on attainment.

Addressing Threats to Internal Validity of the Geographic Approach

A major concern associated with these 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 these factors and not the changing benefit availability that is causing veteran attainment rates to change differentially across states. Table 3.A7 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 time.[44] 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 3.A8 presents these estimates for a number of age groups and education 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. While the point estimates are fairly tight zeroes, the negative coefficients for the civilian population, combined with the appropriation results, suggest another reason that the geographic variation approach may produce underestimates for the effect on veterans.

[44]State appropriations for higher education collected from Grapevine reports produced by Illinois State University's Center for Education Policy and include ARRA stimulus funds.

3.6.4 Demographic Heterogeneity

Certain types of veterans may be more likely to benefit from the additional funds. For example, veterans with low AFQT (aptitude) scores may not be suited for college and may not graduate regardless of the price. On the other hand, younger veterans (younger than 30) may have a high return to finishing, but lack the resources to do so absent the benefit expansion. Table 3.6 illustrates heterogenous treatment effects. While all of the point estimates are statistically indistinguishable, there is suggestive evidence that the benefit expansion increased the college attainment of high ability individuals more than that of low ability individuals with the degree attainment of high ability individuals increasing by nearly nine percentage points, almost double the point estimate for those with AFQT scores below the median.

3.7 Evidence about 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. Under simple formalizations of the college choice problem, individuals are making decisions about where to enroll and how much schooling to obtain based on a comparison of discounted future benefits and current costs. The Post-9/11 GI Bill has primarily two effects. First, it reduces the cost of most instituitions. This may affect enrollment or attainment as a subsidy to education or by loosening credit constraints. Second, the Post-9/11 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.

As a subsidy, a decrease in the cost of schooling will make higher levels of educational attainment more attractive. If separating soldiers have limited resources and face difficulty accessing sufficient credit to finance school, the effect of a decrease in the cost of schooling may be magnified.[45] Indeed, there is evidence that many veterans enlist specifically to overcome credit constraints faced immediately following high school graduation (Barr 2014). As most separating soldiers have limited financial resources and substantial familial responsibilities, these constraints may play an important role in their decision to enroll in college. On the other hand, the relatively high level of financial aid provided by the MGIB makes it less likely that credit constraints are binding.

Turning to persistence, the largest financial returns to education appear to come via degree attainment, so it is somewhat of a puzzle that many individuals enroll in college only to drop out prior to obtaining a degree. One possibility is that credit constrained individuals are unable to weather unexpected financial shocks.[46] The increased resources provided by the Post-9/11 GI Bill will have much stronger effects on persistence if the latter is true.

Other evidence from Massachusetts suggests that college quality may have important effects on persistence and degree completion (Cohodes and Goodman 2013). In contrast to the Massachusett's scholarship, the Post-9/11 GI Bill makes higher tuition schools more appealing. If veterans respond to these incentives, and tuition levels correspond to college quality, quality upgrading may contribute to the overall increase in degree attainment. Below, I provide evidence for how each of these mechanisms contributes to higher degree attainment levels.

3.7.1 Enrollment

An open question is whether the positive effects on degree attainment are a result of increased completion rates, increased enrollment, or both. In order to better understand the portion contributed through each channel, I first estimate specifications similar to equation (3.2) for enrollment. Table 3.7 presents the standard difference-in-differences estimate of whether an individual has enrolled in any program within three years of separation. Post-

[45] This result is well known and a sizable literature has tested for the presence of credit constraints in the human capital investment decision (see Lochner and Monge-Naranjo 2011 for an overview).

[46] Uncertainty about individual college returns is another explanation to this puzzle (Stange 2011).

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. The DD estimates are substantially larger than the estimates using geographic variation in the size of the benefit expansion (Table 3.8), suggesting that veterans may have been unaware of the variation across geography.[47] While Barr (2013) 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 3.7 and Table 3.8.

