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Postsecondary Success Outcomes for Veteran and Nonveteran Students at a Public University in Georgia

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POSTSECONDARY SUCCESS OUTCOMES FOR VETERAN AND NONVETERAN STUDENTS AT A PUBLIC UNIVERSITY IN GEORGIA

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POSTSECONDARY SUCCESS OUTCOMES FOR VETERAN AND NONVETERAN STUDENTS AT A PUBLIC UNIVERSITY IN GEORGIA

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SUMMARY

Every year, the federal government distributes $11 billion in education benefits to nearly one million veterans (GAO, 2013). Despite the substantial price tag and reach of these benefits, we understand very little about how veteran students fare in postsecondary programs and why outcomes may be different for veteran students. Theory and related evidence predict that veteran students should be less successful than their nonveteran peers, yet the limited past research suggests that they are actually as successful as, if not more successful than, nonveterans. This is the student veteran paradox. I posit seven potential explanations to resolve this paradox: bias in past research, background characteristics of veterans, enrollment behaviors of veterans, maturation from delayed entry, education aid benefits for veterans, unobservable factors associated with selection into the military, or the direct effects of military service. I use OLS regression and logistic regression to assess three metrics of student success: grades, retention, and completion. I also leverage variations in the GI Bill program to assess whether higher levels of funding lead to better student success outcomes. Finally, I use matching to test whether unobservable factors associated with military enlistment or the direct effects of military service could drive veteran student success. Student veterans hold many characteristics that predict lower probabilities of college success, but veterans and nonveterans generally have similar academic outcomes. When controlling for background characteristics, enrollment patterns, age, and term of entry, predicted first year GPA is lower for veterans, but veterans are more likely to return after the first year and are more likely to graduate. Generally, students with higher levels of veteran education benefits

have better retention and graduation outcomes, but aid levels seem to have little impact on first year grades. Veterans still have lower grades than similar matched nonveterans, but the veterans are more likely to return after the first year and are more likely to graduate. For retention and graduation, these results rule out the bias, background characteristics, and maturation explanations, but support the enrollment patterns and funding explanations. The results are consistent with the direct effects explanation, but the selection explanation cannot be ruled out completely.

CHAPTER 1

INTRODUCTION

1.1 Purpose

Every year, the federal government distributes $11 billion in education benefits to nearly one million veterans (GAO, 2013). Despite the substantial price tag and reach of these benefits, we understand very little about how veteran students fare in the postsecondary programs these benefits subsidize. Additionally, we know little about why those outcomes may be different for veteran students.

Limitations in available data have constrained prior research on student veterans. Publicly available datasets either systematically exclude most veterans through sampling procedures or only include veterans and, thus, lack mechanisms to make fair comparisons with nonveteran students. Despite these limitations, past research has generally found that veterans perform as well as or better than nonveterans in higher education. These findings are surprising because theory and evidence from analogous research fields suggest that veterans should perform worse. Student veterans, for example, are more likely to be male, to be older, to have disabilities—all of which have negative effects on student success. Additionally, related research has shown that veterans have worse labor market outcomes than comparable nonveterans.

Are student veterans actually performing as well in college as nonveterans, and, if so, why? To understand the apparent paradox that veteran students exhibit in postsecondary outcomes, I examine the issue within a competing hypothesis framework. I propose seven ways to make sense out of this paradox. The observed veteran advantage could be due to: 1) bias introduced through data and methodology limitations; 2) differences in background characteristics between veterans and nonveterans; 3) differences in enrollment behaviors between veterans and nonveterans; 4) maturity gains from entering college at a slightly older age; 5) education funding benefits from the GI Bill and related programs; 6) unobservable factors associated with selection into the military; or 7) the direct effects of military service. Although these explanations are exhaustive, they're not mutually exclusive. Some combination of these explanations is possible, although they all cannot be true.

To examine the seven hypotheses, I use an institutional dataset of Georgia State University (GSU) student records to assess student veteran college success outcomes in a large, public university. The GSU data includes the entire population of students entering the university between 2003 and 2015. The use of GSU data avoids many of the significant limitations and problems that are associated with other datasets that have been or could be used to examine postsecondary outcomes of veteran students. I use a mix of methodological approaches to examine the hypotheses. Testing each hypothesis will shed light on the underlying causal mechanisms that contribute to the veteran advantage in postsecondary education. While the findings are important for understanding college success for veteran students, the results will also yield policy implications for nonveteran students as well.

1.2 Policy Relevance

Additional evidence regarding each of the seven competing hypotheses will be highly relevant for policymaking. The results will be relevant regardless of whether they support or undermine each hypothesis. Evidence surrounding the bias hypothesis probably has the greatest potential for impacting policy. If veterans are more likely to drop out of college than nonveterans, then veterans aren't meeting the full education potential that the GI Bill enables. Since employment and income outcomes are substantially better for degree holders than those who drop out of college, it's important to understand the value of the benefits veterans are receiving from the publicly funded provisions of the GI Bill and related programs. Moreover, if results indicate that veterans are less successful than nonveterans, program structures could be altered to better support veterans who attend college.

Degree completion among veterans is also relevant to the larger national conversation on college completion. Many states have now publicly pledged to increase the proportion of the labor force that holds a postsecondary degree or certificate. Some states have also implemented performance-based funding systems for their public colleges and universities. Under these systems, all or part of a school's appropriation from the state is determined by degree completion numbers, rather than enrollment numbers (as has traditionally been the case). Since these funding structures could perversely incentivize limiting college access for groups with historically low completion rates, the funding formulas also include weights to encourage the enrollment of higher-risk students (e.g., first-generation students). New York included veteran students in these at-risk groups for funding-formula purposes, but Ohio ultimately chose not to include veterans after deliberate consideration (National Conference of State Legislatures 2015, Ohio Board of Regents 2012). Veteran students aren't given extra weight under the proposed formulation in Georgia, putting the state in line with most states that have taken up the issue of performance-based funding (State of Georgia Higher Education Funding Commission 2012, National Conference of State Legislatures 2015). If administrators suspect that veterans will be less likely to complete degree programs (even if that hunch is completely unsubstantiated), then veterans could face declining access to postsecondary education in states with performance-based funding formulas that neglect veterans. If the past research is misleading and veterans actually are less likely to finish, then these policies can incorporate protections for veteran students. Providing more reliable research on college success for student veterans can combat clichés and stereotypes in other settings as well.

Accurately assessing the postsecondary success outcomes for veteran students will improve the situation for veteran students, regardless of the results. If veteran students fare just as well as nonveteran students in terms of retention and degree completion, then myths about the quality of veteran students can be dispelled. If veteran students are actually less successful than nonveteran students, then policies for performance-based funding can be better structured with incentives to protect veteran students. Moreover, colleges and universities can also redirect institutional resources to better serve their veteran students.

If the veteran advantage is due to the education funding benefits bestowed upon veterans, then this has implications more broadly for education finance. Veteran education benefits are unique among large-scale student aid programs in that they are neither need-based nor academic merit-based. Additionally, continued funding does not depend on satisfying requirements regarding grades or academic progress. If this type of funding is an effective tool for improving student success, it might be used by the public or nonprofit sectors for other groups with lower college completion rates (e.g., minorities) or other groups that deserve benefits (e.g., mothers). Additionally, policymakers might consider extending comparable levels of service-based aid benefits to induce participation in other public service ventures, such as the Peace Corps, AmeriCorps, or Teach for America.

If veteran students actually benefit from entering college slightly older, then this implies revisions for traditional college completion strategies. Delayed entry into college has been discouraged in the US, especially in recent years. Finding support for the maturation hypothesis would suggest that this strategy is misguided and that delaying entry could improve the odds of college success (at least for certain kinds of students).

If the veteran advantage is due to the direct effects of military service, then this suggests that the chances of college success can increase by undertaking military service or similar activities (at least for some individuals). Like the maturation hypothesis, finding support for the service effects hypothesis implies that at least some students will benefit from delaying entry into college. However, support for this hypothesis would also suggest that this interim period be used for military service or other activities that cultivate the same qualities as the military. These other activities could include other public service initiatives such as the Peace Corps or AmeriCorps. Encouraging the eventual college-bound individuals to take service-based gap years before enrollment would yield not only individual benefits from bolstering the odds of student success, but also public benefits stemming from the public service activities undertaken during this period.

The selection hypothesis is also relevant for public policy, albeit less than the other hypotheses. Finding support for this claim would imply that those who choose to enlist have certain qualities that give them an advantage in situations like college. This also implies that said qualities will be advantageous in certain employment situations. Groups working on veterans' employment in the public (e.g., Department of Veterans Affairs, Department of Labor) and nonprofit (e.g., Hire Heroes USA) sectors could harness this knowledge to improve labor market outcomes for veterans. Moreover, governments could also utilize this knowledge to adjust veterans' preference systems to steer veterans toward jobs that leverage those characteristics.

1.3 Contribution to the Literature

I build on an uneven body of literature that assesses student success outcomes for veterans in postsecondary education. Most of the work in this area has focused on grades as a short-term metric for student success. Differences between veteran and nonveteran students in long-term metrics like first year retention and degree completion have largely been neglected. This has mostly been due to the significant limitations in publicly available data for identifying and/or tracking student veterans. I use a unique dataset of institutional student records from Georgia State University (GSU) to provide the first reliable assessments of retention and completion in a public university.

The use of GSU data avoids many of the significant limitations that are associated with other datasets that have been used to examine postsecondary outcomes of veteran students. The nationally representative, publicly available datasets with relevant variables either do not properly identify and track veterans, or they misclassify their educational outcomes. The most frequently used dataset for tracking postsecondary outcomes is the Department of Education's Integrated Postsecondary Education Data System (IPEDS), which uses data reported by higher education institutions. However, IPEDS data only includes first-time, full-time freshmen that begin in the fall semester. This excludes about 37% of the total undergraduate population (Snyder and Dillow, 2013, p. 326). Moreover, veterans are likely to be overrepresented in this excluded group since they are more likely than traditional direct-from-high-school students to attend part-time (due to families, jobs, etc.), enter in the spring or summer (depending sometimes on when deployment ends), or transfer from a 2-year to a 4-year school. Other postsecondary datasets from the Department of Education are similarly problematic in that they include veterans but are only cross-sectional and do not track outcomes (in the case of the National Postsecondary Student Aid Study), or are longitudinal but make similar exclusions about first-time student status (in the case of the Beginning Postsecondary Students). Data from the Census Bureau (including the decennial census, the American Community Survey, and the Current Population Survey) is extensive and includes information on veteran status, but lacks sufficient detail on educational attainment outcomes. Census data uses the "some college, no degree" category to include not only dropouts from associates and bachelor’s degree programs, but also students who have successfully completed postsecondary certificate programs. Since census data is self-reported, it is also more susceptible to errors due to misunderstanding or misrepresentation. The decennial National Survey of Veterans does have data on post-secondary outcomes, but it is also self-reported and only includes veterans. A recent initiative to assess veteran student success called the Million Records Project relies on education records rather than self-reporting, but it too only includes veterans. For both the National Survey of Veterans and the data from the Million Records Project, the exclusion of nonveterans prevents making fair comparisons between veterans and nonveterans with regard to postsecondary education outcomes.

This is the first methodologically rigorous study to assess postsecondary student success among contemporary veterans. No study to date has used a multivariate analysis of retention and completion metrics to disentangle the effects of military service from other variables that are highly correlated with both veteran status and student success. This study is also one of the few to examine the effects of service-based financial aid, as distinct from need-based aid and academic merit-based aid.

1.4 Overview of Chapters

This dissertation includes six chapters. In chapter 2, I review the theoretical foundations underpinning my hypotheses and discuss the current evidence on postsecondary success for veteran students. I show that two threads of the literature lead to the student veteran paradox: theory and related evidence predict that veteran students should be less successful than their nonveteran peers, yet the limited past research suggests that they are actually as successful as, if not more successful than, nonveterans. I posit seven potential explanations to resolve this paradox: bias in past research, background characteristics of veterans, enrollment behaviors of veterans, maturation from delayed entry, education aid benefits for veterans, unobservable factors associated with selection into the military, or the direct effects of military service. In chapter 3, I discuss the GSU dataset and detail my methodological approach. I use OLS regression and logistic regression to assess three metrics of student success: grades, retention, and completion. I also leverage variations in the GI Bill program to assess whether higher levels of funding lead to better student success outcomes. Finally, I use matching to test whether unobservable factors associated with military enlistment or the direct effects of military service could drive veteran student success. In chapters 4 and 5, I present and discuss the results of the analyses. Student veterans hold many characteristics that predict lower probabilities of college success, but veterans and nonveterans generally have similar academic outcomes. When controlling for background characteristics, enrollment patterns, age, and term of entry, predicted first year GPA is lower for veterans, but veterans are more likely to return after the first year and are more likely to graduate. Generally, students with higher levels of veteran education benefits have better retention and graduation outcomes, but aid levels seem to have little impact on first year grades. Veterans still have lower grades than similar matched nonveterans, but the veterans are more likely to return after the first year and are more likely to graduate. In chapter 6, I discuss what these findings mean within the competing hypothesis framework and their further real-world implications. For retention and graduation, these results rule out the bias, background characteristics, and maturation explanations, but support the enrollment patterns and funding explanations. The results are consistent with the direct effects explanation, but the selection explanation cannot be ruled out completely.

CHAPTER 2

LITERATURE REVIEW

2.1 Introduction

This research examines postsecondary student success for veterans and what drives those outcomes. This chapter reviews bodies of literature to show how researchers have measured postsecondary student success, why veterans and nonveterans might perform differently on these measures, and what past research has shown regarding veteran success on these metrics and in related areas.

Researchers typically use three different measures of postsecondary student success: grades, retention, and graduation. These student success outcomes could vary between veterans and nonveterans for several reasons. Student veterans are more likely than nonveterans to be male, black, Hispanic, lower socioeconomic status, less academically prepared, nontraditional students, disabled, and less integrated (both academically and socially). Past research has shown that student success outcomes are worse for students who hold each of these characteristics. On the other hand, veteran students usually receive generous financial benefits for education, and having unmet financial need inhibits student success. Additional important factors associated with student success include discontinuous enrollment, course withdrawals, and college GPA, but no research examines whether these vary between veterans and nonveterans. Past research on veteran success has shown that veterans have worse labor market outcomes than nonveterans. However, prior work on student success for veterans has shown that veterans are as successful as nonveterans; some even conclude that student veterans perform better than nonveterans on student success outcomes.

These lines of research lead to a paradox: theory and related research predict that student veterans should be less successful than nonveteran peers, but the limited research on student veteran success indicates that veterans are at least as successful as, if not more successful than, nonveterans. I offer seven explanations that can potentially resolve this paradox and provide research hypotheses suggested by each. I conclude by summarizing and looking ahead to the next chapter.

2.2 Measures of Postsecondary Student Success

Researchers use three primary metrics to gauge postsecondary student success. The simplest is grades. Grades can measure short-term student success during a single semester or course (e.g., McGregor, Reece, and Garner, 1997). More frequently, grades are averaged over an academic year–typically the first year (e.g., Kuh et al., 2008).

Persistence, or retention, examines whether students who initially enroll return to college in subsequent semesters. These terms are typically used interchangeably, but some researchers use retention with reference to the institutional perspective (i.e., whether a student returns to her initial institution) and use persistence with reference to the student perspective (i.e., whether a student returns to college, perhaps somewhere different from her initial institution). Researchers typically measure persistence after the first academic year, from fall-to-fall semesters (e.g., Kuh et al., 2008; Snyder and Dillow, 2013), or, less commonly, between fall and spring semesters or through the second or third academic year.

Degree completion, or graduation, indicates that a student successfully finished the degree or certificate program in which he or she initially enrolled. Program completion has wider implications than the other success outcomes, as those who finish degrees are more likely to get jobs and tend to make more money than those who drop out of postsecondary programs. As a result, researchers and stakeholders have shown substantial interest in degree completion in recent years. Researchers measure completion rates across various time spans, but most do so at 150% of typical program length (e.g., those who finished an associate's degree within 3 years of beginning, or finished a bachelor's degree within 6 years of beginning) (e.g., Snyder and Dillow, 2013). Occasionally, researchers use shorter or longer periods (e.g., Adelman, 1999, 2006; Goldin et al., 2006).

These three metrics are obviously interconnected. Earning poor grades can preclude retention, and returning to an institution is necessary to eventually graduate (although a student could skip the second fall semester before returning to finish). As one would expect, students who make good grades are more likely to persist throughout the first year, and persistence is a necessary prerequisite for degree completion.

2.3 Factors Associated with Veteran Status and Postsecondary Student Success

Some factors associated with being a veteran are also correlated with college success. These can distort the relationship between veteran status and student success if they aren't statistically accounted for. These factors could cause veterans to perform differently than nonveteran students in college.

2.3.1 Sex

Sex is the most notable factor: the vast majority of student veterans are men, but men are less likely than women to succeed in college. Diversity in the US military has increased over time as positions that have historically only been open to men have been opened to women as well. Women made up only 1.6% of those enlisted for active duty when the all-volunteer force began in 1973, but comprised 14.8% in 2014 (Office of the Under Secretary for Defense 2015, Table D-13). As the military is still disproportionately male, consequently, so are veterans. As of 2010, women comprised only about 8% of the total veteran population, although they represent a growing segment of veterans (Patten and Parker 2011). Women hold a larger share of the student veteran population than of the overall veteran population, presumably reflecting the broader trend in which women are more likely to pursue higher education. Both Radford and Wun (2009) and Cole and Kim (2013) report that about 27% of undergraduate military students[1] are women, although Cate (2014b) reports that 21.1% of student veterans between 2002 and 2010 were women.

Women currently outperform men on all metrics of postsecondary success. Women are more likely than men both to attend and to graduate from college (e.g., Freeman 2004). Among new high school graduates in 2011, 72% of women enrolled in a 2- or 4-year college, but only 65% of the men did (Snyder and Dillow, 2013, p. 333). The share of degrees earned annually by women has increased steadily since 1970, with

[1] Veteran, active duty, and reserves are combined into one category in these analyses, though veterans make up nearly 75% of the combined group.

women overtaking men as the majority in 1978 at the associate's level and in 1982 at the bachelor's level (Snyder and Dillow, 2013, p. 448). Women earned 57% of the bachelor's degrees and 62% of the associate's degrees awarded in 2011 (Snyder and Dillow, 2013, p. 448). Women also earn higher grades in college than men (Adelman 1995; Kuh and Hu 1999; Spitzer 2000). The female advantage in college entry and completion holds across all income quartiles, across racial groups, and across all types of family structures & backgrounds (Bailey and Dynarski 2011; Buchmann and DiPrete 2006). The shrinking gaps between men and women in postsecondary enrollment and completion can largely be explained by the decline in discrimination between the sexes, the increasing returns to higher education for women, and advances in contraception that improved family planning (Goldin and Katz 2001; Goldin, Katz, and Kuziemko 2006; Hock 2007). The subsequent reversal of the gender gap is largely due to variations in enrollment patterns between the sexes (men are more likely to take time off or enroll part-time, both of which decrease the probability of degree completion) and the superior academic performance of females (Buchmann and DiPrete 2006; Ewert 2012).

2.3.2 Race

Veterans are more likely than nonveterans to be black. Since the beginning of the all-volunteer force in 1973, blacks have been overrepresented in the military and especially the Army (Segal, Thanner, & Segal 2007). In 2014, blacks made up 18.9 percent of active duty enlisted service members but only 13.5 percent of the civilian labor force between the ages 18 and 44 (Office of the Under Secretary for Defense 2015). Hispanics are underrepresented in the military but they represent a growing segment, especially in the Marine Corps (Segal, Thanner, & Segal 2007). This is probably at least partly due to the fact that citizenship was a prerequisite for enlistment until fairly recently. In 2014, Hispanics comprised 20 percent of the 18-44 civilian labor force in 2014 but only 13.2 percent of active duty service members (Office of the Under Secretary for Defense 2015).

These representation trends impact racial representation among student veterans. Radford and Wun (2009) report that among undergraduates in 2007-2008, blacks were overrepresented among military students (18.3% compared with 18.1% of financially-independent civilians and 10.3% of financially-dependent civilians). Additionally, Hispanics were underrepresented among military students (12.8% compared with 15.1% of financially-independent civilians and 13.5% of financially-dependent civilians). However, they use data from the National Postsecondary Student Aid Study (NPSAS), which only identifies military students who filed the Free Application for Federal Student Aid (FAFSA). This subset may not be representative for veterans, as filing the FAFSA is not necessary for using GI Bill benefits. Using data from the 2012 National Survey of Student Engagement (NSSE), Cole and Kim (2013) corroborate that blacks are overrepresented among military students in four-year institutions. They report that 10.6% of military students, but only 7.1% of civilian students, are black. However, they also report that Hispanics are slightly overrepresented as well; 7.8% of military students are Hispanic but only 6.8% of civilian students are (Cole and Kim 2013).

The racial gaps in postsecondary enrollment are shrinking. Among new high school graduates in 1985, 57% of whites, but only 40% of blacks and 46% of Hispanics, enrolled in a 2- or 4-year college. In 2011, the college enrollment of new graduates had risen to 69% for whites, 65% for blacks, 64% for Hispanics, and 85% for Asians (who weren't tracked until 2003; Snyder and Dillow, 2013, p. 334). The racial gaps in graduation rates, however, have not closed in a similar manner. Out of full-time students who began a 4-year degree in 1996, 58% of whites but only 39% of blacks and 46% of Latinos finished within six years. For the cohort entering college in 2005, the 6-year graduation rate had improved slightly for whites (62%) and for Latinos (51%) but had barely changed for blacks (40%) (Snyder and Dillow, 2013, p. 527). The graduation rates look substantially different for full-time students at 2-year institutions: for the students that began in 2008, 30% of whites graduated within three years, compared with 27% of blacks, 35% of Latinos, and 34% of Asians. These proportions fluctuated very little over the previous eight years for all groups except Latinos, for whom the graduation rate improved by about 5 percentage points (Snyder and Dillow, 2013, p. 532).

Studies that control for socioeconomic status and academic preparation (test scores and grades) show substantially smaller or non-existent effects of race on college enrollment and college graduation (Kao and Thompson, 2003; Adelman, 1999, 2006). Carnevale and Strohl (2013) argue that the racial gaps in postsecondary graduation are also due in part to differences in college choice between these groups, since blacks and Hispanics are less likely than whites to enroll at more selective institutions. In 2009, 25% of white freshmen enrolled at the most selective colleges in the country, while only 9% of blacks and 12% of Hispanics did. Instead, 72% of black freshmen and 74% of Hispanic freshmen (but only 53% of whites) started college at open-access institutions, which have worse graduation rates than selective schools, even for students with similar SAT scores

(Carnevale and Strohl, 2013, p. 51).[2] Sibulkin and Butler (2005) show that black students are much more likely than white students to have a child while enrolled, which also substantially reduces the probability of graduating.

2.3.3 Socioeconomic Status

Military participation is also associated with socioeconomic status (SES). SES, or class, typically refers to the education and/or income level (which are highly correlated) for a person or household. Those from a lower socioeconomic status are more likely to enlist in the military instead of directly entering college or the workforce (Kleykamp 2006). This, in turn, means that students with military service are also from a lower SES. Cole & Kim (2013) find that 61.8% of military students are first-generation students, while only 42.8% of civilian students are.

Socioeconomic status is one of the strongest predictors of college student success. Individuals with parents who either are low-income or do not have a college degree are less likely to enroll in college, even when controlling for additional factors (Baker and Velez, 1996; Choy, 2001; Aronson, 2008). In 2011, 83% of recent high school graduates from the top family income quintile enrolled in college, in contrast to 54% of those in the lowest quintile (Snyder and Dillow, 2013, p. 335). Low SES students who do enroll are less likely than other students to finish degrees (Choy, 2001; Warburton, Bugarin, and Nuñez, 2001; Aronson, 2008). Among students who graduated high school in 1992 and had enrolled in college before 2000, 43% of the first-generation college students had left

[2] The authors argue that open-access schools have poorer outcomes not because of selection, but because they spend significantly less on instruction per student.

without a degree but only 20% of students with a parent who had at least a bachelor's degree had done so (Chen, 2005). Low SES students typically receive less support from their families, both in terms of financial assistance and advice surrounding college decisions. They are also more likely to attend high schools that have fewer financial resources and have substandard college advising capacity (Wells and Lynch, 2012). Socioeconomically disadvantaged students are less likely to take rigorous courses in high school and are older, get lower grades, work more hours while enrolled, attend less selective institutions, are more likely to attend part-time, and are more likely to have discontinuous enrollment across multiple institutions—all of which reduce the odds of retention and graduation (Warburton, Bugarin, and Nuñez, 2001; Aronson, 2008; Goldrick-Rab, 2006).

2.3.4 Academic Preparation

Those who enlist in the military are less academically prepared for college. Having lower high school grades is associated with choosing military enlistment over entering college directly (Elder et al., 2010). However, it is uncertain whether those veterans who eventually enroll in college are less academically prepared than nonveterans who attend college.

