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DISSERTATION

Three Essays on Education Policy

Empirical Analyses of the Challenges and Opportunities with For-Profit Colleges, Military Enlistment and Immigration

Alessandro Malchiodi

RAND PARDEE RAND GRADUATE SCHOOL

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This document was submitted as a dissertation in December 2013 in partial fulfillment of the requirements of the doctoral degree in public policy analysis at the Pardee RAND Graduate School. The faculty committee that supervised and approved the dissertation consisted of Paco Martorell (Chair), Robert Bozick, and Trey Miller.

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Preface

This dissertation analyzes three diverse topics in education policy that concern non-traditional students: for-profit colleges, the effect of military enlistment on education, and immigration. It aims at empirically isolating mechanisms that determine the success of these specific institutions, contexts and challenges as measured by the educational outcomes of students exposed to them. The first and third essays rely on survey data and provide some indications of the robustness of their results, while the second essay uses administrative data and an econometric design to estimate causal effects. This dissertation should be of particular interest to academics and policy makers both in the U.S. and internationally who are facing the dilemmas of budget cuts, obsolescing programs and increasing mismatches between education credentials and labor markets. Against a picture of record youth unemployment, such as in the European Union, this research provides an empirical account of some patterns that have emerged as increasing shares of non-traditional students demand education and both public policies and the market respond. This work benefitted from generous financial support from RAND National Security Research Division and RAND Project Air Force for the analysis of the effects of military enlistment on education; future work will study the economic returns to for-profit education using the same framework. The essay on immigrant students was made possible by a broader project funded by the Spencer Foundation. RAND Labor and Population and RAND Education provided initial funding for the essay on for-profit colleges.

Abstract

This dissertation comprises three essays that empirically examine the educational outcomes of for-profit college students, military enlistees and immigrant youth. All of these are groups of "non-average" students that, in different contexts, pose challenges to the traditional provision of education. Therefore, their outcomes need to be studied in order to assess the need and room for public policy measures to intervene.

The first essay, Academic and Early Labor Market Outcomes of For-Profit College Students, employs a selection on observables framework on a nationally representative longitudinal study and finds that, compared to their peers in the public sector, for-profit students experience higher debt; when starting at 4-year institutions, lower 4-year degree completion rates; when starting at 2-year institutions, higher 2-year degree completion rates, but higher unemployment and lower earnings. Results are robust to departures from a selection-on-observables-only assumption.

The second essay, The Effect of Military Enlistment on Education, aims at identifying causal effects by comparing veterans to non-veterans who applied to enlist and are similar in the characteristics that the military uses to screen applicants. The results indicate that enlistees delay college but eventually enroll at comparable rates to similar non-enlistees; furthermore, enlistment positively impacts degree attainment at two-year institutions but negatively impacts degree attainment at four-year ones.

The third essay is entitled Home-Country Academic Quality, Time Spent in the U.S., and the Math Achievement of Immigrant High School Students. By virtue of augmenting

survey data with scores from international education assessments, it shows that home-country academic quality has a positive and significant relationship with mathematics achievement in the U.S., and that such relationship tends to decrease in size as a function of time since migration. This evidence suggests that one reason for the segmentation of immigrant assimilation along national lines, a phenomenon documented in the literature, is the diversity in academic background.

Summary

Education will be among the decisive factors to determine the prosperity of countries in the era of post-industrial development. However, its provision faces important challenges in the form of reduced budgets, increasingly diverse student bodies in terms of backgrounds and needs, and rapidly evolving and ever specialized labor markets. As a result, alternative demands, pathways and players emerge whose policy implications need to be understood. This dissertation is composed of three essays that examine some key policy challenges confronting nontraditional instances of education: Academic and Early Labor Market Outcomes of For-Profit College Students; The Effect of Military Enlistment on Education; Home-Country Academic Quality, Time Spent in the U.S., and the Math Achievement of Immigrant High School Students.

While the abstracts of each individual essay describe the research questions, data, methods and findings, the remainder of this summary briefly presents the main policy lessons that this dissertation provides.

Policy lesson 1: For-profit colleges must be acknowledged for having expanded the supply of higher education to underserved segments of the population (older students, minorities, students with higher risk to drop out). However, when compared to public institutions, 4-year for-profits have on average have failed to bring students to graduation, and 2-year for-profits have on average failed to adequately place them on the labor market, exposing them to a higher likelihood of unemployment and lower-paying jobs. Policies of the U.S. government aiming at restricting Title IV (federal student aid

programs) eligibility based on loan repayment (which is linked to the earning capacity of graduates) embed the right incentives to help break some of these vicious links.

Policy lesson 2: Policy makers need to rethink the role of public investment in higher education, in light of the evidence suggesting a link between for-profit colleges and higher indebtedness and default rates. For-profits can be a substitute to public higher education but might end up costing more to taxpayers because of high default rates, which shift the entrepreneurial risk from the market to students and public finances. Also in this case, public policies conditioning Title IV eligibility on loan repayment are a step in the right direction, but the evidence presented in this dissertation indicates that a broader reflection on the optimal level of investment in public higher education deserves further research.

Policy lesson 3: Education is an important dimension of military service, and one of the mechanisms through which the compensation policy for armed forces can achieve the objective of attracting and retaining an optimal level and composition of manpower. Military enlistment causes enlistees to delay higher education, but to eventually enroll at similar rates to non-enlistees. If enlistment can combine opportunities for both on-the-job training and formal education that leads to obtaining academic credentials, it might become more palatable to individuals who are concerned for the portability of their skills back to the civilian sector, i.e. those who plan on serving for a limited time.

Policy lesson 4: Black enlistees take longer than their peers to catch up with enrollment in higher education. Furthermore, high-aptitude enlistees suffer from a much stronger negative impact of enlistment on their prospects of obtaining a degree from a 4-

year institution. Policy makers might want to consider targeting black and high-aptitude enlistees with specific measures in order to ensure equal opportunity for higher education across race/ethnicity. Also, it might be inefficient for the military to forego the ability of high-aptitude enlistees to earn 4-year degrees, which could foster the skill set of the armed forces as a whole.

Policy lesson 5: Among immigrant students, home-country academic quality is a significant predictor of high-school achievement in the U.S. This implies that there is no one-size-fits-all approach that will maximize the learning of the whole student body, be they native or born abroad. It also suggests that there are important cross-fertilization effects that would get lost in any strategy for the composition of classes involving some degree of sorting by background. Losing such gains would reduce societal welfare not only in the present but also in the future, when new or future citizens struggle to find their way into the society and labor market.

Policy lesson 6: The legacy of home-country academic quality decreases over time, and it does so faster the further apart home-country academic quality is from the U.S. average: even the students coming from the most-disadvantaged country-specific academic backgrounds can eventually catch up with their native peers, but allowing enough time is of the essence in decreasing the mediating role that educational input received before migration continues to exert on current learning. The provision of education needs to be "patient" and at the same time challenging enough with students who need to catch up, while ensuring that the stock of learning that immigrants from better-performing school systems infuse into the receiving country does not get dispersed.

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Acknowledgments

The work for the first essay of this dissertation, Academic and Early Labor Market Outcomes of For-Profit College Students in the U.S., was started with proposal funding from RAND Education and RAND Labor and Population: without this initial spark, I would probably have never ended up pursuing these topics. RAND Project Air Force and RAND National Security Research Division generously funded the second essay. In particular, I would like to thank Michael Kennedy and John Winkler for their interest in my work and their understanding when my research plans had to change due to data limitations. I owe a very special thanks to John Winkler for extending the deadline of the dissertation funding and allowing me to complete my work remotely even if I had to physically leave PRGS in a rush. I am also grateful to Paco Martorell, Dave Loughran, Trey Miller and Beth Asch for involving me in their research on military manpower, which has been among the most fruitful learning experiences during my Ph.D. The third essay, Home-Country Academic Quality, Time Spent in the U.S., and the Math Achievement of Immigrant High School Students, was developed under financial support from the Spencer Foundation, and I thank Robert Bozick and Trey Miller for entrusting me with the opportunity to develop research in such an interesting area as immigration. Further acknowledgments are noted at the beginning of each essay.

Working with my dissertation committee has been very enriching, and I was lucky to benefit from the mentorship of three inspiring researchers. Furthermore, it was a true pleasure from a personal point of view, and I have enjoyed every single one of the many

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Introduction to the Dissertation

This dissertation is composed of three essays: Academic and Early Labor Market Outcomes of For-Profit College Students; The Effect of Military Enlistment on Education Home-Country Academic Quality, Time Spent in the U.S., and the Math Achievement of Immigrant High School Students.

The search for the topics of each of the essays was initially motivated by my interest in the policies and economics of human capital formation. As a passionate student of policy analysis, because of my origins I often reflect on the struggles that Europe as a continent, a community and a Union, and Italy as a country, are currently facing. I believe that education is at the heart of at least three of the key policy challenges that will determine the success or prolonged decline of Italy and the other countries of the old continent. First, the soaring levels of youth unemployment and precarious and low-paying jobs that most university graduates are forced to accept signal a clear mismatch between the supply and demand of qualifications and skills on the job market. Second, this structural difficulty in achieving an efficient supply of qualifications and skills is worsened by the continuing cuts to public education budgets that governments are forced to operate especially in times of economic downturns. Third, paradoxically in this context of diminishing resources, schools are called upon additional duties of fundamental importance, such as welcoming increasing shares of students from other cultures and providing a pillar of their formation as citizens of the receiving countries.

While shrinking, privileged shares of the population are relatively shielded from the failure of public policies to confront these three challenges, an increasing number of youths and young adults are left underserved in their search for a viable path of human capital formation, and, as the recent financial crisis has exacerbated, many men and women in the midst of their prime working age are forced to find new ways to market themselves by returning to school. This dissertation aims at shedding some light on these three policy challenges. As a graduate student in the United States, I believe that there are many lessons that can be learned from the diversity of experiences that this country witnesses and nurtures within itself. Among these experiences is a very dynamic and multi-faceted education sector: these essays analyze three policy issues that concern non-traditional students and investigate some of the mechanisms and outcomes that have creatively emerged in addressing their needs.

A methodological common denominator also ties these three essays together. The Pardee RAND Graduate School has been an excellent environment for me to lay the foundations of a solid understanding and practice of empirical methods in applied microeconomic analysis, and to grow in my ability to apply these skills across a whole host of policy questions. All of these three essays rely on a strong content of empirical work, including the use of survey and administrative data, data augmentation, imputation, modeling, identification and robustness analyses. They show that, while empirical analyses of secondary data may not provide the answer to all questions, there exists creative ways of patching sources together and extracting interesting patterns with policy-relevant content.

Essay 1: Academic and Early Labor Market Outcomes of For-Profit College Students in the U.S.[1]

Alessandro Malchiodi

Abstract

For-profit colleges and universities in the U.S. have been the fastest-growing type of higher education institutions over the last two decades. They typically target underserved students who seek training with flexible scheduling and direct labor market applicability. However, they have recently come under considerable scrutiny, as they encourage students to take on large amounts of government-subsidized debt that they cannot repay, while providing little in the way of marketable skill potential. While government has recently issued more stringent regulation for access to federal student aid, evidence on the outcomes of for-profit college graduates is still far from definitive.

This study offers one of the first empirical examinations of students in the for-profit sector. In particular, I examine the academic and early labor market experiences of

[1] I would like to thank Robert Bozick, Paco Martorell and Trey Miller for their patience and invaluable academic mentorship. Financial support was provided through proposal funding by RAND Labor and Population and RAND Education. Participants in the 2011 All California Labor Economics poster session provided useful comments. This essay also benefitted from feedback from presentations in 2012 at the PRGS Corporate Unit Review to RAND's President Michael Rich, to the PRGS Board of Governors, and to Dr. Subra Suresh, director of the National Science Foundation.

students who attend for-profit colleges and compare them to those of students who attend traditional public colleges. This analysis uses a nationally representative longitudinal study that tracks students who first enrolled in a postsecondary education institution in the 2003/2004 academic year until six years later.

Using a selection on observables framework, I control for a variety of background characteristics related to socioeconomic status and academic preparation. I find that, when compared to their peers at public institutions, for-profit students starting at a 2-year institution have higher 2-year degree completion but lower probability of advancing and pursuing a 4-year degree, higher debt, lower probability of employment and lower earnings; if starting at a 4-year institution, they have lower 4-year degree completion and higher debt.

I examine the sensitivity of the estimates to violations of the "selection only on observables" assumption and find that it is unlikely that the significant effects found can be explained away by selection bias.

1. Introduction

For-profit colleges and universities – defined as degree granting postsecondary institutions developed and managed by private, profit-seeking organizations – are the fastest growing segment of the U.S. higher education market[2]. In 2010 they accounted for

[2] For ease of communication throughout this essay, I use the shorthand expression "for-profit colleges" to include all private for-profit postsecondary colleges and universities. Similarly, I use the shorthand expression "public colleges" to include all public non-profit postsecondary colleges and universities.

9.6% of total fall enrollment in degree-granting institutions, compared to 0.2% in 1970 (author's calculations based on NCES Digest of Education Statistics, 2011, Table 198[3]). The largest increase in the number of enrollments at for-profit institutions happened between 2000 and 2010: +348%, compared to +37% at all U.S. degree-granting postsecondary institutions (ibid.). However, after a decade of extensive growth, there were for the first time signs of a decline in enrollments at the nation's largest for-profit colleges in 2011 (The Chronicle of Higher Education, March 11, 2012[4]).

Part of this latest slowdown in for-profits' success may be related to the recent wave of criticism that has led to government investigations and regulations. For-profit colleges have been compared to subprime mortgages (Lynch, Engle and Cruz, 2010), and investigations have been conducted into their business practices. For example, it has been considered opportunistic for these institutions to target particular types of students, such as veterans, based on their higher likelihood of obtaining federal student aid then used to pay for tuition. As students subsequently find themselves unable to repay - default rates are 15% at for-profit institutions vs. 5% at private vs. 7% at public (U.S. Department of Education[5]) – the question has been raised about whether the academic preparation delivered by these institutions offers a sensible earning potential relative to the cost of attending. Moreover, in some cases fraudulent behavior has been uncovered (Kutz, 2010) involving, for example, encouraging students to misreport information in their federal

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[3] http://nces.ed.gov/programs/digest/d11/tables/dt11_198.asp (as of 6/18/2012).

[4] http://chronicle.com/article/Big-For-Profit-Colleges-Suffer/131120/ (as of 6/18/2012).

[5] http://www.ed.gov/news/press-releases/default-rates-rise-federal-student-loans (as of 6/18/2012).

student aid applications. In response to these concerns, "gainful employment" regulations will enter into effect on July 1, 2012, conditioning an institution's eligibility for federal aid on loan repayment by its students[6].

In order to provide some basic facts that can inform this policy debate, I will attempt to answer four research questions. First, I will study what kinds of students for-profit colleges attract, in order to build an empirical understanding of the segments of the population that these institutions serve. Controlling for differences in observed characteristics, I will in turn consider outcomes six years after first enrollment. I will also examine debt accumulation in order to shed light on the relationship between the for-profit business model and the students' financial situation. Furthermore, I will try to gage the extent to which teaching characteristics such as flexible hours that are typically associated with for-profit colleges result into higher chances of graduating. Finally, I will inquire into whether the practical training and high-demand skills offered by these institutions actually translate into better prospects in the labor market.

In spite of the outstanding market performance and increasing public scrutiny, there are not many empirical studies on for-profit colleges that address the research questions outlined above. The academic literature on for-profit colleges has concentrated on four

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[6] See, for example, http://www.ed.gov/news/press-releases/gainful-employment-regulations (as of 6/18/2012). Gainful employment is defined as meeting one of the following three criteria: at least 35 percent of former students are repaying their loans (defined as reducing the loan balance by at least $1); the estimated annual loan payment of a typical graduate does not exceed 30 percent of his or her discretionary income; or the estimated annual loan payment of a typical graduate does not exceed 12 percent of his or her total earnings.

themes: characteristics of the institutions, financial aid, characteristics of the students and student outcomes.

A number of studies describe the characteristics of these institutions. A traditional peculiarity of for-profit colleges is their emphasis on practical, job-market oriented training that earned them the name of “trade schools” in their early days (Bailey, Badway and Gumport, 2001). These schools also claim that their value proposition includes flexibility in developing convenient schedules and paths to degree completion for students (Bailey, Badway and Gumport, 2001). While the range of institutions that compose this sector is very wide along several dimensions (size, breadth and quality of educational offering, accreditation, online vs. in-classroom teaching) (Bennett, Lucchesi and Vedder, 2010), they share a common financial model that relies heavily on tuition as the primary revenue source (Coleman and Vedder, 2008). However, these peculiar characteristics have not configured an entirely separate market. Evidence exists of some degree of substitutability between for-profit and public initiative in the provision of higher education: exploiting the discontinuity created by the approval or rejection by narrow margins of community college bond measures, Cellini (2009) found that increases in public funding to community colleges crowded out for-profits.

A separate question regards what financial resources the students use to pay tuition at for-profit institutions. Currently, only study to date includes for-profit institutions that are not Title IV eligible, and it found that the availability of federal student aid programs leads to higher tuition levels (Cellini and Goldin, 2012), supporting claims of opportunistic behavior by these institutions. This evidence is consistent with the fact that

almost all students at for-profits need financial aid (Lynch, Engle and Cruz, 2010), and this is in line with the higher average debt levels reported at for-profits than in other sectors (Baum and Steele, 2010).

Existing studies have also focused on describing the peculiar characteristics of students attending for-profit colleges. In a seminal work on for-profits, Apling (1993) showed that their students were more likely to be women, represent racial and ethnic minorities, come from families with lower income and lower educational achievement, and lack a high school diploma or equivalent certification. These patterns have been largely confirmed in all subsequent work irrespective of the data source, e.g. in Coleman and Vedder (2008) and Lynch et al. (2010). This study uses nationally representative data that includes a large subsample of for-profit enrollees to further study these patterns by examining other characteristics such as family composition, academic preparation and work commitments.