Table 3.9 presents the DD results by subgroup. Interestingly, the enrollment estimates for the low and high ability are opposite in magnitude from the degree attainment estimates, suggesting that the higher benefit levels may have pulled weaker ability students into school, who then failed to graduate.[48]

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.[49] This suggests that the other half of the degree attainment effect is coming through the increased persistence channel. This increased persistence may be a result of (1) increased school quality, or (2) the increased level of resources available to veterans holding school choice constant. While I cannot pin down these pieces completely, I can provide some evidence as to the contributions of each.

[47] Reconciling this with the effects on degree attainment suggests that veteran enrollment was responsive to awareness of the general 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.

[48] Another explanation is that they primarily entered certificate programs. I am still exploring this possible explanation.

[49] 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.

3.7.2 School Choice

Recent evidence points to the importance of school quality in influencing completion rates (Bound et al 2010, Cohodes and Goodman 2013). The Post-9/11 GI Bill reduces the relative cost of more expensive institutions, so we might expect individuals to gravitate toward these types of schools. If college quality is positively correlated with cost, this may result in quality upgrading that translates into higher degree attainment. This is a difficult question to address for several reasons. First, college quality is very difficult to measure, particularly outside of the most selective set of institutions. Second, disentangling the effect of the benefit expansion on school quality is complicated by the fact that veteran enrollment was also affected. 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 inframarginal enrollees are actually enrolling in more expensive schools. Third, due to limitations in the coverage of the NSC data, I am likely to substantially 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 in Table 3.10.

The first row presents the simple change in enrollment for eligible veterans from the pre to the post-period. Most of the increase in enrollment occurs at four-year schools. While small in percentage terms (2.5 pp), the increase in for-profit enrollment is over 50%. Columns (6)-(8) indicate 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 $293 per year in benefits under the Post-9/11 GI Bill (column (8)). While it appears that veterans are enrolling in more expensive institutions, column (9) indicates that they are not 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.[50] The second set of results uses the geographic variation in the benefit expansion. These results are largely consistent

[50]Ongoing work will disentangle these results further by controlling for underlying propensities to enroll.

with the overall changes in the first row. A $1,000 increase in maximum benefits in the state results in enrollment at a school that with, on average, roughly $20 more in annual benefits under the Post-9/11 GI Bill (column (6)). The last two columns contain 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 quality. Next, I turn to an examination of the effect of additional resources holding school choice constant.

3.7.3 Persistence

Restricting the sample to eligible veterans enrolled during the fall of 2007 or 2008, I use variation generated by the Post-9/11 GI Bill for those considering enrollment in 2009 that occurs at the school level. 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.[51]

Table 3.11 presents estimates of the effect of an additional $1,000 in combined benefits on the likelihood of enrollment the following fall (two semesters out).[52] Individuals are 0.5 percentage points more likely to be enrolled for every $1,000 of additional aid. Effect sizes are similar when including school fixed effects. Table 3.12 allows each benefit type to independently affect veteran enrollment. While statistically indistinguishable from 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 may be a result of the

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

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

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 persistent 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 increase in persistence translated to degree attainment, we would expect to find an overall increase in degree attainment of close to (5*0.005*0.45) 1.25 percentage points. This is unrealistic because many veterans drawn to enroll for another year will still drop out prior to obtaining a degree. Assuming that 50 percent will eventually receive a degree suggests that degree attainment would increase by close to 0.6 percentage points through the increased completion channel.[53] 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.

3.7.4 Explaining Lower than Expected Persistence Results: Switching Costs

A potential explanation for the observed gap 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. While this may be the case because those enrolling later had access to additional years of higher benefit levels, another explanation is that many veterans already enrolled in 2008 did not to switch to the new benefit. Recent research across a variety of fields suggests that information frictions and switching costs may play a much greater role than previously thought (Bettinger et al 2012, Keys et al 2014). In the case of switching between GI Bills, the choice seems obvious for most individuals; if the benefit of switching to the new GI Bill minus the cost is positive, which it is for most, they

[53] Recall that slightly less 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.

should switch. However, as has been demonstrated in other populations, veterans may not switch if they are unaware of the benefit expansion, are unaware of their individual benefit from switching, or find it too costly to do so.[54]