Academic preparation is one of the strongest predictors of postsecondary student success. This includes any pre-collegiate indicators that illustrate academic readiness for college-level work. These measures include high school GPA, class rank, standardized test scores, and rigor of high school curriculum. Adelman (2006) finds that the intensity and quality of one's high school curriculum are the strongest pre-enrollment factor related to bachelor's degree completion, but finds all of them to be significant and highly correlated with each other.

2.3.5 Nontraditional Student Attributes

Veterans are also more likely to be nontraditional college students. The nontraditional classification has varying definitions, but usually designates any student who does not begin postsecondary education directly after high school. Typically, seven characteristics define nontraditional students: delayed postsecondary enrollment, part-time attendance, financial independence, full-time employment while enrolled, the presence of dependents, single parenthood, or a non-standard high school diploma (Horn and Carroll 1996). Enrolled veterans exhibit many of these nontraditional student characteristics. Student veterans are more likely than nonveteran students to delay enrollment (due to military service, primarily), be financially independent, have dependents, work off-campus, and attend part time (Radford and Wun 2009, Radford 2011, Cole & Kim 2013).

Although estimates from other scholars may vary with their definitions of "nontraditional," Horn and Carroll (1996) estimate that 54% of all undergraduates in 1992 are nontraditional in at least one of these seven factors and Choy (2002) estimates that the proportion is 73% nearly a decade later. Nontraditional students fare far worse than their traditional peers in terms of first year retention rates and overall graduation rates (Horn and Carroll 1996; Choy 2002; Taniguchi and Kaufman 2005). Among students attempting to earn a bachelor's degree, 54% of traditional students but only 31% of nontraditional students had done so within five years (Horn and Carroll, 1996).

Four attributes of nontraditional students linked to student veterans warrant additional discussion.

2.3.5.1 Delayed Entry

Many see delayed entry into college as the primary characteristic of a nontraditional student, though some interchange this with being an older student (because students who delay entry into postsecondary education are, by necessity, older than students who enroll directly after high school). Although some studies report that older students earn slightly better grades (Leppel 1984; Spitzer 2000), students who wait to enter college generally fare worse than those who do not. Among students who began college in 1995, about one third had waited at least a year after high school graduation to enroll. Six years later, 58% of immediate entrants had earned some sort of degree but only 40% of delayed entrants had (Horn, Cataldi, and Sikora, 2005). When controlling for related factors, Bozick and DeLuca (2006) estimate that students who delay entry into college for a year are 64% less likely to finish a bachelor’s degree than those who enroll immediately after high school. Students who wait to enroll in college tend to be from lower SES families (Horn, Cataldi, and Sikora, 2005; Bozick and DeLuca, 2006; Goldrick-Rab and Han, 2011) and are almost six times as likely to be from the bottom family income quintile as from the top (Goldrick-Rab and Han, 2011). SES, however, doesn’t fully explain the differential outcomes related to enrollment delays. Recent research suggests that part of the explanation is that students who delay entry are also more likely to be married or partnered, more likely to have children, and more likely to work more than 35 hours per week, all of which decrease the odds of finishing a degree (Roksa and Velez, 2012; Goldrick-Rab and Han, 2011).

2.3.5.2 Enrollment Status

Part-time enrollment is surprisingly common. Among those entering 4-year colleges in 1995, 53% of students enrolled part-time (i.e., fewer than 12 credit hours) for at least one semester during college (Adelman, 2006). Variation in enrollment patterns accounts for the largest share of the gap between traditional and nontraditional students in persistence and completion (Taniguchi and Kaufman, 2005; Horn and Carroll, 1996). The negative relationship between part-time status and student success consistently appears in other research as well (Carroll, 1989; O'Toole, Stratton, and Wetzel, 2003; Adelman, 2006). It's unclear why this is the case, though some theorize that these students are less academically and socially integrated[3] (Taniguchi and Kaufman, 2005). Adelman (2006, p. 79) finds that enrolling part-time at any point in one's college career decreases the probability of attaining a bachelor's degree within 6 years by 25 percent.

2.3.5.3 Employment

The relationship between employment during college and student success is generally negative, although this varies depending on how much one works. About 80% of undergraduates report working at some point during their college career (Roksa and Velez, 2012). Working part-time at a lower intensity (15 hours per week or fewer) has a

[3] Student integration refers to the level of involvement and embeddedness a student experiences on campus. I discuss student integration later in this section.

positive effect on retention and GPA, but working full-time (35 hours per week or more) has a negative effect on both. The relationship is stronger for retention than for grades, and the benefits of light employment are stronger for on-campus jobs (Pascarella and Terenzini, 2005; Riggert et al., 2006; Roksa and Velez, 2012).

2.3.5.4 Dependents

A number of studies have shown that students with dependents are less likely to finish college (Adelman 1999, 2006; Bozick & DeLuca, 2005; Goldrick-Rab and Han, 2011). Roksa and Velez (2012) estimate that being married or cohabitating reduces the odds of degree completion by nearly 40% and that becoming a parent reduces the odds by 50%. There is some dissent on this point in older research, though. Grosset (1991) finds that having dependents is positively associated with short-term persistence. Additionally, Astin (1975) finds that the presence of children increases the probability of degree completion for men, but reduces it for women. Taniguchi and Kaufman (2005) find no difference in the probability of degree completion between married students and those who have never married, although they find that divorced students are less likely to finish.

2.3.6 Funding

Veterans are entitled to substantial postsecondary education after military service. Although the original GI Bill for World War II veterans covered tuition and living expenses, the subsequent versions during the Vietnam period and the early all-volunteer force period were less comprehensive and required veterans to make initial contributions to receive benefits later. The Post-9/11 GI Bill, passed in 2008, is the most recent legislative update to the postsecondary education benefits for veterans. It is similar to the original GI Bill in its high availability and levels of benefits. Veterans serving on active duty for at least 90 days after September 10th, 2001, are eligible for benefits, although to receive the maximum amount individuals must have served for at least 36 months (or have been discharged earlier due to a service-related disability). The benefits package includes a payment for tuition and fees (up to the cost of the most expensive in-state public school), a housing allowance (equivalent to the housing payment awarded to military personnel at the E-5 rank with dependents living in the institution's zip code, with exceptions for those enrolled in distance learning programs or in foreign schools), and a stipend for textbooks and supplies (up to $1,000 per year). I estimate that a veteran who qualifies for the full award amount and attends GSU would receive $14,448 for the fall 2017 semester. Veterans who need additional funds because they attend out-of-state or private schools may receive supplementary tuition assistance through the Yellow Ribbon Program, an additional program through the VA. The Post-9/11 GI Bill benefits can be used for apprenticeships, on-the-job training, flight school, or degrees and certificates at traditional colleges, universities, and trade schools. Veterans can transfer these benefits to spouses and dependents, although this generally requires a commitment to four additional years of service beyond the initial 36 months (Department of Veterans Affairs 2012).

Scholars working on the postsecondary educational attainment of veterans have been most interested in evaluating the causal impact of the initial GI Bill and its legislative kin. Impact estimates conclude that the provision of these education benefits has had a positive effect on the education level of US veterans serving in World War II (Bound & Turner 2002; Stanley 2003), Korea (Stanley 2003), and Vietnam (Mattila 1978), although Nam (1964) provides a dissenting opinion on the WWII era. These positive effects have not accrued equitably, however, with the WWII GI Bill package only improving educational attainment for white men and black men born outside the South (Turner and Bound 2003). Lemieux and Card (2001) also found evidence for positive impacts of the Canadian equivalent of the GI Bill, although they did not address distributional aspects. Zhang (forthcoming) finds that the Post-9/11 GI Bill increased the enrollment of veterans in postsecondary education, but the effect was largest just after implementation.

A large body of research contains mixed conclusions on the relationship between financial aid and college success. Since many unobservable traits are correlated with both financial aid levels and student success, conclusions from past research have been weakly supported and occasionally contradictory. For the most part, the literature suggests that financial aid has a small positive effect on retention and graduation, with the largest effects coming from higher aid levels or from aid focused on the poorest students (see Hossler et al., 2009, for an extensive overview, but also St. John, 2004; Singell, 2004; Pascarella and Terenzini, 2005; Long, 2008). Most of the conflicting findings center on merit aid programs that are awarded based on high school performance and require maintaining a college GPA above a specified threshold. Merit aid programs may not improve degree completion because the college GPA requirement incentivizes lighter course loads, because many students lose merit-based aid early in their college career, or because many recipients are high-quality students who would have graduated even without the merit aid (see Sjoquist and Winters 2014). Despite these debates on the causal impact of aid, researchers typically agree that as unmet financial need increases, the odds of persisting and graduating decrease (Pascarella and Terenzini, 2005).

2.3.7 Disabilities

Veterans may also perform differently in higher education because they are more likely than nonveterans to have physical and mental injuries that can impede success in the classroom. Veterans, especially those exposed to combat, are at increased risk of injury. According to the Department of Defense (2015), 52,313 have been wounded in action in Operation Iraqi Freedom (Iraq before August 2010), Operation Enduring Freedom (Afghanistan), and Operation New Dawn (Iraq after August 2010). This is about 2% of the approximately 2.5 million who have served in these conflicts (Baker, 2014). However, these statistics exclude wounds inflicted during non-combat situations (heat exhaustion, assault, suicide attempts, etc.) and, usually, “invisible” wounds that affect the brain and the mind. Traumatic brain injuries (TBIs) are widespread among those who have served in Iraq and Afghanistan, due in part to the prevalence of improvised explosive devices (IEDs) in these conflicts. TBIs include brain damage ranging from mild concussions to major head trauma, and can result in problems with memory, planning, attention, and problem solving (Okie, 2005; Baker, 2014). Researchers estimate between 6% and 19% of Iraq and Afghanistan veterans suffered a TBI (Taylor et al., 2012; Hoge et al., 2008; Tanielian et al., 2008).

Veterans are at a higher risk for such mental health problems as posttraumatic stress disorder, depression, alcohol abuse, and other related mental health issues. Based on the requisite post-deployment screenings by the Department of Defense, Hoge, Auchterlonie, and Milliken (2006) estimate that 19.1% of Iraq veterans and 11.3% of Afghanistan veterans returned with at least one of these mental health problems. However, subsequent research on Iraq veterans suggests that these earlier estimates actually understate the prevalence due to the proximity of the screening to the end of service (Milliken, Auchterlonie, and Hoge, 2007). Most research has focused on PTSD, with researchers estimating prevalence rates between 8% and 25% for veterans of Iraq and Afghanistan, with higher rates among the Iraq veterans and those who served in the National Guard or Reserves (Hoge et al., 2004; Vasterling et al., 2006; Hoge, Auchterlonie, and Milliken, 2006; Hoge et al., 2007; Seal et al., 2007; Erbes et al., 2007; Schell and Marshall, 2008; Smith et al., 2008).

Research examining the prevalence of injuries among students who have served is less extensive. Active duty and veteran students have higher rates of psychological symptoms than their civilian peers (Barry, Whiteman, and Wadsworth, 2014), with one survey reporting that 46% of the veteran students surveyed experienced significant symptoms of PTSD (Rudd, Goulding, and Bryan, 2011). In interviews, student veterans also report having attendance and attention issues due to combat-related physical pain (DiRamio, Ackerman, and Mitchell, 2008; Ackerman, DiRamio, and Garza Mitchell, 2009).

Disabilities can inhibit students from succeeding in college. Physical disabilities may render it difficult for a student to get to classes, to write notes or assignments, and, if in pain, to fully pay attention when present. Mental disabilities can impede a student's comprehension, attention, motivation, and communication. Students with disabilities comprise a fairly small proportion of postsecondary students—about 11% of all postsecondary students in 2008 (Government Accountability Office, 2009). Disabled students report lower postsecondary GPAs than their non-disabled peers (Wagner et al. 1991). These disabled students were less likely to persist in or complete postsecondary programs than students without disabilities (Hurst and Smerdon, 2000; Murray et al., 2000; Quick, Lehmann, & Deniston 2003; Mamiseishvili & Koch, 2012). Horn, Berktold, and Bobbit (1999) estimate that only 53% of disabled students were still enrolled or had graduated within five years, in contrast to 64% of students without disabilities.

2.3.8 Student Integration

Some research suggests that military students are less academically and socially integrated than their civilian peers. Integration entails having positive interactions with faculty and peers, making friends, and getting involved with student organizations. Military students report feeling less supported on campus and being less engaged in college life than nonmilitary students, although the nonmilitary students over 25 reported about the same levels of support and engagement as the military students (Cole and Kim 2013).

Student integration (both academic and social) is positively associated with persistence (Astin 1993; Tinto, 1993; Pascarella and Terenzini, 2005; Kuh et al., 2008). Social integration improves the chances of finishing college because it fosters a support system for the individual (Tinto, 1993).

2.4 Additional Factors Associated with Postsecondary Student Success

Some additional factors are strong predictors of postsecondary student success. These warrant brief discussions even though there is no evidence to suggest that these factors are also associated with veteran status.

2.4.1 Continuous enrollment

A student is continuously enrolled if he or she reenrolls for each non-summer term until leaving the institution permanently. This makes a distinction between "stopping out" (i.e., after beginning college, taking at least a semester off before returning to college) and dropping out (i.e., after beginning college, leaving school permanently without a degree). Aside from academic background, continuous enrollment is the variable with the strongest association with degree completion (Adelman, 1999, 2006). Continuous enrollment increases the probability of finishing a bachelor's degree by 43 percent (Adelman, 2006).

2.4.2 College GPA

Although college GPA is an outcome measure of student success, it is also a contributor to retention and graduation. College grades signal that the student is an appropriate academic fit for the institution, but grades also reflect student effort. Grades are strongly associated with degree persistence, both when measured as first year GPA and overall GPA trend (Adelman, 1999, 2006).

2.4.3 Withdrawal percentage

The number of course withdrawals and repeats is another strong predictor of degree completion (Adelman, 1999, 2006). This can indicate academic unpreparedness, but it also prolongs the collegiate time frame and depletes financial resources. Adelman (2006) estimates that having course withdrawals and repeats that exceed 20% of attempted credits reduces the probability of graduating by 49%.

2.5 Prior Research on Postsecondary Student Success Measures for Veteran Students

Early attempts to assess postsecondary outcomes while controlling for additional variables generally suggest that veteran students fare at least as well as comparable, nonveteran peers. These researchers focus on college grades, probably because they allow for simpler comparisons over shorter time periods. Love and Hutchison (1946) matched a small number of veterans and nonveterans (n=208) at a single university on academic college (e.g., business, arts & sciences) and standardized test scores, finding a slight (but non-significant) grade advantage for the veteran students. Garmezy and Crose (1948) matched a larger number (n=809) of veterans and nonveterans on additional attributes (sex, age, race, marital status, in addition to college and college aptitude), also finding a slight, but non-significant, GPA advantage for the veteran students. Gowan (1949) examined 511 freshmen at Iowa State College, finding a clear advantage in first-year GPA for veteran students after controlling for high school GPA and standardized test scores. Frederiksen and Schrader (1951) utilized an ANCOVA procedure to control for college readiness, finding that veterans received slightly higher grades than nonveterans of equal ability. However, this significant difference was relatively small (approximately the difference between a C and a C+) and was only statistically significant at some of the individual institutions in the sample (Frederiksen and Schrader 1951). Joanning (1975) corroborated an overall positive grade differential for veteran students, but noted that only the subset of veteran students with pre-service college experience outperformed the nonveteran students, with the other veteran students performing about the same as the nonveterans. McGregor, Reece, and Garner (1997) show that among community college students in 1996, veterans earned higher grades and were less likely to withdraw from courses than nonveterans.

Among all adults in the US, veterans have a lower average educational attainment than nonveterans. This gap has decreased somewhat over time for Vietnam-era veterans (Teachman 2005) but not for veterans of the subsequent all-volunteer force (Teachman 2007). Teachman (2007) speculates that this is due to the rise of alternate pathways to college, as they allow those who would have enlisted for college benefits to pursue college directly without service.

Over the past decade, the general public and the research community have focused more attention on the postsecondary outcome measures for those veterans who actually pursue higher education. Recent media reports have claimed that 88% of veterans drop out within their first year of postsecondary education and only 3% eventually finish (e.g., Betar 2012, Briggs 2012, Wood 2012). Researchers and veteran groups have widely criticized these estimates, however. I could not find the primary source(s) that inspired these articles,[4] but Cate (2014b) claims that the research used early education benefit termination as a proxy for dropping out (an operationalization that artificially inflated the dropout rate).

Self-reported data suggests that veterans could be completing education programs at higher rates than nonveterans. The Department of Veterans Affairs irregularly conducts nationwide surveys of veterans covering a wide range of issues. The most recent survey (in 2010) asks about education benefits usage and outcomes. Of those who had used education benefits, 66.6% reported finishing the program for which they were used (Westat 2010, 147). When analyzing the survey by service era, Cate (2014a) finds that veterans serving in World War II and the following years report the highest completion rates (80%), followed by veterans serving in the Korean conflict (73%). Completion rates were slightly lower but stable for veterans serving from the end of the Korean conflict until August 2001 (68%). Only 51% of veterans serving after 9/11 reported finishing the program funded by their education benefits, but it is likely that many of these students entered after the implementation of the Post-9/11 GI Bill in 2009 and are still enrolled.

An analysis in Ohio found mixed results for veteran student success at public institutions in the state (Ohio Board of Regents 2014). Veterans had higher graduation rates in Ohio community colleges (33.8% of veterans graduated, compared with 24.3% for the total cohort of students), but lower graduation rates in universities (51.7% of veterans finished while 60.0% of all students finished). In both community colleges and

[4] Although most of these articles refer to reports from the University of Colorado Denver, the Colorado Workforce Development Council, and/or the U.S. Senate Committee on Health, Education and Labor and Pensions, no direct citations were provided and the primary source(s) could not be located as of the time of writing.

universities, the veterans had higher completion rates than minorities, those entering at age 22 or older, and those deemed academically unprepared (Ohio Board of Regents, 2014). The authors acknowledge significant data quality and sampling issues in the analysis. Practices for identification and data collection for veterans varied across institutions. Additionally, the Veterans' Services Office identified those student veterans who had sought out services, which could be an unrepresentative sample of student veterans. The veterans identified made up less than 1% of students.

The contemporary, large-scale empirical research is limited to two major endeavors spearheaded by the Student Veterans of America: the Million Records Project (MRP) and the National Veteran Education Success Tracker (NVEST). The Student Veterans of America, the Department of Veterans Affairs, and the National Student Clearinghouse collaborated to analyze postsecondary success outcomes for veteran students. The Department of Veterans Affairs identified student veterans who had utilized GI Bill benefits and released their information to the National Student Clearinghouse. The National Student Clearinghouse matched the educational records of these student veterans to the data supplied by the Department of Veterans Affairs, then stripped the data of personal identifiers. The de-identified individual-level records for the student veterans were then shipped to the Student Veterans of America for analysis.

The MRP study sample included 898,895 veterans who utilized either Montgomery GI Bill or Post-9/11 GI Bill benefits (or both) between 2002 and 2010 to earn a postsecondary degree or certificate. The sample excludes students who used other veterans' education benefits besides the GI Bills (such as the Tuition Assistance program from the Department of Defense) and also students utilizing transferred GI Bill benefits (such as spouses and children of veterans). Student veterans in the sample were enrolled at either 2- or 4-year institutions in the public, nonprofit, and for-profit levels. The sample is not representative across institutional sectors, as veterans at for-profit institutions are under-represented. This is due, in part, to the fact that the National Student Clearinghouse receives data from a smaller proportion of for-profit institutions (68% as of 2013, compared with 99% of public institutions and 93% of nonprofit institutions for the same year). As a result, the aggregate estimate of degree completion for veteran students by Cate is likely biased upward. In the sample, 79% of the student veterans initially enrolled in public institutions, with 11% at nonprofit institutions and 10% at for-profit institutions (Cate 2014b, p31).

To examine postsecondary completion rates in the MRP, the authors exclude students with an initial postsecondary enrollment date of 2011 or later. This drops about 8% of their initial sample, leaving 788,915 cases. In the reduced sample, 51.7% of student veterans achieved a postsecondary degree or certificate. Interestingly, 40.8% of those who completed a postsecondary degree or certificate had already earned at least one credential before utilizing GI Bill benefits. This could indicate degree attainment before enlistment, degree attainment during service (or directly after) through Prior Learning Assessment credits, or strategic use of benefits after service (e.g., using DOD tuition assistance for an associate's degree and saving GI Bill benefits for a bachelor's degree). Out of those who completed associates and bachelor's degrees, the average times to completion were 5.1 years and 6.3 years, respectively.

The NVEST project is similar in methodology to the MRP, but exclusively looks at the success of student veterans that use the Post-9/11 GI Bill. The sample includes

822,327 student veterans who used the Post-9/11 GI Bill between August 2009 and December 2013 and had records in the National Student Clearinghouse. The education records extended until September 2015. Among these student veterans who used the Post-9/11 GI Bill, 53.6% completed a postsecondary education program and 18% were still enrolled in 2015. Again, this excludes any student veterans who were enrolled during the time frame but did not use the Post-9/11 GI Bill and any student veterans who did use it at non-reporting postsecondary institutions or for on-the-job training (Cate, Lyon, Schmeling, and Bogue, 2017).

The research projects from the Student Veterans of America are the first real attempts to understand the postsecondary outcomes of modern student veterans. However, the studies suffer from some major limitations that limit their utility. The most significant issue is that the study samples include only veterans. The lack of a suitable comparison group within each study prevents us from drawing reliable conclusions across groups of interest (such as whether veterans finish degrees at the same rates as nonveterans enrolled in similar places). Both projects also aggregate student veterans in all degree levels when calculating completion rates. Although the author of the MRP report does note that varying approaches prevent direct comparisons with completion statistics from other research, he later goes on to do just this: "In fact, student veterans are attaining degrees at a rate similar to that of all students—traditional and non-traditional" (Cate 2014b, p.53). The NVEST report emphasizes this idea even more, noting in the executive summary that student veterans using the Post-9/11 GI Bill “perform better than their peers” and “are more likely to graduate” (Cate et al. 2017, p.viii).

2.6 Prior Research on Veteran Success in Related Areas

Related research on labor market outcomes for veterans and nonveterans is useful here. Angrist and Kreuger (1994) initially find an apparent wage premium for WWII veterans over nonveterans, but after using an instrument to correct for selection bias the authors find that these veterans earn no more than (and probably less than) comparable nonveterans. Kleykamp (2013) finds a small wage premium for post-9/11 veterans but only for those with a high school education or lower. Additionally, Kleykamp reports that the common bivariate statistics on unemployment for post-9/11 veterans significantly understate the labor market penalty these veterans pay compared to statistically similar nonveterans, in part because veterans are more likely to be male and less likely to be high school dropouts. This line of research illustrates how factors associated with military service can distort relationships between veteran status and labor market outcomes, but it also suggests that veterans perform worse (at least in terms of employment and wages) than nonveterans who otherwise hold the same characteristics on other relevant traits. However, Kleykamp (2013) does find that post-9/11 veterans are more likely than comparable nonveterans to enroll in college.

The evidence on veterans employed in the public sector is scarce and mixed. In the federal workforce, Lewis (2013) finds that veterans advance more slowly than nonveterans hired into the same grades. However, Johnson (2015) finds that those receiving veterans' preference advance at least as quickly as nonveterans after controlling for job and worker characteristics.

2.7 The Student Veteran Paradox

Veterans have many attributes that have been shown to hinder student success. Moreover, the evidence from research on labor market outcomes for veterans suggests that veterans should perform worse in college as well. On the whole, theory and related evidence suggest that veteran students should perform worse than nonveteran students. However, past research indicates this is not the case. Despite the limitations in scope and rigor, the prior research on student veterans has been fairly consistent: veterans perform at least as well as, if not better than, their nonveteran peers. Consistent findings that student veterans have outcomes that are equivalent to or better than nonveterans are very surprising.

This presents a logical paradox.[5] Theory and related evidence from labor market outcomes suggest that veteran students are less likely than nonveterans to succeed in college, yet this contradicts the evidence that student veterans are at least as likely to succeed. This is logically inconsistent and both cannot be true. Thus, the *student veteran paradox* can be summarized as follows: veteran students should be less likely to succeed than nonveteran students; but veteran students are *not* less likely to succeed and may, in fact, be *more likely* to succeed.[6]

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[5] In the most common type of logical paradox, sound reasoning produces a conclusion that is contradictory or nonsensical.

[6] The Million Records Project report also contains a discussion of a paradox regarding veteran students (Cate 2014b) but the usage is slightly different here.