While consistent findings have emerged from the literature on institutions' and students' characteristics, and financial aid, evidence on student outcomes is quite mixed. Regarding degree completion, Bennett et al. (2010) report that "for-profit institutions have the highest graduation rate within 150% of normal time among the three sectors when all programs are considered", but "the lowest 6-year graduation rate among the sectors when only bachelor's degree programs are considered". In contrast, an earlier study by Grubb (1993) had reported no substantial differences in completion rates. These results suggest that it is important to stratify the analyses by 2-year vs. 4-year institutions. In terms of labor market outcomes, using the NLS72 Grubb (1993) found higher

likelihood of employment coming from community colleges as opposed to for-profits, and no differences in monthly earnings. Chung (2008) employed a selection on observables approach using NELS data and did not find statistically significant differences in employment rates and limited positive effects on women's earnings. Cellini and Chaudhary (2011) found no statistically significant differences in earning gains at private 2-year colleges (mainly for-profits). Using the NLSY97 allowed them to implement a fixed-effects analysis, but constrained them to a survey design aimed at sampling young individuals. This resulted in a failure to capture the older students, who represent a large share at for-profits. The only study that has found negative returns to for-profit college education as compared to not-for-profit has not been published yet (Turner, 2011). From the information presented in the abstract[7], Turner's work is based on a panel design and uses IRS earnings data. Finally, in a recent study Cellini (2012) estimates that public per-student cost of 2-year for-profits is lower than that of community colleges. However, when both public and private costs are considered the opposite is true, so higher returns are required at for-profits in order to yield positive net benefits – a circumstance that has not been found in the literature yet.

This essay seeks to improve on the existing literature in addressing three main shortcomings. Firstly, all existing studies rely on quite small sample sizes of for-profit college students. On the contrary, I will be able to observe three times as many for-profit college students as in the NLSY97, with the additional advantage of having for-profit status reported by the institution itself and not by the student. Secondly, I will try to

[7] http://works.bepress.com/nicholas_turner/6/ (as of 6/18/2012).

minimize concerns about selection in examining debt, graduation and early labor market outcomes, by virtue of exploiting a very rich set of observable student characteristics. Finally, in terms of identification of the causal effect of for-profit colleges, the most convincing research design published thus far has been that of Cellini and Chaudhary (2011), which however suffers from the important limitation of not including the so called "returning adults" in the study sample. While the nature of the data for this study does not leave any room for a direct improvement over the research design, it allows overcoming an equally important concern from a policy perspective by not restricting the attention to youth alone[8], since returning adults are a very important segment of the for-profit student population. In order to analyze the potential implications of omitted variable bias for my results, I will assess in detail the extent to which unobserved selection drives the estimates. In fact, in an article published after this essay was originally written, Deming, Goldin and Katz (2012) used the same dataset (Beginning Postsecondary Students Longitudinal Study, BPS:04/09) and reached very similar conclusions, warning against a causal interpretation of the findings notwithstanding the rich set of controls. They suggest that unobserved selection could be at play biasing towards finding negative effects of for-profit attendance (their matching estimator estimates smaller negative effects than their ordinary least squares one), but do not further address these issues. In this sense, the part of this essay dealing with selection bias represents an important addition to the work of Deming, Goldin and Katz (2012).

[8] Additional strengths of the dataset are described in the next section.

The remainder of this essay is organized as follows. Section 2 describes the data. Sections 3 and 4 present the results of descriptive and regression analyses, respectively. Section 5 introduces some discussion and treatment of selection on unobservables in this study and Section 6 concludes and highlights some of the key policy implications of my results. Finally, the Appendix presents the full set of results as well as some robustness checks.

2. Data

In order to address my research aims, I draw from a nationally representative sample of recent college entrants from the Beginning Postsecondary Students Longitudinal Study of 2004/2009 (BPS:04/09).

The BPS:04/09, sponsored by the National Center for Education Statistics (NCES), tracks the postsecondary experiences of a nationally representative cohort of students who first enrolled in a postsecondary education institution in the 2003/2004 academic year. The information was collected using a two-stage design, with subsequent sub-sampling procedures for the follow-up waves. The first stage is a sample of 1,630 colleges and universities from all the postsecondary institutions maintained within the NCES' Integrated Postsecondary Education Data System (IPEDS)[9]. The second stage is a

[9] For eligibility to the first stage, the college or university had to be located in the 50 states, the District of Columbia or Puerto Rico, had to offer a program designed for high-school graduates lasting at least 3 months or 300 hours open to persons other than the employees of the same institution and had to be eligible to distribute Title IV funds. This first stage included an oversampling of public two-year institutions, public

sample of students from the 1,360 institutions that provided students' information[10], of whom 18,640 were first time beginning undergraduate students in the 2003-04 school year[11]. Sampled students were initially interviewed (Wave 1) about their high school and early college experiences during their first year of college through either a web or telephone survey. Sample members were subsequently re-interviewed in the spring of 2006 (Wave 2) and in the spring/summer of 2009 (Wave 3), approximately three and six years after they had first started college respectively. These interviews were supplemented with enrollment verification and transcript information from the National Student Clearinghouse, federal financial aid and loan information from the Department of Education, and college admission test score information from the College Board and ACT. Across the three waves, 16,120 sample members were retained for an overall weighted response rate of 72.6 percent (68.6 percent weighted) as reported in the BPS:04/09 Methodology Report (Wine, Janson and Wheeless, 2011).

These figures indicate that a sizeable percentage of the original sample of students is lost between the first and third waves of the study, both because of interview nonresponse and because some students are progressively not confirmed to have been FTBs. Interview

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four-year institutions, and private nonprofit four-year institutions to permit state-level analyses in 12 states. This imbalance in the sample will be corrected for in my analyses by applying sampling weights.

[10] Not all 1,630 IPEDS institutions provided students' information. Within an institution students were sampled based on fixed sampling rates for each sampling type. The two sampling types were first-time beginners (FTB) and other undergraduates.

[11] The samples for the two follow-ups were progressively cleaned to remove ineligible students (false-positives) and include students that were not originally classified as FTBs (false-negatives), with the size shrinking from 44,670 eligible FTBs in Wave 1 to 23,090 in Wave 2 and 18,640 in Wave 3 (the final BPS:04/09 sample).

nonresponse can introduce bias if non-random. In the context of this study, if nonresponse was correlated to dropout and dropout was higher at for-profits, there could be, if any, a bias towards finding positive effects of for-profit attendance: if students who drop out tend to be the least successful ones, I would be "artificially removing" relatively more unsuccessful students from the sample of for-profit attendees, therefore comparing a more positively selected sample of for-profit students to an averagely selected sample of students at public institutions.

The BPS:04/09 has a number of very desirable design features for the research hypotheses outlined for this study. First, the longitudinal nature of the study captures the complete academic trajectories of each sampled student, from the first year of college, through graduation, and into the labor market – therefore permitting a broad examination of the dynamics of persistence, retention, performance, degree completion, debt accumulation and early achievements in the workforce. Second, the sample is composed of beginning postsecondary students of any age, and this is particularly advantageous for a study on for-profit institutions, which tend to attract older students who have already spent some time in the labor force before enrolling in college. Third, the BPS:04/09 includes administrative records on federal financial aid applications (the FAFSA) as well as student loan and Pell Grant disbursement. The reliance on administrative records significantly reduces the concerns with reporting errors by respondents. Lastly, the large sample will provide sufficient statistical power to study the relatively small sub-sample of students attending for-profit colleges.

Among the 1,360 schools at which study respondents enrolled in 2003/2004, 230 are for-profit colleges. Of the 16,120 panel respondents, 1,810 first enrolled in a for-profit college (Wine, Janson and Wheeless, 2011).

3. Descriptive Analyses

As mentioned above, my sample contains first-time beginning undergraduate students. I define my 'treatment group' as for-profit college students and my 'control group' as public college students, who ex-ante are expected to represent a more comparable group than private non-profit students. Also, I restrict my attention to the study of 4-year and 2-year institutions, as there are too few less-than-2-year institutions in the data to be analyzed separately. According to the U.S. Department of Education, 4-year institutions award at least a bachelor's degree, and 2-year institutions award at least an associate degree but their programs have less than 4 year duration.

The weighted distribution of first-time beginner college enrollees in my sample is described in Table 1.

Table 1. Type of Postsecondary Institution Attended by First-Time College Enrollees in 2003-04: Weighted Population Fractions and Frequencies.

		Four-Year Institutions	Two-Year Institutions	N (rows)
Public				
	col%	0.64	0.93	
	row%	0.36	0.64	
	cell%	0.29	0.51	
	cell n	578,070	1,019,948	1,598,018
For-Profit				
	col%	0.05	0.06	
	row%	0.39	0.61	
	cell%	0.02	0.04	
	cell n	44,325	70,129	114,453
Private				
	col%	0.31	0.01	
	row%	0.96	0.04	
	cell%	0.14	0.01	
	cell n	281,017	11,484	292,501
N (columns)		903,412	1,101,560	2,004,972

80% of first-time beginner college students in 2004 were enrolled in public institutions, 14% in private non-profit institutions and 6% in private for-profit institutions[12]. The distribution of students across 2-year and 4-year institutions looks fairly similar at public and for-profit colleges, while private institutions present a radically different picture. In fact, roughly 60% of first-time beginners in 2004 studied at a 2-year institution in the public and for-profit sectors (64% and 61%, respectively). On the other hand, only 4% of the first-time beginners in the private sector were enrolled in a

[12] For convenience I use the term "private" to refer to private non-profit institutions and "for-profit" to refer to private for-profit institutions.

2-year institution. This large difference in the distribution of students across 2-year and 4-year institutions suggests that the private sector would not represent an ideal comparison group.

In order to describe for-profit college students, in Table 2 I compare mean characteristics across institution types.

Table 2. Socio-demographic and Academic Characteristics of First-Time College Enrollees, by Type of Institution First Attended in 2003-04: Weighted Population Means.

	Four-Year Institution			Two-Year Institution		
	Public		For-Profit	Public		For-Profit
Sociodemographic Background						
Female	0.55		0.60	0.56		0.54
Race/ethnicity						
Asian/Pacific Islander	0.07		0.06	0.05	***	0.02
Black	0.09	***	0.22	0.14	**	0.23
Hispanic	0.10	***	0.21	0.16		0.29
White	0.70	***	0.44	0.60		0.53
Other	0.05		0.07	0.05		0.05
Parent's highest education						
High school or less	0.27	***	0.56	0.48	***	0.62
Some college	0.19	*	0.24	0.24		0.20
Bachelor's degree	0.54	***	0.21	0.28	***	0.18
Age first enrolled	19.1	***	24.2	23.6		24.1
Dependent on parents	0.93	***	0.52	0.63	***	0.44
Academic Preparation						
High school GPA > 3.0	0.92	***	0.52	0.60	**	0.48
No high school degree	0.02	***	0.14	0.11	***	0.20
Postsecondary Enrollment Characteristics						
Delayed enrollment	0.10	***	0.45	0.46	**	0.58
Part time enrollment	0.10	***	0.25	0.51	***	0.12
Has dependents	0.04	***	0.34	0.24	***	0.37
Single parent	0.02	***	0.23	0.12	***	0.25
Working full time	0.09	***	0.43	0.32		0.31
N	4,643		370	5,549		521

Note. Asterisks indicate the p-value of Wald tests of mean differences (H_0: difference = 0):
* p < .1, ** p < .05, *** p < .01

For-profit students tend to comprise a much higher share of minorities (especially Black and Hispanic). Furthermore, they tend to come from families with less educated parents. In particular, at 4-year institutions the average highest parental education in for-profits is high school or less, while the highest educated parent of students at public institutions has a bachelor's degree or above. On average, for-profit college students enroll later in life: at 4-year institutions there is a positive and significant average difference of 5 years in the age at first enrollment. Also, for-profit college students start higher education with more deficient academic preparation from high school. Roughly 50% of them have a high school GPA below 3.0, and they are from two (at 2-year institutions) to seven times (at 4-year institutions) more likely not to have earned a high school diploma than their counterparts who enroll at public institutions. Finally, their enrollment is characterized by some elements that are typically associated with higher risk of dropping out. For-profit students at 4-year institutions are more likely to be enrolled part-time. Interestingly, the percentage of students working full time while enrolled at 4-year for-profits is 43%, while the percentage reporting to be enrolled part-time is 25%. This means that 18% of students at 4-year for-profits are both working full-time and enrolled full-time – something not seen at other types of institutions. The reverse is true at 2-year institutions, where the majority of enrollees at public colleges do not study full time. However, the percentages of students working full-time at 2-year institutions are comparable across the public and for-profit sector (32% and 31%, respectively). More than a third of for-profit college students have dependents, and

around a fourth are single parents: a non-academic commitment that is likely to require a lot of these students' time.

Given all these significant differences in observed students' characteristics, I expect to find interesting results by examining mean outcomes across these groups (Table 3).

Table 3. Academic and Labor Market Outcomes of First-Time College Enrollees, by Type of Institution First Attended in 2003-04: Weighted Population Means.

	Four-Year Institution			Two-Year Institution		
	Public		For-Profit	Public		For-Profit
Bachelor's degree	0.60	***	0.16	0.11	***	0.00
Associate degree	0.04	***	0.15	0.14	**	0.20
Debt (amount still owed)	10,341	***	17,641	5,029	***	10,026
Employed (if out of school)	0.86	***	0.82	0.78	**	0.71
Still enrolled	0.34	***	0.16	0.29	***	0.16
Employed or enrolled	0.90	*	0.85	0.85	***	0.76
Employed	0.57	***	0.69	0.55		0.60
Annual income	33,663		32,774	31,724	***	27,412
N	4,643		370	5,549		521

Note. Asterisks indicate the p-value of Wald tests of mean differences (H0: difference = 0):
* p < .1, ** p < .05, *** p < .01

Degree completion rates reported in the first two rows of Table 3 speak to the lower achievement for for-profit students 6 years after starting. Only 16% of students who started at a 4-year for-profit managed to attain a bachelor's degree, as compared to 60% at public institutions. The figure for associate degree achievement is higher for for-profit students at 4-year institutions (15% vs. 4%). One possible interpretation is that more students who enroll in a 4-year program at for-profits tend to leave once they complete the requirements for a 2-year degree, or simply that 4-year for-profits are more likely to

offer 2-year degrees than their public counterparts. Regardless, overall graduation rates are much higher at public (64%) institutions than at for-profits (31%). 2-year for-profit institutions appear to be doing a significantly better job at graduating their students with associate degrees than 2-year public colleges. However, virtually no student transfers from a 2-year for-profit to attain a bachelor's degree, while 11% of students at 2-year public colleges do that. At 2-year institutions too, overall graduation rates look significantly better at public (25%) colleges than at for-profits.

While lower academic achievement is a major concern, student loans and debt burdens are one of the aspects that have attracted most criticism about for-profit colleges. Ostensibly, for-profit institutions try to maximize their revenue both by targeting students who are more likely to be eligible for federal loans and by strategically setting tuition levels in line with the maximum amount that can be financed through such facilitated borrowing schemes. Evidence presented in Table 3 is consistent with these hypotheses. The amount of loans that for-profit college students still owe six years after starting is much higher than their counterparts who enrolled at public institutions. Outstanding student debt accumulated to finance a 2-year program or less is twice as high ($ 10,026 vs. $ 5,029) on average for for-profit students than for their public counterparts. This is especially striking because it is a higher relative average difference than what can be observed at 4-year institutions ($ 17,641 vs. $ 10,341), while the time to accumulate it (i.e. the average time for which tuition has to be paid) is expected to be much shorter (because the nominal program duration is shorter).

One question that naturally follows the empirical examination of student debt is whether the larger monetary investment made at a for-profit institution is accompanied by a higher return on the labor market. Table 3 reports job-related outcomes six years after first enrollment. On the extensive margin, among those currently not enrolled, former for-profit college students are significantly less likely to be employed. Especially in a time of unfavorable macroeconomic conditions (2009), a difference of 4 to 7 percentage points in the likelihood of employment with respect to public college students seems quite relevant. Furthermore, the probability of being still enrolled six years after starting is much lower at for-profit colleges, which, coupled with lower graduation rates, implies higher dropout. It also implies that a significantly higher share of for-profit college students neither will be employed nor enrolled six years after starting. Alongside with financial considerations due to their inability to earn, it is equally important to remark that they are not accumulating human capital (neither through on-the-job learning nor through academic learning) and thus they are depleting its stock. On the intensive margin, I observe self-reported yearly income (again six years after first enrollment). While the difference is not statistically significant among students at 4-year institutions, 2-year for-profit college students appear to be learning significantly less than 2-year public college ones. This suggests they will struggle even more to repay their outstanding debt, which is already higher to start with.

4. Regression Analyses

The descriptive analyses of Section 3 highlighted substantial differences in outcomes between for-profit and public college students. If the treatment of beginning postsecondary schooling at a for-profit institution (vs. at a public institution) was orthogonal to other factors which affect the academic and labor market outcomes, taking differences in mean outcomes (such as those presented in above) would yield consistent point estimates. However, Table 2 has shown that selection into for-profit college attendance exists, as the baseline characteristics of students at the two types of institutions differ significantly on average. Therefore, "selection bias" makes it difficult to determine how for-profit attendance affects student outcomes. In particular, students attending for-profit schools may have other attributes that could independently affect later-life outcomes. Randomizing students to either non-profit or for-profit colleges could solve this problem, as this would ensure that the two groups are comparable in all dimensions other than for-profit college attendance. However, such randomization would be very difficult to implement. Similarly, there is no obvious "natural experiment" (Shadish, Cook and Campbell, 2002) setup that mimics random assignment.