Figure 3.5 plots the share of enrollees receiving MGIB benefits during the fall semester between 2006 and 2012. Although nearly all veterans are likely to receive greater benefits under the Post-9/11 GI Bill, the figure shows that during the first semester of implementation (fall 2009) forty percent of enrolled veterans were still using the MGIB. 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, implied by those that choose not to switch. Figure 3.6 plots the distribution of implied switching costs for veterans enrolled and using their MGIB benefits during the fall of 2008 who remained enrolled and using their MGIB benefits during the fall of 2009. While a small fraction 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 share of veterans choosing to continue MGIB receipt is quite large, with roughly fifty percent continuing to receive the old benefit during the fall of 2009. An open question is what explains the large share that did not switch. 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.

Table 3.14 presents estimates from specifications that regress the decision to continue MGIB benefit usage on a variety of veteran observables and the estimated gain from switching. The sample is restricted to individuals enrolled and receiving MGIB benefits during the fall of 2008 who continued enrollment during the fall of 2009. Veterans' decisions to switch benefits are responsive to the size of the gain from switching; an additional thousand dol-

[54]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 hour.

lars in benefits results in a 1.5 percentage point greater chance that a veteran will switch.[55] Older individuals and those with lower aptitude levels are also less likely to switch benefits, suggesting that policies should be particularly attentive to the presentation of information and the simplification of the process for these groups. Overall, I view this as evidence that those already enrolled were less likely to access the more generous benefit and thus the effect on year-to-year persistence was lower than that for veterans entering school after the new GI Bill's implementation.

3.8 Discussion and Conclusion

Overall, my results suggest that the Post-9/11 GI bill increased degree attainment in my sample by over 30%, roughly 0.4 percentage points per $1,000 of additional 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 veterans are attending more expensive schools, there is little evidence that increased school quality has contributed to higher persistence levels. In contrast, estimates of the effect of the benefit expansion on the year-to-year persistence of veterans indicates sizable effects of higher benefit levels, with an additional $1,000 increasing one year persistence by 0.5 percentage points. The effect of additional aid on persistence is likely substantially larger than this as many veterans enrolled prior to the benefit expansion did not switch to the new benefit. The very large implied costs of switching between the benefits provides further evidence that more focus needs to be placed on overcoming information frictions that prevent programs from operating as expected.[56]

These results also inform our understanding of how financial aid affects degree attain-

[55] I have data on MGIB but not Post-9/11 GI Bill usage. In the discussion, I am implicitly assuming that all veterans who received MGIB benefits during the fall of 2008 who are still enrolled during the fall of 2009, but are not observed receiving MGIB benefits, are receiving Post-9/11 GI Bill benefits.

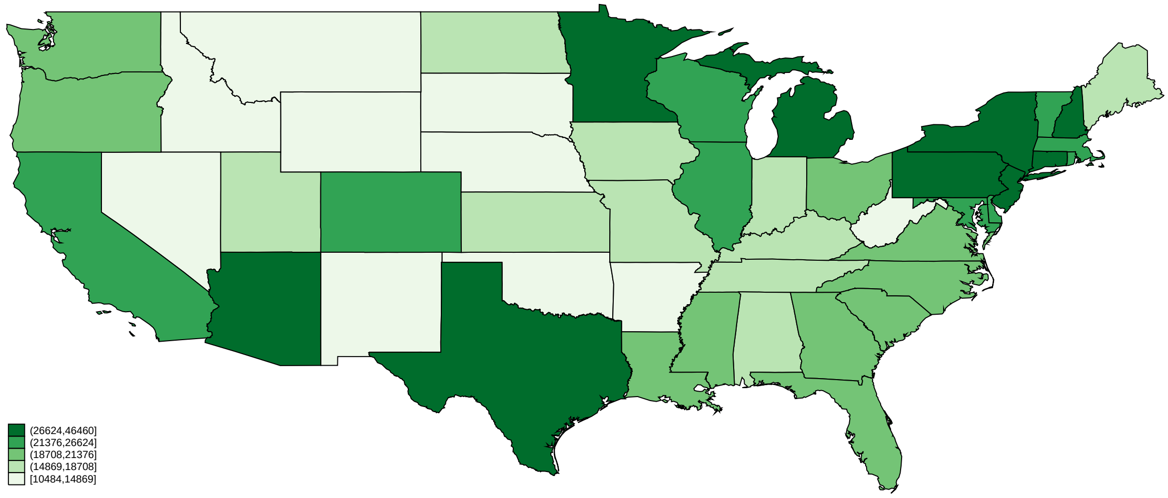
[56] The lack of quality upgrading and the inefficient switching results motivate an ongoing experiment with a branch of the military in which we are randomly assigning prompts and school quality information to potentially remove information frictions for separating soldiers.