2.8 The Competing Explanations and Hypotheses

The rest of this study attempts to explain the student veteran paradox. I examine veteran student success within a competing hypothesis framework to understand the apparent advantage that veteran students exhibit in postsecondary outcomes. I propose seven ways to resolve the student veteran paradox and make sense out of the veteran advantage seen in past research. These explanations are exhaustive but not mutually exclusive. The observed veteran advantage could be due to: 1) bias introduced through data and methodology limitations; 2) differences in observable background characteristics between veterans and nonveterans; 3) differences in enrollment behaviors between veterans and nonveterans; 4) maturity gains from entering college at a slightly older age; 5) education funding benefits from the GI Bill and related programs; 6) unobservable factors associated with selection into the military; or 7) the direct effects of military service. These seven explanations for resolving the student veteran paradox yield a set of testable research hypotheses. I discuss each explanation and related hypotheses in greater detail.

2.8.1 Explanation 1: Bias

The past research on outcomes for veterans in higher education has been limited in both scope and rigor. Most revolves around evaluating the impact of the GI Bill on the stock of educated veterans. Other researchers have examined how the college experiences for veteran and nonveteran students differ. More direct attempts to analyze the postsecondary success of veteran students have either focused on outcomes in the very short run (like semester grades) or been hindered by significant limitations in the data available. The earlier research is likely inapplicable to more recent veterans since the transition to an all-volunteer force. The most recent attempts of this sort argue that the college completion rates of veteran students are comparable to nonveteran students, yet these conclusions have been drawn from studies that only analyze data on veterans. Without an appropriate comparison group of nonveteran students in the same study, the researchers risk biased results and unsubstantiated conclusions.

The bias explanation suggests that the veteran paradox doesn't exist and has been a product of substantial limitations in past research. If the bias explanation is true, then when examining student success with an appropriate dataset and comparison group either veterans will not hold characteristics that hinder student success or veterans will no longer be at least as successful as nonveterans on the outcome measures:

H1a: Veterans do not hold characteristics that inhibit student success

H1b: Veterans have worse student success outcomes than nonveterans

2.8.2 Explanation 2: Background Characteristics

If veteran and nonveteran students differ on background characteristics that predict student success, then these differences could explain varying student success outcomes between veterans and nonveterans. These background characteristics include sex, race, socioeconomic status, and academic background. Although theory and past research predict that veterans should hold characteristics on these variables that make them less successful students, it is important to test these predictions nonetheless. Sometimes relationships are only partially understood by researchers and occasionally relationships change directions in multivariate settings.

Though unlikely, the group differences explanation suggests that background characteristics explain the veteran advantage:

H2: Veterans have worse student success outcomes than nonveterans after controlling for background characteristics

2.8.3 Explanation 3: Enrollment Patterns

Differing patterns related to college enrollment could explain differing student success outcomes. These enrollment patterns include levels of basic funding (grants, loans, and scholarships), part-time status, and hours of transfer credit. Again, these factors seem unlikely to explain the veteran paradox since past research has shown that veterans are more likely to enroll part-time and have similar levels of overall funding (Radford and Wun, 2009; Radford, 2011). Still, these differences in enrollment behaviors might play out in unexpected ways that are not predicted by theory.

If this explanation is true, then enrollment behaviors will explain the veteran advantage:

H3: Veterans have worse student success outcomes than nonveterans after controlling for enrollment pattern variables

2.8.4 Explanation 4: Maturation

The veteran advantage could also be due to simple maturation. Used here, maturation refers to gains in maturity that are a result of getting older.[7] Those who enter

[7] This kind of maturation does not include maturity gains that could be a result of military service.

college after serving in the military are necessarily older than their traditional student peers who enter directly from high school. Past research has shown that delayed entry, on the whole, is negatively associated with postsecondary success (Horn, Cataldi, and Sikora 2005; Bozick and DeLuca 2006), but this relationship might be curvilinear, with shorter delays in entry being a boon to student success while longer delays hinder it. At least for some kinds of students, a short delay (a few years or less, perhaps) could be advantageous, as it can allow for extra time for growth and maturity. This is the driving idea behind the practice of taking a "gap year" between high school and college (Jones 2004). However, I could find no research on how these short, intentional delays impact education outcomes. Longer delays (perhaps at least 5-10 years) likely hinder student success because older students are more likely to have dependents and full-time jobs. Additionally, students who enter at a substantially older age may also face technological, pedagogical, and/or discriminatory barriers, simply because they have been out of the educational system for so long. If the negative effect of long-term delays in college entry is very large, this could mask any positive effects that short-term delays may have. Since about half of student veterans are between twenty and thirty (Cate 2014b; Radford and Wun 2009), I propose that the veteran advantage could be attributable to the gains in maturity that accompany short-term (but not long-term) delays in college entry.

If the maturation explanation is true, then age will explain the veteran advantage:

H4: Veterans have worse student success outcomes than nonveterans after controlling for age

2.8.5 Explanation 5: Funding

The veteran advantage could be due to the education funding benefits that veterans receive. Although some research suggests that on average veteran students receive the same amount of financial assistance as nonveteran students (Radford and Wun, 2009), the aid that veteran students receive is substantially different in nature. While most large-scale programs award student aid on the basis of either need or academic merit, military education benefits are awarded on the basis of service. Aside from being fundamentally different in nature, military funding generally comes free from academic requirements in order to maintain continued funding (e.g., making satisfactory academic progress for federal aid, or maintaining a high GPA in the case of Georgia's HOPE Scholarship). Although such requirements may incentivize students with traditional sources of student aid to perform well academically, these requirements also mean that funding could potentially be cut off midstream. Such disruptions in aid can cause academic progress to stall even if the student is still in good standing with respect to institutional requirements. Even aside from these possibilities, the education benefits for military students are generous and could increase the likelihood of student success and contribute to the veteran advantage.

The funding explanation suggests that aid explains the relationship between veteran status and success outcomes. Since aid could be associated with other relevant factors, I expect a higher "dosage" of veteran funding will have a greater impact:

> H5: Students with higher levels of veteran education benefits tend to have better student success outcomes than otherwise similar students

2.8.6 Explanation 6: Selection

Unobservable characteristics associated with enlistment could also explain differences in student success. As these factors are unobserved here, prior literature must be supplemented with a degree of speculation. Past research has shown that enlistment is associated with social isolation, alternative youth family structures (i.e., living with single-, step-, or foster-parents), and histories of adolescent fighting (Elder et al., 2010; Spence, Henderson, and Elder, 2013; Teachman and Tedrow, 2014). Additionally, those who enlist could be more impulsive since it's easier to enter the military than college. Moreover, immaturity could be associated with signing up if the enlisted are delaying life decisions concerning education and career. I could find no research to support these latter correlations and, if true, it is uncertain how veteran students would compare to civilian students on these dimensions at the time of college entry. It seems that these factors would reduce student success. On the other hand, unobserved characteristics might also improve student success. Those who sign up for the military may be more mature than others, as evidenced by their willingness to risk personal safety to serve the country. Additionally, they could be more willing to delay short-term gratification in pursuit of long-term goals. At least for those who aren't pursuing a military career, enlistment necessarily puts personal and professional matters on hold during service (although this is less true for those in the National Guard or reserves). Moreover, if the veterans who eventually go to college enlisted with the intent of using the subsequent education benefits, they might be actually better at planning and execution functions that could be a boon in the classroom. Kleykamp (2006) finds that college aspirations are associated with enlisting (over entering the labor market or remaining unemployed), but no research supports superior planning capacities among those who enlist.

The selection explanation suggests that unobserved factors help veterans succeed in college. On the whole this explanation seems less plausible, but if it is true then the veteran advantage will disappear when attempts to include these unobservable factors are used:

H6: Veterans have worse student success outcomes than nonveterans who match characteristics associated with enlistment

2.8.7 Explanation 7: Direct Effects

The veteran advantage could be due to the direct effects of military service. Proponents have long argued that military service builds character. A majority of veterans report that military service helped them develop independence, self-discipline, and the ability to cope with adversity, and those with more combat experience reported greater gains on the latter two (Elder and Clipp 1989). Self-discipline probably stems from habit-building and repeated challenges during training. Self-discipline likely also plays into one's ability to cope with adversity, as self-control is one component of resilience (Meredith et al. 2011). Although physical training and combat experiences may naturally improve the ability to cope with adversity, this outcome could also be the product of resilience training programs during service. These programs were introduced to prevent readjustment problems and use evidence-based practices from positive psychology to build resilience. Several have been introduced, but the Army's Comprehensive Soldier Fitness program has received the most focus (Bowles and Bates

2010, Casey 2011, Adler 2009). Grit is a related non-cognitive trait that might be associated with military participation. Duckworth and colleagues (2007) define grit as "perseverance and passion for long-term goals" and find it to be a strong predictor of overall educational attainment among adults and first-year GPA among students in elite universities. Although the effects of the military on grit have not been directly examined, grit has been associated with whether cadets complete the first year of training at military academies and whether soldiers complete the Army Special Operations Forces selection course (Duckworth et al. 2007, Maddi et al. 2012, Eskreis-Winkler et al. 2014). Military service could improve performance under pressure since soldiers are required to act under high-stress situations (especially during combat). However, to my knowledge this has not yet been examined.

The direct effects explanation suggests that something about military service makes veterans better students. If the direct effects explanation is true, then veteran status will have a positive effect on student success even after controlling for the variables related to the above explanations:

H7: Veterans have student success outcomes at least as good as nonveterans after controlling for all the aforementioned factors

2.9 Conclusion

Researchers have shown that many factors drive performance on student success outcomes and that veterans and nonveterans vary substantially with respect to these factors. This, coupled with research on veteran success in the labor market, suggests that student veterans should be less successful than nonveterans. Yet, the limited research assessing student success outcomes for veteran and nonveteran students indicates that veterans are at least as successful as nonveterans. This is the student veteran paradox: veterans should be less likely to succeed than nonveterans, but veterans are as likely to succeed and may even be more likely to succeed. I propose seven possible explanations that could resolve the paradox. Each explanation leads to testable research hypotheses. The next chapter details methodological approaches to testing these hypotheses.

CHAPTER 3

DATA AND METHODOLOGY

3.1 Introduction

Theory and related evidence predict that veteran students should be less successful than their nonveteran peers, yet the limited past research suggests that they are actually as successful as, if not more successful than, nonveterans. This is the student veteran paradox. I posit seven potential explanations to resolve this paradox: bias in past research, background characteristics of veterans, enrollment behaviors of veterans, maturation from delayed entry, education aid benefits for veterans, unobservable factors associated with selection into the military, or the direct effects of military service. Each potential explanation leads to a testable research hypothesis. In this chapter, I explain the data and methodology I use to test this set of hypotheses.

I first detail the unique dataset used for this dissertation. I use administrative records from Georgia State University that have been de-identified by the Office of Institutional Research. As this dataset includes all students who enrolled at GSU during the study period, I avoid the limitations faced by most other researchers. Most of the commonly used, publicly available datasets only include first-time, full-time freshman or students who have filed the FAFSA; this almost certainly excludes a large segment of the student veteran population. Other researchers have used surveys of veterans, but these studies lack an appropriate comparison group of nonveterans. This research avoids those limitations.

After detailing how I operationalize each variable, I explain the methodological approaches used to test the hypotheses. I use OLS regression and logistic regression to assess three metrics of student success: grades, retention, and completion. I supplement these models with two additional analyses to strengthen the design. I leverage variations in the GI Bill program to assess whether higher levels of funding lead to better student success outcomes. I also use matching to test whether unobservable factors associated with military enlistment or the direct effects of military service could drive veteran student success. Finally, I conclude by reviewing the data and methodology and by looking ahead to the next chapter.

3.2 Data

Georgia State University (GSU) is a large, public research university in Atlanta, Georgia. GSU collects student-level data on an extensive set of variables for each student in the university. The Office of Institutional Research has provided a de-identified subset of this data for this research. The student data covers demographic and academic information for all GSU students in the population, including both veterans and nonveterans. The population includes students who first enrolled at GSU between the fall term in 2003 and the spring term of 2015. As recent cohorts have not had time to finish a bachelor’s degree, these will only be used in the analysis of short-term student success outcomes.

Georgia State University is a unique institution in some respects. First, GSU is a large, urban, public research university. About 51,000 students were enrolled in the fall of 2016 and about 25,000 of those were undergraduates. The campus is located in downtown Atlanta. As a result, GSU has historically had a small residential student population. Despite growth in recent years, only 21 percent of undergraduate students lived on campus in 2016. This might cause GSU students to be less academically and socially integrated than students on traditional campuses. Second, GSU enrolls more students from groups with historically low rates of degree completion than any other college in Georgia. In 2013, 56 percent received Pell grants, 30 percent were first-generation students, and 60 percent were minorities. Third, graduation rates have significantly improved at GSU in recent years. Among first-time, full-time freshmen, the six-year graduation rate was 31 percent in 2003 but had risen to 53 percent by 2013. In the same year, these graduation rates were higher for minority students: 66 percent for Latinos, 57 percent for blacks, and 51 percent for whites. Students who receive Pell Grants are no less likely to graduate. Administrators at GSU attribute the improvements to the supplemental instruction program (a peer tutoring program), freshman learning communities (major-based residence hall assignments with additional support), micro-grant programs to cover small gaps in tuition payments, and data analytics systems that support advising and administrative decision-making. As a result of these factors, studying GSU students may impact the results. Because of GSU's success graduating black and low income students, veterans at GSU could be more successful than veterans at other large universities in or near Atlanta (e.g., Georgia Tech, University of Georgia, Emory University).

The data used in this dissertation include records for students who entered GSU from the fall semester of 2003 until the spring semester of 2015. This includes 96,237 students enrolled for a bachelor's degree. Graduation information is available through the spring semester of 2017. For the graduation models, I use as many cases as possible; so for the six-year models, I include every cohort that has at least six years of data after entry. As a result, sample sizes for the graduation models differ. Veterans are a small proportion of the students seeking bachelor's degrees. There are only 2,144 identified student veterans, which comprise 2.23% of the students during the study period.

3.3 Data Limitations

The nature of the data used for this dissertation overcomes many of the limitations inherent in past research on student veterans. However, the GSU data has two main shortcomings. First, GSU does not systematically collect veteran status from each student. Instead, veterans (along with students who are active duty, National Guard, reserves, or military dependents) are identified as such in the data system when they contact the GSU Military Outreach Center in order to utilize military education benefits. Alternatively, individuals can identify themselves as veterans to the Military Outreach Center, which the office encourages but cannot mandate. Thus, some student veterans at GSU are not identified. This could include veterans who have exhausted education benefits prior to entry at GSU, have transferred education benefits to dependents, or have chosen to save benefits for later usage. However, I expect that unidentified veterans are a very small proportion of the total GSU veteran population, because the low eligibility bar for the Post-9/11 GI Bill (serving after 9/11/2001 for at least 90 days, or fewer if honorably discharged) and the prevalence of additional veteran education benefits (e.g., the Department of Defense Tuition Assistance program) mean that most student veterans are likely to qualify for at least some benefits. Still, it is impossible to know how large this group is and how their exclusion will introduce bias into the results.

Second, missing values also limit the utility of the data. Academic background variables are missing for large numbers of students, particularly veterans. Students who delayed entry more than five years from high school graduation or who enter with more than 30 hours of transfer credit are generally missing both high school GPA and ACT/SAT test scores. More than 45% of nonveterans and more than 65% of veterans are missing at least one academic background variable.

Additionally, some students are missing data on traditional measures of socioeconomic status. In the GSU dataset, this information is taken from FAFSA submissions. As a result, students who never file the FAFSA during their tenure at GSU have missing values. Although 87% of nonveterans and 88% of veterans file a FAFSA at some point while at GSU, veterans using the GI Bill do not have to file in order to use benefits. Thus, the veterans with the most funding may be missing values for the SES variables. If this is the case, excluding these students could bias student success outcomes for veterans.

Some of the common pitfalls of administrative records datasets are also present here. Across the college career, some students are missing values for other variables in some semesters. For some students, time-invariant characteristics (like race) change over time at GSU.

I take several steps to address these missing data problems. Since the delayed entry students are required to take the Compass exam to determine remedial course placement, I convert scores from the Compass exam to ACT score equivalencies. I compute the ACT score equivalencies for both Compass and SAT scores using concordances developed by the College Board and ACT (Dorans 1999, ACT 2009). For the other missing academic background values, I use imputation methods. This is appropriate since the academic background variables only serve as control variables in these analyses. Since the missing values are systematically missing for certain types of students but not necessarily systematically biased, these are classified as missing at random (MAR). Researchers typically recommend multiple imputation methods in this situation (Rubin 1987, Horton and Kleinman 2007). These methods involve creating several sets of imputed values for the cases with missing data, computing estimates (e.g., regression coefficients) with each of set of imputed values, then combining these results with special rules. However, one downside is that the combining rules are unreliable for some estimates that are easy to interpret, like average partial effects and average marginal effects. Because of this, I use a crude imputation method of imputing a zero for the missing value and adding a dummy variable for each academic background variable to indicate whether a case has an imputed value for it. This approach allows the inclusion of the missing cases but the dummy variable prevents the introduction of bias. Long and Kurlaender (2009) have also used this approach to deal with missing values in postsecondary education research. To ensure this crude method does not skew results, I also perform multiple imputations with chained equations (MICE) using predictive mean matching (Horton and Kleinman 2007; White, Royston, and Wood 2011). In the MICE procedure, I compute a set of 20 imputations and use the 3 nearest neighbors in the predictive mean matching procedure. The results from both imputation methods are very similar and reported in the next chapter.

For socioeconomic status, I use an alternative measure available for students regardless of FAFSA-filing behavior. I use the student's ZIP code to add mean income in the home ZIP code as a proxy for SES. To ensure the reliability of this approach, I also compute the models for the subset of students who file the FAFSA using first-generation student status and gross financial need[8] as measures of SES. When these results differ, I estimate the models using the Heckman method to correct for sample selection bias (Heckman 1979, Wooldridge 2009). The Heckman correction uses a two-stage approach to first model selection into the sample and then use the results to model the outcome of interest. In this case, values on covariates are unobserved for students who do not file the FAFSA. I use the Heckman procedure to first model FAFSA filing behavior, and then estimate student success outcomes. I find that results from both approaches are very similar and are reported in the following chapter.

When other variables have missing values in certain semesters or vary across semesters, I make adjustments based on observed data when reasonable. When variables that do not change over time (e.g., race, high school GPA) are missing in some semesters, I use values for that student from earlier or later semesters. When the variables that should not change over time actually do vary across semesters, I use the most commonly reported value for that student. However, I make no adjustments for veteran status or the student success outcomes.

[8] Gross financial need is the difference between the cost of attendance and the estimated family contribution.

3.4 Variables

3.4.1 Dependent Variables

3.4.1.1 First-Year GPA

This variable indicates the student's cumulative college GPA after one full academic year (i.e., after two non-summer terms). For students who do not complete the entire first academic year, I use the cumulative GPA at the last point during that academic year.

3.4.1.2 First-Year Retention

This variable indicates that the student is still enrolled one year after his or her initial enrollment term. This is a computed dichotomous variable, coded 1 if the student is enrolled one year after the matriculation term. For summer entrants, I code 1 if the student is enrolled in either the summer or fall term one year later.

3.4.1.3 Graduation

I compute a set of variables to identify degree completion. I use a dichotomous variable to denote whether a student ever finished his or her degree, and a set to denote whether he or she finished within 4, 5, 6, 7, and 8 years from the matriculation term. These are computed from the graduation term variable and the matriculation term variable. I use data from the National Student Clearinghouse to compute a set of graduation from anywhere variables for the primary models. I also construct a set of graduation-from-GSU variables for use in an alternate specification.

3.4.2 Independent Variables

3.4.2.1 Veteran Status

This is the primary independent variable of interest, which indicates prior participation in the military. This is a dichotomous variable, coded 1 for veterans. Students are classified as veterans if they have used veteran education benefits at any time during their college career or if they have identified themselves as a veteran without benefits to the Veteran Outreach Office. This variable is constructed from the GSU veteran type variable.

Students who report that they are either active duty, a member of the National Guard, or a Reservist in the armed forces are not classified as veterans. There are likely few (if any) students who enroll while on active duty, since this would impose time, energy, and geographic limitations to postsecondary participation. Those in the National Guard or the Reserves likely exhibit irregular enrollment patterns as these students may be deployed while enrolled (Ackerman et al 2009; DiRamio et al 2008; Cate 2014b). These deployments cause discontinuous enrollment. If activated mid-semester, the student could lose credits that were in-progress at that time and may need to delay reenrollment for several months until the next term begins. Those who re-enroll may also be misaligned with course sequences that include intermittently offered classes, further delaying degree completion (Rumann and Hamrick 2010). Additionally, those using veteran survivor/dependent benefits are not classified as veterans.

3.4.2.2 Female

This dichotomous variable represents sex. I code female students as 1 and male students as 0.

3.4.2.3 Race

This variable indicates the race/ethnicity of the student and is constructed from two separate race and ethnicity variables. Race is self-reported and students can select any applicable race from Asian, black, American Indian, white, Pacific Islander, and other. Students can also identify as Hispanic in the ethnicity question. I recode race into white, black, Hispanic, Asian, other/mixed categories. I also include a category for those who did not report a race. I classify any student ever reporting Hispanic status on the ethnicity question as being Hispanic, so that Hispanic is a mutually exclusive race category. Students who select more than one race are classified as Other/Biracial. Students who have different races in different semesters are classified as the most commonly reported race during the student's academic career.

3.4.2.4 Socioeconomic Status

This variable is a measure of class. I use the mean household income by ZIP code as a proxy for socioeconomic status. I use income data from the 2006-2010 American Community Survey compiled by the Michigan Population Studies Center. Mean ZIP income is a continuous variable in ten thousands of dollars. In line with Long and Kurlaender (2009), the models also include a squared term. As an alternate specification of SES I also use the gross financial need and first generation student status variables, but these are only available for FAFSA-filers. I use a continuous measure of gross need in thousands of dollars. I follow the GSU protocol for first generation status and classify the student as first generation if both the mother and father have some college experience, a postsecondary certificate, an associate's degree, a high school diploma or lower listed as the highest earned degree. If at least one parent has earned a bachelor's degree, the student is classified as not a first generation student. The student is classified as unknown first generation student status if the student reports either unknown/other levels of education for both parents, or unknown/other education for one parent and the other has a high school diploma or lower.

3.4.2.5 High School Grade Point Average

This variable serves as one of the academic background indicators. I take the value calculated by the GSU based on high school courses and grades, which disregards +/- variations and additional weighting given to AP courses. Because the effects of academic preparation vary with level of preparation, the models also include a squared term to capture nonlinear effects of high school GPA (Kuh et al. 2008, Long and Kurlaender 2009, Curs and Harper 2012).

3.4.2.6 Standardized Test Scores

These two variables (for math and English) serve as additional academic background indicators. I use scores from the ACT, SAT, and Compass exams. The Compass exams are math and English subject tests that determine remedial course placements. Older students aren't required to take the SAT or the ACT before matriculating at GSU, but they are required to take the Compass exams. Since most students only take one of these standardized exams, I convert the Compass and SAT scores into ACT-equivalent scores by using the concordances developed by the College Board and ACT (Dorans 1999, ACT 2009). The ACT scores range from 1 to 36. I use the highest score if a student has multiple scores for an exam or scores from multiple kinds of tests. I use both an ACT math-equivalent score and an ACT English-equivalent score. The models also include squared terms to capture nonlinear relationships (Kuh et al. 2008, Long and Kurlaender 2009).

3.4.2.7 Advanced Placement Credit

This variable serves as one of the academic background indicators. This is a continuous measure of the number of credit hours GSU awarded for AP testing. It is coded zero for those with no AP credit or a missing value. The models also include a squared term to include nonlinear relationships.

3.4.2.8 Student Aid

These variables indicate the amount of student aid received by type in the first semester. I separate student aid into grants, loans, and scholarships. Although both scholarships and grants do not require payment, scholarships are typically merit-based and grants are typically need-based. I use thousands of dollars for each. Aid from the GI Bill is not included any of these categories (see below). GI Bill payment amounts are only available for most veterans using the Post-9/11 GI Bill in or after the fall 2009 semester.

3.4.2.9 Transfer Hours

This continuous variable indicates the number of hours of transfer credit. The models also include a squared term to capture nonlinear effects.

3.4.2.10 Part-Time Status

I construct this dichotomous variable from the attempted credit hours to indicate the enrollment status of the student (i.e., whether the student has attended part-time rather than full-time). Students sometimes fluctuate between part- and full-time statuses across semesters. In the graduation models, I follow Adelman (2006) and code a student as part-time if he or she drops below 12 credit hours for any non-summer term. For the first year GPA and retention models, I only use the first year to compute part-time status.

3.4.2.11 Early College GPA

Although this is one of the dependent variables of interest, it is also a predictor of degree completion. I use first semester GPA in the primary models but also compute alternate specifications using first year GPA. I only use this as an independent variable in the graduation models.

3.4.2.12 Age at Matriculation

This variable indicates the age of the student at the time of matriculation. To impose the fewest assumptions on the shape of the relationships, I use a set of dummy variables for each age in the dataset. In the models, I use age 18 as the reference group.