In the absence of randomization or a clean natural experiment, the strategy I take to mitigating selection bias is a "selection on observables" approach. In fact, none of the few existing studies on for-profits has presented a superior identification strategy so far. In particular, I use multivariate regression to control for differences between for-profit and public college students. Specifically, I estimate regression models of the form:

$$Y_i = \alpha FP_i + X_i'\gamma + \varepsilon_i,$$

where Y_i is some outcome (e.g., college completion, earnings) for student i and FP_i is an indicator variable for entering college in a for-profit school, X_i is a vector of observable baseline covariates. In particular, I control for age, gender, race, whether the student depends on her parents, has any dependents, and/or is a single parent, whether the student had a job prior to enrollment, high school GPA, high school degree completion, delayed enrollment, part-time enrollment, full-time employment at the time of first enrollment, parents' education and State of the institution. For continuous outcome variables (e.g., debt burden, earnings), I use ordinary least squares regression (OLS). For binary outcomes (e.g., college degree attainment), I use logistic regression.

The advantage of this approach is that it is convenient to implement and the results can be easily interpreted. In the ideal case where the covariate vector X_i contains all of the relevant differences between for-profit and public college students, the other determinants of the outcome (captured by the residual e_i) are uncorrelated with FP_i and the least squares regression estimates of β_1 are consistent for the causal effect of for-profit attendance. If omitted variables that remain after I control for observable factors are correlated with both the outcome and FP_i, the parameter β_1 still can be interpreted as the partial correlation between the outcome and for-profit enrollment "holding constant" the variables in X_i.

Table 4. OLS and Logit Estimates of For-Profit College Attendance "Effects".

Started at a 2-year institution	Employed	Still Enrolled	Earnings	Debt	4-year Degree	2-year Degree
Sample Mean	0.78	0.29	30,323	6,448	0.12	0.17
Average ME	-0.06 ***	-0.12 ***	-2,769 ***	4,532 ***	-0.11 ***	0.04 ***
	0.03	*0.02*	*1,032*	*556*	*0.01*	*0.02*
(Pseudo-) R^2	0.04	0.04	0.06	0.05	0.17	0.05
N	4,304	6,056	3,347	6,057	6,032	6,056
Started at a 4-year institution	**Employed**	**Still Enrolled**	**Earnings**	**Debt**	**4-year Degree**	**2-year Degree**
Sample Mean	0.84	0.33	33,473	11,616	0.58	0.05
Average ME	-0.01	-0.11 ***	87	6,657 ***	-0.28 ***	0.11 ***
	0.03	*0.03*	*1,471*	*1,229*	*0.03*	*0.03*
(Pseudo-) R^2	0.04	0.02	0.09	0.08	0.15	0.12
N	3,311	5,005	2,838	5,006	5,005	4,628

Note. Robust standard errors in Italic. Pseudo-R^2 are reported for the logit models.
For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time enrollment, whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled, state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

Table 4 presents the results of these multivariate regressions of outcomes observed six years after first enrollment. Once selection on observables is accounted for, there still remain significant differences in outcomes between students who start at for-profit and public postsecondary institutions. While several of the differences are smaller in absolute terms than the ones in raw means (Table 3), all statistically significant results retain economic significance too. One notable difference with respect to Table 3 is that starting at a for-profit college did not seem to affect the probability of employment among students who started at 4-year institutions and were no longer enrolled. At a first look it appears as though early labor market outcomes were not affected by for-profit attendance among students at 4-year institutions. At 2-year institutions on the contrary, students who started in for-profit schools were clearly disadvantaged in the labor market, both in terms of finding a job and in self-reported annual earnings.

Results are also mixed when looking at patterns of degree completion, although the interpretation requires some caution. At 2-year institutions, for-profit college students were 4 percentage points more likely than public college ones to have completed a 2-year degree. This is in principle a point scored by 2-year for-profit institutions in terms of bringing their students to completion, but it has to be balanced against the fact that this sample includes students at 2-year public colleges who then transferred to 4-year institutions to complete a bachelor's degree. The negative sign on the point estimate for the average marginal effect of 2-year for-profit college attendance on 4-year degree attainment suggests that students at 2-year for-profit colleges are much less likely to transfer. At 4-year institutions, for-profit college students were 28 percentage points less

likely to have completed a bachelor's degree; however, they were more likely to have completed an associate degree than students who started at public institutions. As discussed in Section 3, my analysis is not conclusive about whether this result depends merely on the fact that more students at 4-year for-profits enroll in 2-year programs than at 4-year public universities or, instead, that more students at 4-year for-profits simply decide to leave with a lower level degree than that for which they had originally aimed.

5. Selection on Unobservables

The results presented in Section 4 suggest that for-profit attendance, as compared to attendance of a public school, has a negative effect on employability and earnings, and a negative effect on bachelor's degree completion at 4-year institutions, but a positive effect on associate degree completion at 2-year institutions. However, it is hard to interpret these results beyond their face value. As with any empirical study, the question remains of whether these point estimates are consistent. Although the BPS data contains rich background information on students, I recognize that in a non-experimental evaluation such as this, there are likely to be confounding factors that could lead to "selection on unobservables". This problem happens when determinants of the outcome that are not observed are correlated with the treatment. This can result in omitted variable bias: erroneously attributing the effects of some of these other correlated factors to the treatment (starting at a for-profit colleges). The direction of omitted variable bias when comparing students at for-profit and public institutions is unclear, but would probably tend to penalize the former in a comparison of outcomes. To address this concern in

absence of a natural experiment, I supplement the regression analyses by borrowing from an insight developed in Altonji, Eder and Taber (2005). The work of these authors explains how the influence of the inclusion of baseline covariates on estimates of α can be informative about the amount of selection bias due to *unobservable* factors.

The essence of this insight is that under some assumptions a relationship can be formalized between selection on observables and selection on unobservables. Once selection on observables is measured, selection on unobservables can be gauged. The assumptions can be described as follows:

(1) the elements of X are chosen at random from the true full set of factors W that determine Y;

(2) the numbers of elements in X and W are large and none of their elements dominates the distributions of FP and Y;

(3) the regression of FP^* on $Y^* - \alpha FP$ is the same as the regression of the part of FP^* that is orthogonal to X on the corresponding part of $Y^* - \alpha FP$.

These assumptions are strong but no more objectionable than the standard OLS assumptions. In fact, 1) and 2) are plausible when working with large-scale survey data. As Altonji, Elder and Taber (2005) describe, many of the factors that compose W are often left out of surveys (due to financial and technical constraints): it is easier to think that X is chosen 'at random' rather than to eliminate bias. 3) is a technical assumption that is weaker than the independence assumption of OLS.

Under 1), 2) and 3) the following holds:

$$\frac{E(\varepsilon|FP = 1) - E(\varepsilon|FP = 0)}{Var(\varepsilon)} = \frac{E(X'\gamma|FP = 1) - E(X'\gamma|FP = 0)}{Var(X'\gamma)}$$

This condition states that "selection on unobservables is the same as selection on observables". This relationship contains information about how much selection on unobservables there would need to be in order for the bias to fully account for $\hat{\alpha}$. It can be shown that $bias(\hat{\alpha}) = \frac{Var(FP)}{Var(\widetilde{FP})} * [E(\varepsilon|FP = 1) - E(\varepsilon|FP = 0)]$, where $\widetilde{FP}$ is the residual of a regression of FP on X. So, following Altonji, Eder and Taber (2005) I take these steps:

a) Estimate γ under $H_0: FP = 0$ (i.e. imposing $\alpha = 0$);

b) Estimate $\frac{E(X'\gamma|FP=1)-E(X'\gamma|FP=0)}{Var(X'\gamma)}$;

c) Estimate the shift in unobservables $E(\varepsilon|FP = 1) - E(\varepsilon|FP = 0)$, under assumptions (1), (2) and (3) above, as $\frac{E(X'\gamma|FP=1)-E(X'\gamma|FP=0)}{Var(X'\gamma)} * Var(\varepsilon)$;

d) Compute $Var(FP)$ from the sample and $Var(\widetilde{FP})$ as the variance of the residual of a regression of FP on X;

e) The ratio $\hat{\alpha}/\left\{\frac{Var(FP)}{Var(\widetilde{FP})} * [E(\varepsilon|FP = 1) - E(\varepsilon|FP = 0)]\right\}$ indicates how big the normalized shift in the distribution of the unobservables would have to be relative to the shift in the observables in order to cancel out the effect of FP.

An example might help clarify the outcome of this procedure. If $\hat{\alpha}/\left\{\frac{Var(FP)}{Var(\widetilde{FP})} * [E(\varepsilon|FP=1) - E(\varepsilon|FP=0)]\right\}$ were equal to 5, it would mean that the effect I found is 5 times bigger than the bias (the normalized selection on unobservables). In other words, it would mean that if the true effect was zero the normalized shift in the unobservables would have to be 5 times as large to cancel out the biased effect I found. The shift in unobservables is normalized by $\frac{Var(FP)}{Var(\widetilde{FP})}$ to get the bias. This translates the shift in unosbersvables (whose variance is represented by the denominator, as that is the part of FP that is not correlated to the observables X) into the scale of the effect $\hat{\alpha}$.

Table 5. The Amount of Selection on Unobservables Relative to Selection on Observables Required to Attribute the Entire For-Profit College Effect to Selection.

Started at a 2-year institution	(1) $\frac{Var(FP)}{Var(\widetilde{FP})} * [E(\varepsilon \vert FP=1) - E(\varepsilon \vert FP=0)]$	(2) $\hat{\alpha}$	(3) Implied Ratio
Employed	0.13	0.70	5.33
Earnings	-268.32	-2,769	10.32
Debt	211.66	4,532	21.41
4-year Degree	0.00	0.11	105.03
2-year Degree	0.06	1.34	23.26
Started at a 4-year institution	(1) $\frac{Var(FP)}{Var(\widetilde{FP})} * [E(\varepsilon \vert FP=1) - E(\varepsilon \vert FP=0)]$	(2) $\hat{\alpha}$	(3) Implied Ratio
Employed	1.75	0.94	0.53
Earnings	-87.26	87	-1.00
Debt	643.36	6,657	10.35
4-year Degree	0.02	0.26	14.06
2-year Degree	0.21	4.33	20.47

The steps and results of the procedure described above are reported in Table 5. The main result is contained in Column (3), which is the ratio of Column (2) to Column (1) and answers the question: how big would selection on unobservables (relative to selection on observables) have to be in order to cancel out the effect of for-profit college attendance on the outcome of interest? Because this is not a statistical test, there is no cutoff against which to compare the values reported in Column (3). However, they can be benchmarked considering two factors. First, a ratio of 1 means that if what I did not know about these students was as relevant to these outcomes as what I knew, my effects would be entirely drawn by selection on unobservables. Therefore, any value greater than 1 is an indication pointing towards the right direction, i.e. that selection on unobservables is less of a concern. Second, while it is plausible to think that there must be unobservable factors that affect these outcomes and are not included in my data, it is harder to imagine that they would represent a very high share of the variation in baseline characteristics. This is because the BPS is a very rich dataset and already contains many of the obvious candidates for controls in a multivariate regression.

With this key to interpretation in mind, the ratios for the debt and graduation outcomes look very large and suggest that it is unlikely that the entire effects can be explained by selection bias. Furthermore, the ratios for employment and earnings among students who started at 2-year institutions (top panel) are higher than 1, so the indication they provide - although much weaker - is also against a 'selection-bias-only' explanation for the effects found. On the other hand, the ratios for employment and earnings among students who started at 4-year institutions (bottom panel) do not pass the critical

threshold of 1, which is consistent with the fact that their respective effects were not significantly different from 0 in my regression models.

One additional piece of unobserved information that might bear implications on the analyses presented in this essay regards the heterogeneous quality of the institutions in my sample. This heterogeneity is at play both within the for-profit sector, whereby some 4-year institutions have earned a respected reputation while others have faced significant negative publicity, but possibly even more strongly within the public sector. More importantly, when drawing comparisons across the two sectors, it might be problematic to include high-quality public institutions (such as state flagships) that do not find matching institutions of similar quality among for-profits. In this case, there could be a bias towards finding negative effects of for-profit attendance. Although the rich set of controls employed in this study should in part alleviate this problem, it is plausible that the analyses of 2-year institutions represent a better comparison than those of 4-year institutions, as the former include a subset of the public sector that is less likely to contain peaks of excellence and more likely to accept comparable students to the ones typically enrolling at for-profits.

6. Policy Implications and Conclusions

At the start of academic year 2003/2004 for-profit institutions accounted for 5.7% of the total college population. Students at such colleges are typically older, and are more likely to be female and/or part of a minority group. They are also more at risk to drop out than the average college student. Differences in outcomes are also significant. When

compared to public colleges, starting at a 2-year institution is negatively associated with the probability of employment (-8% with respect to the population mean at public institutions), with earnings (-9%) and with the probability of advancing and pursuing a 4-year degree (-91%), but positively associated with 2-year degree completion (+25%). Starting at a 4-year institution is negatively associated with 4-year degree completion (-49%). Also, students starting at for-profits accumulate a much larger debt burden than their colleagues at public institutions (+70% at 2-year for-profits and +57% at 4-year for-profits). It is unlikely that the statistically significant effects of for-profit college attendance on debt and completion can be explained solely by selection bias. A similar conclusion can be drawn on the significant effects on labor market outcomes, although the statistical evidence supporting this claim is weaker. As a result, at least part of these effects shall be treated as causal.

My analyses acknowledge the role of for-profit institutions in expanding the supply of higher education to underserved segments of the population. This is true both because students with disadvantaged backgrounds might have traditionally found it harder to access postsecondary education and because more recently budget cuts have significantly reduced the number of courses offered at public institutions. On the other hand, my results suggest that students who start at for-profit institutions are often at disadvantage relative to their counterparts at public schools, in terms of debt (when starting both at 2-year and at 4-year institutions), academic achievement (when starting at 4-year institutions) and early labor market outcomes (when starting at 2-year institutions). This is an indication that on average for-profit institutions will be less likely to meet the

thresholds for income-to-debt ratio and loan repayment rates recently set by the U.S. government. In a consistent fashion, a recent report by Charles River Associates found that “18 percent of for-profit postsecondary programs would not satisfy the debt limit requirement of the gainful employment proposal” and “33 percent of students in for-profit postsecondary programs would be impacted” (Guryan and Thompson, 2010).

Moreover, the difficulty that students who start at for-profit institutions face repaying their loans seriously calls into question the efficiency of the entire business model from a perspective of societal welfare. In fact, the risk appears to be at least in part shifted away from the entrepreneurs when federal student loans represent a relevant source of revenue for these institutions. The risk of a student’s default is born by the government and, ultimately, by the taxpayers whose money is at stake. This cycle results in an inefficient allocation of resources similar to a subsidy. The results presented here ultimately pertain to a problem of optimal level of investment in public higher education. This study is only able to provide a snapshot of the outcomes of the expansion of the for-profit sector. However, these analyses can be an important piece of information to consider when evaluating spending cuts in public higher education. Budget reductions yield immediate savings and undoubtedly benefit public finances in the short run, but the for-profit model alternative to public provision is only apparently costing no money to the taxpayer. In fact, the cost of defaults on student loans has to be taken into account, as well as the potential longer-run harms caused by frictions faced by students once they enter the labor market.

It is important to emphasize that this study does not constitute a comprehensive cost-benefit analysis. While the much higher cost of attending for-profit institutions versus public colleges would only reinforce the generally negative effects of for-profit college attendance, aspects such as the more flexible "educational supply chain" of for-profit college students might instead represent efficiency gains. Another important limitation of this study lies in its inability, due to the survey design, to analyze the very relevant margin represented by those individuals who decide not to enroll in higher education. Future research in this field will have to try to address the question of whether for-profit college attendance is better than no college attendance at all. Furthermore, efforts will be required to expand this line of research beyond the United States, as significant international trends have already been documented (Kinser and Levy, 2006). Developing countries such as Brazil (Douglass, 2012) have seen a significant rise of similar types of institutions to for-profits in recent years, and the natural question is whether they can play a role in increasing human capital, given that concerns have been expressed about quality (Council for Higher Education Accreditation and United Nations Educational, 2011). Finally, the renewed need for fiscal discipline in the European Union, and in the Euro countries in particular, will cause major cuts to public investments in higher education in the coming years – the question again will be whether private for-profit institutions are an efficient and sustainable solution to fill that possible gap.

Appendix: Robustness Checks

This Appendix presents the results of alternative model specifications.

Firstly, I examine alternative definitions of labor market outcomes to the one presented in Table 4. Table 9 shows that for-profit students, six years after starting, are less likely to be still enrolled and to be employed or enrolled (i.e. more likely to be idle).

Secondly, I employ multinomial logits to study graduation and labor market outcomes to test the robustness of the results obtained with binary models. Table 7 reports results for an outcome defined as 4-year degree vs. 2-year degree vs. certificate vs. no degree, which confirm the patterns presented in Table 4 (for-profit students starting at 2-year institutions are more likely to complete a 2-year degree, for-profit students starting at 4-year institutions are less likely to complete a 4-year degree); Table 7 also adds the interesting result that for-profit students starting at 2-year institutions are more likely to complete a certificate degree. Table 10 reports results for an outcome defined as out of school and employed vs. out of school and unemployed vs. still in school: with respect to being out of school and not employed, for-profit students are relatively less likely than students in public schools to be still enrolled; for-profit students starting at 2-year institutions are also relatively less likely to be out of school and employed.

Furthermore, all the models presented in Table 6 through Table 10 report results both with and without analysis weights. The use of analysis weights allows to account for panel nonresponse and to ensure a calibrated population coverage (Wine, Janson and Wheeless, 2011). The results are generally robust to the use of analysis weights. In particular, the implied ratios discussed in the section on selection on unobservables stay

at comparable orders of magnitude for the main results: 2-year degrees (among students starting at 2-year institutions) and 4-year degrees (among students starting at 4-year institutions) in Table 6, debt in Table 8 and employment (conditional on being out of school) in Table 9.

Table 6. Logit Estimates of For-Profit College Effects on Academic Outcomes in 2009.