ment for older non-traditional students. Similar to other older students, veterans frequently enroll as a way to transition between occupations. Despite the growing importance of non-traditional students, who now account for roughly half of all enrollment and financial aid expenditures, the existing research focuses entirely on the traditional student population (Dynarski and Scott-Clayton 2012). I have shown here that even at high initial levels of support, additional financial aid helps veterans to obtain a college degree. The effects on college success may be even larger for similar civilian students who likely receive less aid than veterans under the MGIB.

A natural question is whether this aid intervention is a good investment. Prior estimates of the effect of degree attainment on earnings and employment suggest that this increase in education levels will help hundreds of thousands of veterans transition into the workforce. Whether or not the benefit expansion is a beneficial social investment is another question. Back of the envelope calculations of the direct costs of each additional degree produced by the benefit expansion suggest costs between $75,000 and $150,000, higher than most financial aid interventions.[57] While existing work suggests large private returns to education, ongoing work will explore the degree to which prior research generalizes to veterans.

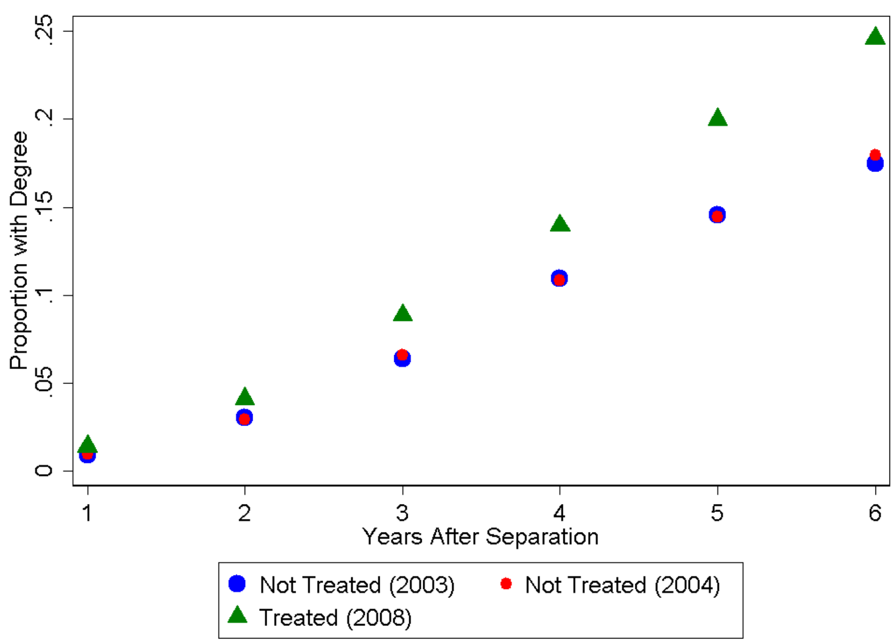
[57] See Appendix B for the details of these calculations.