3.4.2.13 Term of Entry

Since GSU has improved graduation rates over time, I use a set of dummy variables that indicate term of entry. The earliest term, fall 2003, is used as the reference group.

3.4.2.14 Post-9/11 GI Bill Usage

This is a set of two dichotomous variables. The first indicates that a veteran is using funding from the newer, Post-9/11 GI Bill. The second indicates that a veteran first used funding from an older form of the GI Bill, but later used funding from the Post-9/11 GI Bill. I construct these from the veteran type variable used by GSU. These variables are only used in the aid quasi-experiment.

3.4.2.15 Post-9/11 GI Bill Aid

These previous aid variables (grants, loans, and scholarships) do not include aid from the GI Bill. GI Bill payment amounts are only available for most veterans using the Post-9/11 GI Bill and only from the fall 2009 semester onward. I use thousands of dollars and only use this amount in the aid quasi-experiment.

3.4.2.16 Veteran Dependent

This dichotomous variable identifies students who have used education benefits that have been transferred from a veteran to a dependent or survivor. Veteran dependents are identified as such if they have used dependent/survivor benefits at any point during their time as a student. As some veterans also receive dependent/survivor benefits, I only include dependents/survivors who have not also used veteran benefits during college. I construct this from the veteran type variable used by GSU.

3.4.2.17 Geographic Military Presence

This variable is only used to model propensity to enlist in the military. Although Kleykamp (2006) uses county-level recruiter density, this data is seemingly no longer available in the Defense Manpower Data Center. Instead, I calculate the military recruits per capita by ZIP code tabulation area (ZCTA). I use the Department of Defense recruitment data to tabulate the number of military enlistments by ZCTA between 2002 and 2006 (Christensen 2015), then divide this by the ZTCA population from the Census Bureau (Bittner 2013).

3.5 General Methodology and Justification

To resolve the student veteran paradox, I examine how veteran status and related factors are associated with the three key student success outcomes: grades, retention, and completion.

3.5.1 The Bias Explanation

H1a: Veterans do not hold characteristics that inhibit student success

H1b: Veterans have worse student success outcomes than nonveterans

I test the bias explanation by replicating the student veteran paradox with the GSU data. In contrast to past research, I use data that includes veterans who didn’t file the FAFSA, veterans who attend part time, veterans who began at another institution, and nonveteran students who can serve as a fair comparison group. Theory suggests that veterans should be less successful students because they hold characteristics that predict worse student success outcomes, but past research has shown that veterans are at least as successful as nonveteran students. I first compare means and proportions of characteristics for veterans and nonveterans. I then compare means and proportions of the student success outcomes for veterans and nonveterans. For both, I also report differences and whether they are statistically significant. If the student veteran paradox stems from bias in past research due to limitations in data and methods, then either veterans at GSU will not hold characteristics that inhibit student success, or veterans at GSU will be less successful than nonveterans on the basic outcome measures.

3.5.2 The Background, Enrollment, and Maturation Explanations

H2: Veterans have worse student success outcomes than nonveterans after controlling for background characteristics

H3: Veterans have worse student success outcomes than nonveterans after controlling for enrollment pattern variables

H4: Veterans have worse student success outcomes than nonveterans after controlling for age

I test the background, enrollment, and maturation explanations by comparing student success outcomes for veterans and nonveterans while controlling for additional factors. To do so, I construct regression models for each of the three student success outcomes of interest. As GPA is a continuous variable, I use OLS regression. Since retention is a binary outcome, I use a logit model.

Graduation is usually measured as a dichotomous variable over a given time span, noting whether the student successfully completed a bachelor's degree in, say, six years (four years and eight years are also used, although less commonly). This practice, however, leads to a time-framing problem: a student who finishes in 7 years will be identified as a non-completer when operationalized as such (even if we know the student eventually did finish). This also leads to a censoring problem, in which students who have not yet graduated during the study time period will either be discarded or counted as non-completers (even if they will go on to finish the degree).

Although survival analysis is most suited to this type of situation, there are two important shortcomings. First, survival analysis results are difficult to interpret in a meaningful way. The results may show that certain types of students have differing hazard functions or conditional probabilities of graduation, but this is difficult to clearly map onto traditional ways of thinking about degree completion, such as graduating within six years. Second, and most significantly, estimating hazard functions is computationally difficult when individuals are not abundant in each time period. Since veterans are a very small proportion of students at GSU, estimation problems arise.

Because of these issues, I use a set of logit models to measure degree completion over various time spans. I measure completion at four, five, six, seven, and eight years from matriculation. I also use a time-neutral measure of completion that denotes whether a student ever completed a degree, although I only use students who enter during or before the fall 2011 semester so there are at least six years of data available.

I refer to these as the primary models throughout the dissertation. Veteran status is the primary variable of interest in these models, but I control for background characteristics, enrollment patterns, and age at matriculation. Background characteristics include sex, race, socioeconomic status, and academic background. Enrollment behaviors include aid levels, transfer hours, and part-time status. I control for age with a set of dummy variables to impose the fewest assumptions about the shape of the relationships between age and the measures of student success. I also include a set of dummy variables for term of entry to control for institutional changes over time.

For these models, I report the average partial effects and average marginal effects. These represent the average change in the predicted outcome due to a one-unit increase in the independent variable, holding the other variables in the model constant. In the GPA model, these are the changes in predicted first year GPA. For the logit models, these are the changes in the predicted probabilities of retention and graduation. Instead of reporting the average marginal effects for the age and term of entry variables, I compute the predicted outcomes for each value and graphically represent them. I also include the full regression models in the appendix.

3.5.3 The Funding Explanation

H5: Students with higher levels of veteran education benefits tend to have better student success outcomes than otherwise similar students

I test the funding explanation with two approaches that examine how levels of military education benefits impact student success. First, I use a quasi-experimental design to leverage changes in the extent of GI Bill benefits to tease out the effects of the aid. The Post-9/11 GI Bill applies to anyone who served after 9/11, but the law was passed in 2008 and went into effect in the fall of 2009. The Post-9/11 GI Bill had less restrictive eligibility requirements and more generous benefit levels. Veterans who were already using older versions of the GI Bill could upgrade to the newer, more generous version if eligible. Second, I use award amounts for veterans with the Post-9/11 GI Bill to assess the effects of different levels of aid. If the education funding is driving student success for veterans, then a higher "dosage" of the aid should result in even greater success.

The first approach repeats the primary models from the previous chapter but adds three dummy variables to identify types of veteran benefit usage. The veteran variable still identifies all veterans. The first two new variables identify veterans who upgrade from older versions of the GI Bill to the more generous Post-9/11 GI Bill, and veterans who exclusively use the Post-9/11 GI Bill. As these two variables show the effect of using the Post-9/11 GI Bill beyond the base effect of being a veteran, they are technically interaction terms. Since the models control for changes over time, the interaction terms essentially serve as difference-in-differences estimators. The third new variable is a dummy variable to indicate whether a student used survivor/dependent education benefits. These students are nonveterans who receive veteran education funding. If the aid explanation is driving the veteran paradox, then outcomes should be similar for veterans and veteran dependents, and even better for those veterans using the more generous post-9/11 GI Bill.

The second approach repeats the primary models from the previous chapter on the subset of Post-9/11 GI Bill recipients with award data, dropping the veteran dummy variable and adding a continuous measure of GI Bill aid. In the GSU data, these award amounts are only available for veterans using the Post-9/11 GI Bill. Thus, I restrict the analysis to veterans using the Post-9/11 GI Bill who have award amount data for the first semester. If the aid explanation is driving the veteran paradox, then outcomes should be better for veterans who have larger GI Bill awards.

The Selection and Direct Effects Explanations

H6: Veterans have worse student success outcomes than nonveterans who match characteristics associated with enlistment

H7: Veterans have student success outcomes at least as good as nonveterans after controlling for all the aforementioned factors

The veteran paradox could be explained by unobserved factors associated with selection into the military. Those who volunteer to serve in the armed forces may be inherently more mature or better at long-term planning. On the other hand, they could be more impulsive or less prepared for life after high school. These factors would impact student success outcomes. The fact that these are unobserved (at least in this research) makes it very difficult to control for them.

I test the selection and direct effects hypotheses by repeating the primary models after using matching to create a balanced comparison group of nonveterans. Matching is typically used to deal with selection on observables, but if the relevant unobservables can be predicted by the observables, the procedure will also control for them. This is a strong, unverifiable assumption and illustrates a key weakness in this dissertation. However, matching is the most appropriate way to address selection within this context and limitations of this research.[9] I create the matched group of nonveterans using the sex, race, socioeconomic status, academic background, age, and geographic military presence variables. If selection explains the veteran paradox and the observables can predict the unobservables, then the effects of veteran status on retention and graduation will diminish or vanish after matching. If selection explains the veteran paradox and the observables cannot predict the unobservables, then the effects of veteran status on retention and graduation may stay the same after matching. If the direct effects of military service are driving the veteran paradox, then the effects of veteran status will remain or even increase after matching.

3.6 Conclusion

The seven explanations to resolve the student veteran paradox each yield unique research hypotheses. I use a dataset of student-level administrative records from Georgia State University to test these hypotheses. I use OLS regression, logistic regression, and a combination of survival analysis and logistic regression to assess grades, retention, and completion, respectively. Additionally, I include two supplementary analyses to bolster my results: a quasi-experiment to address funding and propensity score matching to address selection. The specifications used are straightforward, logical, and largely already

[9] An instrumental variables approach is more appropriate for dealing with unobservables related to selection. I also attempted to use geographic military presence as an instrumental variable. Although the results suggested that the primary models are robust to unobservables, these were ultimately excluded because of the weakness of the instrument and the incompatibility of the instrumental variables approach and situations with a binary treatment and a binary outcome.

established in the literature. In the next chapter, I present and discuss the first set of results from these analyses.

CHAPTER 4

FINDINGS FROM PRIMARY MODELS

4.1 Introduction

Student veterans appear to be at least as successful as student nonveterans despite many characteristics that predict that veteran students should be less successful. I argue that seven competing explanations could potentially resolve this paradox: bias in past research, background characteristics, enrollment behaviors, maturation, aid, unobserved factors associated with selection, and direct effects of military service. This chapter presents findings from several analyses that test competing hypotheses from the bias, maturation, background, and enrollment explanations. To some extent, they also speak to the aid, selection, and direct effects explanations; however, these are addressed more directly in the following chapter. These results are the first steps in understanding and explaining the student veteran paradox.

I first test the bias explanation by corroborating both parts of the student veteran paradox. First, theory predicts that veterans should be less successful students because they hold characteristics that inhibit student success. I compare means and proportions for veterans and nonveterans to confirm this half of the student veteran paradox. Veterans are more likely than nonveteran students to be male and black. Veterans have weaker academic backgrounds, are of lower socioeconomic status, and enter college later. On the other hand, veterans enter with more transfer hours and, contrary to expectations, are *less* likely to attend part-time. Second, past research has shown that veterans are at least as successful as nonveteran students. I report student success outcomes for each group to substantiate the second half of the student veteran paradox. Veterans and nonveterans at this university generally have similar academic outcomes, with one exception. In contrast to past findings, first year GPA is lower for veterans. However, veterans are as likely as nonveterans to return after the first year. Surprisingly, veterans are more likely than nonveterans to graduate within four or five years, but have similar graduation rates within six years and beyond. The bias explanation can be ruled out for retention and completion, but not for first year grades.

I next test the background, enrollment, and maturation explanations by examining the relationship between veteran status and student success outcomes while controlling for additional factors. These models incorporate background characteristics, enrollment patterns, and age of entry, along with controls for institutional changes over time. When controlling for other factors, predicted first year GPA is lower for veterans, but veterans are more likely to return after the first year and are more likely to graduate. The background explanation can be ruled out, but differing enrollment patterns help explain the veteran paradox for retention and completion. The maturation explanation explains why the difference in first year GPA widens in the multivariate model, but does not explain the differences that emerge in retention and completion.

I also report the results of alternative versions of the primary models to justify methodological assumptions and illustrate model robustness. I repeat the primary models with alternate specifications of the graduation, early college GPA, and socioeconomic status variables, and with alternate imputation methods for the academic background variables. The results from these model variations are similar to those from the preferred primary models and provide additional confidence in them. I conclude by reviewing the results from the primary models and by looking ahead to the next chapter, which presents secondary models that extend the results presented here.

4.2 Characteristics of Nonveteran and Veteran Students

I begin testing the bias explanation by characteristics held by nonveterans and veterans:

H1a: Veterans do not hold characteristics that inhibit student success

Nonveteran and veteran students are substantially different at Georgia State University (GSU). Table 1 compares characteristics for nonveteran and veteran students. As expected, student veterans hold many characteristics that predict lower probabilities of college success.

Table 1. Characteristics of Nonveterans and Veterans

VARIABLES	Nonveterans	Veterans	Difference	
Percentage Female	60	39	-21	**
Race, Percentage				
White	36	31	-5	**
Black	37	48	11	**
Hispanic	8	8	0	
Asian	11	3	-8	**
Other/Biracial	4	5	1	+
No Race Given	5	5	0	
Mean ZIP Income	$78,709	$74,001	-$4,708	**
Percentage Filed FAFSA	87	88	1	
Gross Financial Need	$14,767	$14,750	-$17	
Percentage 1st Generation Student	20	23	3	**
Percentage Not 1st Generation Student	57	52	-5	**
Percentage Unknown 1st Gen Status	23	25	2	*
High School GPA	3.29	3.16	-0.13	**
AP Credit	1.3	0.3	-0.9	**
ACT Math Score Equivalent	23.1	21.9	-1.3	**
ACT English Score Equivalent	22.5	22.1	-0.4	**
Grants (first-semester)	$868	$788	-$80	**
Loans (first-semester)	$1,936	$1,783	-$154	**
Scholarships (first-semester)	$1,288	$535	-$754	**
Percentage Transfer Student	51	73	22	**
Transfer Hours	38.6	53.2	14.6	**
Percentage Part-Time Student (first-year)	31	26	-5	**
Percentage Part-Time Student (ever)	51	43	-8	**
Age at Matriculation	21.9	26.6	4.7	**

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

Characteristics are very different for nonveteran and veteran students, generally in ways that are in line with expectations. Although 60% of nonveterans are women, only 39% of veterans are female. Veterans are more likely to be black (48% of veterans versus 37% of nonveterans) and less likely to be Asian (3% versus 11%). Veterans are of lower socioeconomic status on most measures. The mean income in the home ZIP code is about $5,000 lower for veterans than nonveterans, and veterans are 3 percentage points more likely to be first-generation students. However, the gross financial need—unadjusted cost of attendance minus expected family contribution—is slightly lower for veterans. Academic background variables are missing for large numbers of students, particularly students who delayed entry more than five years and transfer students. For those with data, mean high school GPA is .13 points lower for veterans, and mean ACT-equivalent scores are about a point lower for math and half a point lower for English.[10]

Veterans also differ from nonveterans on enrollment variables. Nonveterans have higher amounts of each kind of aid than veterans, though it is important to note that GI Bill payments are not included in the aid variables.[11][12] Veterans have, on average, about $750 less in scholarships than nonveterans, but the differences are smaller for grants (about $80 less) and loans (about $150 less).[13][14] Veterans have nearly an additional

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[10] ACT scores range from 1 to 36.

[11] The VA makes GI Bill payments on behalf of the student rather than through traditional aid protocols. These payments are only available for some veterans and only for those who entered in the fall of 2009 or later. I utilize these in chapter 6.

[12] I estimate that a veteran qualifying for the full award amount at GSU would receive $14,448 for the fall 2017 semester.

[13] Scholarships are typically merit-based and grants are typically need-based, while loans require repayment.

[14] GI Bill payments are applied after other forms of aid, so having GI bill aid doesn't prevent veterans from using other types of aid.

semester of transfer hours, but are less likely to be part-time students (both in the first year and throughout the college career). Because of military service, veterans are older than nonveteran students. Mean age at entry for veterans is nearly five years older than for nonveterans.

4.3 Student Success Outcomes for Nonveteran and Veteran Students

I test the second half of the bias explanation by comparing student success outcomes for veterans and nonveterans:

H1b: Veterans have worse student success outcomes than nonveterans

In general, veterans perform at least as well as nonveterans on basic student success outcomes. Table 2 compares mean first year GPA and proportions for retention and graduation for both nonveterans and veterans. Student success outcomes are similar for nonveterans and veterans on nearly every measure.

Table 2. Postsecondary Outcomes for Nonveterans and Veterans

VARIABLES	Nonveterans	Veterans	Difference	
First-Year GPA	2.94	2.84	-0.10	**
Percentage Returning after First-Year	75	76	1	
Percentage Graduating Anywhere				
in Four Years	39	47	8	**
in Five Years	54	57	3	+
in Six Years	61	61	0	
in Seven Years	64	63	-1	
in Eight Years	65	66	1	
Ever	68	69	1	
Percentage Graduating from GSU				
in Four Years	34	41	7	**
in Five Years	46	49	3	*
in Six Years	51	53	2	+
in Seven Years	52	54	2	
in Eight Years	53	56	3	
Ever	54	57	3	*

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

The only measure on which veterans perform worse on is first year GPA, which is .10 points lower for veterans. Veterans are at least as successful as nonveterans on other outcomes. About 70% of nonveterans and veterans return after the first year. Four-year graduation rates are higher for veterans: within four years, 39% of nonveterans graduate from any school but 47% of veterans do. Veterans also have higher four-year graduation rates from GSU (32 percent versus 28 percent). Graduation rates within five years and beyond are similar for nonveterans and veterans, however, both from GSU and from any school.

These results show that veterans are at least as successful as nonveteran students on all outcome measures besides first year GPA. For graduation and retention (but not first year GPA), this seems to rule out the explanation that the veteran paradox stems from upwardly biased results from the past research on veterans. Most of the research on the veteran advantage in grades, though, was completed more than two decades ago. These findings are likely no longer relevant since contemporary veterans are different. Some veterans who were drafted into the military would have otherwise entered college directly. After the introduction of the all-volunteer force in 1974, it’s likely that many of those who were interested in and ready for college simply did not choose enlistment.

4.4 Student Success Outcomes for Nonveteran and Veteran Students While Controlling for Other Factors

I use the primary models in the analysis to test the background characteristics, enrollment patterns, and maturation explanations:

H2: Veterans have worse student success outcomes than nonveterans after controlling for background characteristics

H3: Veterans have worse student success outcomes than nonveterans after controlling for enrollment pattern variables

H4: Veterans have worse student success outcomes than nonveterans after controlling for age

The primary models illustrate the relationships between veteran status and the postsecondary student success outcomes while controlling for background characteristics, enrollment patterns, age at matriculation, and institutional changes over time. For each model, I present the average partial and average marginal effects. These effects represent the average change in the predicted outcome due to a one-unit increase in the independent variable, controlling for the other variables in the model.[15] In the OLS models for GPA, these are equivalent to the coefficients unless there is a squared term, in which case the average marginal effect is computed using both terms. In the logit models, these are the changes in the predicted probabilities for retention and graduation. For the continuous independent variables that are treated as dummy variables (age and time), I present the predicted GPA and predicted probabilities of retention and graduation for each value. The discussion focuses on the average partial/marginal effects and the predicted outcomes but the regression coefficients for the full models can be found in the appendix.

[15] Average marginal effects apply to continuous independent variables while average partial effects apply to binary independent variables. The interpretation is the same for both, although computation varies slightly.

4.4.1 First Year GPA

Student veterans have lower first-year grade point averages than comparable nonveteran students (table 3). The predicted first year GPA for veterans is 0.16 points lower than for nonveteran students with the same characteristics. Surprisingly, this is even larger than the bivariate difference of means reported in the previous section (average first year GPA for veterans is 0.10 points lower than for nonveterans). There is a veteran disadvantage in first year grades that widens after controlling for background characteristics, enrollment patterns, and age.

Table 3. First Year GPA, Average Partial & Marginal Effects

VARIABLES	
Veteran	-0.155**
	(0.017)
Female	0.089**
	(0.005)
Race	
Black	-0.270**
	(0.007)
Hispanic	-0.087**
	(0.010)
Asian	-0.098**
	(0.009)
Other/Biracial	-0.151**
	(0.014)
No Race Given	-0.100**
	(0.013)
Mean ZIP Income (in $10,000s)	0.008**
	(0.001)
High School GPA	0.513**
	(0.010)
AP Credit	0.023**
	(0.001)
ACT Math Score Equivalent	0.013**
	(0.001)
ACT English Score Equivalent	0.008**
	(0.001)
Grants (first-semester, in $1,000s)	-0.037**
	(0.002)
Loans (first-semester, in $1,000s)	-0.024**
	(0.001)
Scholarships (first-semester, in $1,000s)	0.092**
	(0.002)
Transfer Hours	0.003**
	(0.000)
Part-Time Student (first-year)	-0.328**
	(0.006)

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1
Calculations for academic background variables exclude students with imputed values

The effects of the background variables in the model generally align with predictions from theory. Females have a predicted GPA nearly a tenth of a point higher than similar males. The predicted first year GPA is lower for all minority groups than for comparable white students. This gap is largest for blacks (.27 points below comparable white students), but still notable for students who are other or biracial (.15 lower), unwilling to provide a race (.10 lower), Asian (.10 lower), or Hispanic (.09 lower). It is surprising that even Asians, who typically are more successful students, have lower grades than comparable whites.

Mean income is positively related to first year GPA, but the effect is smaller than expected. An additional $10,000 in mean ZIP code income increases predicted GPA by slightly less than a hundredth of a point for comparable students. Students with stronger academic backgrounds have higher grades in the first year. High school grades have the strongest impact: having a high school GPA an additional letter grade higher increases predicted first year GPA by half a letter grade. An additional hour of AP credit increases predicted first year GPA by about .02 points and an additional point on either the math or English section of the ACT increases it by about .01 points.

The effects of enrollment variables are generally aligned with expectations. An additional $1,000 in scholarships increases predicted GPA by nearly a tenth of a point, but the same amount in grants or loans decreases it by .04 and .02, respectively. An additional hour of transfer credit improves predicted GPA but the effect is very small (.003). Enrollment intensity, however, has a substantial impact. Part time students have a predicted GPA a third of a letter grade lower than comparable full-time students.

The expected first year GPA is higher for students who are older or slightly younger than for comparable 18 year olds (the reference group). Figure 1 shows the predicted First Year GPA for comparable students at each matriculation age. Although the advantages are relatively modest for the ages near 18, the advantage grows quickly with age: those who enter at age 30 have an expected GPA half a letter grade higher than those who begin at 18 with the same characteristics. The advantage is at least as high for almost every age until 65, when the advantage begins to fizzle out.

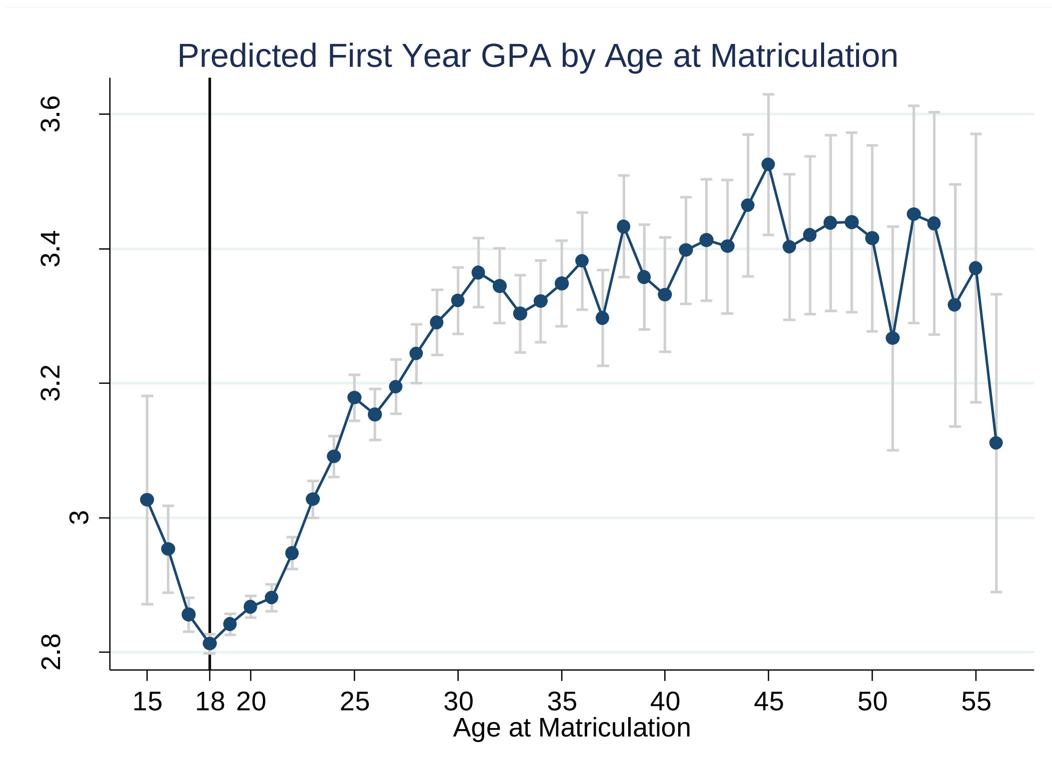

Figure 1. Predicted First Year GPA by Age at Matriculation

First year GPA for comparable students has been generally increasing over time. Figure 2 shows predicted first year GPA for each term of entry. Expected first year GPA only shows a slight upward trend from the fall of 2003 through the fall semester of 2007;

but students who enter GSU in or after the spring 2008 semester have significantly higher expected grades than comparable students who entered in the fall of 2003 (the reference group). A student who entered in the fall of 2013 has an expected first year GPA .21 points higher than a similar student who started a decade earlier. This pattern is consistent with grade inflation, which has been noted at universities more generally over the past three decades (Rojstaczer and Healy, 2012). Binder, Ganderton, and Hutchens (2002) also suggest that state merit-based aid programs can lead to collegiate grade inflation, though Henry and Rubenstein (2002) find no evidence of merit aid causing high school grade inflation and the mechanisms should be similar in both high schools and colleges. On the other hand, better advising at GSU could better match students with appropriate courses, which could improve success.