		4-year Degree *unweighted*	4-year Degree *weighted*	2-year Degree *unweighted*	2-year Degree *weighted*
Started at a 2-year institution	Sample Mean	0.12	0.10	0.17	0.15
	Odds Ratio	0.11 ***	0.04 ***	1.34 **	1.42 **
	Implied Ratio	105.03	-5.44	23.26	31.51
	Pseudo R^2	0.17		0.05	
	N	6,032	6,032	6,056	6,056
Started at a 4-year institution	Sample Mean	0.58	0.55	0.05	0.06
	Odds Ratio	0.26 ***	0.29 ***	4.33 ***	3.58 ***
	Implied Ratio	14.06	-5.86	20.47	1.27
	Pseudo R^2	0.15		0.12	
	N	5,005	5,005	4,628	4,628

Note. Robust standard errors for the unweigthed models.
For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time enrollment, whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled, state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

Table 7. Multinomial Logit Estimates of For-Profit College Effects on Academic Outcomes in 2009.

		4-year Degree *unweighted*	4-year Degree *weighted*	2-year Degree *unweighted*	2-year Degree *weighted*
Started at a 2-year institution	RRR w.r.t. no degree	0.13 ***	0.05 ***	1.33 **	1.38 **
	Pseudo R^2	0.10		0.10	
	N	6,057	6,057	6,057	6,057
Started at a 4-year institution	RRR w.r.t. no degree	0.31 ***	0.33 ***	2.72 ***	2.30 ***
	Pseudo R^2	0.15		0.15	
	N	5,006	5,006	5,006	5,006

		Certificate Degree *unweighted*	Certificate Degree *weighted*
Started at a 2-year institution	RRR w.r.t. no degree	2.40 ***	2.50 ***
	Pseudo R^2	0.10	
	N	6,057	6,057
Started at a 4-year institution	RRR w.r.t. no degree	0.85	0.74
	Pseudo R^2	0.15	
	N	5,006	5,006

For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time enrollment, whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled, state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

Table 8. OLS Estimates of For-Profit College Effects on Debt.

		$ Owed in 2009 *unweighted*	$ Owed in 2009 *weighted*
Started at a 2-year institution	Sample Mean	6,448	5,477
	Marginal effect	4,532 ***	4,815 ***
	Implied Ratio	21.41	24.20
	R^2	0.05	0.07
	N	6,057	6,057
Started at a 4-year institution	Sample Mean	11,616	11,144
	Marginal effect	6,657 ***	6,935 ***
	Implied Ratio	10.35	14.41
	R^2	0.08	0.08
	N	5,006	5,006

Note. Robust standard errors for the unweigthed models.
For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time enrollment, whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled, state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

Table 9. OLS and Logit Estimates of For-Profit College Effects on Labor Market Outcomes in 2009.

		Employed (if out of school) *unweighted*	Employed (if out of school) *weighted*	Still enrolled *unweighted*	Still enrolled *weighted*
Started at a 2-year institution	Sample Mean	0.78	0.78	0.29	0.28
	Odds Ratio	0.70 ***	0.63 ***	0.51 ***	0.52 ***
	Implied Ratio	5.33	-30.66	9.04	-3.70
	R^2	0.04		0.04	
	N	4,304	4,304	6,056	6,056
Started at a 4-year institution	Sample Mean	0.84	0.85	0.33	0.32
	Odds Ratio	0.94	1.03	0.58 ***	0.47 ***
	Implied Ratio	0.53	-10.87	2.82	-1.59
	(Pseudo-) R^2	0.04		0.02	
	N	3,311	3,311	5,005	5,005

		Employed or enrolled *unweighted*		Employed or enrolled *weighted*		Employed *unweighted*		Employed *weighted*	
Started at a 2-year institution	Sample Mean	0.84		0.84		0.55		0.56	
	Odds Ratio	0.61	***	0.55	***	1.18		1.10	
	Implied Ratio	5.68		-17.27		12.11		10.21	
	R^2	0.04				0.02			
	N	6,056		6,056		6,051		6,051	
Started at a 4-year institution	Sample Mean	0.90		0.90		0.57		0.58	
	Odds Ratio	0.81		0.85		1.37	**	1.63	***
	Implied Ratio	1.63		-9.52		6.35		5.12	
	(Pseudo-) R^2	0.04				0.02			
	N	4,936		4,936		5,005		5,005	

		Annual income *unweighted*	Annual income *weighted*
Started at a 2-year institution	Sample Mean	30,323	31,331
	Marginal Effect	-2,769.41 ***	-4,934.83 ***
	Implied Ratio	10.32	-726.40
	R^2	0.06	0.10
	N	3,347	3,347
Started at a 4-year institution	Sample Mean	33,473	33,544
	Marginal Effect	87.24	-887.67
	Implied Ratio	-1.00	-364.32
	(Pseudo-) R^2	0.09	0.11
	N	2,838	2,838

Note. Robust standard errors for the unweigthed models.
For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

Table 10. Multinomial Logit Estimates of For-Profit College Effects on Labor Market Outcomes in 2009.

		Still in school *unweighted*	Still in school *weighted*	Out of school, employed *unweighted*	Out of school, employed *weighted*
Started at a 2-year institution	RRR w.r.t. out of school, not employed	0.40 ***	0.37 ***	0.73 **	0.64 **
	R^2	0.04		0.04	
	N	6,057	6,057	6,057	6,057
Started at a 4-year institution	RRR w.r.t. out of school, not employed	0.56 **	0.49 ***	0.95	1.04
	R^2	0.03		0.03	
	N	5,006	5,006	5,006	5,006

Note. P-values based on robust standard errors for the unweigthed models.
For-profit college attendance is equal to 1 if the first attended institution is a for-profit college, 0 if it is a public college.
Controls: age, gender, race, parent's education, high school GPA, delayed enrollment, no high school degree, part-time enrollment, whether the student is dependent, whether the student has dependents, single parent status, full-time job while enrolled, state of first postsecondary institution fixed-effects.
*** p < .10, ** *p* < .05, *** p < .01

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Essay 2: The Effect of Military Enlistment on Education[13]

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Abstract

Applicants for volunteer military service face a tradeoff between enlistment and education often at an early stage of their adulthood, an important decision that will have implications for their labor market trajectories. The Armed Forces provide significant educational benefits to their members and veterans, but evidence is scarce concerning their effects. In order to investigate the effects of enlistment on educational outcomes over the lifecycle, we assemble data on cohorts of military applicants and supplement it with records on college enrollment and graduation. We compare outcomes of veterans to non-veterans who applied to join the military and are similar on the information that the military uses to screen applicants. We find that enlistees delay college but eventually enroll at comparable rates to similar non-veterans. Enlistment positively impacts degree attainment at two-year institutions and negatively impacts degree attainment at four-year ones. Also, high-aptitude individuals suffer from much larger

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negative effects on enrollment in the short run than their lower-aptitude peers, and subsequently in the long run are the only group displaying negative effects on four-year degree attainment. On the other hand, Whites suffer from less negative effects on four-year degree attainment than African-Americans. Finally, in the long run Military service shifts Air-Force enlistees towards degrees at two-year colleges more than within any other Service.

1. Background and Motivation

The decision of whether to enlist for volunteer military service is highly intertwined with education, both from an individual and from a public policy perspective. Returns to college attendance are an important determinant of this choice, and macroeconomic fluctuations in labor market conditions are related to the ease with which the armed forces meet their recruitment target and strategically allocate resources to educational benefits (Asch et al., 1999b). At the micro level, research has documented the importance of college aspirations for enlistment (Kleykamp, 2006). However, in the short run, even for individuals who aspire to a degree, military enlistment acts as an alternative to higher education. Military service is competing with colleges for talent (Asch et al., 1999a), and in fact service members typically enlist directly after high school and serve at a time when their peers are most likely to be in college. Following service, supplementing human capital through higher education is one of the aids to a smooth return to the civilian labor market (Schmitz, Dale and Drisko, 1988). In this context, policies are in place to facilitate enrollment among enlisted personnel, in the form of educational benefits including tuition assistance and loan repayment programs, and veterans, who receive financial support under the Montgomery GI Bill — Active Duty (1984) and the Post-9/11 Veterans Education Assistance Improvements Act of 2010 (Post-9/11 GI Bill). The Government

Accountability Office reported in May 2011 that the Department of Veterans Affairs had distributed over $5.7 billion within the framework of the Post-9/11 GI Bill, and projected an additional $8 billion for fiscal year 2011[14]; the total cost of the program is expected to reach $90 billion over 15 years[15]. With respect to the Montgomery GI Bill, which provided $1,321 per month (Radford and Weko, 2011), the Post-9/11 GI Bill includes a more generous full payment of tuition and fees, together with a housing allowance and a stipend for books and supplies. The number of beneficiaries of this program has been calculated to be as high as 817,000 as of January 2013[16]. Altogether, these policies create the potential for military service as a pathway to higher education. Highlighting the importance of this mechanism, a recent study of the experiences of the Post-9/11 GI Bill beneficiaries found that around one quarter of their sample considered those benefits a major factor in their decision to enroll (Steele et al., 2010). In a context of rising costs of college attendance (Snyder and Dillow, 2012, table 349), the appeal of military educational benefits can be particularly prominent among lower-aptitude youth who do not qualify for merit-based scholarships and cannot afford tuition and living expenses, even with need-based financial aid. As a survey respondent from a cohort of high school graduates from the State of Texas in 2002 described it, “the military is the “next best thing to college.”” (Kleykamp, 2006, p. 286).

Even in the presence of these facilitating conditions for enrollment, graduation is not an outcome directly targeted by the aforementioned policies. Some recent news reports have

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[14] http://161.203.16.70/assets/100/97478.pdf (as of 11/2/2013).

[15] http://chronicle.com/article/As-GI-Bill-Expands-So-Do/136241/ (as of 11/2/2013).

[16] http://www.huffingtonpost.com/2013/01/10/veterans-in-college_n_2447426.html (as of 11/2/2013).

speculated that as many as 88% of veterans drop out of higher education[17], and the graduation rate for returning veterans at four-year universities has been placed at 3% (Cunningham, 2012). On the other end, a recent research brief by Student Veterans of America[18] has challenged these figures, suggesting that 68% of the sample in the 2010 National Survey of Veterans had reported to have completed the training or received the primary degree or certificate for which they were enrolled and receiving benefits from the Department of Veteran Affairs. This mixed evidence on the educational outcomes of veterans has spurred political focus on the efficiency and effectiveness of the Post-9/11 GI Bill, and in April 2012 President Obama signed an executive order that among other things contained a provision for a national-level reporting system of graduation rates of service members and veterans. These concerns are grounded in the inherent dropout risks associated with nontraditional students, to which veterans belong almost by definition. In fact, research has shown instances where higher education institutions have not proven fully ready to cater to their specific needs (O'Herrin, 2011; Steele et al., 2010).

In light of these policy developments, the actual impact of military service on educational outcomes remains an empirical question. Our goal is to go beyond the tradeoff between enlistment as an alternative or a pathway to education, and estimate the effect of volunteer military service on college enrollment and graduation. We aim at informing the policy debate by providing a fresh analytical perspective. On one hand, we are interested in comparing the educational outcomes of enlisted versus non-enlisted applicants, thus avoiding potentially confounding factors that are inherent to any benchmarking of statistics of veterans vis-à-vis the

[17] http://usnews.nbcnews.com/_news/2012/07/02/12509343-thousands-of-veterans-failing-in-latest-battlefield-college?lite (as of 11/2/2013).

[18] http://studentveterans.org/images/Documents/Research_Brief_2013_1.pdf (as of 11/2/2013).

civilian population. On the other hand, we would like to track veterans and non-veterans for several years after their application for volunteer military service, thus accommodating for the different timings of enrollment between the two groups.

A few existing studies investigated the effects of enlistment on educational outcomes. Yu presents data from the 1964 through 1984 March Current Population Surveys, and shows how, for a given cohort, there is a negative effect of enlistment on educational attainment at young ages, but this more than compensated by the late twenties to early thirties (1992), reinforcing our motivation for a lifecycle type of analysis. Yu also cites a number of earlier studies that had all come to similar conclusions, and proposed two possible explanations for the observed patterns: alongside with the introduction of educational benefits, some suggested that "military life [could] inspire veteran's educational aspiration" (1992, p. 388). Also, using data from the 1987 Survey of Veterans, Angrist estimated an increase of 1.4 years in post-service education associated with the use of educational benefits (1993).

2. Data

We obtained data on military applicants from Department of Defense Military Entrance Processing Command (MEPCOM) administrative records. These data files contain electronic records for every individual who submits a formal application for active-component military service. The extract we employ covers the universe of individuals who applied for military service between fiscal year 1989 and fiscal year 2003. We restrict our attention to the typical qualified applicant: individuals who were 17 and older at the time they applied for military service, had no prior military service, obtained a score of 31 or higher on the Armed Forces Qualification Test (AFQT, very few individuals who score below this percentile are admitted to

the military), had at most a high school diploma (excluding those with varying levels of postsecondary education), and had no potentially disqualifying health conditions or potentially disqualifying drug or alcohol use[19]. These sample restrictions leave us with 45 percent of the total universe of individuals applying for military service in those years[20].For each applicant, the MEPCOM data include measures of the key factors the military uses to screen applicants at the time of application. In addition to the screening criteria, these measures include AFQT score (a powerful predictor of labor market earnings - see, for example, Neal and Johnson (1996))[21] and educational attainment. The application record also contains standard demographic information, such as gender, race/ethnicity, date of application, and active-component service to which the individual applied. We define an applicant as having enlisted if, according to MEPCOM records, that individual accesses following his or her application date. To "access" means that the military inducts the individual into military service. An enlistee, by this definition, could serve as little as a single day in the active component, although 92 percent of applicants in our data serve at least six months and 70 percent serve three or more years. Thus, our estimates represent the average effect of serving in the active component, regardless of how long, on educational outcomes in a given year following application. It is also important to remember that a sizable fraction of

[19] Approximately 23 percent of the applicant records are missing health and drug and alcohol information. These records were dropped from the analysis.

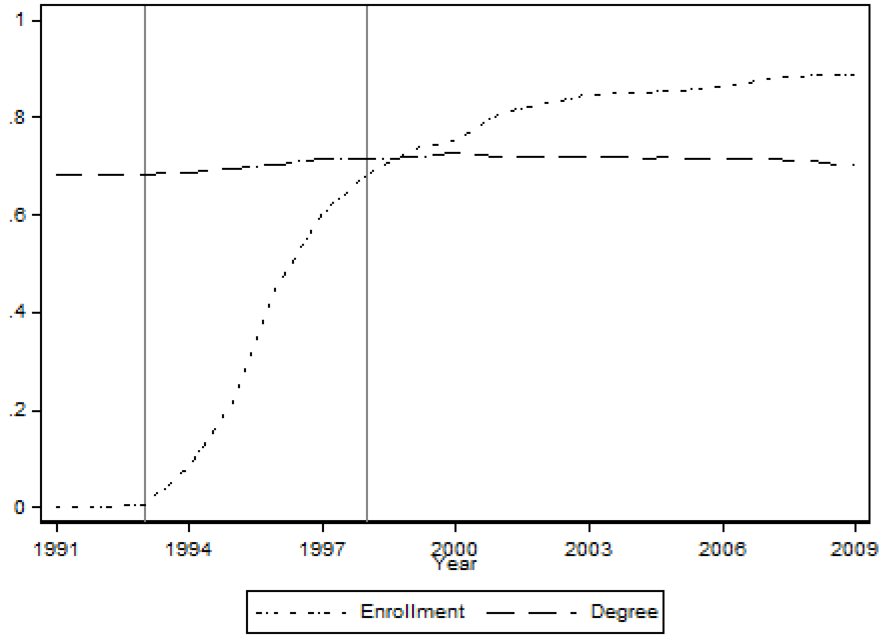
[20] Approximately 10 percent of the sample has two or more application records indicating that their first application was suspended. This could happen either because the applicant decided to withdraw his or her application or because the applicant did not meet enlistment criteria at that time. For individuals who decide to apply again at a later date, we apply sample restrictions and measure all covariates at the time of that individual's last application record in the MEPCOM data.

[21] AFQT scores are derived from selected scores on the eight-component Armed Services Vocational Aptitude Battery (ASVAB): Paragraph Comprehension (PC), Word Knowledge (WK), Mathematics Knowledge (MK), and Arithmetic Reasoning (AR).

active-component enlistees will continue to serve in the reserve components and so, separation from active-component service does not necessarily mean separation from military service altogether.

Our education data come from the National Student Clearinghouse (NSC). Founded in 1993, NSC is a nonprofit organization that contracts with institutions of higher education to verify college enrollment and degree receipt for student loan agencies. The NSC data allow us to track military applicants as they transition in and out of college and complete college degrees. NSC maintains college enrollment data for institutions in years in which they had an active contract with NSC. Between 1993 and 2009, NSC's coverage of college enrollment grew from 13 to 89 percent of all college enrollments (Figure 1). NSC also maintains a degree verification service for participating institutions. In 2009, about 70 percent of all U.S. colleges participated in this service. Participating institutions submit electronic degree records for all available years. Consequently, in earlier years, coverage of college degrees is more complete than coverage of college enrollment. NSC is able to verify about 68 percent of all degrees awarded by U.S. colleges in 1991, the earliest applicant cohort employed in these analyses.

Figure 1: Percentage of U.S. College Enrollment and Degrees Awarded Covered by NSC Data, by Year



Incomplete enrollment and degree coverage in the NSC data has implications for our analysis and sample selection. First, in choosing applicant cohorts for analysis, we face a tradeoff between data coverage and the ability to observe completed college enrollment and degree attainment. Selecting earlier applicant cohorts increases the likelihood that we will observe completed college enrollment and degree attainment. However, the NSC data omit a larger fraction of enrollments and degrees attained in the earlier years of this sample. Selecting later applicant cohorts provides better coverage but allows us fewer years to observe completed enrollment and degree attainment. This trade-off is much more pronounced for college enrollment outcomes, the coverage of which increased sharply between 1993 and 2009. Second, since we expect enlistees to delay college enrollment relative to non-enlistees, we must restrict the NSC data to colleges that are in every year of our sample so that, a priori, enlistees and non-enlistees have equal opportunity to appear as enrolled in these data. If we were to define college

enrollment in a given year as being enrolled in any institution in the NSC data, we would tend to undercount college enrollment of non-enlistees relative to enlistees. This is because enlistees are likely to enroll at a later date when NSC enrollment coverage is more complete. Based on the above considerations and the cost of obtaining data from NSC, we restrict our sample as follows. When examining college enrollment, we restrict our sample to the 1998–2000 applicant cohorts. When examining college degree attainment, we restrict our sample to the 1991–1994 applicant cohorts. Enrollment is defined as enrolling in a college that began contracting with NSC by 1998. Since the coverage rates of degree data are constant over time, degree attainment is defined as attaining a degree from a college contracting with NSC by 1993.