Figure 3.1: Estimate of Annual Combined Maximum Benefits



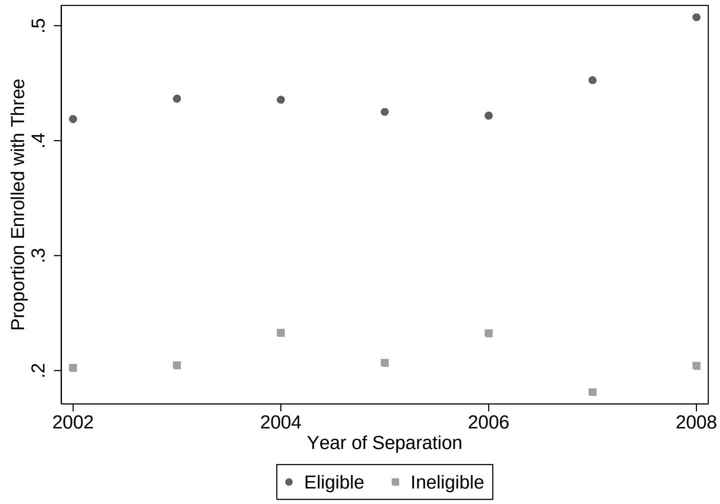
Note: Estimate of annual combined maximum is equivalent to 9*BAH + 24*Credit Maximum. Tuition credit maximums for 2009-2010 obtained from `http://www.gibill.va.gov/gi_bill_info/ch33/tuition_and_fees_2009.htm`. Basic allowances for housing are weighted state averages of zip code BAH levels for 2010.

Figure 3.2: Attainment of any Degree by Separation Year



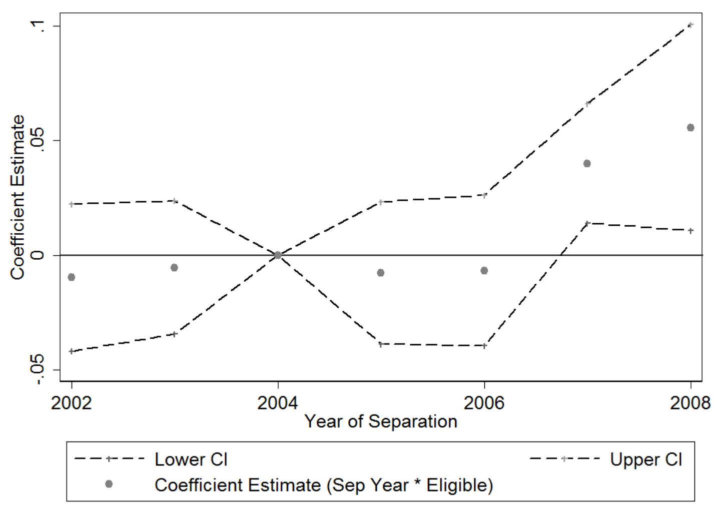
Note: Each marker represents the share of eligible veterans that obtained any degree by years after separation. Statistics are presented separately for the 2003, 2004, and 2008 cohorts. See Table 3.2 or the text for details of the sample restrictions.

Figure 3.3: Three Year Enrollment Rates by Year of Separation



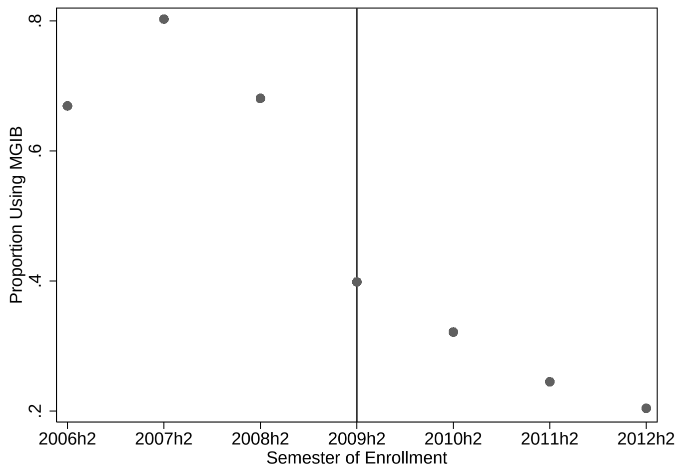
Note: The figure plots the share of veterans enrolling within three years of separation by year of separation. The dark circles represent the enrollment rates of eligible veterans and the light squares represent the enrollment rates of ineligible veterans. See the notes to Table 2 or the text for details of the sample restrictions.