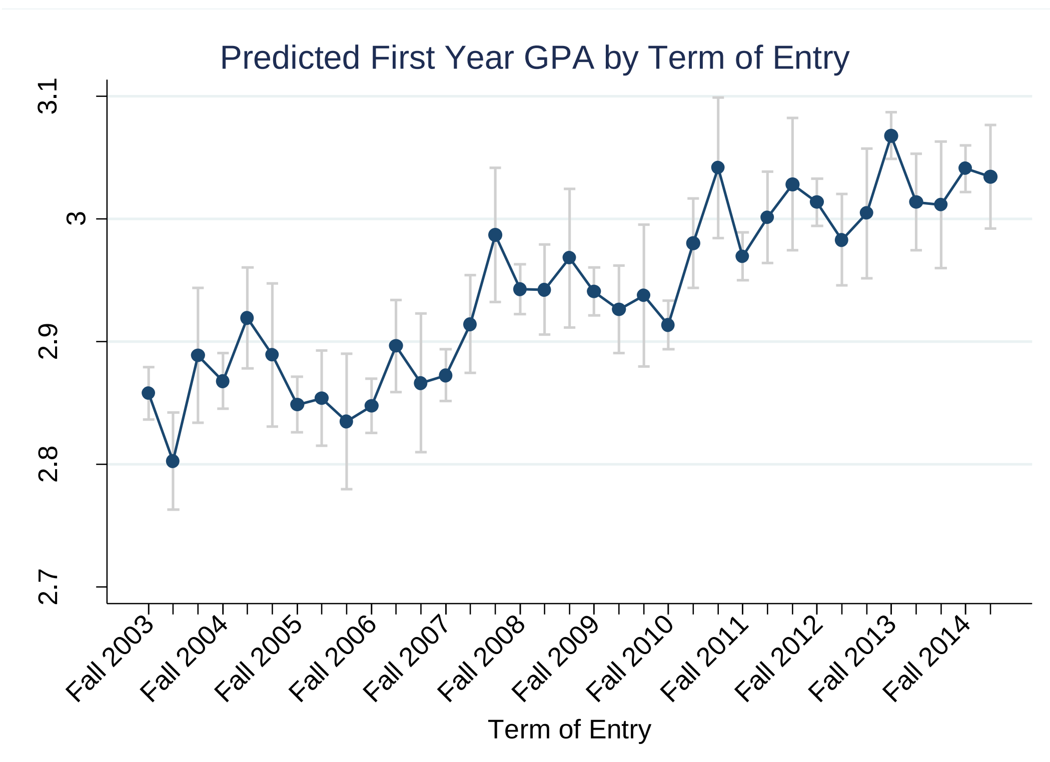

Figure 2. Predicted First Year GPA by Term of Entry

4.4.2 First Year Retention

Veterans are more likely to return after the first year than comparable nonveterans (table 4). The predicted probability of continuing past the first year is 3.6 percentage points higher for veterans than for similar nonveterans. This is in sharp contrast to the bivariate differences reported in the previous section, which showed that veterans and nonveterans have virtually the same first year retention rates. There is a veteran advantage in retention even after controlling for background characteristics, enrollment patterns, and age.

Table 4. First-Year Retention, Average Partial & Marginal Effects

VARIABLES	
Veteran	3.6**
	(0.9)
Female	-0.6*
	(0.3)
Race	
Black	3.3**
	(0.4)
Hispanic	5.3**
	(0.6)
Asian	8.8**
	(0.5)
Other/Biracial	3.9**
	(0.8)
No Race Given	-8.1**
	(0.7)
Mean ZIP Income (in $10,000s)	0.4**
	(0.1)
High School GPA	-2.0**
	(0.5)
AP Credit	1.1**
	(0.1)
ACT Math Score Equivalent	0.2**
	(0.0)
ACT English Score Equivalent	-0.5**
	(0.0)
Grants (first-semester, in $1,000s)	1.2**
	(0.1)
Loans (first-semester, in $1,000s)	0.3**
	(0.1)
Scholarships (first-semester, in $1,000s)	3.6**
	(0.1)
Transfer Hours	0.1**
	(0.0)
Part-Time Student (first-year)	-8.1**
	(0.4)

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1
Calculations for academic background variables
exclude students with imputed values

Some of the background variables in the retention model run counter to expectations. Women have a slightly lower probability of returning after the first year than similar men, but the difference is only about half of a percentage point. Surprisingly, all minority groups are more likely to return after year one than comparable whites. Compared with similar whites, the predicted probability of returning after the first year is 5.3 percentage points higher for Hispanics, 3.9 percentage points higher for those of other and mixed races, and 3.3 percentage points higher for blacks. As expected, though, the advantage is largest for Asians (8.8 percentage points higher). Those who did not provide a race were 8.1 percentage points less likely to return after the first year than similar white students. One possible explanation for these findings is that white students are transferring rather than dropping out. As discussed later, graduation rates from any school are similar for comparable white and minority students.

An extra $10,000 in mean income in one's ZIP code only increases the probability of returning by about half a percentage point for equivalent students. Having a high school GPA a letter grade higher decreases the probability of returning for similar students by 2 percentage points. An additional hour of AP credit increases the predicted retention probability by just over a percentage point. Test scores have divergent, but very small effects: an extra point on the ACT math section increases the predicted retention probability by .2 percentage points but an extra point on the English section decreases it by .5 percentage points.

Enrollment variables have notable, but unsurprising impacts. All forms of aid increase the probability of returning for the second year, but an extra $1,000 has the largest effect as scholarships (3.6 percentage points), followed by grants (1.2 percentage point) and loans (0.3 percentage points). An additional hour of transfer credit increases the probability of retention but only by .01 percentage points. Students who enroll part time at any point in their first year are 8 percentage points less likely to return for the second than similar full-time students.

Surprisingly, the probability of coming back for the second year varies only slightly across age groups. Figure 3 shows the predicted retention probability for each age at entry. Entrants who begin at 17 or 19 are less likely to return than comparable students who start as 18 year olds, as are those who enter in their mid- to late-twenties or mid-thirties. Aside from these groups, retention is generally no more or less likely for other older students.

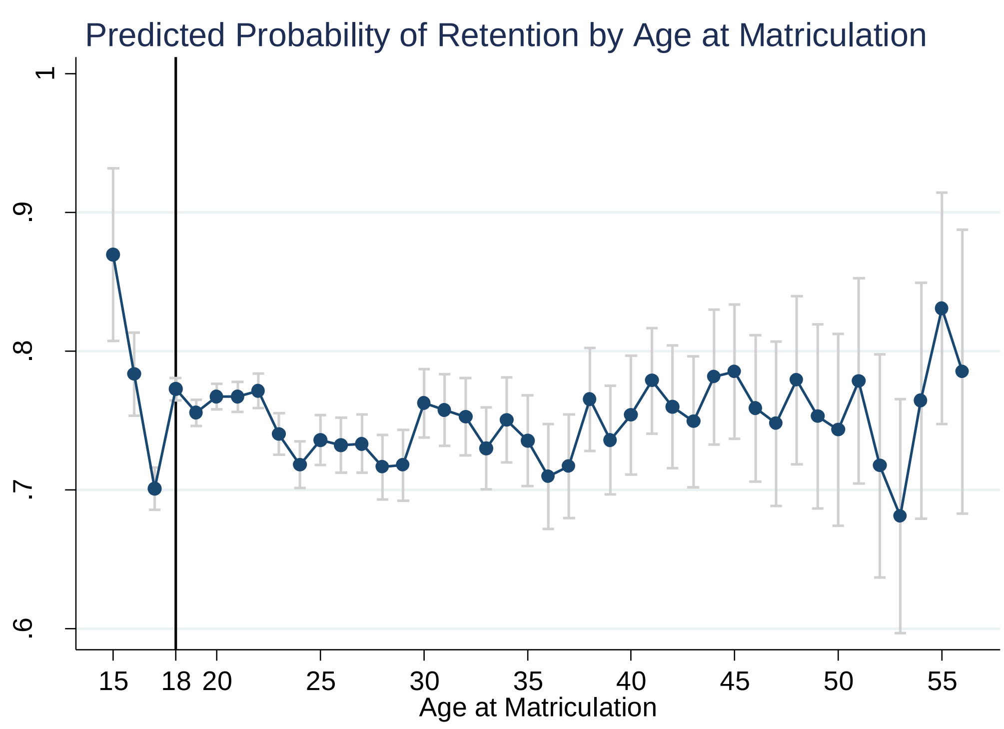

Figure 3. Predicted Probability of Retention by Age at Matriculation

Across entry terms, the likelihood of returning after the first year shows no clear trend. Figure 4 shows the predicted retention probability by term of entry. Compared with the fall of 2003 (the reference term) the probability of returning is lower in subsequent terms through the spring of 2005 and is generally lower from the fall of 2013 onward. It is surprising that retention rates for comparable students have been stagnant or falling since graduation rates at GSU have dramatically increased over the past decade. One possible explanation is that the GSU student body has improved in quality; if this were the case, there should be no relationship between time and retention (or graduation) when controlling for student quality.

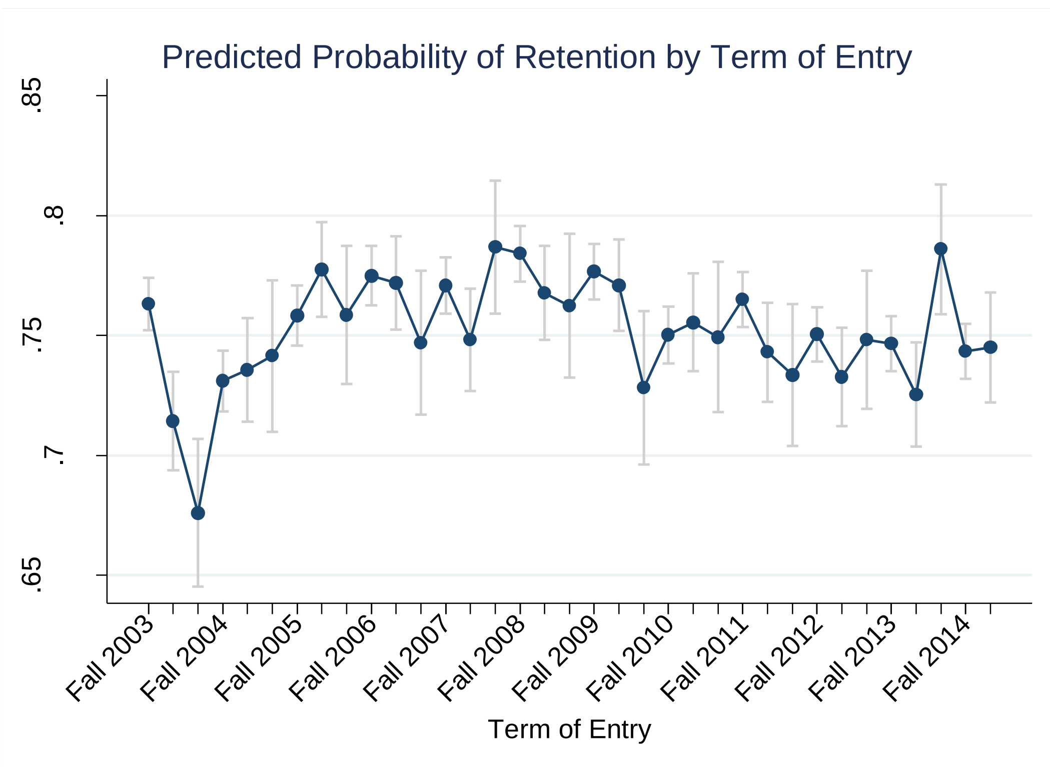

Figure 4. Predicted Probability of Retention by Term of Entry

4.4.3 Degree Completion

Student veterans are more likely to graduate than similar nonveteran peers (table 5). The probability of graduating is higher for veterans than for comparable nonveterans in every time-to-graduation specification and the magnitude increases as the time horizon expands. In the four-, five-, and six-year graduation models, the probability of graduating is about 5 percentage points higher for veteran students than for similar nonveteran students. Veterans are 6 percentage points more likely to graduate within seven years, and slightly less than 8 percentage points more like to graduate within 8 years or to ever graduate within the study period. The differences between veterans and similar nonveterans here are much larger than bivariate graduation differences reported in the previous section. There is a clear veteran advantage in graduation even after controlling for background characteristics, enrollment patterns, and age.

Table 5. Graduation, Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.5**	4.9**	5.4**	6.0**	7.8**	7.5**
	(1.1)	(1.3)	(1.3)	(1.4)	(1.4)	(1.1)
Female	3.1**	3.4**	3.4**	3.2**	3.4**	3.8**
	(0.3)	(0.4)	(0.4)	(0.4)	(0.5)	(0.4)
Race						
Black	1.0*	0.5	0.5	1.0+	0.6	0.7
	(0.4)	(0.5)	(0.5)	(0.6)	(0.6)	(0.5)
Hispanic	-0.5	-0.4	-0.1	0.2	0.2	1.4+
	(0.7)	(0.8)	(0.8)	(0.9)	(1.0)	(0.8)
Asian	3.2**	4.0**	4.7**	5.2**	5.0**	4.2**
	(0.6)	(0.7)	(0.7)	(0.7)	(0.8)	(0.7)
Other/Biracial	-0.8	-2.9**	-0.6	-0.2	-0.6	-0.8
	(0.9)	(1.1)	(1.2)	(1.3)	(1.4)	(1.1)
No Race Given	-2.1**	-4.1**	-5.4**	-5.7**	-6.3**	-7.3**
	(0.8)	(0.9)	(0.9)	(1.0)	(1.0)	(0.9)
Mean ZIP Income (in $10,000s)	0.3**	0.6**	0.6**	0.7**	0.7**	0.5**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
High School GPA	2.6**	5.2**	5.4**	5.2**	5.2**	3.7**
	(0.7)	(0.8)	(0.8)	(0.8)	(0.9)	(0.7)
AP Credit	0.9**	0.7**	0.6**	0.7**	0.8**	0.6**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)
ACT Math Score Equivalent	-0.2**	0.1+	0.2**	0.2*	0.2*	0.3**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
ACT English Score Equivalent	-0.4**	-0.7**	-0.7**	-0.7**	-0.6**	-0.6**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)

Table 5 (continued)

Grants (first-semester, in $1,000s)	-0.3+	-0.0	0.4+	0.5*	0.6*	0.4+
	(0.2)	(0.2)	(0.2)	(0.2)	(0.3)	(0.2)
Loans (first-semester, in $1,000s)	-0.2*	-0.2+	-0.0	-0.0	0.2	-0.1
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
Scholarships (first-semester, in $1,000s)	1.9**	2.1**	2.0**	2.1**	2.1**	2.0**
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)
Transfer Hours	0.5**	0.5**	0.4**	0.3**	0.3**	0.3**
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Part-Time Student (ever)	-19.7**	-15.0**	-10.0**	-7.0**	-5.6**	-4.1**
	(0.3)	(0.4)	(0.4)	(0.4)	(0.5)	(0.4)
First-Semester GPA	14.8**	17.2**	16.5**	15.4**	14.5**	13.7**
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)

Standard errors in parentheses

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

Calculations for academic background variables exclude students with imputed values

Some background variables have notable effects, mostly in line with expectations based on past research. Female students are 3 to 4 percentage points more likely to graduate than comparable males. Asians are also more likely to graduate than comparable whites in every model: the probability of graduating is between 3 and 5.5 percentage points higher than for similar white students. Although black students are slightly more likely to graduate in the four-year model, white, black, Hispanic, and other/biracial students have essentially the same graduation probabilities. This is consistent with past research that shows the effects of race on graduate either shrink substantially or disappear completely when controlling for factors like academic background and socioeconomic status (Kao and Thompson, 2003; Adelman, 1999, 2006). Higher SES students have a higher likelihood of graduating: an extra $10,000 in mean income in the ZIP code increases the predicted probability of graduating by about half a percentage point.

The academic background variables are all significantly associated with graduation, but the effects are largest for high school GPA. For comparable students, having a GPA an additional letter grade higher increases the predicted probability of graduating by 2.5 to 5.5 percentage points. An extra AP credit hour increases the probability of graduating by slightly less than a percentage point. For similar students, an extra point on the ACT subscore increases the probability of graduating by 0.2 percentage points for math and decreases it by 0.6 percentage points for English.

Enrollment variables have large impacts that are consistent with past research. Students who enroll part-time for at least one non-summer semester are much less likely to graduate than full-time students. However, the size of the effect shrinks dramatically as the time horizon increases. Compared with similar full-time students, the probability of graduating for a part-time student is 20 percentage points lower after four years, but is only 6 percentage points lower after eight years. This suggests that, to some extent, part-time students merely take longer to graduate rather than being less likely to do so. Grants and loans have virtually no impact on graduation for similar students, although grants appear to have a small, positive effect on the probability of graduating within seven or eight years. However, an extra $1,000 in scholarships increases the probability of graduating by about 2 percentage points. An additional transfer hour increases the probability of graduating by between a quarter and half of a percentage point, with the effect decreasing as the time horizon increases. Having a first semester GPA a letter grade higher increases the predicted probability of graduating 14 to 17 percentage points.

Older students are less likely to graduate than similar students who begin college at (or shortly after) age 18. Figure 5 shows the predicted probability of graduation by age of entry. Only college students who enter between ages 19 and 23 are more likely to graduate within four years than comparable students who enter at 18. However, over longer time spans older students are less likely to graduate. Compared with similar 18 year olds, the probability of graduating is lower for each age from 22 through 55, and frequently thereafter. As the time horizon expands, the probability curve shifts upward but the pattern remains the same.

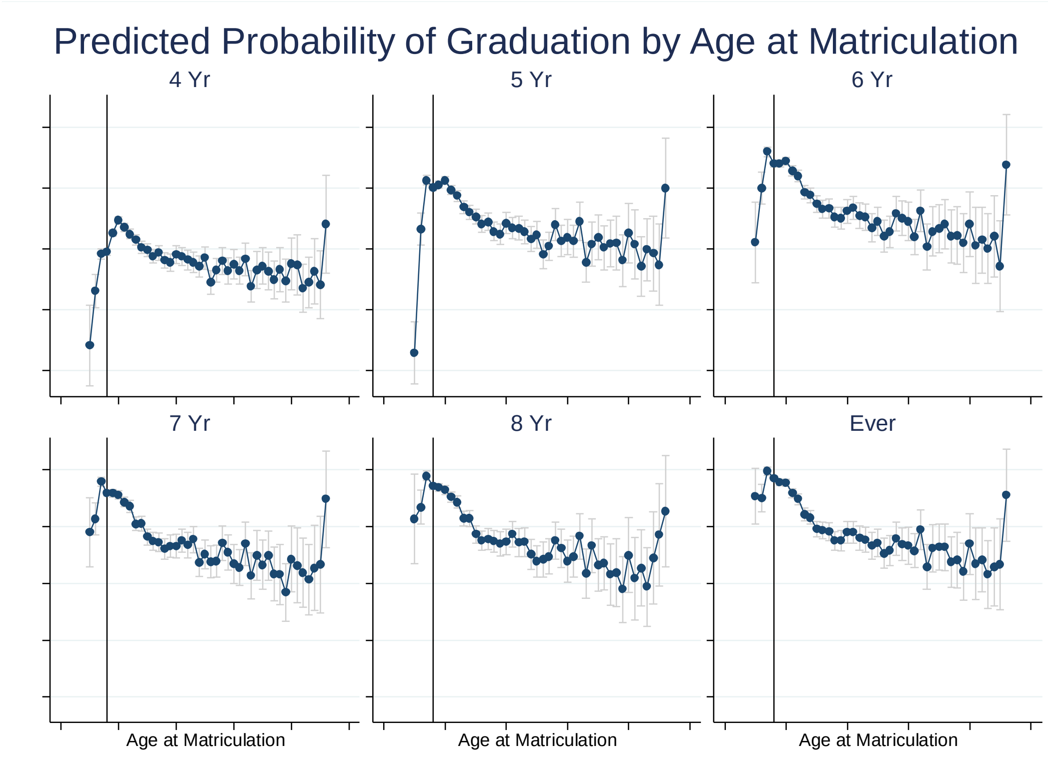

Figure 5. Predicted Probability of Graduating by Age at Matriculation

Across time, the probability of graduating has remained stagnant for similar students. Figure 6 shows the predicted probability of graduation by term of matriculation. Compared with students who entered in the fall of 2003, the likelihood of graduation is lower for students in some later spring and summer entry terms but there is no evidence that graduation rates for comparable students are improving over time. This is also surprising since graduation rates at GSU have been increasing substantially over the past decade. Again, this might be explained by increasing student quality. Another possibility is that GSU has only made progress in graduation rates for first time, full time students who begin in the fall, as these are the students included in those calculations.

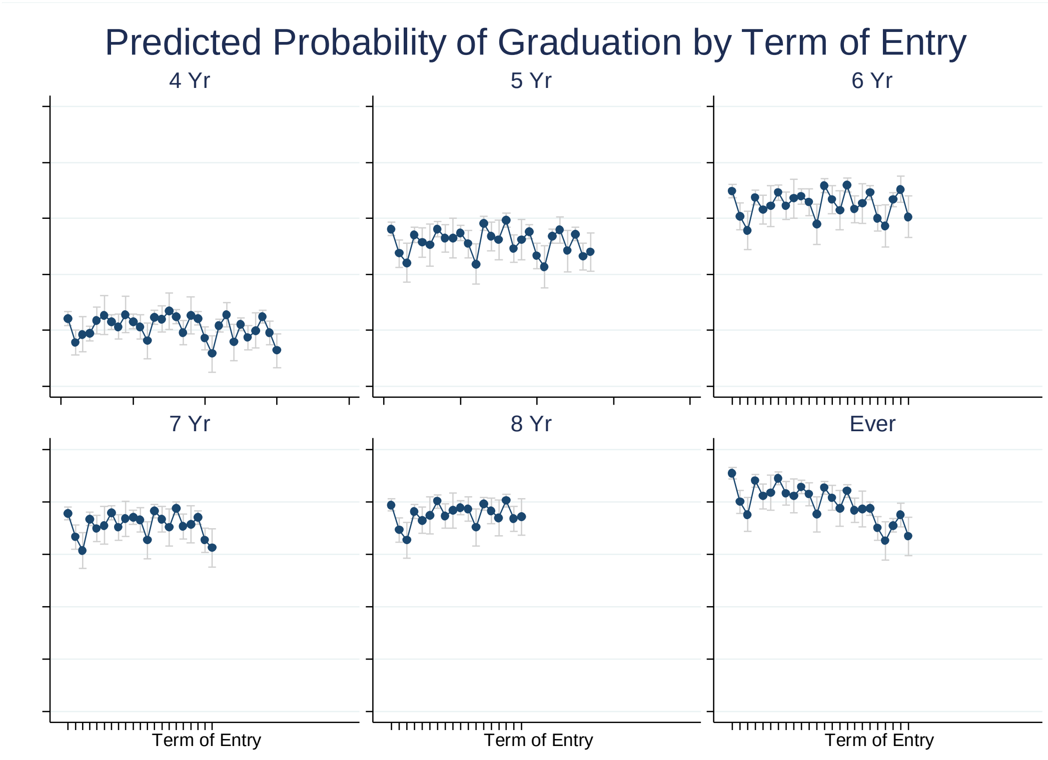

Figure 6. Predicted Probability of Graduation by Term of Entry

4.4.4 Discussion

These models show the relationships between student success outcomes and veteran status while controlling for background characteristics, enrollment patterns, age at matriculation, and institutional changes over time. Even though veterans have lower first year grades than similar nonveterans, they are more likely than comparable nonveterans to return after the first year and to eventually graduate. The surprising finding that older students have higher first year grades explains why the gap between veterans and nonveterans widens when controlling for age of entry, as does the fact that veterans are more likely to enroll full-time. Differences in background characteristics cannot explain veterans' retention and graduation advantage, since that advantage widens when controlling for these characteristics. The enrollment behaviors of veterans, however, do help explain the student veteran paradox. Part of veterans' advantage in retention and graduation is that, at least at GSU, they are more likely to enroll full time and are more likely to begin with transfer credit. However, the veteran advantage still persists after controlling for these factors. This suggests that student veteran paradox is due, at least in part, to military education benefits, unobserved factors associated with enlistment, or the direct effects of military service. These explanations are the subject of the next chapter.