Employing these sample restrictions we measure 68 percent of all college enrollments in the first year of potential enrollment in our sample (1998) and 68 percent of all awarded college degrees in 1993. These statistics imply that we underestimate college enrollment and degree attainment by approximately one-third. This underestimation poses a problem for our estimates of the effect of enlistment on education only insofar as applicants who do and do not enlist are more or less likely to attend and receive degrees from the colleges that are not in our sample. One possible concern is the fact that the NSC data cover a lower percentage of enrollments at for-profit colleges (44 percent at four-year for-profits in 1998). The evidence suggests that veterans have a high propensity to enroll at for-profit institutions (Kutz, 2010; Steele et al., 2012), which would cause us to underestimate the impact of enlistment on college enrollment and completion overall.

However, there is little reason to believe there would be differential enrollment at particular institutions within a given type (e.g., two-year versus four-year institutions), especially once we condition on applicant characteristics. Thus, while the means of our educational outcomes are

likely to be biased downward, we assume that the difference in these outcomes between enlistees and non-enlistees, conditional on applicant covariates, is an unbiased estimate of the causal effect of enlistment on education within a given college type. Cost considerations prohibited us from obtaining data for the entire population of applicants. Therefore, we obtained NSC data on enrollments and degree attainment for a sample of 120,000 male applicants in the 1991–1994 cohorts and a sample of 120,000 male applicants in the 1998–2000 cohorts (for reasons of cost, we did not purchase NSC data for female applicants). In order to ensure a large enough sample to detect reasonable effect sizes for well-defined subgroups, we stratified our sample by race and AFQT category, over-sampling high aptitude Hispanics and African Americans while under-sampling low-aptitude white applicants. We also selected our sample so that half of it consists of Army applicants and the other half consists of applicants to the other three active component services.

3. Descriptive Statistics

Table 11 reports sample characteristics. Roughly half of our sample across both sets of cohorts considered enlisted. The racial composition is quite balanced across sets of cohorts as well as between enlistees and non-enlistees: roughly half of the sample is white, one quarter black and one quarter Hispanic[22]. Educational attainment is also evenly distributed, although there is a slightly higher prevalence of less-than-high-school achievers among non-enlistees. 17 and 18-years-old account for around two thirds of our sample, and there appears to be some tendency for the later cohorts to apply younger on average. Finally, one third of our sample is

[22] As described above, high-aptitude Hispanics and black individuals were oversampled.

represented by high-aptitude individuals, as measured by the AFQT score, and one third covers the lower-aptitude categories included in this study.

Table 12 and Table 13 present mean values of educational outcomes of interest for military applicants. As one could expect, enrollment happens at different time for enlistees than for non-enlistees; the former tend to be enrolled later on after application, and while among non-enlistees the share of individuals currently enrolled starts declining as soon as 2 years after application, it does so only 7 years later among enlistees (Figure 2). As a result, eventually a higher share of them has been enrolled at least once 11 years out (Figure 3). As regards degree attainment, starting from 8 years after application 2-year degrees are more prevalent among enlistees (Figure 4); however, 4-year degrees are more prevalent among non-enlistees all the way through 18 years after application, even though enlistees progressively close a gap which is widest 5 years after application (Figure 5, consistent with the observation that two thirds of individuals who do not enlist and enroll do so in the 2 years following application). The next section discusses our empirical strategy to try to determine whether some of these gaps are causally attributable to enlistment.

Table 11: Sample Characteristics.

	1991-1994 Cohorts			1998-2000 Cohorts		
	All	Non-Enlistees	Enlistees	All	Non-Enlistees	Enlistees
Enlisted	0.51	0.00	1.00	0.51	0.00	1.00
Race/ethnicity						
White	0.50	0.51	0.49	0.50	0.51	0.49
Black	0.25	0.26	0.25	0.25	0.26	0.25
Hispanic	0.25	0.23	0.26	0.25	0.23	0.26
Education						
Less than High School	0.50	0.54	0.46	0.48	0.55	0.41
GED	0.04	0.05	0.03	0.09	0.10	0.09
High School	0.45	0.40	0.49	0.41	0.34	0.48
Age						
17	0.32	0.34	0.30	0.36	0.36	0.35
18	0.32	0.32	0.33	0.33	0.33	0.33
29	0.22	0.20	0.23	0.19	0.19	0.20
20	0.14	0.13	0.14	0.12	0.12	0.12
Service						
Army	0.50	0.49	0.51	0.50	0.49	0.51
Air Force	0.10	0.08	0.11	0.11	0.10	0.12
Marine Corps	0.16	0.17	0.15	0.18	0.18	0.18
Navy	0.24	0.26	0.23	0.22	0.24	0.20
AFQT Category						
I or II	0.33	0.33	0.33	0.33	0.32	0.33
IIIA	0.33	0.33	0.34	0.34	0.33	0.34
IIIB	0.33	0.34	0.33	0.34	0.34	0.33
N	114,827	56,294	58,533	114,564	55,961	58,603

Table 12: Mean Enrollment Outcomes.

Years since Application	Outcome: Current Enrollment			Cumulative Enrollment		
	Sample:			Sample:		
	All	Non-Enlistees	Enlistees	All	Non-Enlistees	Enlistees
0	0.11	0.14	0.05	0.11	0.14	0.09
1	0.13	0.20	0.07	0.19	0.25	0.13
2	0.14	0.21	0.09	0.23	0.30	0.17
3	0.14	0.19	0.11	0.27	0.33	0.21
4	0.14	0.17	0.14	0.30	0.36	0.25
5	0.14	0.15	0.15	0.34	0.38	0.30
6	0.14	0.13	0.15	0.37	0.39	0.35
7	0.13	0.11	0.15	0.39	0.41	0.38
8	0.12	0.10	0.15	0.42	0.42	0.41
9	0.12	0.09	0.09	0.44	0.43	0.44
10	0.07	0.05	0.04	0.45	0.44	0.45
11	0.04	0.03	0.09	0.45	0.45	0.46

Figure 2: Current Enrollment over Time.

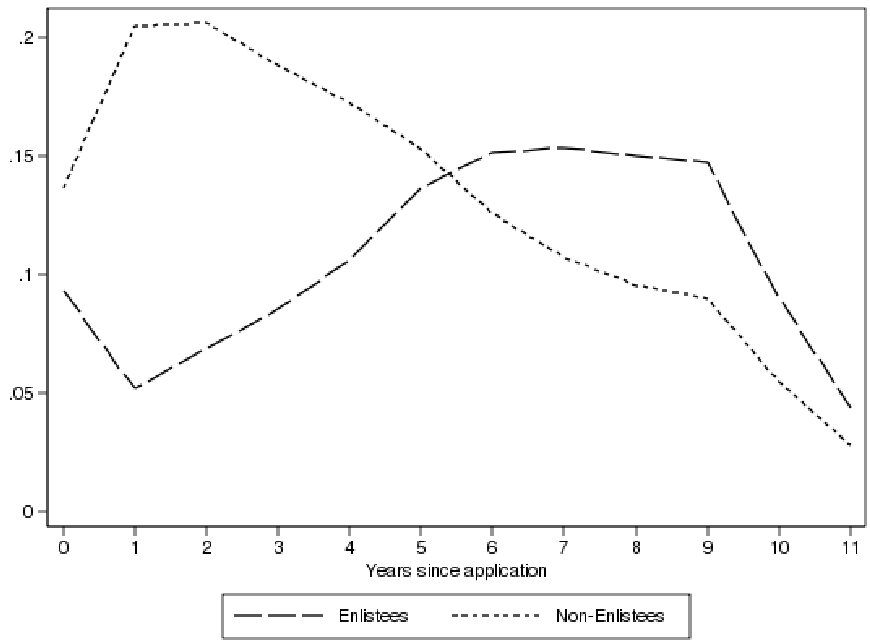


Figure 3: Cumulative Enrollment over Time.

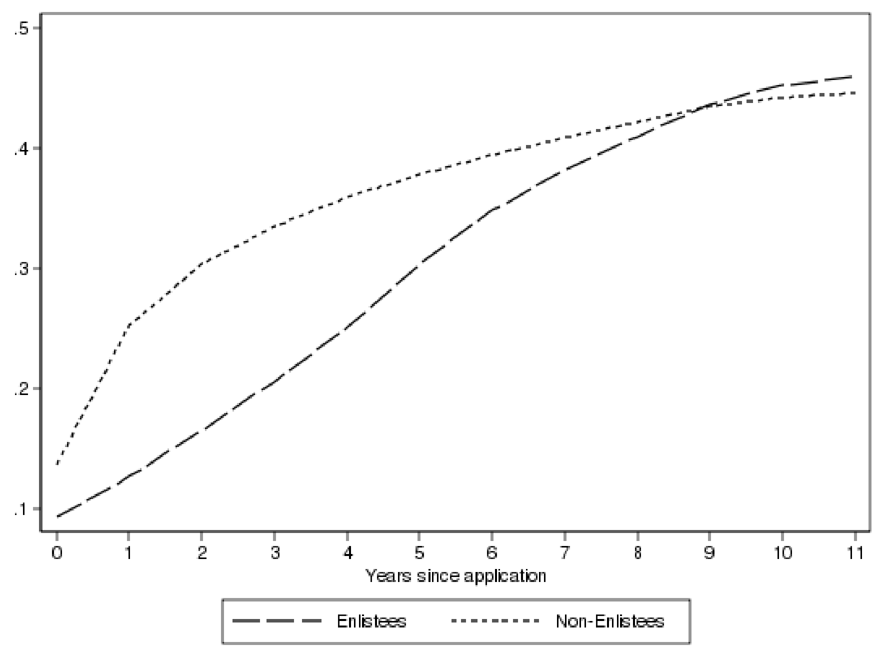


Table 13: Mean Degree Outcomes.

Years since Application	Outcome: Cum. 2-Year Degree Attainment			Outcome: Cum. 4-Year Degree Attainment		
	Sample:			Sample:		
	All	Non-Enlistees	Enlistees	All	Non-Enlistees	Enlistees
2	0.00	0.00	0.00	0.00	0.00	0.00
3	0.01	0.01	0.00	0.00	0.01	0.00
4	0.01	0.01	0.00	0.01	0.02	0.00
5	0.01	0.02	0.01	0.02	0.03	0.00
6	0.02	0.02	0.01	0.03	0.05	0.01
7	0.03	0.03	0.02	0.04	0.06	0.01
8	0.03	0.03	0.03	0.05	0.07	0.02
9	0.04	0.03	0.04	0.06	0.08	0.03
10	0.04	0.04	0.04	0.06	0.09	0.04
11	0.04	0.04	0.05	0.07	0.09	0.05
12	0.05	0.04	0.05	0.08	0.10	0.06
13	0.05	0.04	0.06	0.08	0.10	0.07
14	0.05	0.04	0.06	0.09	0.10	0.08
15	0.06	0.05	0.07	0.09	0.11	0.08
16	0.06	0.05	0.07	0.10	0.11	0.09
17	0.06	0.05	0.07	0.10	0.11	0.09
18	0.06	0.05	0.07	0.10	0.11	0.09

Figure 4: Mean Cumulative 2-Year Degree Attainment over Time.

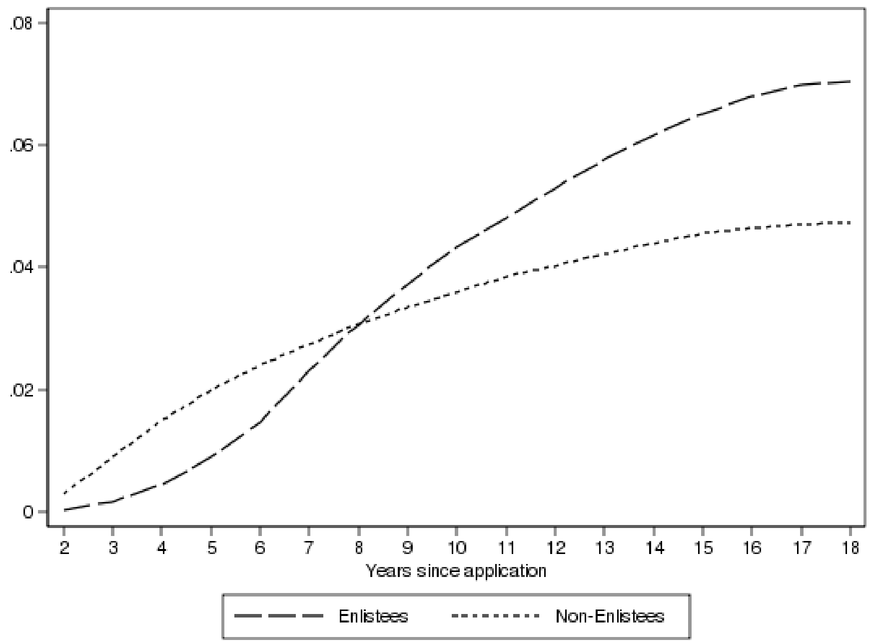
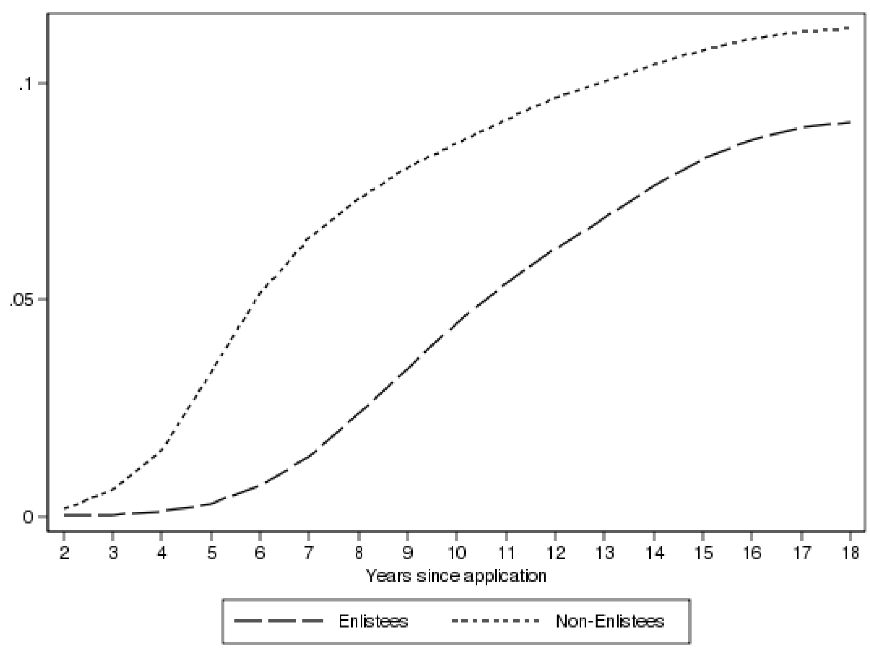


Figure 5: Mean Cumulative 4-Year Degree Attainment over Time.



4. Empirical Strategy

For a given individual, the causal effect of enlistment on a particular educational outcome can be expressed as

$$R_{it} = Y_{it}^{E} - Y_{it}^{NE}, (1)$$

where Y_{it}^{E} denotes the outcome of applicant i in the t^{th} year since applying for military service if the applicant enlists, and Y_{it}^{NE} denotes the same outcome if the applicant never enlists. Since, for any individual applicant, it is impossible to observe the difference R_{it} (i.e., an applicant cannot both enlist and never enlist), we must estimate this difference using data on a population of individuals that enlists and a population of individuals that does not enlist. The average effect of enlistment on a population of enlistees (generically referred to as the effect of "treatment on the treated") can be expressed as:

$$R_{t}^{E} = E(Y_{it}^{E}|D_i = 1) - E(Y_{it}^{NE}|D_i = 1), (2)$$

where D_i is an indicator variable for enlistment. We can generate an unbiased estimate of the first term on the right-hand side of Equation 2 using data on enlistees. It is likely, however, that using data on individuals who never enlist will result in biased estimates of the second term on the right-hand side of Equation 2, the mean outcome of enlistees had they never enlisted (i.e., the counterfactual). This bias results from the fact that individuals choose to enlist in the military and the military chooses which applicants can enlist, and these choices are likely conditional on characteristics of individuals correlated with the outcomes of interest. For example, enlistment might be relatively more common among individuals with a high tolerance for risk, and the military requires enlistees to meet specific aptitude, health, drug and alcohol, and other requirements. Comparisons of mean outcomes made conditional on the characteristics of

individuals that determine enlisted status yield a causal estimate of the effect of enlistment on outcomes if the distribution of potential outcomes is unrelated to enlistment conditional on covariates included in the model. Formally, if we assume the pair $(Y_{it,}^{E} Y_{it,}^{NE})$ is independent from $D_{it}|X_{it}$ for some vector of covariates X, then

$$R_t^E = E(Y_{it}^E|D_i = 1, X_{it}) - E(Y_{it}^{NE}|D_i = 0, X_{it}), (3)$$

which we can estimate directly from data on enlistees and non-enlistees, is an unbiased estimate of the causal effect of enlistment on outcome Y. The key assumption in Equation 3 is that the vector X contains all factors that co-vary with enlistment and outcome Y. We argue, as in Angrist (1998), that restricting our sample to military applicants and employing the rich data on the applicant record make this assumption plausible for our purposes. It is reasonable to assume that enlistees will be more similar to applicants who do not enlist than to individuals in the general population. This is likely to be true in terms of both observable characteristics, such as age, gender, and education, and unobservable characteristics, such as attitudes toward risk and authority. By restricting our sample to applicants, we implicitly control in X for differences in observable and unobservable characteristics across applicants and non-applicants. Within the pool of applicants, there are likely to remain important differences between applicants who do and do not enlist, but we assume that we can control for these remaining differences by employing data available in the applicant record (see section "Limitations of our approach" for more discussion of this particular assumption). Thus, focusing our analysis on applicants allows us to control more completely for differences between enlistees and non-enlistees in the population at large and therefore improve our estimate of the causal effect of enlistment on education in the general population.