Figure 3.4: Effect on Any Degree Five Years After Separation



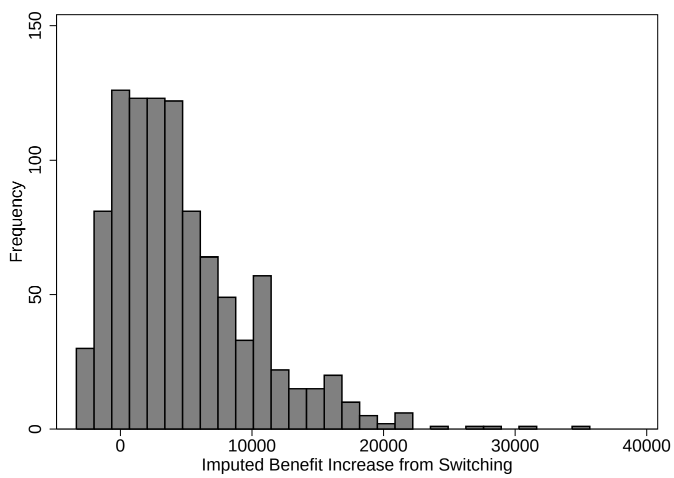
Note: The dependent variable is receipt of any degree within five years of separation (results similar using six years). Dark circles represent the coefficient estimates for each year of separation interacted with eligibility. Smaller and lighter plus signs indicate the 90 percent confidence interval for each point estimate. See the notes to Table 2 for details of the sample restrictions.

Figure 3.5: 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 3.6: 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 that were still enrolled and receiving MGIB benefits during the fall of 2009. See the text for further details.

Table 3.1: 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 that separated between 2003 and 2008. Enrollment statistics present the breakdown of school types for those observed enrolled between Fall 2006 and Fall 2013.

Table 3.2: Degree Effects Five and Six Years After Separation: Over-time Variation

VARIABLES	(1) Associates +	(2) BA +
Five Years after Separation	0.054***	0.038***
	(0.007)	(0.006)
Observations	14,224	14,224
Mean	0.164	0.0992
Six Years after Separation	0.065***	0.049***
	(0.007)	(0.007)
Observations	14,224	14,224
Mean	0.201	0.131

Note: Table shows coefficients from OLS regressions that compare degree attainment levels for eligible 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. 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3.3: Degree Effects Five and Six Years After Separation: DD Regressions

VARIABLES	(1) Associates +	(2) BA +
Five Years after Separation	0.045***	0.033***
	(0.016)	(0.012)
Observations	15,457	15,457
Mean	0.164	0.0994
Six Years after Separation	0.060***	0.045**
	(0.021)	(0.018)
Observations	15,457	15,457
Mean	0.201	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. 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3.4: Degree Effects Using Geographic Variation (6 Years)

VARIABLES	(2) Associates +	(3) BA +
Post 9/11 * Combined Max.	0.0020***	0.0016***
	(0.0005)	(0.0005)
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 (in thousands) 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01). ***(p<0.01).

Table 3.5: 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01)..

Table 3.6: Degree Effects Six Years After Separation: DD Demographic Heterogeneity

VARIABLES	(1) Black	(2) Not Black	(3) Male	(4) Female	(5) Young	(6) AFQT < 60	(7) AFQT ≥ 60
Post 9/11 * Eligible	0.062	0.063***	0.062***	0.012	0.061***	0.046**	0.089***
	(0.041)	(0.021)	(0.022)	(0.050)	(0.020)	(0.018)	(0.032)
Observations	2,529	12,929	13,025	2,433	14,653	8,148	7,310
Mean	0.169	0.207	0.185	0.281	0.201	0.136	0.269

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 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3.7: Enrollment within Three Years DD Estimates