4.5 Variations on Primary Models

In recent years, social scientists have been criticized for publishing statistically significant results that cannot be replicated. At best, this is a result of the many decisions researchers make regarding statistical tests, variable specifications, model specifications, and underlying assumptions. To allay criticisms of this kind, I construct additional variations of the primary models to provide additional evidence for robustness and assumption legitimacy. These model variations use alternate variable specifications and differing sample restrictions. Although these models typically use the same control variables as the primary models, the tables that follow present only the relevant subsets of effects and coefficients.

4.5.1 Alternate Specification: Graduation Site

In the above graduation models, the dependent variables are graduation from any university. This includes Georgia State University and any other college that shares data with the National Student Clearinghouse. Tables 6 a and b show a comparison of relevant results from models that use graduation from GSU (6a) and graduation from anywhere (6b) as the dependent variables. Since veterans are more likely to graduate elsewhere than nonveteran students, graduation models that use GSU graduation as the dependent variable show somewhat weaker coefficients. The overall patterns for veterans are still the same and the average partial effect sizes are almost exactly the same in both specifications, though the effect is slightly smaller for GSU graduation in the four-year model.

Table 6a. Graduation from GSU, Average Partial Effect

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	3.3**	4.6**	5.7**	6.2**	7.2**	7.6**
	(1.1)	(1.3)	(1.4)	(1.6)	(1.7)	(1.4)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 6b. Graduation from Anywhere, Average Partial Effect

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.5**	4.9**	5.4**	6.0**	7.8**	7.5**
	(1.1)	(1.3)	(1.3)	(1.4)	(1.4)	(1.1)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

4.5.2 Alternate Specification: Early College GPA

The graduation models include first semester GPA as an independent variable, but past research has typically used first year GPA instead. First semester GPA was chosen over first year GPA to diminish endogeneity problems and to preserve large sample sizes (as those who do not complete an entire academic year will not have a first year GPA). Tables 7 a and b show the relevant results of versions of graduation models that use first year GPA (7a) and first semester GPA (7b). Graduation models that use first year GPA in place of first semester GPA show the same patterns, although the effects for veteran status are slightly weaker in some time horizons when using first year GPA.

Table 7a. Graduation from Anywhere, Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.2**	4.0**	4.2**	4.7**	6.1**	6.2**
	(1.2)	(1.3)	(1.3)	(1.3)	(1.4)	(1.1)
First-Year GPA	20.9**	22.5**	20.8**	19.4**	18.0**	16.8**
	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.2)
Observations	58,375	51,700	45,232	38,866	32,498	45,229

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, age at matriculation, term of entry.

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

Table 7b. Graduation from Anywhere, Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.5**	4.9**	5.4**	6.0**	7.8**	7.5**
	(1.1)	(1.3)	(1.3)	(1.4)	(1.4)	(1.1)
First-Semester GPA	14.8**	17.2**	16.5**	15.4**	14.5**	13.7**
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, other/biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, age at matriculation, term of entry.

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

4.5.3 Alternate Specification: Socioeconomic Status

In the models presented in the previous section, socioeconomic status is operationalized as mean income by ZIP code (and a squared term). This specification was chosen as other measures of SES are only available for students who filed the FAFSA. Even though 80% of all students at GSU (and 81% of veteran students) filed in at least one academic year, excluding those who did not file notably reduces the sample size. More importantly, FAFSA-filers are less likely to graduate than non-filers, so excluding the latter group risks biasing the results. Moreover, veterans who have full funding from the GI Bill may be less likely to file the FAFSA and possibly more likely to succeed.

Table 8 shows the relevant results of the first year GPA model with varying specifications of SES. The effects are virtually the same across all models. As such, I do not include a correction method for selection among FAFSA filers for GPA.

Table 8. First Year GPA with Alternate SES Specifications, Average Partial & Marginal Effects

VARIABLES	(1) FAFSA vars	(2) income vars (FAFSA filers)	(3) income vars (all students)
Veteran	-0.153**	-0.149**	-0.155**
	(0.019)	(0.018)	(0.017)
1st Gen Status Unknown	-0.046**		
	(0.009)		
1st Generation	-0.008		
	(0.007)		
Gross Financial Need (in $1,000s)	0.000		
	(0.001)		
Mean ZIP Income (in $10,000s)		0.022**	0.018**
		(0.004)	(0.004)
Observations	57,184	63,462	70,893

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 9 shows the relevant results of the retention model with varying specifications of SES. Although the effects display the same patterns, their magnitude is slightly different across the first three models. Thus, I also include a fourth model that uses the Heckman selection method to correct for the selection bias among FAFSA filers. The average partial effects are fairly consistent across models. Compared with nonveterans who hold similar characteristics, the probability of returning after the first year is between 2.5 and 3.6 percentage points higher for veterans.

Table 9. First Year Retention with Alternate SES Specifications, Average Partial & Marginal Effects

VARIABLES	(1) FAFSA vars	(2) income vars (FAFSA filers)	(3) income vars (all students)	(4) FAFSA vars (Heckman Correction)
Veteran	2.5*	3.3**	3.6**	2.9**
	(1.0)	(0.9)	(0.9)	(1.1)
1st Gen Status Unknown	-1.9**			-1.8**
	(0.5)			(0.5)
1st Generation	-3.0**			-2.9**
	(0.4)			(0.4)
Gross Financial Need (in $1,000s)	-0.2**			-0.2**
	(0.0)			(0.1)
Mean ZIP Income (in $10,000s)		1.3**	1.2**	
		(0.2)	(0.2)	
Observations	68,345	75,502	86,416	67,297

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Tables 10 a, b, c, and d show relevant results from versions of the graduation models with varying specifications of SES. Although the effects of the veteran variables differ between models that use mean ZIP income and the FAFSA variables, the model that uses mean ZIP income with an equivalent sample (i.e., only FAFSA filers) looks quite similar to the model that uses gross need and parental education. These models show slightly weaker effects for the veteran variable. Using the Heckman selection method leads to similar, though slightly weaker, average partial effects as the primary set of graduation models. Depending on the time horizon, the probability of graduating is between 4 and 8 percentage points higher for a veteran than a comparable nonveteran. Overall, the consistency in results suggests that using mean ZIP income in the primary models is a justifiable specification for use with the full sample.

Table 10a. Graduation Using FAFSA variables (FAFSA filers only), Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	3.4**	3.7*	3.9*	4.1*	6.5**	5.7**
	(1.3)	(1.5)	(1.5)	(1.6)	(1.7)	(1.3)
1st Gen Status Unknown	-1.7**	-2.7**	-2.9**	-2.3**	-2.7**	-1.5*
	(0.6)	(0.7)	(0.7)	(0.8)	(0.8)	(0.7)
1st Generation	-0.2	-2.2**	-2.8**	-2.2**	-2.2**	-2.7**
	(0.5)	(0.5)	(0.5)	(0.6)	(0.6)	(0.5)
Gross Financial Need (in $1,000s)	-0.3**	-0.3**	-0.3**	-0.2**	-0.2+	-0.2**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
Observations	50,259	43,680	37,393	31,381	25,546	37,393

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 10b. Graduation using mean income variables (FAFSA filers only), Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.2**	4.0**	4.5**	4.6**	6.6**	6.4**
	(1.2)	(1.4)	(1.4)	(1.5)	(1.6)	(1.2)
Mean ZIP Income (in $10,000s)	0.6*	1.1**	1.1**	1.5**	1.1**	1.0**
	(0.3)	(0.3)	(0.3)	(0.4)	(0.4)	(0.3)
Observations	56,595	49,671	43,021	36,551	30,248	43,018

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 10c. Graduation using mean income variables (all students), Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	4.5**	4.9**	5.4**	6.0**	7.8**	7.5**
	(1.1)	(1.3)	(1.3)	(1.4)	(1.4)	(1.1)
Mean ZIP Income (in $10,000s)	0.8**	1.5**	1.7**	1.8**	1.6**	1.4**
	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 10d. Graduation using FAFSA variables with Heckman Correction, Average Partial & Marginal Effects

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	3.6**	3.5*	3.6*	4.0*	6.4**	5.7**
	(1.3)	(1.5)	(1.6)	(1.7)	(1.9)	(1.4)
1st Gen Status Unknown	-1.9**	-2.8**	-3.0**	-2.4**	-2.5**	-1.5*
	(0.6)	(0.7)	(0.7)	(0.8)	(0.9)	(0.7)
1st Generation	-0.3	-2.2**	-3.0**	-2.4**	-2.4**	-2.8**
	(0.5)	(0.5)	(0.6)	(0.6)	(0.7)	(0.5)
Gross Financial Need (in $1,000s)	-0.3**	-0.3**	-0.3**	-0.2*	-0.2	-0.2**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
Observations	49,448	42,935	36,716	30,775	25,087	36,716

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

4.5.4 Missing Data and Imputation

Missing data on academic background variables presents a substantial challenge in this research. Transfer students are not required to provide high school GPA or standardized scores when applying to GSU. Students who have been out of high school for more than five years but have fewer than 30 transfer hours are required to take the Compass exam to determine remedial course placement[16], but high school GPA is not required. Thus, many students are missing values for these academic background variables. As a result, using listwise deletion for missing values on the academic background variables in the above models excludes more than half of the initial sample and also introduces bias into the results.

Tables 11, 12, and 13 a-f show the coefficients on key variables in models with varying academic background measures, sample equivalencies, and imputation methods. Because of challenges in computing average marginal and average partial effects with multiply imputed data, these tables all report regression coefficients. In each table, model 1 shows the results of including academic background using listwise deletion for the missing cases. Model 2 excludes high school GPA from the regression and model 3 excludes both high school GPA and standardized test scores. Compared with the primary model from the previous section, the veteran coefficients are attenuated in the listwise deletion model. As the academic background variables are removed sequentially in models 2 and 3, the veteran coefficients strengthen. Yet, when model 3 is repeated with an equivalent sample from model 1 (i.e., model 4 excludes academic background

[16] The Compass exam was replaced by the Accuplacer exam at GSU in 2017, but this change is outside the timeframe for this study.

variables but omits students who have missing values for those variables), the veteran coefficients are very similar to those from model 1 instead of model 3. This demonstrates that excluding those students with missing academic background variables systematically biases the other coefficients in the model. Thus, an imputation method (or alternative solution) is necessary.

Model 5 shows the results of model 1 (i.e., the full model that includes academic background variables) after missing values on the academic background variables have been imputed by multiple imputation using chained equations. Finally, model 6 repeats the primary models from the previous section. These use the crude method of imputing a zero for missing values and adding a dummy variable to indicate that a case was missing said value. Although the multiple imputation method is more theoretically appropriate, the crude imputation method was chosen for the primary models to allow for estimation of the average partial effects to enhance interpretation. However, the results of both the multiple imputation and the crude imputation methods are very similar; this bolsters confidence in the primary models.

Table 11. First Year GPA with Missing Data & Imputation for Academic Background

VARIABLES	(1) Full (no imputation)	(2) No HS GPA	(3) No HS GPA or Scores	(4) No HS GPA or Scores with sample from (1)	(5) Full (MICE)	(6) Full (crude imputation)
Veteran	-0.031	-0.077**	-0.150**	-0.027	-0.138**	-0.155**
	(0.030)	(0.025)	(0.018)	(0.032)	(0.018)	(0.017)
ACT Math Score Equivalent	0.069**	0.096**			0.084**	0.084**
	(0.009)	(0.007)			(0.007)	(0.007)
ACT Math Score Equivalent squared	-0.001**	-0.002**			-0.002**	-0.002**
	(0.000)	(0.000)			(0.000)	(0.000)
ACT English Score Equivalent	0.050**	0.053**			0.058**	0.062**
	(0.007)	(0.006)			(0.005)	(0.006)
ACT English Score Equivalent squared	-0.001**	-0.001**			-0.001**	-0.001**
	(0.000)	(0.000)			(0.000)	(0.000)
High School GPA	-0.946**				-0.150*	-0.951**
	(0.097)				(0.071)	(0.098)
High School GPA squared	0.232**				0.094**	0.223**
	(0.015)				(0.011)	(0.015)
Missing High School GPA						-0.778**
						(0.155)
Missing ACT Math Score Equivalent						0.993**
						(0.082)
Missing ACT English Score Equivalent						0.789**
						(0.063)

Table 11 (continued)

Observations	38,810	49,239	70,893	38,810	70,435	70,893
R-squared	0.276	0.223	0.210	0.207		0.246

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 12. Retention with Missing Data & Imputation for Academic Background

VARIABLES	(1) Full (no imputation)	(2) No HS GPA	(3) No HS GPA or Scores	(4) No HS GPA or Scores with sample from (1)	(5) Full (MICE)	(6) Full (crude imputation)
Veteran	0.387**	0.182*	0.194**	0.384**	0.126+	0.220**
	(0.129)	(0.091)	(0.057)	(0.129)	(0.066)	(0.057)
ACT Math Score Equivalent	0.267**	0.239**			0.189**	0.250**
	(0.032)	(0.024)			(0.025)	(0.023)
ACT Math Score Equivalent squared	-0.006**	-0.005**			-0.004**	-0.005**
	(0.001)	(0.001)			(0.001)	(0.000)
ACT English Score Equivalent	-0.026	0.049**			0.062**	0.068**
	(0.027)	(0.019)			(0.018)	(0.019)
ACT English Score Equivalent squared	-0.000	-0.002**			-0.002**	-0.002**
	(0.001)	(0.000)			(0.000)	(0.000)
High School GPA	-1.533**				0.085	-0.805*
	(0.356)				(0.186)	(0.317)
High School GPA squared	0.212**				-0.035	0.104*
	(0.055)				(0.031)	(0.050)
Missing High School GPA						-1.313**
						(0.499)
Missing ACT Math Score Equivalent						2.314**
						(0.265)
Missing ACT English Score Equivalent						0.531*
						(0.210)

Table 12 (continued)

Observations	46,049	58,233	86,416	46,049	81,391	86,416

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13a. Graduation with Missing Data & Imputation for Academic Background; Full (no imputation)

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.303*	0.124	0.191	0.212	0.407*	0.438**
	(0.134)	(0.131)	(0.146)	(0.168)	(0.195)	(0.156)
High School GPA	-2.649**	-2.779**	-2.235**	-1.818**	-1.415**	-2.068**
	(0.391)	(0.392)	(0.417)	(0.459)	(0.496)	(0.425)
High School GPA squared	0.430**	0.459**	0.383**	0.319**	0.256**	0.351**
	(0.060)	(0.061)	(0.065)	(0.072)	(0.078)	(0.067)
ACT Math Score Equivalent	0.168**	0.150**	0.064	0.047	0.032	0.073+
	(0.039)	(0.037)	(0.040)	(0.043)	(0.048)	(0.042)
ACT Math Score Equivalent squared	-0.004**	-0.003**	-0.001+	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ACT English Score Equivalent	0.006	0.009	0.005	0.016	0.026	0.009
	(0.031)	(0.030)	(0.032)	(0.035)	(0.038)	(0.033)
ACT English Score Equivalent squared	-0.001	-0.001+	-0.001+	-0.001+	-0.002+	-0.001+
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	35,281	31,209	27,392	23,658	19,925	27,389

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13b. Graduation with Missing Data & Imputation for Academic Background; No HS GPA

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.324**	0.239*	0.295*	0.392**	0.594**	0.501**
	(0.102)	(0.103)	(0.114)	(0.132)	(0.155)	(0.120)
ACT Math Score Equivalent	0.195**	0.140**	0.073*	0.049	0.056	0.090**
	(0.029)	(0.029)	(0.031)	(0.034)	(0.038)	(0.032)
ACT Math Score Equivalent squared	-0.004**	-0.003**	-0.001*	-0.001	-0.001	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ACT English Score Equivalent	0.011	0.034	0.045+	0.042	0.053+	0.043+
	(0.022)	(0.022)	(0.024)	(0.026)	(0.028)	(0.024)
ACT English Score Equivalent squared	-0.001+	-0.002**	-0.002**	-0.002**	-0.002**	-0.002**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	44,507	39,376	34,465	29,650	24,894	34,462

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13c. Graduation with Missing Data & Imputation for Academic Background; No HS GPA or Test Scores

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.246**	0.244**	0.288**	0.336**	0.466**	0.464**
	(0.065)	(0.069)	(0.075)	(0.084)	(0.096)	(0.078)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13d. Graduation with Missing Data & Imputation for Academic Background; No HS GPA or Test Scores, with sample from full model

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.354*	0.135	0.134	0.146	0.356	0.403*
	(0.152)	(0.146)	(0.163)	(0.187)	(0.216)	(0.173)
Observations	29,803	26,318	23,096	19,952	16,818	23,104

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13e. Graduation with Missing Data & Imputation for Academic Background; Full (MICE)

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.245**	0.251**	0.301**	0.348**	0.478**	0.520**
	(0.066)	(0.069)	(0.076)	(0.084)	(0.096)	(0.087)
High School GPA	-0.778**	-1.013**	-0.986**	-0.924**	-0.834**	-0.788**
	(0.195)	(0.200)	(0.221)	(0.242)	(0.282)	(0.241)
High School GPA squared	0.134**	0.179**	0.178**	0.168**	0.154**	0.143**
	(0.032)	(0.034)	(0.037)	(0.040)	(0.047)	(0.040)
ACT Math Equivalent	0.170**	0.127**	0.080**	0.060*	0.062+	0.075*
	(0.026)	(0.026)	(0.028)	(0.030)	(0.034)	(0.030)
ACT Math Equivalent squared	-0.004**	-0.003**	-0.001*	-0.001	-0.001	-0.001+
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ACT English Equivalent	0.005	0.021	0.024	0.022	0.016	0.006
	(0.020)	(0.020)	(0.021)	(0.023)	(0.025)	(0.024)
ACT English Equivalent squared	-0.001	-0.001**	-0.001**	-0.001*	-0.001+	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	65,125	57,731	50,611	43,601	36,660	43,601

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

Table 13f. Graduation with Missing Data & Imputation for Academic Background; Full (Crude Imputation)

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.262**	0.263**	0.306**	0.352**	0.482**	0.481**
	(0.066)	(0.069)	(0.075)	(0.084)	(0.096)	(0.078)
Missing High School GPA	-4.967**	-4.032**	-3.212**	-2.389**	-1.660*	-2.643**
	(0.564)	(0.556)	(0.590)	(0.643)	(0.692)	(0.594)
High School GPA	-3.360**	-2.908**	-2.430**	-1.931**	-1.476**	-2.021**
	(0.355)	(0.353)	(0.377)	(0.413)	(0.446)	(0.383)
High School GPA squared	0.533**	0.485**	0.420**	0.344**	0.276**	0.349**
	(0.055)	(0.055)	(0.059)	(0.065)	(0.071)	(0.061)
Missing ACT Math Score Equivalent	1.889**	1.584**	0.991**	0.753*	0.852*	1.188**
	(0.313)	(0.312)	(0.336)	(0.367)	(0.404)	(0.348)
ACT Math Score Equivalent	0.216**	0.172**	0.113**	0.092**	0.097**	0.127**
	(0.027)	(0.027)	(0.029)	(0.032)	(0.035)	(0.030)
ACT Math Score Equivalent squared	-0.005**	-0.004**	-0.002**	-0.002**	-0.002*	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Missing ACT English Score Equivalent	0.005	0.203	0.262	0.204	0.247	0.250
	(0.240)	(0.243)	(0.261)	(0.285)	(0.311)	(0.268)
ACT English Score Equivalent	0.020	0.053*	0.061**	0.055*	0.060*	0.053*
	(0.021)	(0.022)	(0.023)	(0.025)	(0.028)	(0.024)
ACT English Score Equivalent squared	-0.001*	-0.002**	-0.002**	-0.002**	-0.002**	-0.002**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, AP credit, AP credit squared, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA, age at matriculation, term of entry.

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

4.6 Conclusion

Student veterans at GSU hold characteristics that predict they will be less successful students. Yet, veterans are at least as successful as nonveteran students at GSU on all measures besides first year GPA. At least for retention and graduation, these findings corroborate the student veteran paradox and rule out the bias explanation. When controlling for background characteristics, enrollment behaviors, age of entry, and institutional changes over time, veterans have lower grades after the first year, but are more likely to return in the second year and to eventually graduate. Veterans have lower mean first-year grades than nonveterans, but this is partly because veterans are older and, surprisingly, older students earn higher grades. The veteran advantage in retention and graduation persists even after controlling for background characteristics and age of entry, though some of the advantage is due to higher full time enrollment and more transfer credit. Enrollment patterns help explain the veteran advantage in retention and graduation, but limitations in past research, background characteristics, and age at matriculation do not.

Controlling for enrollment patterns does not completely explain the student veteran paradox for retention and graduation. Compared with similar nonveterans, the probability of continuing past the first year is still 4 percentage points higher for a veteran and the probability of graduating is between 5 and 9 percentage points higher for a veteran. This suggests that the veteran advantage is also due to GI Bill aid, unobserved factors associated with selection, or the direct effects of military service. In the next chapter, I report the results of the secondary analyses that build on the primary models and test these explanations for the veteran paradox.

CHAPTER 5

FINDINGS FROM SECONDARY MODELS

5.1 Introduction

This dissertation has presented seven competing explanations that can resolve the student veteran paradox: bias in past research, background characteristics of veterans, enrollment behaviors of veterans, maturation from delayed entry, education aid benefits for veterans, unobservable factors associated with selection into the military, or direct effects of military service. For retention and graduation, the previous chapter ruled out the bias, background, and maturation explanations, but found support for the enrollment explanation. Even after controlling for enrollment patterns like part-time status and transfer hours, though, veterans are more likely to return for the second year and are more likely to graduate. This suggests that the veteran paradox is, in part, due to at least one of the remaining explanations: the education aid, unobservable factors associated with selection, or direct effects of military service. I examine these explanations in this chapter.

I test the aid explanation with two variations on the primary models. First, I assess outcomes for veterans using different types of GI Bill benefits and for nonveterans using veteran survivor/dependent benefits. Second, I assess outcomes for a subset of veterans who have GI Bill award amount data available. If aid explains the veteran paradox, then outcomes should be better for veterans using the more generous version of the GI Bill, for veteran survivors and dependents, and for veterans with higher GI Bill awards. Although aid explains some of the veteran advantage in retention and graduation, it does not account for all of it. Additionally, aid levels seem to have little impact on first year grades.

I test the selection and direct effects explanations by repeating the primary models after matching veterans to similar nonveterans. The matching approach can compensate for misspecifications in the primary models and can account for selection on the observables. Additionally, it can also account for selection on the unobservables but *only if* the unobservables are correlated with the observables and the correlations do not differ for veterans and nonveterans. These are strong assumptions that are impossible to verify. If selection explains the veteran paradox and the observables can predict the unobservables, then the effects of veteran status on retention and graduation will diminish or vanish after matching. If the direct effects of military service are driving the veteran paradox, then the effects of veteran status will remain or even increase after matching. Veterans still have lower grades than similar matched nonveterans, but the veterans are more likely to return after the first year and are more likely to graduate. The retention and graduation effects are generally slightly higher than those from the primary models in the previous chapter. These results after matching are consistent with the direct effects explanation. However, the selection explanation cannot be ruled out completely.

5.2 Student Success Outcomes for Students with Military Education Benefits

I test the funding explanation with two variations on the primary models:

H5: Students with higher levels of veteran education benefits tend to have better student success outcomes than otherwise similar students

The two approaches used here examine how levels of military education benefits impact student success. The first repeats the primary models but adds two terms to identify veterans using the more generous Post-9/11 GI Bill (those that upgrade and those that use it exclusively). It also includes a dummy variable to identify the survivors and dependents of veterans. The second repeats the primary models with the small subset of veterans who have GI Bill payment amounts. If funding is driving the veteran paradox, then outcomes should be better for veterans using the more generous Post-9/11 GI Bill, for veteran survivors and dependents (who are nonveterans who receive veteran aid without military service), and for veterans with higher amounts of GI Bill payments.