We estimate the effect of enlistment on educational outcomes employing the following probit

model:

$$prob(Y_{it} = 1) = prob(\alpha_t + \beta_t D_i + X_i \theta_t + \varepsilon_{it} > 0), (4)$$

where Y_{it} is an indicator for whether applicant i was enrolled (or had ever enrolled) or obtained a college degree in the t^{th} year following application, D_i is an indicator for whether the applicant enlisted, X_i is a vector of applicant characteristics (percentile of AFQT score, age, education and race/ethnicity) and ε_{it} is an idiosyncratic, normally distributed error term. All of our regression analysis estimates are weighted by the appropriate sample weights.

5. Results

The following section presents the results with a set of graphics. The underlying point estimates are reported in Table 14, Table 15 and Table 16.

Enrollment

Figure 6 graphs the estimated effect of military enlistment on current college enrollment by years since application for the 1998–2000 applicant cohorts[23].

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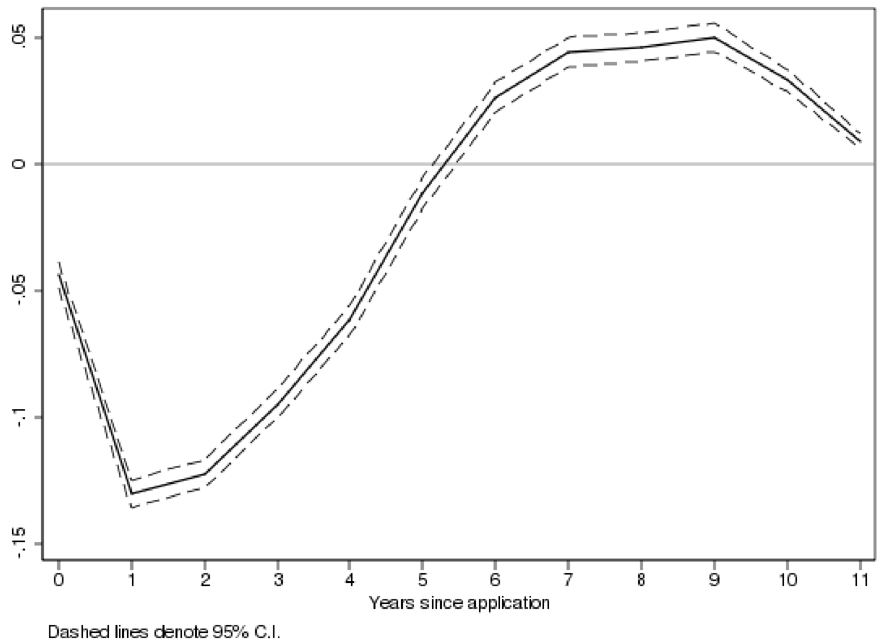
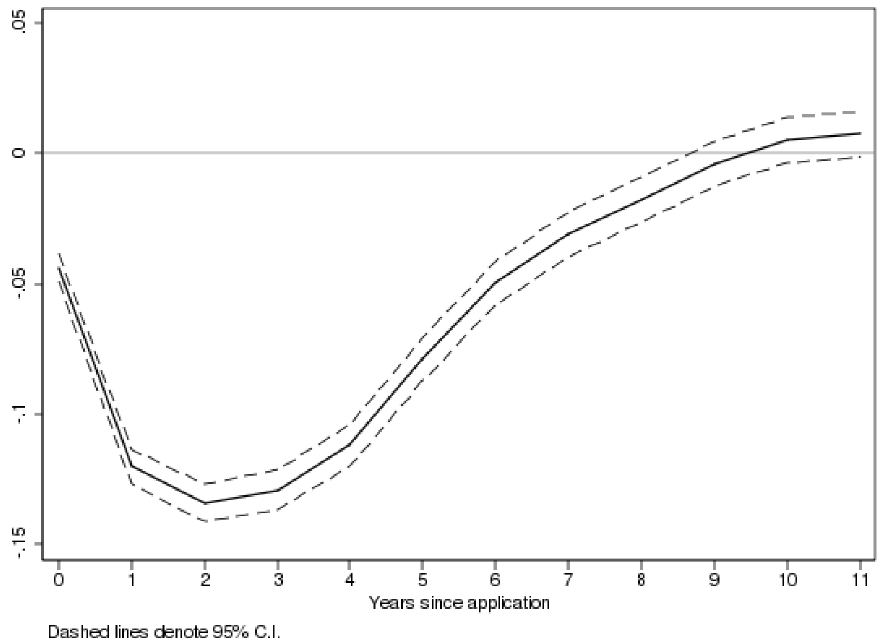
[23] We observe up to 11 years of enrollment data (1998–2009) for these cohorts. Enrollment data for the first nine years since application cover all applicant cohorts. For the tenth year since application, the data cover the 1998-1999 cohorts and, for the 11th year since application, the data cover the 1998 cohort only.

Figure 6: Estimated Effect of Enlistment on College Enrollment, by Years since Application and Service



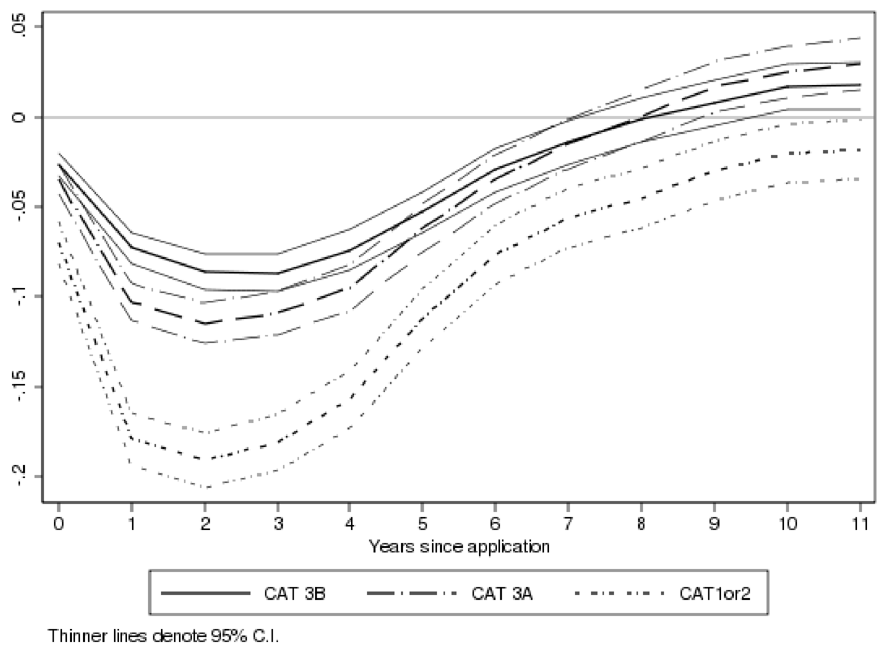
In the first year following enlistment, our estimates imply that military enlistment lowers the probability of current enrollment by 13 percentage points. However, the negative effect of enlistment on current enrollment diminishes with years since application. By the fifth year following application, our estimates imply that enlistment has no effect on current enrollment; 6–11 years following application, the estimates imply that enlistment has a positive effect on current enrollment. These results are consistent with the hypothesis that enlistment delays college education. Estimates for cumulative enrollment (Figure 7) tell a similar story.

Figure 7: Estimated Effect of Enlistment on Cumulative College Enrollment, by Years since Application



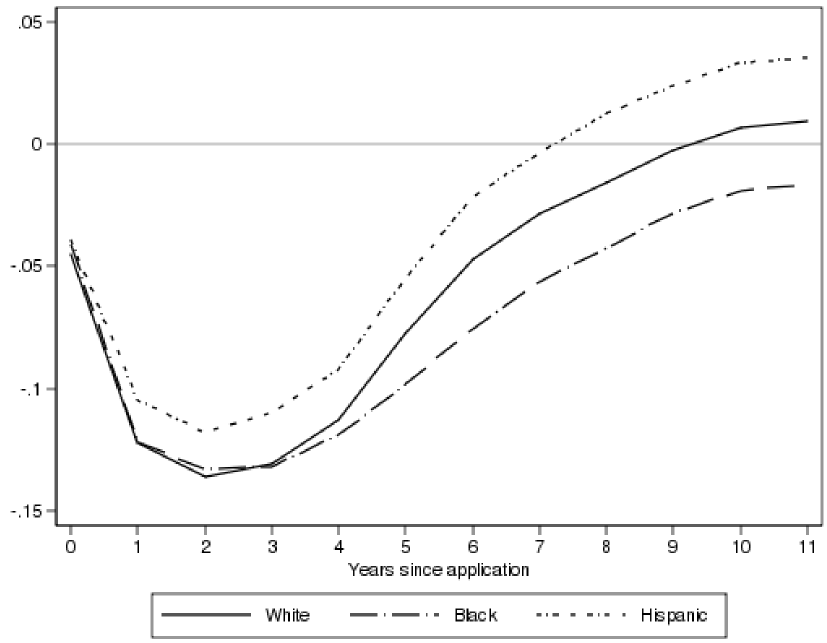
Enlistment lowers the likelihood of ever enrolling in college in the first eight years following application. However, by nine years following application, enlistment has no effect on cumulative enrollment. The estimates imply a 0.7 percentage point positive effect of enlistment on cumulative enrollment 11 years following application, suggesting that, while enlistment delays college education, in the longer run it results in higher levels of college enrollment. The estimates graphed in Figure 8 indicate that enlistment has a larger negative effect on cumulative enrollment for Category I and II enlistees than for Category III enlistees in the first few years following application. This is unsurprising, since high-aptitude youth overall are more likely to attend college.

Figure 8: Estimated Effect of Enlistment on Cumulative College Enrollment, by Years since Application and AFQT Category



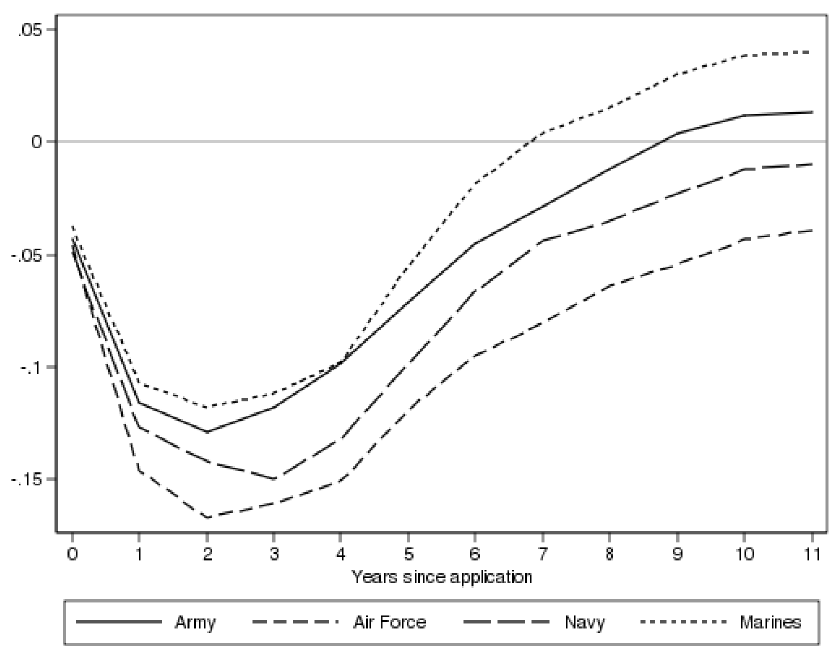
In the longer run, the point estimates imply that enlistment has a positive effect on cumulative enrollment only for the two lower-aptitude groups. Figure 9 reveals no statistically significant difference in the estimated effect of enlistment on cumulative enrollment across race/ethnicity categories, although it suggests that African-American enlistees take longer than Hispanic enlistees to catch up with their non-enlistee counterparts. Finally, Figure 10 shows that, while enlistment has a positive effect on the cumulative enrollment of Marine Corps enlistees 11 years after the application, the same effect is negative for Air Force enlistees.

Figure 9: Estimated Effect of Enlistment on Cumulative College Enrollment, by Years since Application and Race/Ethnicity



Note: Effects on Black applicants are statistically different from those on Hispanic ones from year 5 at 95% confidence level.

Figure 10: Estimated Effect of Enlistment on Cumulative College Enrollment, by Years since Application and Service



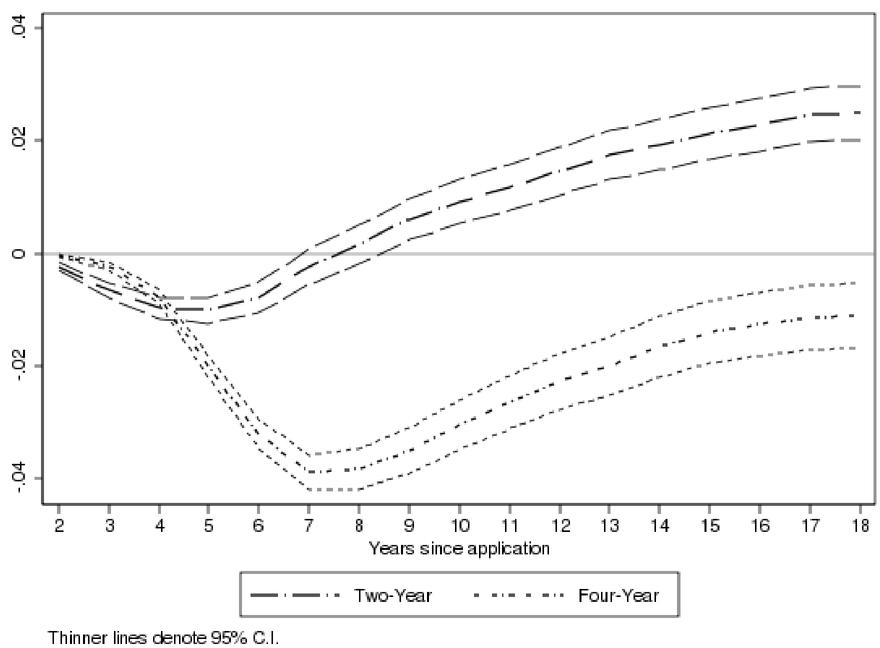
Note: Effects on Air Force applicants are statistically different from those on Marines applicants from year 1 and from those on

Army applicants from year 2 at 95% confidence level.

Degree Attainment

Figure 11 graphs the estimated effect of military enlistment on the probability of earning a college degree by year since application for the 1991–1994 cohorts[24].

Figure 11: Estimated Effect of Enlistment on College Degree Attainment, by Years since Application and Institutional Level



Consistently with the results for college enrollment, the college degree results imply that enlistment delays college education. Enlistment lowers the probability of completing a two-year college (four-year) degree by 1.2 (4.3) percentage points within five (seven) years of application.

[24] We observe up to 18 years of degree data for these cohorts (1991–1994). Degree data for the first 15 years since application cover all applicant cohorts. For the 16th year since application, the data cover the 1991–1993 cohorts; for the 17th year since application, the data cover the 1991–1992 cohorts; and, for the 18th year since application, the data cover the 1991 cohort only.

The negative effect of enlistment decreases thereafter but, while it becomes positive for two-year degrees at year 9, it stays negative for four-year degrees all the way through year 18.

As with enrollment, our estimates imply that the negative effect of enlistment on degree attainment in the short run is greatest for Category I and II enlistees (see Figure 12 and Figure 13).