VARIABLES	(1) Enroll
Post 9/11 * Eligible	0.077**
	(0.029)
Observations	15,459
Mean	0.460

Note: Table shows coefficients from difference-in-differences regressions that compare thee-year enrollment rates 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. 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3.8: Enrollment within Three Years Geographic Variation Estimates

VARIABLES	(1) Enroll
Post 9/11 * Combined Max.	0.001**
	(0.001)
Observations	14,224
Mean	0.460

Table shows coefficients from regressions of whether a veteran enrolls within three years of separation on the size of the annual estimate of the combined Post-9/11 GI Bill benefit (in thousands) 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 state 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 state. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 3.9: Enrollment within Three Years: DD Demographic Heterogeneity

VARIABLES	(1) Black	(2) Not Black	(3) Male	(4) Female	(5) Age $\leq$ 30	(6) AFQT $<$ 60	(7) AFQT $\geq$ 60
Enrollment	-0.013	0.111***	0.091***	-0.081	0.075**	0.098**	0.058
	(0.052)	(0.028)	(0.033)	(0.112)	(0.029)	(0.038)	(0.035)
Observations	2,527	12,932	13,026	2,433	14,650	8,151	7,308
Mean	0.450	0.462	0.446	0.534	0.465	0.406	0.517

Table shows coefficients from difference-in-differences regressions that compare thee-year enrollment rates 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 state 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 state. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

Table 3.10: Enrollment within Three Years: School Choice

VARIABLES	(1) 4-Year	(2) 2-Year	(3) Public	(4) For-profit	(5) Private Non-profit	(6) School Combined	(7) Housing Allowance	(8) Tuition	(9) Graduation Rate	(10) Open Admission
Post 9/11	0.048***	0.013	0.027***	0.025***	0.010***	0.433***	0.170*	0.293***	-0.721	0.008
	(0.007)	(0.008)	(0.009)	(0.004)	(0.003)	(0.114)	(0.091)	(0.073)	(0.467)	(0.012)
Observations	14,219	14,219	14,219	14,219	14,219	6,324	6,360	6,324	6,318	6,562
Mean	0.186	0.261	0.377	0.0451	0.0247	15.46	12.33	2.786	27.77	0.684
Post 9/11 * Combined Max.	0.0008	0.0003	0.0008	-0.0001	0.0004**	0.0195***	0.0117	0.0085**	0.0340	-0.0021
	(0.0009)	(0.0008)	(0.0005)	(0.0003)	(0.0002)	(0.0072)	(0.0072)	(0.0036)	(0.0281)	(0.0013)
Post 9/11 * Housing Allow.	0.0034*	-0.0017	0.0007	-0.0007	0.0017*	0.0306	0.0201	0.0096	0.1097	-0.0062***
	(0.0020)	(0.0024)	(0.0028)	(0.0009)	(0.0009)	(0.0234)	(0.0256)	(0.0136)	(0.0837)	(0.0021)
Post 9/11 * Tuition Max.	0.0004	0.0006	0.0008	-0.0001	0.0002	0.0179**	0.0104	0.0084**	0.0227	-0.0015
	(0.0008)	(0.0007)	(0.0006)	(0.0003)	(0.0002)	(0.0075)	(0.0076)	(0.0033)	(0.0334)	(0.0012)

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 state 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 state. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 3.11: 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 3.12: 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 3.13: 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 3.14: Exploring the Decision Not to Switch

VARIABLES	(1) MGIB Next Year
Black	-0.011
	(0.036)
AFQT Score	-0.001
	(0.001)
Age at Sep.	0.015***
	(0.005)
Married	0.034
	(0.025)
HS Grad at Sep.	0.008
	(0.059)
Estimated Gain From Switching	-0.016***
	(0.002)
Observations	1,883
Mean	0.511

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Table shows coefficients from regressions of an indicator variable for veteran MGIB usage for veterans enrolled and receiving MGIB benefits during the fall of 2008 and enrolled during the fall of 2009. Estimated gain from switching is in thousands. For example, for an additional $1,000 in the estimated gain from switching, individuals are 1.5 percentage points less likely to continue MGIB receipt. Each cell presents the estimate from a single regression. Robust standard errors are in parentheses. Significance levels indicated by: * (p<0.10) **(p<0.05), ***(p<0.01).