Some caution is warranted for the results of the aid analyses. These two analyses are dividing the already-small veteran population into even smaller subpopulations. Veteran dependents, who were not classified as veterans in the earlier analyses, are also a small group. These small group sizes lead to high standard errors in many cases.[17] Additionally, having certain aid types or levels may be related to other unobserved traits. Veterans who used older forms of the GI Bill were required to make contributions while enlisted, so they may have stronger intentions toward earning a degree. Veterans using the Post-9/11 GI Bill receive a pro-rated award if they serve for less than three years, so higher levels of funding might be associated with perseverance.

[17] The large standard errors stem from the low variance of the independent variables. For example, there are fewer than 150 veterans in the entire dataset who upgrade their GI Bill and fewer than 50 who do so in their first year. Thus, the variance is very low for the convert dummy variables, which causes large standard errors.

5.2.1 Aid Type

I use the primary models but add two new variables to identify veterans who upgraded from the older GI Bill to the newer Post-9/11 GI Bill, and veterans who exclusively use the Post-9/11 GI Bill. These variables show the impact of upgrading to or exclusively using the Post-9/11 GI Bill beyond the effect of being a veteran. I also add a dummy variable for nonveterans who use veteran survivor/dependent benefits. Table 14 shows the results. Since the Post-9/11 GI Bill variables are essentially interaction terms, those coefficients show the effects for those veterans in addition to the effects of the veteran variable.

Table 14. Aid Levels and Student Success, Average Partial Effects

VARIABLES	(1) First Year GPA	(2) First Year Retention	(3) 4 Year Graduation	(4) 5 Year Graduation	(5) 6 Year Graduation
Veteran	-0.167**	3.6**	4.7**	4.8**	5.6**
	(0.025)	(1.3)	(1.6)	(1.7)	(1.6)
Only Uses Post-9/11 GI Bill	0.030	-0.0	-0.3	-1.5	-4.1
	(0.034)	(1.9)	(2.3)	(2.8)	(3.3)
Converts to Post-9/11 GI Bill (first year)	-0.333*	8.6			
	(0.148)	(8.0)			
Converts to Post-9/11 GI Bill (ever)			0.3	7.6+	8.5*
			(4.0)	(4.3)	(4.2)
Veteran Survivor/Dependent	-0.034	5.8**	1.9	4.2+	3.5
	(0.031)	(1.7)	(2.1)	(2.3)	(2.5)
Observations	70,893	86,416	65,125	57,731	50,611

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA (graduation models only), age at matriculation, term of entry.

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

Consistent with the primary models from the previous chapter, veterans have lower first year GPAs, but higher retention and graduation probabilities than comparable nonveterans. Veterans who only use the Post-9/11 GI Bill have virtually the same outcomes as other veterans. Surprisingly, veterans who begin using an older form of the GI Bill and upgrade to the Post-9/11 GI Bill have first year GPAs a third of a grade lower than other similar veterans who exclusively used the older GI Bill, but have probabilities of graduating within six years 8.5 percentage points higher than similar veterans using the older GI Bill (and thus, 14.1 percentage points higher than similar nonveterans).

The survivors and dependents of veterans are nonveterans who receive veteran aid without military service. As such, they can help isolate the effects of the aid. Survivors and dependents have first year grades and graduation probabilities similar to comparable nonveterans, but have a probability of returning after the first year 5.8 percentage points higher. Although this is higher than the veteran advantage in retention, it is not significantly different.

5.2.2 Aid Amount

I use the primary models with the subset of Post-9/11 GI Bill recipients with payment data, dropping the veteran dummy variable and adding a continuous measure of GI Bill aid. Table 15 shows the results.

Table 15. Aid Levels and Student Success Among Post-9/11 Veterans, Average Marginal Effects

VARIABLES	(1) First Year GPA	(2) First Year Retention	(3) Four Year Graduation	(4) Five Year Graduation
GI Bill Awards (first-semester, in $1,000s)	-0.022	2.9*	-0.9	27.0**
	(0.023)	(1.4)	(2.7)	(9.4)
Observations	556	669	310	156

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA (graduation models only), age at matriculation, term of entry.

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

Among Post-9/11 veterans, the GI Bill payments appear to have no relationship with grades. However, levels of GI Bill payments are associated with higher retention and graduation rates. For similar veterans using the Post-9/11 GI Bill, an extra $1,000 in veteran benefits increases the probability of returning for the second year by 2.9 percentage points and the probability of graduating within five years by 27 percentage points. Levels of benefits appear to have no effect on the probability of graduating within four years. There isn't enough data to construct a six-year graduation model and low sample sizes may affect other estimates.[18]

5.2.3 Discussion

Overall, the results from these analyses suggest that aid partially explains the veteran paradox for retention and graduation. Like veterans, the survivors and dependents of veterans who receive education benefits are more likely to return after the first year than other similar nonveterans. Veterans who start college using the GI Bill and then receive increased benefits under the Post-9/11 GI Bill are more likely to graduate within six years than other comparable veterans. Among Post-9/11 veterans with award data, larger GI bill payments are associated with higher probabilities of returning after the first year and graduating within five years.

Yet, these results suggest that veteran aid does not fully explain the veteran advantage in retention and graduation. Veterans who exclusively use the more generous Post-9/11 GI Bill are no more likely to return and graduate than other similar veterans. Moreover, veteran survivors and dependents have essentially the same chances of

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[18] Award amounts vary based on the amount of time served after 9/11.

graduating as other, similar nonveterans. It could be the case that generous eligibility might induce college enrollment for those veterans and veteran survivors/dependents who are unsuited for it. The models control for academic background, but some unobservable factors could be relevant. On the whole, education benefits for veterans seem to have negligible impacts on grades but seem to improve retention and graduation. However, these still do not fully explain the veteran paradox in retention and, especially, graduation.

5.3 Student Success Outcomes for Veterans and Matched Nonveterans

I test the selection and direct effects explanations by repeating the primary models after matching:

H6: Veterans have worse student success outcomes than nonveterans who match characteristics associated with enlistment

H7: Veterans have student success outcomes at least as good as nonveterans after controlling for all the aforementioned factors

The approach used here relies on matching to create a comparison group of nonveterans who are most similar to the veterans. To create the matched comparison group, I use variables related to veteran status and student success, but exclude variables that could be affected by veteran status. This includes sex, race, socioeconomic status, academic background, age, and geographic military presence.[19] As is generally

[19] Although some suggest that variables related to the treatment but unrelated to the outcome should be excluded from the matching procedure, this practice is generally acceptable as long as including the variable does not lead to unbalanced groups. Using geographic military presence did not disrupt the balance.

recommended, I include the dummy variables that identify students with imputed values for the academic background variables.

Recommendations vary on how to select a matching method, but many suggest choosing the method that achieves the best balance among the important covariates. I create matched comparison groups using the most commonly recommended approaches. Table 16 shows the standardized differences between veterans and nonveterans for each of the matching approaches. As shown in the previous chapter, there are very substantial differences between veterans and nonveterans in the unmatched data. Each of the matching methods improves the balance between veterans and nonveterans. Standardized differences above 20 are typically seen as problematic. Only kernel matching produced differences that exceeded this threshold.

Table 16. Standardized Differences Between Veterans and Nonveterans Before and After Matching

VARIABLES	Unmatched	PS 1:1 NN	PS k:1 NN	PS Kernel	Mahalanobis
Female	-43.0	-0.7	-1.0	-12.7	-4.6
Race					
Black	24.3	0.1	-1.0	8.6	-0.6
Hispanic	2.6	-3.4	0.0	1.0	0.5
Asian	-3.0	1.7	0.0	-13.9	0.6
Other/Biracial	3.7	2.4	0.8	1.7	0.2
No Race Given	-1.6	4.2	3.0	0.4	0.2
Mean ZIP Income	-18.4	-4.0	-1.2	-6.6	1.0
High School GPA	-64.4	0.1	-0.4	-22.4	0.2
Missing High School GPA	62.1	-0.1	0.4	21.4	-0.4
AP Credit	-31.0	1.8	1.2	-12.7	1.3
ACT Math Score Equivalent	-50.6	4.1	4.5	-17.6	-0.2
Missing ACT Math Score Equivalent	44.9	-4.3	-5.0	15.1	0.1
ACT English Score Equivalent	-50.2	2.0	3.1	-17.0	0.0
Missing ACT English Score Equivalent	50.2	-2.4	-3.2	16.9	0.0
Age at Matriculation	68.3	5.3	2.4	23.8	0.1
Recruitment Density	2.6	2.3	2.0	1.1	2.0
Overall Mean Difference	17.1	2.1	1.5	6.2	0.2
Observations	94,635	5,036	9,445	94,264	4,032

Matching methods involve a trade-off between bias and variance, since adding multiple matches per veteran will increase the size of the comparison group but at the risk of adding nonveterans who are less similar to veterans on the relevant variables. As such, I chose propensity score matching using four nearest neighbors with replacement. Although Mahalanobis matching makes the two groups most similar, it comes at the cost of a smaller comparison group. The propensity score nearest neighbors approach strikes the best balance between bias and variance, as it yields very similar groups but with a much larger comparison group.[20]

Table 17 shows the results of the primary models on the preprocessed sample. Veterans have predicted first year GPAs .15 lower than comparable, matched nonveterans. Veterans are more likely to return after the first year and to eventually graduate. Compared with similar nonveterans from the matched sample, the predicted retention probability is 5.1 percentage points higher for veterans and the predicted graduation probability is between 5.0 and 8.2 percentage points higher for veterans.[21] The results after matching are very similar to those using the unmatched data presented in the previous chapter. The effect is nearly identical for first year GPA and is about one and a half percentage points higher for retention. The effects are generally between half and one percentage point higher for graduation, but are slightly lower in the seven- and eight-year models.

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[20] I also performed the subsequent analysis after Mahalanobis matching and the results were nearly identical to those presented here.

[21] Technically, the standard errors used in these regressions are incorrect because they are used in second-stage regressions and do not account for the first stage (in which the propensity scores were estimated). I also compute the adjusted AI standard errors in Stata and use them to manually recalculate the p-values. All estimates are at least statistically significant at the .05 level.

Table 17. Student Success Regressions on Matched Sample, Average Partial Effects

VARIABLES	(1) First Year GPA	(2) First Year Retention	(3) 4 Year Graduation	(4) 5 Year Graduation	(5) 6 Year Graduation	(6) 7 Year Graduation	(7) 8 Year Graduation	(8) Ever Graduates
Veteran	-0.149**	5.1**	5.0**	5.5**	6.2**	5.8**	7.0**	8.2**
	(0.023)	(1.2)	(1.4)	(1.5)	(1.6)	(1.8)	(2.0)	(1.6)
Observations	6,962	8,685	6,423	5,692	4,956	4,173	3,471	4,953

Standard errors in parentheses. Models also include the following variables: female, black, Hispanic, Asian, Other/Biracial, no race given, mean ZIP income, mean ZIP income squared, high school GPA, high school GPA squared, missing high school GPA, AP credit, AP credit squared, ACT math score equivalent, ACT math score equivalent squared, missing ACT math score equivalent, ACT English score equivalent, ACT English score equivalent squared, missing ACT English score equivalent, grants, loans, scholarships, transfer hours, transfer hours squared, part time student, first semester GPA (graduation models only), age at matriculation, term of entry.

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

5.3.1 Discussion

Even after taking into account the support for the enrollment and aid explanations, the veteran paradox still remains, especially for graduation. This leaves the selection and direct effects explanations. The results from propensity score matching cannot completely rule out the selection explanation. The factors associated with selection are unobserved here, which makes it very difficult to control for them. If the unobservables are correlated with the observed variables used here, and if those correlations are equivalent for veterans and nonveterans, then the propensity score matching analysis has controlled for them. However, these assumptions, while not implausible, are strong and are impossible to verify.

Past research has shown that military enlistment is associated with social isolation, histories of adolescent fighting, weaker family structures, greater geographic military institutional presence, lower socioeconomic status, weaker academic backgrounds, but also with higher education aspirations (Kleykamp, 2006; Elder et al., 2010; Spence, Henderson, and Elder, 2013; Teachman and Tedrow, 2014). On the whole, these findings suggest that the relevant unobservables are more likely to inhibit student success than bolster it. Together, the enlistment literature, logic, and the matching results suggest that the selection explanation is unlikely to be driving the veteran paradox for retention and graduation. However, it cannot be ruled out completely.

The results of the matching analysis are consistent with the direct effects explanation. Even though the threat of selection cannot be eliminated, past research and the matching results suggest that the direct effects explanation is more likely. Although far from conclusive, on the whole this suggests that actual military service leads to better student success outcomes.

5.4 Conclusion

The results from the previous chapter ruled out the bias, background, and maturation explanations for the veteran paradox, but found support for the enrollment explanation. The findings from this chapter assess the aid, selection, and direct effects explanations. The results suggest that aid partially explains the veteran paradox for retention and for graduation. The matching results cannot completely rule out the selection explanation, but suggest that it is unlikely. Overall, the results from all the previous analyses (and, especially, the final matching analysis), suggest that the effects of actually serving in the military partially explain the veteran paradox for retention and graduation. In the final chapter, I discuss additional implications of these results and directions for further research.

CHAPTER 6

CONCLUSIONS

6.1 Summary of Findings

The purpose of this dissertation is to understand how veterans fare in college and why those outcomes manifest themselves. Theory and related evidence predict that veteran students should be less successful than their nonveteran peers, yet the limited past research suggests that they are actually at least as successful as, if not more successful than, nonveterans. This is the student veteran paradox. I have proposed and tested seven potential explanations to resolve this paradox: bias in past research, background characteristics of veterans, enrollment behaviors of veterans, maturation from delayed entry, education aid benefits for veterans, unobservable factors associated with selection into the military, or the direct effects of military service.

The descriptive statistics for both groups show that veterans do hold characteristics that inhibit success, but are at least as successful as nonveterans in retention and graduation (but not first year grades). The primary models show this is partly due to enrollment patterns, but not background characteristics or simple maturation. Veterans at GSU begin with more transfer credit and, surprisingly, are less likely to enroll part-time. Still, veterans are more likely to return for the second year and to eventually graduate than similar nonveterans, even when controlling for enrollment behaviors. The results from the funding analyses suggest that part, but not all, of this can be attributed to education aid benefits. The matching results seem to suggest that another part stems from the direct consequences of military service. However, the findings could not completely rule out the possibility that the veteran advantage is due to unobservable factors that are associated with enlisting in the military. Overall, at least for retention and graduation, these results rule out the bias, background characteristics, and maturation explanations, but support the enrollment patterns and funding explanations. The results are consistent with the direct effects explanation, but the selection explanation cannot be ruled out completely.

6.2 Limitations

This dissertation makes several original contributions, but is limited in many respects. The conclusions from the primary models are strong, but those from the secondary models are much more weakly supported. The funding analyses rely on very small subgroups of veterans. More significantly, the selection explanation could not be adequately ruled out. Controlling for unobservable factors associated with selection is very difficult under optimal circumstances. There are no strong instrumental variables for contemporary veterans. The matching approach used only controls for unobservables if they are correlated with the observables. This is impossible to assess.

There are also external validity issues related to Georgia State University. Despite attempts to expand the study, data only included students at a single university. GSU has actively worked to improve retention and graduation in the past decade. Even though the models control for term of entry, it is possible that interventions have disproportionately affected veteran students. Even if this isn't the case, GSU could be different on other dimensions or could be an anomaly for veteran success.

6.3 Policy Implications

The results from this dissertation have many implications for public policy. First, this research suggests that veteran education benefits are not being wasted, at least when used at institutions like GSU. Some recent media reports claim that veterans squander these resources because they drop out of college at much higher rates than nonveterans (e.g., Betar 2012, Briggs 2012, Wood 2012). At least at GSU, this is far from the case.

Many states have either implemented performance-based funding models for public colleges and universities or are considering it. Under such systems, the state appropriation a college receives is determined, at least in part, by institutional performance on student retention and graduation. These systems typically have mechanisms in the formulae to prevent disincentives for enrolling high-risk students (e.g., first-generation students and minority students). Some states have considered adding veterans to these protected groups, but the results of this research suggest that is unnecessary.

This research also implies that changing enrollment patterns is one way to improve the graduation and retention. Part of the reason veterans fare well is because they are much less likely to enroll part-time, and part-time student fare worse. This is perhaps an indirect effect of GI Bill benefits, if they allow veterans to delay work in order to enroll full-time. Although the findings on part-time students are not new, the overall results serve as a reminder that these patterns are malleable and altering them could improve outcomes for students. Colleges could encourage full-time attendance or beginning at a two-year institute in various ways, through information or with financial incentives (e.g., aid, tuition discounts, etc.).

At the ground level, many colleges and universities have limited resources. Many argue that programs centered on veteran student support should be high priorities. Although it is important to provide adequate support for veteran students (particularly in mental health), the results of this work suggest that extensive programs for veterans may not be necessary. In a world of scarce resources, institutional funds for support programs would be more useful if targeted toward higher risk groups.

6.4 Directions for Future Research

This dissertation makes several original contributions, but it also raises additional questions to be addressed in the future research. Most importantly, additional research should examine outcomes for veteran students beyond Georgia State University. Although the university is not unique in its support for veterans or in the size of the veteran population, external validity is not addressed in this study. GSU has improved graduation rates over the past decade; although this study does control for institutional change over time, veterans could be less successful at other institutions. Research should look particularly at veteran success at for-profit colleges, which take in a disproportionate amount of money spent on veteran education benefits.

This study finds support for the explanation that military service improves the odds of retention and graduation. Yet this raises questions about the specific mechanism(s) of action through which this occurs. Past research has suggested that non-cognitive factors such as resilience and grit are associated with graduation. Many contemporary soldiers go through resilience training programs during service, and veterans report that service improves independence, self-discipline, and ability to deal with adversity (Meredith et al., 2011; Elder and Clipp, 1989). Additionally, grit has been associated with both college graduation and persistence through military training (Duckworth et al., 2007; Maddi et al., 2012; Eskreis-Winkler et al. 2014). Alternatively, improved performance could be a policy feedback effect. Mettler (2002) finds that GI Bill usage increased civic engagement among WWII veterans, postulating that this might stem from a sense of obligation from receiving those benefits. Analogously, student veterans might work harder out of a sense of obligation from receiving various veteran benefits. Future research should focus on whether veteran and nonveteran students differ on these various non-cognitive factors and whether they can account for the direct effects of military service on retention and graduation.

One of the major shortcomings of this research is the inability to rule out unobservable factors related to selection into the military. Additional causal inference work should aim to address the selection explanation. Unfortunately, this is a difficult task. There are no apparent strong instruments that could address this for contemporary veterans. Longitudinal datasets that capture these factors during high school may hold promise.

Moreover, additional research should investigate other things that mediate the relationships between veteran status and student success outcomes. Postsecondary institutions vary widely in programs, policies, and resources devoted to veteran students. These may improve veteran experiences on campus, but researchers should examine whether they have any impact on student success outcomes for veterans.

More generally, education researchers should expand research beyond first-time, full-time students who enter in the fall. Most research focuses on this subpopulation because these are the students included in required data submissions to the Department of Education, and, subsequently, are the students included in the publicly-available datasets. This epitomizes the research joke about only looking for car keys under the streetlamp. Simplifying samples to first-time, full-time, fall entrants eases many analyses, but it excludes the majority of students who attend institutions like GSU. Although findings for veterans in this analysis were consistent, student success outcomes for minorities were less favorable when using an unrestricted sample of all students. The federal government should require data reporting for all students, but until that happens researchers should also strive to find datasets that are representative of actual student bodies. Doing so will not only prevent biased findings, but also improve the policy recommendations they inspire. Institutional research offices are obvious data sources, as are statewide data systems that are (usually) more complete.

Additionally, education researchers should focus on adding nuance to the relationships between student success outcomes. The conventional narrative is that earning good grades is necessary for first year retention, and that both grades and retention are necessary for graduation. This is obviously true, but the results from this dissertation suggest that the relationships are complex. Veterans have lower first-year grades than comparable nonveterans, but are more likely to return and graduate. Similarly, minority students have lower grades and higher retention rates than comparable whites, but only Asians have higher graduation rates while all other races graduate at virtually the same rates as similar white students. On the whole, findings like this suggest that factors and interventions that improve the leading measures of success may not influence the lagging measures in the same ways.

Appendix

Table 18. First Year GPA, Full Regression Results

VARIABLES		
Veteran	-0.155**	(0.017)
Female	0.089**	(0.005)
Race		
Black	-0.270**	(0.007)
Hispanic	-0.087**	(0.010)
Asian	-0.098**	(0.009)
Other/Biracial	-0.151**	(0.014)
No Race Given	-0.100**	(0.013)
Mean ZIP Income (in $10,000s)	0.018**	(0.004)
Mean ZIP Income squared	-0.001**	(0.000)
High School GPA	-0.951**	(0.098)
High School GPA squared	0.223**	(0.015)
Missing High School GPA	-0.778**	(0.155)
AP Credit	0.024**	(0.001)
AP Credit squared	-0.001**	(0.000)
ACT Math Score Equivalent	0.084**	(0.007)
ACT Math Equivalent squared	-0.002**	(0.000)
Missing ACT Math Score Equivalent	0.993**	(0.082)
ACT English Score Equivalent	0.062**	(0.006)
ACT English Equivalent squared	-0.001**	(0.000)
Missing ACT English Score Equivalent	0.789**	(0.063)
Grants (first-semester, in $1,000s)	-0.037**	(0.002)
Loans (first-semester, in $1,000s)	-0.024**	(0.001)
Scholarships (first-semester, in $1,000s)	0.092**	(0.002)
Transfer Hours	0.002**	(0.000)
Transfer Hours squared	0.000**	(0.000)
Part-Time Student (first-year)	-0.328**	(0.006)
Age at Matriculation		
13	0.146	(0.667)
14	0.343	(0.252)
15	0.214**	(0.079)
16	0.141**	(0.033)
17	0.044**	(0.012)
19	0.029**	(0.010)
20	0.055**	(0.013)
21	0.069**	(0.014)
22	0.135**	(0.016)
23	0.215**	(0.018)
24	0.278**	(0.019)

Table 18 (continued)

25	0.365**	(0.021)
26	0.341**	(0.022)
27	0.382**	(0.023)
28	0.431**	(0.025)
29	0.478**	(0.027)
30	0.511**	(0.028)
31	0.552**	(0.028)
32	0.532**	(0.030)
33	0.491**	(0.031)
34	0.509**	(0.033)
35	0.536**	(0.034)
36	0.569**	(0.038)
37	0.484**	(0.038)
38	0.620**	(0.040)
39	0.545**	(0.041)
40	0.519**	(0.045)
41	0.585**	(0.042)
42	0.600**	(0.047)
43	0.591**	(0.052)
44	0.652**	(0.055)
45	0.712**	(0.054)
46	0.589**	(0.056)
47	0.607**	(0.061)
48	0.626**	(0.068)
49	0.626**	(0.069)
50	0.603**	(0.071)
51	0.454**	(0.086)
52	0.638**	(0.083)
53	0.625**	(0.085)
54	0.503**	(0.093)
55	0.559**	(0.103)
56	0.298**	(0.113)
57	0.765**	(0.129)
58	0.782**	(0.143)
59	0.721**	(0.153)
60	0.520**	(0.201)
61	0.475*	(0.236)
62	0.530**	(0.112)
63	0.540**	(0.146)
64	0.469**	(0.150)
65	0.466**	(0.167)
66	0.601+	(0.333)
67	0.691**	(0.211)
68	0.675**	(0.202)

Table 18 (continued)

69	0.056	(0.472)
70	0.133	(0.333)
71	0.482	(0.385)
72	0.209	(0.472)
73	0.844	(0.667)
74	-0.126	(0.667)
75	0.441	(0.668)
76	0.735	(0.472)
79	-0.418	(0.667)
81	0.337	(0.667)
85	1.414*	(0.667)
Term of Entry		
Spring 2004	-0.055*	(0.023)
Summer 2004	0.031	(0.030)
Fall 2004	0.010	(0.016)
Spring 2005	0.062**	(0.024)
Summer 2005	0.031	(0.032)
Fall 2005	-0.009	(0.016)
Spring 2006	-0.004	(0.022)
Summer 2006	-0.023	(0.030)
Fall 2006	-0.010	(0.016)
Spring 2007	0.039+	(0.022)
Summer 2007	0.009	(0.031)
Fall 2007	0.015	(0.015)
Spring 2008	0.057*	(0.023)
Summer 2008	0.129**	(0.030)
Fall 2008	0.085**	(0.015)
Spring 2009	0.085**	(0.022)
Summer 2009	0.110**	(0.031)
Fall 2009	0.083**	(0.015)
Spring 2010	0.069**	(0.021)
Summer 2010	0.080*	(0.031)
Fall 2010	0.056**	(0.015)
Spring 2011	0.123**	(0.022)
Summer 2011	0.184**	(0.031)
Fall 2011	0.112**	(0.015)
Spring 2012	0.144**	(0.022)
Summer 2012	0.171**	(0.029)
Fall 2012	0.156**	(0.015)
Spring 2013	0.125**	(0.022)
Summer 2013	0.147**	(0.029)
Fall 2013	0.210**	(0.015)
Spring 2014	0.156**	(0.023)
Summer 2014	0.154**	(0.028)