Figure 12: Estimated Effect of Enlistment on Two-Year College Degree Attainment, by Years since Application and AFQT Category

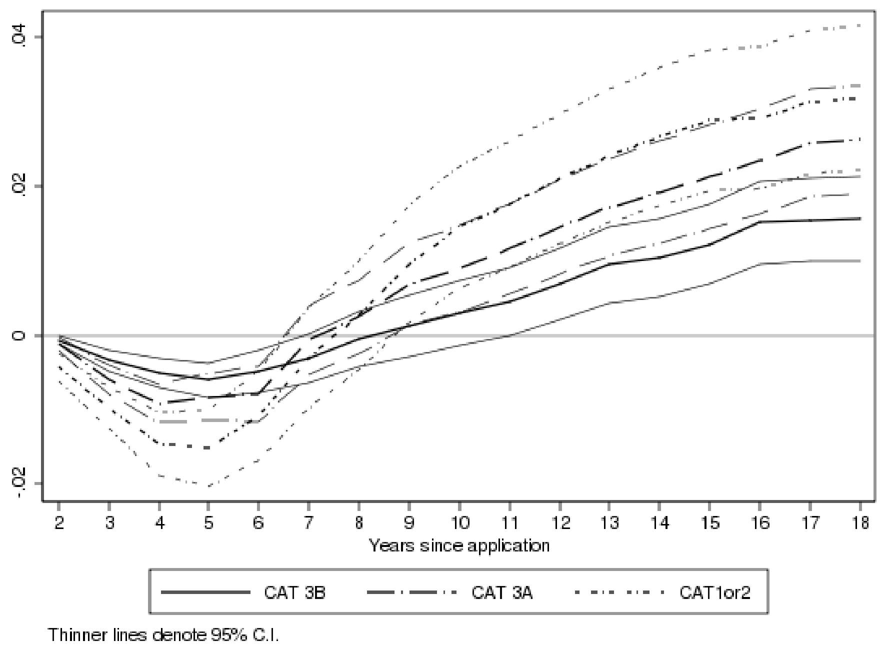
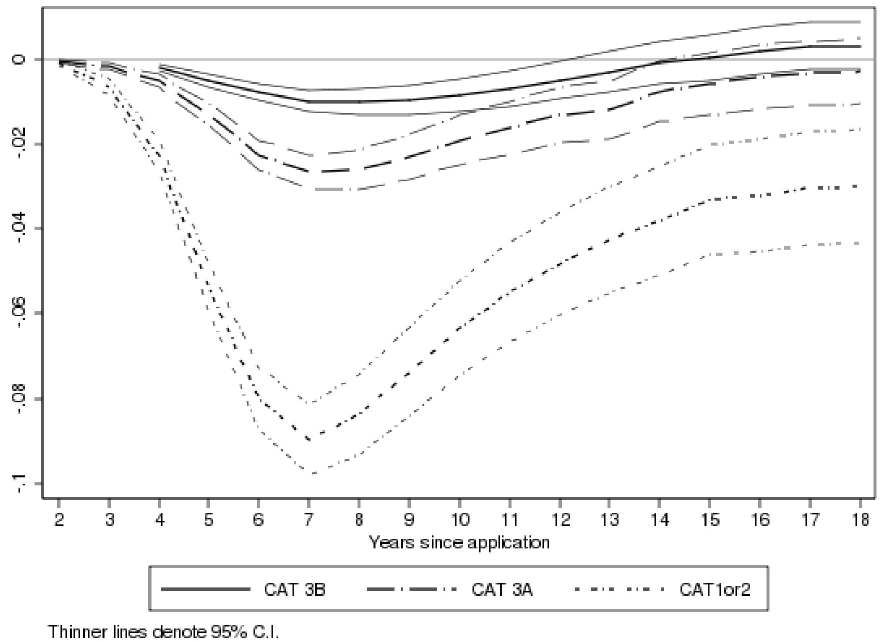


Figure 13: Estimated Effect of Enlistment on Four-Year College Degree Attainment, by Years since Application and AFQT Category



This is consistent with the results for enrollment, and with the expectation that high-aptitude individuals should be even more successful relative to their low-aptitude peers at 4-years institutions that at two-year ones. In the longer run, 18 years following application, the point estimates suggest a negative effect of enlistment on four-year college degree attainment for Category I and II youth and a positive effect on two-year college degree attainment for youth of all Categories.

Figure 14 shows a statistically significant positive effect of enlistment on two-year degree attainment across all racial/ethnic groups from year 11, while Figure 15 suggests that the effect of enlistment on four-year college degree attainment is less negative for White enlistees than for African-American ones.

Figure 14: Estimated Effect of Army Enlistment on Two-Year College Degree Attainment, by Years since Application and Race/Ethnicity

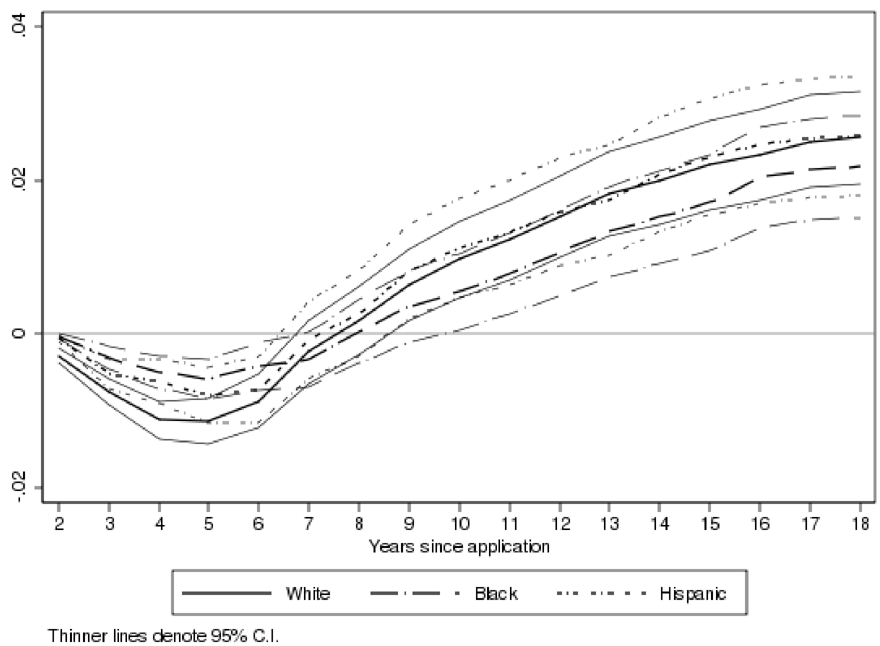
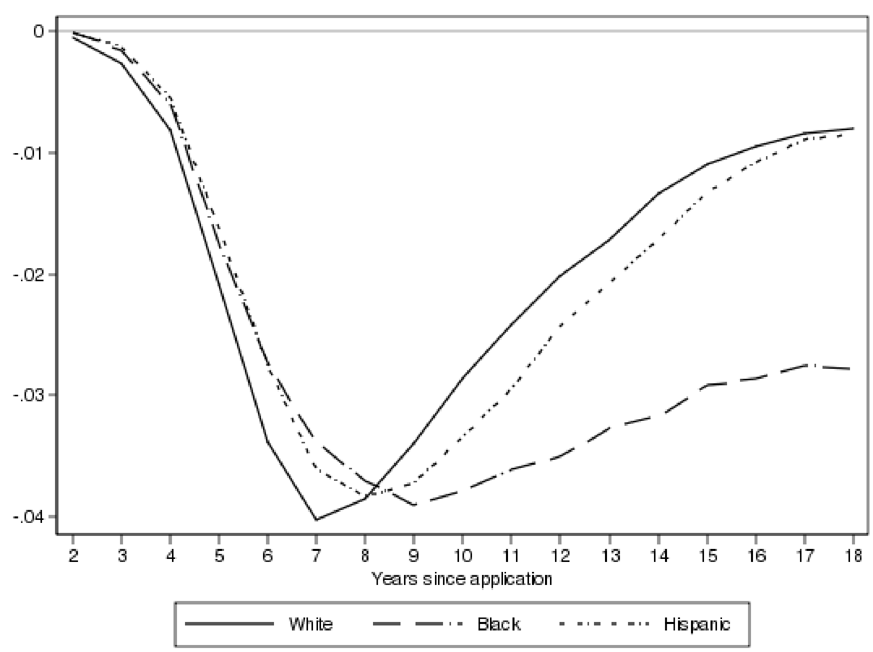


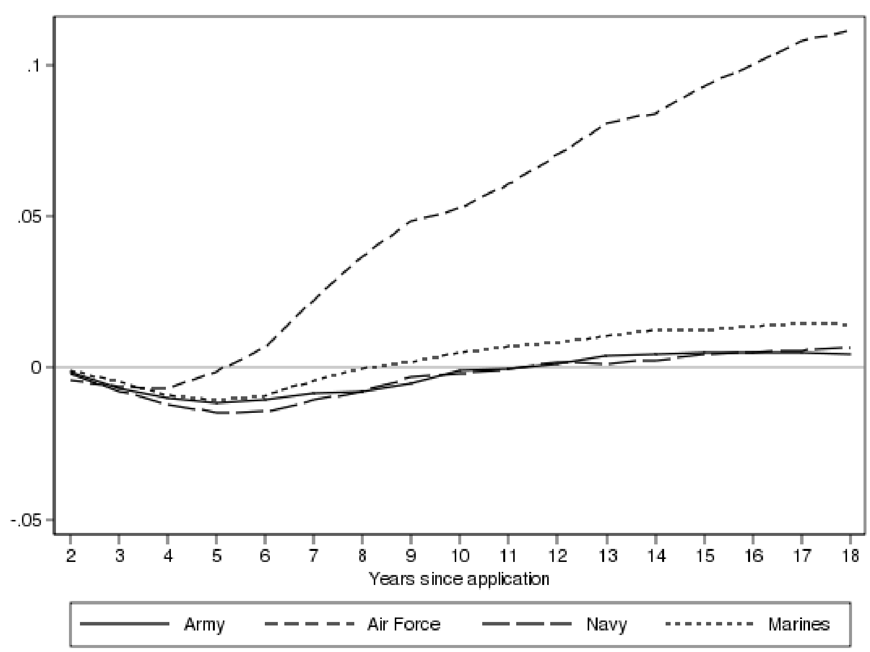
Figure 15: Estimated Effect of Army Enlistment on Four-Year College Degree Attainment, by Years since Application and Race/Ethnicity



Note: Effects on White applicants are statistically different from those on Black applicants from year 12 at 95% confidence level.

Finally, Figure 16 shows that Air-Force enlistees draw the strong positive effect of enlistment on two-year degree attainment. This is consistent with the fact that Air Force enlistees are automatically enrolled in the Community College of the Air Force[25]. In fact, 62 percent of the degrees of Air Force enlistees that we observe in our data were awarded by it. No comparable institution exists for the Army, Marines or Navy, and consistently we do not observe any "concentration" of degrees from a single college for the other Services. Four-year degree attainment is not affected by enlistment in a statistically significant way across Services (Figure 17).

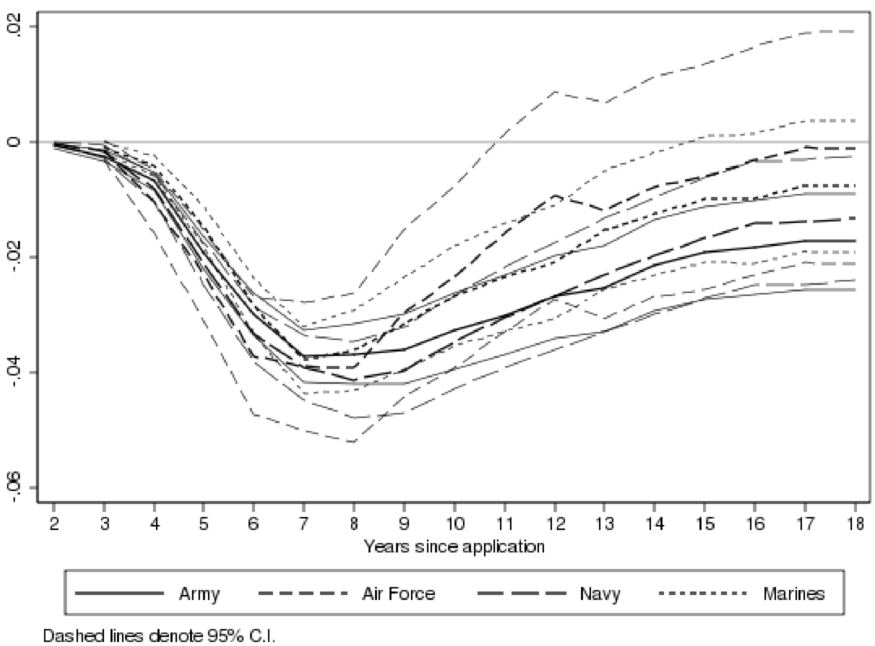
Figure 16: Estimated Effect of Army Enlistment on Two-Year College Degree Attainment, by Years since Application and Service



Note: Effects on Air Force applicants are statistically different from those on all other Services applicants at 95% confidence level from year 7.

[25] http://www.airforce.com/benefits/enlisted-education/ (as of 3/25/2013).

Figure 17: Estimated Effect of Army Enlistment on Four-Year College Degree Attainment, by Years since Application and Service



6. Limitations

Although our methods allow us to control for many of the factors that lead individuals to apply for military service and the military to admit those individuals, we acknowledge that differences could remain. A qualified applicant who chooses to enlist could differ from a qualified applicant who does not enlist for reasons unrelated to their propensity to apply or differences in available covariates. For example, it is plausible that, even among applicants, individuals with a higher willingness to accept the regimentation, strenuous physical work, and danger that can be associated with military employment are more likely to enlist. If these characteristics are correlated with educational outcomes, then our estimates could be biased even after adjusting for the extensive set of controls we use here. Two other such factors are the stochastic arrival of civilian job offers (or job losses) and school admissions. Given our extensive

controls, we assume that applicants who do and do not enlist are equally well prepared for civilian jobs and postsecondary education, but there is nonetheless a random component to job offers and school admissions. Take two equally well-qualified applicants. One applicant randomly receives an attractive civilian job offer and the other does not. All else equal, it is reasonable to assume that the applicant receiving this job offer is less likely to enlist than the applicant who does not receive such an offer. The same might be true of school admissions. If this “good luck” has a lasting, beneficial effect on civilian labor market outcomes and postsecondary education, then Equation 3 will tend to underestimate the causal effect of military service on outcomes. By the same token, an individual who loses his job during the application process might be more likely to join the military. While theoretically plausible, there are two reasons why we might expect the stochastic arrival of job offers and school admissions to be practically unimportant in this context. First, our sample has already applied for military service, and so the job offer or offer of school admission must arrive between the time the individual goes through the application process (which entails visiting a military entrance processing station, taking the ASVAB, completing a physical exam, and undergoing drug testing and criminal background checks) and when that individual makes the decision whether to enlist. The median number of months between application and enlistment in our sample is six; this window is relatively short compared to estimates of mean duration to first full-time job among individuals without postsecondary education: for example, using the 1979 National Longitudinal Survey of Youth, Eckstein and Wolpin (1995) report a mean duration of 4.8 quarters for high school dropouts and 2.5 quarters for high school graduates. Second, these stochastic events must have a lasting effect on labor market outcomes and educational attainment. That is, the effect of

landing a good job following high school or being admitted to a good school by chance must persist and not be countered by equally probable "bad luck" in the future.

Furthermore, we acknowledge that the absence of women in our sample limits the scope of our analyses. The Armed Forces Health Surveillance Center reported that "as of 30 September 2011, women composed 14.5 percent (n=204,706) of the active components of the U.S. Army, Navy, Marine Corps, and Air Force" (Armed Forces Health Surveillance Center, 2011, p.18). Future research should address the potential for gender-specific effects of military enlistment on education: according to the academic literature on the economics of manpower, it is especially for females that "the military has faced increased competition from colleges for high-quality youth" (Asch, Hosek and Warner, 2007, p.1079).

Finally, we estimate the effect of ever having enlisted on outcomes rather than the effect of a specific length of military service. Although we observe years of military service in our data, it is less plausible that the covariates available on the applicant record are sufficient to control for differences between enlistees who serve for different periods of time. Put another way, over time, the pool of enlistees still serving in the active component becomes increasingly select, both because those individuals are choosing to remain in service and because the military wants them to remain in service. Thus, while it is of considerable interest to understand how characteristics of military service, such as years of service or military occupational specialty, affect outcomes, estimating such effects requires isolating exogenous variation in those characteristics, which is beyond the scope of this research.

7. Conclusion

Our work has sought to estimate the causal effect of military enlistment on educational attainment. In the All-Volunteer era, making such an estimate is complicated by the fact that military service is highly selective: individuals volunteer for military service, and the military chooses among those volunteers on the basis of a wide range of criteria that are themselves correlated with education. To mitigate the bias this type of selection can impart to empirical estimates, we restricted our analysis to qualified military applicants, controlling for a wide-range of applicant characteristics.

Our estimates clearly indicate that enlistment causes enlistees to delay their college education, but to eventually enroll at similar rates to comparable non-enlistees. We also find that military service shifts enlistees toward degrees at 2-year colleges, although this effect might be largely due to the special case of the Air Force providing an "internal" community college. Furthermore, we highlight how high aptitude applicants suffer from the largest negative effects of enlistment on enrollment and degree attainment. Finally, it appears that black applicants struggle more than their peers to absorb the negative impact of enlistment on education.

In the All-Volunteer era, the overriding objective of compensation policy is to attract and retain the force necessary to meet the nation's national security objectives. If individuals believe they will be well served by this experience, more might be willing to enlist, and education is an important dimension of military service. The estimates reported in this essay suggest that, on average, these individuals will obtain as much, or more, formal education as they otherwise would have. However, the Military might want to consider targeting black and high-aptitude

enlistees specifically in order to ensure equal opportunity for higher education across race/ethnicity and aptitude level.

Table 14: Point Estimates of the Effects of Enlistment on Enrollment.