3.9 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 veteran 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. As 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 state, separation year, sex, and 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 last 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.[58]

C. "Pre-Processing" Methods

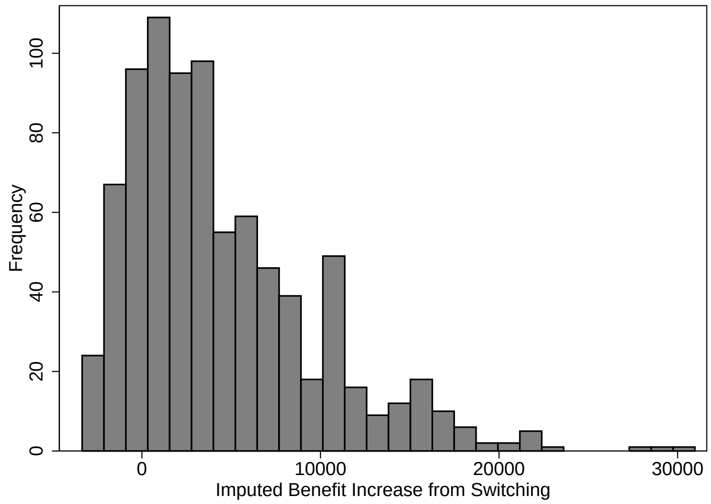
A recent paper suggests that "pre-processing" the data to select a non-experimental comparison group results in superior (i.e., less biased) estimates. As a robustness check I use a similar approach to select ineligible veterans that are similar to eligible veterans. In practice, this amounts to estimating 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.[59] I implement this process separately in the pre and post-period and then run the DD on the pre-processed data.[60]

[58] 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.

[59] There is substantial common support.

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

Figure 3.A1: 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 3.6, this restricts the set of veterans to those enrolled at the same institution the following year. See the text for further details.

Table 3.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 3.A2: 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 3.A3: Degree Effects Five and Six Years After Separation: Pre-processed Data

VARIABLES	(1) Associates +	(2) BA +
Five Years after Separation	0.051**	0.047***
	(0.024)	(0.016)
Observations	15,136	15,136
Mean	0.164	0.0992
Six Years after Separation	0.064**	0.062***
	(0.027)	(0.020)
Observations	15,136	15,136
Mean	0.201	0.131

Note: Data have been pre-processed as described in the text and appendix. 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. 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 state 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 state. Significance levels indicated by: * ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table 3.A4: Degree Effects Using Simulated Geographic Variation (6 Years)

VARIABLES	(1) Any Degree	(2) BA +
Post 9/11 * Combined Simulated	0.0040**	0.0037**
	(0.0016)	(0.0015)
Observations	14,224	14,224
Mean	0.201	0.131

Note: Table shows coefficients from regressions of degree attainment levels on the simulated size of the annual estimate of the combined Post-9/11 GI Bill benefit interacted with whether the individual separated between August 1, 2007 and July 31, 2008 (the post-period). The simulated benefits are generated under the assumptions that veterans in each state enroll following an identical distribution as the national distribution of school types chosen by eligible veterans in 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 3.A5: 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 3.A6: 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 3.A7: 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 (Grapevine reports). 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 3.A8: 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).

Table 3.A9: 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).

3.10 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 that 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 anyways. I assume that the pre-period benefts 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.

Table 3.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 that 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.[61]

Finally, there is an increase in costs for inframarginal enrollees who do not respond to the higher level of benefits. I assume that these individuals are attending for 1.5 years on average and

[61] 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.

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 3.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 that will complete a degree.

These estimates abstract away from at least two important pieces. First, inefficient switching to the new benefit implies that the cost per degree may be substantially overestimated as the assumptions underlying Table 3.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 will also lead to overestimates of the cost per degree.

Table 3.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

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