Table 18 (continued)

Fall 2014	0.183**	(0.015)
Spring 2015	0.177**	(0.024)
Constant	1.524**	(0.176)
Observations	70,893	

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Table 19. First-Year Retention, Full Regression Results

VARIABLES		
Veteran	0.220**	(0.057)
Female	-0.037*	(0.018)
Race		
Black	0.189**	(0.022)
Hispanic	0.314**	(0.036)
Asian	0.546**	(0.034)
Other/Biracial	0.226**	(0.047)
No Race Given	-0.412**	(0.035)
Mean ZIP Income (in $10,000s)	0.071**	(0.012)
Mean ZIP Income squared	-0.003**	(0.001)
High School GPA	-0.805*	(0.317)
High School GPA squared	0.104*	(0.050)
Missing High School GPA	-1.313**	(0.499)
AP Credit	0.070**	(0.005)
AP Credit squared	-0.002**	(0.000)
ACT Math Score Equivalent	0.250**	(0.023)
ACT Math Equivalent squared	-0.005**	(0.000)
Missing ACT Math Score Equivalent	2.314**	(0.265)
ACT English Score Equivalent	0.068**	(0.019)
ACT English Equivalent squared	-0.002**	(0.000)
Missing ACT English Score Equivalent	0.531*	(0.210)
Grants (first-semester, in $1,000s)	0.071**	(0.009)
Loans (first-semester, in $1,000s)	0.020**	(0.004)
Scholarships (first-semester, in $1,000s)	0.212**	(0.008)
Transfer Hours	0.014**	(0.001)
Transfer Hours squared	-0.000**	(0.000)
Part-Time Student (first-year)	-0.452**	(0.019)
Age at Matriculation		
13, omitted	-	
14, omitted	-	
15	0.713*	(0.293)
16	0.067	(0.097)
17	-0.398**	(0.038)
19	-0.101**	(0.034)
20	-0.033	(0.042)
21	-0.033	(0.047)
22	-0.008	(0.052)
23	-0.188**	(0.055)
24	-0.308**	(0.058)
25	-0.213**	(0.062)
26	-0.232**	(0.066)
27	-0.227**	(0.069)

Table 19 (continued)

28	-0.317**	(0.072)
29	-0.309**	(0.078)
30	-0.061	(0.082)
31	-0.089	(0.085)
32	-0.118	(0.089)
33	-0.245**	(0.089)
34	-0.131	(0.097)
35	-0.215*	(0.099)
36	-0.352**	(0.107)
37	-0.314**	(0.107)
38	-0.044	(0.118)
39	-0.212+	(0.116)
40	-0.110	(0.131)
41	0.037	(0.125)
42	-0.076	(0.137)
43	-0.137	(0.142)
44	0.054	(0.159)
45	0.077	(0.160)
46	-0.082	(0.161)
47	-0.146	(0.175)
48	0.040	(0.194)
49	-0.115	(0.198)
50	-0.171	(0.201)
51	0.036	(0.236)
52	-0.312	(0.220)
53	-0.497*	(0.217)
54	-0.050	(0.259)
55	0.392	(0.322)
56	0.079	(0.332)
57	0.321	(0.382)
58	-0.331	(0.388)
59	-0.128	(0.435)
60	-0.963+	(0.493)
61	-0.177	(0.602)
62	0.143	(0.297)
63	0.296	(0.382)
64	0.467	(0.418)
65	-0.640	(0.405)
66	-1.735**	(0.657)
67	-0.304	(0.566)
68	0.224	(0.505)
69	-1.721*	(0.827)
70	0.903	(1.149)
71	-1.165	(0.887)

Table 19 (continued)

72	-0.453	(1.006)
73	-1.237	(1.172)
74	-0.310	(1.429)
75, omitted	-	
76	-0.411	(1.019)
77, omitted	-	
79	-0.414	(1.307)
81, omitted	-	
83, omitted	-	
85, omitted	-	
88, omitted	-	
91, omitted	-	
Term of Entry		
Spring 2004	-0.272**	(0.064)
Summer 2004	-0.468**	(0.084)
Fall 2004	-0.183**	(0.048)
Spring 2005	-0.158*	(0.069)
Summer 2005	-0.125	(0.096)
Fall 2005	-0.029	(0.049)
Spring 2006	0.087	(0.070)
Summer 2006	-0.027	(0.092)
Fall 2006	0.071	(0.051)
Spring 2007	0.053	(0.069)
Summer 2007	-0.095	(0.093)
Fall 2007	0.046	(0.049)
Spring 2008	-0.087	(0.070)
Summer 2008	0.145	(0.096)
Fall 2008	0.128*	(0.050)
Spring 2009	0.027	(0.069)
Summer 2009	-0.004	(0.096)
Fall 2009	0.081	(0.049)
Spring 2010	0.046	(0.068)
Summer 2010	-0.199*	(0.095)
Fall 2010	-0.076	(0.049)
Spring 2011	-0.045	(0.069)
Summer 2011	-0.080	(0.097)
Fall 2011	0.010	(0.048)
Spring 2012	-0.116+	(0.069)
Summer 2012	-0.169+	(0.090)
Fall 2012	-0.074	(0.047)
Spring 2013	-0.173**	(0.067)
Summer 2013	-0.087	(0.090)
Fall 2013	-0.096*	(0.048)
Spring 2014	-0.214**	(0.069)

Table 19 (continued)

Summer 2014	0.139	(0.093)
Fall 2014	-0.114*	(0.047)
Spring 2015	-0.105	(0.074)
Constant	-1.435*	(0.559)
Observations	86,416	

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Table 20. Graduation, Full Regression Results

VARIABLES	(1) 4 Year	(2) 5 Year	(3) 6 Year	(4) 7 Year	(5) 8 Year	(6) Ever
Veteran	0.262**	0.263**	0.306**	0.352**	0.482**	0.481**
	(0.066)	(0.069)	(0.075)	(0.084)	(0.096)	(0.078)
Female	0.184**	0.175**	0.184**	0.179**	0.190**	0.220**
	(0.020)	(0.020)	(0.022)	(0.024)	(0.027)	(0.023)
Race						
Black	0.057*	0.027	0.025	0.055+	0.034	0.042
	(0.026)	(0.026)	(0.028)	(0.031)	(0.034)	(0.029)
Hispanic	-0.029	-0.018	-0.004	0.013	0.013	0.080+
	(0.040)	(0.041)	(0.045)	(0.050)	(0.056)	(0.048)
Asian	0.185**	0.214**	0.263**	0.301**	0.296**	0.257**
	(0.035)	(0.036)	(0.039)	(0.043)	(0.048)	(0.042)
Other/Biracial	-0.046	-0.151**	-0.030	-0.011	-0.035	-0.046
	(0.055)	(0.056)	(0.063)	(0.070)	(0.078)	(0.065)
No Race Given	-0.124**	-0.213**	-0.285**	-0.303**	-0.342**	-0.402**
	(0.046)	(0.046)	(0.048)	(0.050)	(0.052)	(0.049)
Mean ZIP Income (in $10,000s)	0.046**	0.081**	0.090**	0.103**	0.093**	0.083**
	(0.015)	(0.015)	(0.016)	(0.017)	(0.019)	(0.017)
Mean ZIP Income squared	-0.002*	-0.003**	-0.004**	-0.004**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
High School GPA	-3.360**	-2.908**	-2.430**	-1.931**	-1.476**	-2.021**
	(0.355)	(0.353)	(0.377)	(0.413)	(0.446)	(0.383)
High School GPA squared	0.533**	0.485**	0.420**	0.344**	0.276**	0.349**
	(0.055)	(0.055)	(0.059)	(0.065)	(0.071)	(0.061)
Missing High School GPA	-4.967**	-4.032**	-3.212**	-2.389**	-1.660*	-2.643**
	(0.564)	(0.556)	(0.590)	(0.643)	(0.692)	(0.594)
AP Credit	0.060**	0.042**	0.037**	0.041**	0.047**	0.036**
	(0.006)	(0.006)	(0.007)	(0.009)	(0.010)	(0.008)

Table 20 (continued)

AP Credit squared	-0.002**	-0.002**	-0.001**	-0.001**	-0.002**	-0.002**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ACT Math Score Equivalent	0.216**	0.172**	0.113**	0.092**	0.097**	0.127**
	(0.027)	(0.027)	(0.029)	(0.032)	(0.035)	(0.030)
ACT Math Equivalent squared	-0.005**	-0.004**	-0.002**	-0.002**	-0.002*	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Missing ACT Math Score Equivalent	1.889**	1.584**	0.991**	0.753*	0.852*	1.188**
	(0.313)	(0.312)	(0.336)	(0.367)	(0.404)	(0.348)
ACT English Score Equivalent	0.020	0.053*	0.061**	0.055*	0.060*	0.053*
	(0.021)	(0.022)	(0.023)	(0.025)	(0.028)	(0.024)
ACT English Equivalent squared	-0.001*	-0.002**	-0.002**	-0.002**	-0.002**	-0.002**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Missing ACT English Score Equivalent	0.005	0.203	0.262	0.204	0.247	0.250
	(0.240)	(0.243)	(0.261)	(0.285)	(0.311)	(0.268)
Grants (first-semester, in $1,000s)	-0.017+	-0.001	0.020+	0.027*	0.033*	0.022+
	(0.010)	(0.010)	(0.011)	(0.014)	(0.016)	(0.012)
Loans (first-semester, in $1,000s)	-0.010*	-0.008+	-0.001	-0.000	0.010	-0.005
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
Scholarships (first-semester, in $1,000s)	0.110**	0.108**	0.107**	0.118**	0.119**	0.121**
	(0.009)	(0.009)	(0.010)	(0.012)	(0.014)	(0.011)
Transfer Hours	0.056**	0.041**	0.034**	0.033**	0.033**	0.033**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Transfer Hours squared	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Part-Time Student (ever)	-1.130**	-0.770**	-0.539**	-0.392**	-0.319**	-0.244**
	(0.021)	(0.021)	(0.022)	(0.025)	(0.027)	(0.024)
First-Semester GPA	0.862**	0.904**	0.894**	0.860**	0.823**	0.807**
	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)	(0.014)
Age at Matriculation						

Table 20 (continued)

13, omitted	-	-	-	-	-	-
14	-0.019	-1.696	0.247			
	(1.212)	(1.215)	(1.461)			
15	-2.478*	-3.822**	-1.316**	-0.738*	-0.636	-0.388
	(1.028)	(1.022)	(0.339)	(0.312)	(0.412)	(0.288)
16	-0.783**	-0.707**	-0.428**	-0.504**	-0.422*	-0.427**
	(0.189)	(0.139)	(0.141)	(0.150)	(0.166)	(0.141)
17	-0.035	0.127**	0.236**	0.248**	0.241**	0.175**
	(0.047)	(0.043)	(0.048)	(0.053)	(0.058)	(0.052)
19	0.363**	0.049	0.010	-0.001	-0.026	-0.100*
	(0.039)	(0.039)	(0.042)	(0.046)	(0.051)	(0.044)
20	0.591**	0.130**	0.052	-0.034	-0.076	-0.105+
	(0.049)	(0.050)	(0.054)	(0.058)	(0.064)	(0.056)
21	0.464**	-0.044	-0.126*	-0.182**	-0.219**	-0.333**
	(0.056)	(0.057)	(0.061)	(0.066)	(0.071)	(0.063)
22	0.341**	-0.146*	-0.221**	-0.264**	-0.319**	-0.449**
	(0.061)	(0.062)	(0.067)	(0.073)	(0.080)	(0.069)
23	0.231**	-0.340**	-0.498**	-0.589**	-0.631**	-0.739**
	(0.066)	(0.068)	(0.072)	(0.078)	(0.085)	(0.074)
24	0.093	-0.427**	-0.550**	-0.583**	-0.631**	-0.791**
	(0.071)	(0.072)	(0.076)	(0.083)	(0.090)	(0.078)
25	0.042	-0.501**	-0.695**	-0.820**	-0.911**	-1.004**
	(0.075)	(0.076)	(0.080)	(0.085)	(0.092)	(0.081)
26	-0.076	-0.626**	-0.777**	-0.891**	-1.019**	-1.021**
	(0.081)	(0.082)	(0.086)	(0.092)	(0.099)	(0.087)
27	-0.012	-0.588**	-0.766**	-0.912**	-0.992**	-1.044**
	(0.084)	(0.085)	(0.090)	(0.096)	(0.104)	(0.091)
28	-0.159+	-0.745**	-0.906**	-1.027**	-1.031**	-1.202**

Table 20 (continued)

	(0.088)	(0.089)	(0.093)	(0.099)	(0.107)	(0.093)
29	-0.197*	-0.795**	-0.928**	-0.978**	-1.073**	-1.199**
	(0.095)	(0.096)	(0.101)	(0.109)	(0.117)	(0.101)
30	-0.043	-0.608**	-0.815**	-0.981**	-1.040**	-1.058**
	(0.098)	(0.101)	(0.106)	(0.114)	(0.124)	(0.107)
31	-0.079	-0.690**	-0.761**	-0.882**	-0.908**	-1.057**
	(0.099)	(0.100)	(0.105)	(0.112)	(0.123)	(0.106)
32	-0.148	-0.692**	-0.885**	-0.961**	-1.050**	-1.155**
	(0.107)	(0.109)	(0.115)	(0.121)	(0.133)	(0.115)
33	-0.192+	-0.754**	-0.908**	-0.867**	-1.044**	-1.182**
	(0.110)	(0.112)	(0.116)	(0.126)	(0.134)	(0.117)
34	-0.273*	-0.876**	-1.081**	-1.259**	-1.250**	-1.284**
	(0.116)	(0.117)	(0.123)	(0.131)	(0.140)	(0.123)
35	-0.108	-0.804**	-0.977**	-1.115**	-1.368**	-1.239**
	(0.119)	(0.121)	(0.126)	(0.134)	(0.145)	(0.126)
36	-0.600**	-1.139**	-1.215**	-1.245**	-1.335**	-1.411**
	(0.134)	(0.133)	(0.139)	(0.147)	(0.159)	(0.138)
37	-0.350**	-1.000**	-1.145**	-1.236**	-1.285**	-1.365**
	(0.130)	(0.132)	(0.138)	(0.148)	(0.160)	(0.137)
38	-0.167	-0.630**	-0.853**	-0.928**	-1.018**	-1.165**
	(0.141)	(0.143)	(0.149)	(0.159)	(0.170)	(0.149)
39	-0.360*	-0.911**	-0.920**	-1.084**	-1.148**	-1.258**
	(0.142)	(0.143)	(0.150)	(0.161)	(0.175)	(0.150)
40	-0.229	-0.845**	-0.978**	-1.282**	-1.364**	-1.288**
	(0.154)	(0.157)	(0.167)	(0.178)	(0.191)	(0.167)
41	-0.359*	-0.914**	-1.230**	-1.345**	-1.290**	-1.367**
	(0.144)	(0.146)	(0.154)	(0.164)	(0.182)	(0.152)
42	-0.136	-0.574**	-0.805**	-0.943**	-0.943**	-1.011**
	(0.167)	(0.170)	(0.180)	(0.196)	(0.207)	(0.181)

Table 20 (continued)

43	-0.686**	-1.279**	-1.396**	-1.486**	-1.573**	-1.637**
	(0.181)	(0.186)	(0.198)	(0.207)	(0.221)	(0.196)
44	-0.342+	-0.964**	-1.141**	-1.137**	-1.106**	-1.330**
	(0.195)	(0.196)	(0.209)	(0.221)	(0.236)	(0.208)
45	-0.262	-0.850**	-1.094**	-1.297**	-1.428**	-1.302**
	(0.190)	(0.196)	(0.204)	(0.220)	(0.232)	(0.203)
46	-0.366+	-1.026**	-1.021**	-1.137**	-1.401**	-1.309**
	(0.200)	(0.199)	(0.208)	(0.220)	(0.234)	(0.207)
47	-0.547**	-0.956**	-1.220**	-1.450**	-1.592**	-1.558**
	(0.208)	(0.208)	(0.224)	(0.239)	(0.273)	(0.221)
48	-0.337	-0.946**	-1.205**	-1.461**	-1.558**	-1.526**
	(0.230)	(0.236)	(0.244)	(0.267)	(0.298)	(0.243)
49	-0.567*	-1.250**	-1.330**	-1.774**	-1.848**	-1.718**
	(0.235)	(0.240)	(0.251)	(0.266)	(0.301)	(0.250)
50	-0.223	-0.777**	-1.021**	-1.196**	-1.262**	-1.254**
	(0.264)	(0.258)	(0.270)	(0.292)	(0.324)	(0.266)
51	-0.242	-0.971**	-1.371**	-1.306**	-1.662**	-1.587**
	(0.307)	(0.304)	(0.323)	(0.330)	(0.364)	(0.311)
52	-0.727**	-1.352**	-1.282**	-1.432**	-1.481**	-1.519**
	(0.275)	(0.278)	(0.278)	(0.306)	(0.333)	(0.277)
53	-0.598*	-1.061**	-1.422**	-1.552**	-1.804**	-1.765**
	(0.275)	(0.277)	(0.301)	(0.316)	(0.356)	(0.297)
54	-0.374	-1.127**	-1.218**	-1.351**	-1.314**	-1.640**
	(0.338)	(0.337)	(0.342)	(0.373)	(0.399)	(0.336)
55	-0.647+	-1.322**	-1.720**	-1.292**	-0.915*	-1.600**
	(0.373)	(0.377)	(0.407)	(0.422)	(0.454)	(0.391)
56	0.523	-0.007	-0.015	-0.124	-0.499	-0.367
	(0.459)	(0.450)	(0.481)	(0.500)	(0.532)	(0.480)
57	-0.899+	-1.844**	-2.231**	-2.250**	-2.375**	-1.898**

Table 20 (continued)

	(0.530)	(0.560)	(0.573)	(0.575)	(0.642)	(0.520)
58	-0.296	-0.905*	-1.405**	-1.504**	-1.205*	-1.692**
	(0.447)	(0.458)	(0.473)	(0.492)	(0.554)	(0.468)
59	-0.306	-0.425	-0.863	-0.926	-0.859	-0.640
	(0.550)	(0.516)	(0.613)	(0.640)	(0.680)	(0.641)
60	-0.437	-1.360+	-0.742	-0.409	0.633	-0.106
	(0.738)	(0.731)	(0.761)	(0.778)	(1.109)	(0.868)
61	-0.762	-1.688*	-1.479*	-1.326	-1.287	-1.437*
	(0.828)	(0.831)	(0.742)	(0.892)	(0.937)	(0.700)
62	-2.473**	-3.085**	-1.883**	-2.595**	-2.441**	-2.405**
	(0.763)	(0.773)	(0.568)	(0.678)	(0.698)	(0.559)
63	-1.030+	-1.329*	-1.856*	-2.373+	-2.546*	-2.248**
	(0.620)	(0.602)	(0.730)	(1.242)	(1.236)	(0.734)
64	-1.430+	-1.268	-1.680+	-1.761*	-2.296*	-2.244**
	(0.813)	(0.879)	(0.864)	(0.881)	(1.163)	(0.860)
65	-2.175*	-2.963**	-2.490*	-2.240+	-2.072+	-2.025*
	(1.075)	(1.082)	(1.130)	(1.151)	(1.210)	(0.901)
66, omitted	-	-	-			-
67	-1.927+	-1.887*	-1.550*	-1.229	-2.097*	-2.050**
	(1.108)	(0.863)	(0.777)	(0.881)	(0.930)	(0.776)
68	-1.184	-2.088*	-1.749*	-1.790+	-2.153+	-2.101**
	(0.864)	(0.870)	(0.790)	(0.914)	(1.204)	(0.773)
69, omitted	-	-	-	-	-	-
70, omitted	-	-	-	-	-	-
71, omitted	-	-	-	-	-	-

Table 20 (continued)

72, omitted	-	-	-	-	-	-
73, omitted	-	-	-	-	-	-
76, omitted	-	-	-	-	-	-
79, omitted	-	-	-			-
83, omitted	-	-	-	-	-	-
85, omitted	-	-				
Term of Entry						
Spring 2004	-0.255**	-0.230**	-0.242**	-0.247**	-0.264**	-0.335**
	(0.077)	(0.072)	(0.072)	(0.072)	(0.072)	(0.074)
Summer 2004	-0.165	-0.316**	-0.370**	-0.381**	-0.368**	-0.473**
	(0.101)	(0.096)	(0.096)	(0.096)	(0.096)	(0.097)
Fall 2004	-0.157**	-0.058	-0.063	-0.059	-0.072	-0.097+
	(0.054)	(0.048)	(0.049)	(0.050)	(0.051)	(0.053)
Spring 2005	-0.020	-0.127+	-0.179*	-0.160*	-0.162*	-0.274**
	(0.080)	(0.077)	(0.077)	(0.078)	(0.078)	(0.080)
Summer 2005	0.034	-0.153	-0.143	-0.126	-0.112	-0.234*
	(0.109)	(0.105)	(0.105)	(0.106)	(0.107)	(0.109)
Fall 2005	-0.036	-0.001	-0.013	0.006	0.038	-0.065
	(0.054)	(0.050)	(0.050)	(0.051)	(0.052)	(0.054)
Spring 2006	-0.085	-0.088	-0.141+	-0.149*	-0.118	-0.243**
	(0.076)	(0.073)	(0.074)	(0.074)	(0.075)	(0.077)
Summer 2006	0.041	-0.090	-0.072	-0.057	-0.060	-0.269**
	(0.102)	(0.100)	(0.101)	(0.102)	(0.102)	(0.104)

Table 20 (continued)

Fall 2006	-0.032	-0.037	-0.051	-0.044	-0.030	-0.169**
	(0.054)	(0.049)	(0.050)	(0.051)	(0.052)	(0.054)
Spring 2007	-0.088	-0.142*	-0.107	-0.070	-0.044	-0.250**
	(0.075)	(0.072)	(0.073)	(0.074)	(0.075)	(0.076)
Summer 2007	-0.237*	-0.327**	-0.313**	-0.277**	-0.239*	-0.471**
	(0.104)	(0.100)	(0.100)	(0.101)	(0.102)	(0.103)
Fall 2007	0.014	0.052	0.054	0.024	0.012	-0.178**
	(0.052)	(0.048)	(0.049)	(0.050)	(0.051)	(0.052)
Spring 2008	-0.007	-0.071	-0.083	-0.061	-0.065	-0.292**
	(0.078)	(0.076)	(0.077)	(0.078)	(0.079)	(0.079)
Summer 2008	0.076	-0.104	-0.183+	-0.149	-0.139	-0.407**
	(0.103)	(0.100)	(0.101)	(0.102)	(0.103)	(0.104)
Fall 2008	0.020	0.084+	0.064	0.056	0.048	-0.213**
	(0.052)	(0.048)	(0.049)	(0.051)	(0.052)	(0.053)
Spring 2009	-0.148*	-0.185*	-0.174*	-0.138+	-0.148*	-0.430**
	(0.075)	(0.073)	(0.073)	(0.074)	(0.075)	(0.076)
Summer 2009	0.034	-0.102	-0.120	-0.114	-0.128	-0.414**
	(0.104)	(0.102)	(0.103)	(0.105)	(0.105)	(0.106)
Fall 2009	0.000	-0.026	-0.016	-0.044		-0.410**
	(0.051)	(0.048)	(0.049)	(0.051)		(0.052)
Spring 2010	-0.209**	-0.253**	-0.259**	-0.281**		-0.616**
	(0.072)	(0.070)	(0.071)	(0.071)		(0.072)
Summer 2010	-0.377**	-0.353**	-0.329**	-0.354**		-0.741**
	(0.108)	(0.104)	(0.104)	(0.104)		(0.104)
Fall 2010	-0.075	-0.070	-0.084+			-0.585**
	(0.051)	(0.048)	(0.049)			(0.052)
Spring 2011	0.040	-0.011	0.018			-0.478**
	(0.075)	(0.073)	(0.074)			(0.075)

Table 20 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Summer 2011	-0.250*	-0.208*	-0.245*			-0.697**
	(0.106)	(0.104)	(0.105)			(0.105)
Fall 2011	-0.061	-0.049				
	(0.051)	(0.048)				
Spring 2012	-0.203**	-0.260**				
	(0.075)	(0.072)				
Summer 2012	-0.127	-0.217*				
	(0.101)	(0.097)				
Fall 2012	0.018					
	(0.050)					
Spring 2013	-0.151*					
	(0.075)					
Summer 2013	-0.342**					
	(0.101)					
14, omitted				-	-	-
Constant	-1.303*	-1.088+	-0.994	-1.336+	-2.008*	-0.936
	(0.644)	(0.636)	(0.674)	(0.731)	(0.790)	(0.682)
Observations	65,125	57,731	50,611	43,601	36,660	50,608

Standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

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