Years since Application	Outcome: Current Enrollment	Outcome: Cumulative Enrollment										
	Sample: All	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service			
			Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
0	-0.0437***	-0.0437***	-0.0700***	-0.0347***	-0.0268***	-0.0450***	-0.0414***	-0.0391***	-0.0431***	-0.0462***	-0.0489***	-0.0375***
	-0.00263	*-0.00263*	*-0.0061*	*-0.00399*	*-0.00328*	*-0.00349*	*-0.00466*	*-0.00429*	*-0.00371*	*-0.00891*	*-0.00531*	*-0.00543*
1	-0.130***	-0.120***	-0.179***	-0.103***	-0.0731***	-0.122***	-0.122***	-0.105***	-0.116***	-0.146***	-0.127***	-0.107***
	-0.00278	*-0.00338*	*-0.00729*	*-0.00522*	*-0.00441*	*-0.00444*	*-0.00635*	*-0.00581*	*-0.00486*	*-0.0108*	*-0.00685*	*-0.00713*
2	-0.122***	-0.134***	-0.191***	-0.115***	-0.0862***	-0.136***	-0.133***	-0.118***	-0.129***	-0.167***	-0.142***	-0.118***
	-0.00285	*-0.00368*	*-0.0077*	*-0.00578*	*-0.00492*	*-0.00484*	*-0.00693*	*-0.00645*	*-0.00532*	*-0.0117*	*-0.00742*	*-0.0079*
3	-0.0945***	-0.129***	-0.181***	-0.109***	-0.0868***	-0.131***	-0.132***	-0.110***	-0.118***	-0.161***	-0.150***	-0.112***
	-0.0029	*-0.00389*	*-0.00797*	*-0.00618*	*-0.00528*	*-0.00511*	*-0.00732*	*-0.00687*	*-0.00564*	*-0.0122*	*-0.00783*	*-0.00833*
4	-0.0613***	-0.112***	-0.157***	-0.0952***	-0.0741***	-0.113***	-0.119***	-0.0921***	-0.0988***	-0.151***	-0.132***	-0.0980***
	-0.00295	*-0.00406*	*-0.00817*	*-0.00651*	*-0.00559*	*-0.00533*	*-0.00765*	*-0.0072*	*-0.00589*	*-0.0127*	*-0.00824*	*-0.00867*
5	-0.0113***	-0.0789***	-0.112***	-0.0619***	-0.0531***	-0.0774***	-0.0986***	-0.0548***	-0.0710***	-0.119***	-0.0986***	-0.0550***
	-0.00305	*-0.00421*	*-0.00835*	*-0.00678*	*-0.00587*	*-0.00553*	*-0.00793*	*-0.00752*	*-0.00609*	*-0.0131*	*-0.00861*	*-0.00906*
6	0.0266***	-0.0496***	-0.0765***	-0.0347***	-0.0296***	-0.0473***	-0.0756***	-0.0216***	-0.0453***	-0.0949***	-0.0665***	-0.0183**
	-0.00305	*-0.00431*	*-0.00842*	*-0.00698*	*-0.00611*	*-0.00565*	*-0.00816*	*-0.00774*	*-0.00623*	*-0.0133*	*-0.00888*	*-0.0093*
7	0.0443***	-0.0309***	-0.0561***	-0.0151**	-0.0143**	-0.0286***	-0.0565***	-0.00352	-0.0289***	-0.0806***	-0.0438***	0.00417
	-0.00296	*-0.00438*	*-0.00845*	*-0.00711*	*-0.00627*	*-0.00573*	*-0.00831*	*-0.00786*	*-0.00631*	*-0.0134*	*-0.00904*	*-0.00943*
8	0.0462***	-0.0177***	-0.0452***	0.000441	-0.0017	-0.0161***	-0.0426***	0.0125	-0.0123*	-0.0638***	-0.0352***	0.0155
	-0.00288	*-0.00441*	*-0.00843*	*-0.00719*	*-0.00639*	*-0.00577*	*-0.00842*	*-0.00795*	*-0.00637*	*-0.0135*	*-0.00913*	*-0.0095*
9	0.0501***	-0.00399	-0.0300***	0.0165**	0.00786	-0.00229	-0.0281***	0.0240***	0.00354	-0.0543***	-0.0227**	0.0303***
	-0.00284	*-0.00444*	*-0.00841*	*-0.00726*	*-0.0065*	*-0.0058*	*-0.00851*	*-0.00802*	*-0.00641*	*-0.0135*	*-0.0092*	*-0.00957*
10	0.0331***	0.00521	-0.0204**	0.0249***	0.0167**	0.00691	-0.0189**	0.0334***	0.0119*	-0.0434***	-0.0123	0.0385***
	-0.00224	*-0.00445*	*-0.00838*	*-0.00729*	*-0.00656*	*-0.00582*	*-0.00855*	*-0.00805*	*-0.00643*	*-0.0135*	*-0.00924*	*-0.00958*

	Outcome: Current Enrollment	Outcome: Cumulative Enrollment											
Years since Application	Sample: All	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service				
			Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
11	0.00931***	0.00748*	-0.0182**	0.0293***	0.0174***	0.00928	-0.0167*	0.0354***	0.0133**	-0.0392***	-0.00974	0.0401***
	-0.00148	*-0.00446*	*-0.00838*	*-0.0073*	*-0.00658*	*-0.00582*	*-0.00857*	*-0.00806*	*-0.00644*	*-0.0135*	*-0.00925*	*-0.00959*
N	114,564	114,564	37,764	38,400	38,400	57,600	28,800	28,164	56,964	12,219	24,627	20,754

Robust standard errors in *Italic*
*** p<0.01, ** p<0.05, * p<0.1
The models include controls for percentile of AFQT score, age, education and race/ethnicity.

Table 15: Point Estimates of the Effects of Enlistment on 2-Year Degree Attainment.

Years since Application	Outcome: 2-Year Degree Attainment										
	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service			
		Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
2	-0.00240***	-0.00426***	-0.00117***	-0.000631**	-0.00285***	-0.000310*	-0.000625**	-0.00153***	-0.00396**	-0.00210***	-0.000892**
	-0.00036	*-0.00095*	*-0.00043*	*-0.0003*	*-0.00049*	*-0.00018*	*-0.0002*	*-0.00039*	*-0.00159*	*-0.00056*	*-0.00034*
3	-0.00661***	-0.00976***	-0.00601***	-0.00341***	-0.00754***	-0.00311***	-0.00520***	-0.00669***	-0.00633**	-0.00772***	-0.00464***
	-0.00067	*-0.0014*	*-0.00101*	*-0.00075*	*-0.00087*	*-0.00077*	*-0.00095*	*-0.00089*	*-0.00305*	*-0.0011*	*-0.00106*
4	-0.00971***	-0.0146***	-0.00922***	-0.00515***	-0.0112***	-0.00501***	-0.00615***	-0.0102***	-0.00691	-0.0119***	-0.00900***
	-0.00095	*-0.00215*	*-0.00129*	*-0.001*	*-0.00124*	*-0.00106*	*-0.00144*	*-0.00129*	*-0.00441*	*-0.00155*	*-0.00148*
5	-0.0101***	-0.0152***	-0.00828***	-0.00608***	-0.0113***	-0.00586***	-0.00800***	-0.0116***	-0.00118	-0.0147***	-0.0107***
	-0.00118	*-0.00262*	*-0.0016*	*-0.00122*	*-0.00153*	*-0.0013*	*-0.00187*	*-0.00156*	*-0.00528*	*-0.00191*	*-0.00195*
6	-0.00784***	-0.0108***	-0.00790***	-0.00491***	-0.00875***	-0.00420***	-0.00727***	-0.0107***	0.00663	-0.0145***	-0.00922***
	-0.00137	*-0.00305*	*-0.00189*	*-0.00143*	*-0.00178*	*-0.00156*	*-0.00218*	*-0.0019*	*-0.00619*	*-0.00222*	*-0.00229*
7	-0.00236	-0.00304	-0.00069	-0.00321*	-0.00236	-0.00338*	-0.0008	-0.00839***	0.0224***	-0.0106***	-0.0042
	-0.0016	*-0.00346*	*-0.00232*	*-0.00167*	*-0.00206*	*-0.00183*	*-0.00254*	*-0.0022*	*-0.00714*	*-0.00263*	*-0.00281*
8	0.00157	0.00269	0.00243	-0.00051	0.0017	0.000313	0.0027	-0.00763***	0.0368***	-0.00787***	-0.00019
	-0.00175	*-0.00373*	*-0.00255*	*-0.00191*	*-0.00224*	*-0.00212*	*-0.00286*	*-0.0024*	*-0.0078*	*-0.00291*	*-0.00323*
9	0.00608***	0.00956**	0.00680**	0.00123	0.00638***	0.00354	0.00813***	-0.00503*	0.0485***	-0.00314	0.00197
	-0.00189	*-0.004*	*-0.00279*	*-0.00207*	*-0.00241*	*-0.00238*	*-0.00312*	*-0.00259*	*-0.00832*	*-0.00322*	*-0.00342*
10	0.00917***	0.0145***	0.00891***	0.00296	0.00970***	0.00551**	0.0112***	-0.0011	0.0526***	-0.00195	0.00495
	-0.00199	*-0.00419*	*-0.00295*	*-0.00221*	*-0.00254*	*-0.00253*	*-0.0033*	*-0.00277*	*-0.00869*	*-0.00338*	*-0.00364*
11	0.0117***	0.0176***	0.0116***	0.00460**	0.0123***	0.00789***	0.0132***	-0.00044	0.0606***	-0.00035	0.00705*
	-0.00208	*-0.00434*	*-0.0031*	*-0.00234*	*-0.00265*	*-0.00269*	*-0.00347*	*-0.00287*	*-0.00918*	*-0.00351*	*-0.00385*
12	0.0146***	0.0210***	0.0145***	0.00689***	0.0153***	0.0106***	0.0159***	0.00126	0.0702***	0.00192	0.00834**
	-0.00215	*-0.00445*	*-0.00321*	*-0.00245*	*-0.00273*	*-0.00282*	*-0.00359*	*-0.00297*	*-0.00941*	*-0.00363*	*-0.00399*

Years since	Outcome: 2-Year Degree Attainment											
Application	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service				
		Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
13	0.0174***	0.0241***	0.0172***	0.00943***	0.0183***	0.0133***	0.0175***	0.00391	0.0807***	0.00142	0.0104**
	-0.00223	*-0.00458*	*-0.00335*	*-0.00259*	*-0.00282*	*-0.003*	*-0.00367*	*-0.00308*	*-0.00981*	*-0.00377*	*-0.00408*
14	0.0193***	0.0267***	0.0192***	0.0104***	0.0200***	0.0152***	0.0208***	0	0.0840***	0	0.0126***
	-0.00229	*-0.00472*	*-0.00347*	*-0.00267*	*-0.0029*	*-0.00309*	*-0.00379*	*-0.00318*	*-0.0101*	*-0.0039*	*-0.0042*
15	0.0213***	0.0289***	0.0212***	0.0122***	0.0220***	0.0171***	0.0231***	0.00478	0.0930***	0.00433	0.0125***
	-0.00235	*-0.00481*	*-0.00356*	*-0.00274*	*-0.00297*	*-0.0032*	*-0.00387*	*-0.00324*	*-0.0104*	*-0.00397*	*-0.00426*
16	0.0229***	0.0292***	0.0234***	0.0151***	0.0233***	0.0204***	0.0247***	0.00508	0.0998***	0.00496	0.0137***
	-0.00239	*-0.00486*	*-0.00362*	*-0.00286*	*-0.00302*	*-0.00332*	*-0.00393*	*-0.00328*	*-0.0106*	*-0.00403*	*-0.00433*
17	0.0246***	0.0313***	0.0258***	0.0155***	0.0251***	0.0214***	0.0255***	0.00515	0.108***	0.00569	0.0146***
	-0.00242	*-0.00492*	*-0.00369*	*-0.00288*	*-0.00306*	*-0.00337*	*-0.00396*	*-0.0033*	*-0.0108*	*-0.0041*	*-0.00438*
18	0.0250***	0.0319***	0.0263***	0.0156***	0.0256***	0.0218***	0.0258***	0.00471	0.111***	0.0066	0.0143***
	-0.00243	*-0.00493*	*-0.00371*	*-0.0029*	*-0.00307*	*-0.00339*	*-0.00398*	*-0.00331*	*-0.0109*	*-0.00412*	*-0.00439*
N	114,827	38,095	38,332	38,400	57,600	28,800	28,427	57,227	11,085	28,050	18,449

Robust standard errors in *Italic*
*** p<0.01, ** p<0.05, * p<0.1
The models include controls for percentile of AFQT score, age, education and race/ethnicity.

Table 16: Point Estimates of the Effects of Enlistment on 4-Year Degree Attainment.

Years since Application	Outcome: 4-Year Degree Attainment										
	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service			
		Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
2	-0.000407**	-0.000811**	-0.000517**		-0.000521**	-0.0000842	-0.0000796	-0.000660***		-0.00017	
	-0.00013	*-0.00029*	*-0.00021*		*-0.00019*	*-0.000072*	*-7.42E-05*	*-0.00025*		*-0.00014*	
3	-0.00227***	-0.00635***	-0.00171***		-0.00262***	-0.00154***	-0.00123***	-0.00245***	-0.00134	-0.00172***	-0.000940**
	-0.00034	*-0.00098*	*-0.0004*		*-0.00045*	*-0.000468*	*-0.00037*	*-0.00049*	*-0.00089*	*-0.0006*	*-0.00044*
4	-0.00766***	-0.0227***	-0.00516***	-0.00191***	-0.00816***	-0.00595***	-0.00543***	-0.00689***	-0.0104***	-0.00807***	-0.00417***
	-0.00062	*-0.00185*	*-0.00079*	*-0.00041*	*-0.00082*	*-0.000849*	*-0.000801*	*-0.00083*	*-0.0028*	*-0.0012*	*-0.00092*
5	-0.0202***	-0.0538***	-0.0130***	-0.00493***	-0.0208***	-0.0175***	-0.0162***	-0.0192***	-0.0231***	-0.0211***	-0.0147***
	-0.001	*-0.00285*	*-0.0013*	*-0.00084*	*-0.00127*	*-0.00145*	*-0.0015*	*-0.0014*	*-0.00407*	*-0.0019*	*-0.00177*
6	-0.0322***	-0.0800***	-0.0225***	-0.00774***	-0.0338***	-0.0273***	-0.0275***	-0.0299***	-0.0372***	-0.0334***	-0.0285***
	-0.00132	*-0.00372*	*-0.00179*	*-0.00103*	*-0.00169*	*-0.00186*	*-0.00201*	*-0.0019*	*-0.00519*	*-0.00247*	*-0.00249*
7	-0.0389***	-0.0897***	-0.0266***	-0.00984***	-0.0402***	-0.0339***	-0.0361***	-0.0373***	-0.0390***	-0.0392***	-0.0379***
	-0.00156	*-0.00426*	*-0.00204*	*-0.0013*	*-0.00199*	*-0.00216*	*-0.00243*	*-0.00232*	*-0.00571*	*-0.00286*	*-0.00294*
8	-0.0383***	-0.0837***	-0.0259***	-0.0100***	-0.0385***	-0.0370***	-0.0383***	-0.0368***	-0.0392***	-0.0413***	-0.0362***
	-0.00183	*-0.00485*	*-0.0024*	*-0.00154*	*-0.00235*	*-0.00237*	*-0.00285*	*-0.00272*	*-0.0066*	*-0.00334*	*-0.00358*
9	-0.0351***	-0.0737***	-0.0231***	-0.00969***	-0.0340***	-0.0390***	-0.0372***	-0.0360***	-0.0297***	-0.0397***	-0.0315***
	-0.00207	*-0.00533*	*-0.0027*	*-0.00177*	*-0.00266*	*-0.0026*	*-0.00325*	*-0.00306*	*-0.00743*	*-0.00381*	*-0.00407*
10	-0.0305***	-0.0634***	-0.0190***	-0.00853***	-0.0286***	-0.0379***	-0.0334***	-0.0328***	-0.0235***	-0.0347***	-0.0268***
	-0.00225	*-0.00567*	*-0.00296*	*-0.00196*	*-0.00289*	*-0.00279*	*-0.00352*	*-0.00333*	*-0.00806*	*-0.00411*	*-0.00445*
11	-0.0265***	-0.0550***	-0.0161***	-0.00682***	-0.0242***	-0.0361***	-0.0294***	-0.0301***	-0.0158*	-0.0305***	-0.0235***
	-0.00241	*-0.00597*	*-0.00317*	*-0.00213*	*-0.00311*	*-0.00293*	*-0.00379*	*-0.00355*	*-0.00873*	*-0.00443*	*-0.00478*
12	-0.0228***	-0.0482***	-0.0131***	-0.00491**	-0.0202***	-0.0350***	-0.0243***	-0.0269***	-0.00928	-0.0268***	-0.0209***
	-0.00255	*-0.0062*	*-0.00338*	*-0.00228*	*-0.00327*	*-0.00312*	*-0.00403*	*-0.00373*	*-0.00916*	*-0.00475*	*-0.005*

Years since Application	4-Year Degree Attainment										
	Sample: All	Sample: AFQT			Sample: Race/Ethnicity			Sample: Service			
		Cat 1 or 2	Cat 3A	Cat 3B	White	Black	Hispanic	Army	Air Force	Navy	Marines
13	-0.0200***	-0.0425***	-0.0119***	-0.00292	-0.0173***	-0.0327***	-0.0208***	-0.0255***	-0.0119	-0.0231***	-0.0153***
	-0.00265	*-0.00638*	*-0.00352*	*-0.0024*	*-0.0034*	*-0.00324*	*-0.00424*	*-0.00384*	*-0.00953*	*-0.00497*	*-0.00523*
14	-0.0166***	-0.0381***	-0.00754**	0	-0.0134***	-0.0317***	-0.0171***	-0.0214***	0	-0.0198***	-0.0125**
	-0.00275	*-0.00652*	*-0.00367*	*-0.00257*	*-0.00353*	*-0.00339*	*-0.00443*	*-0.00397*	*-0.00975*	*-0.00519*	*-0.00545*
15	-0.0140***	-0.0331***	-0.00576	0.000388	-0.0109***	-0.0292***	-0.0133***	-0.0193***	-0.00606	-0.0167***	-0.00993*
	-0.00282	*-0.00664*	*-0.00376*	*-0.00267*	*-0.00361*	*-0.00357*	*-0.00459*	*-0.0041*	*-0.00996*	*-0.00534*	*-0.00558*
16	-0.0126***	-0.0321***	-0.00415	0.00207	-0.00954***	-0.0286***	-0.0108**	-0.0184***	-0.00318	-0.0141***	-0.00984*
	-0.00289	*-0.00674*	*-0.00383*	*-0.00277*	*-0.00369*	*-0.00368*	*-0.0047*	*-0.00419*	*-0.0101*	*-0.00546*	*-0.00573*
17	-0.0114***	-0.0303***	-0.00328	0.0032	-0.00835**	-0.0275***	-0.00890*	-0.0172***	-0.00097	-0.0139**	-0.00761
	-0.00292	*-0.0068*	*-0.00388*	*-0.0028*	*-0.00373*	*-0.00376*	*-0.00476*	*-0.00425*	*-0.0102*	*-0.0055*	*-0.00582*
18	-0.0111***	-0.0300***	-0.00284	0.00322	-0.00799**	-0.0279***	-0.00849*	-0.0173***	-0.00099	-0.0132**	-0.00771
	-0.00294	*-0.00682*	*-0.00391*	*-0.00283*	*-0.00375*	*-0.00378*	*-0.00478*	*-0.00427*	*-0.0103*	*-0.00553*	*-0.00585*
N	114,827	38,095	38,332	38,400	57,600	28,800	28,427	57,227	11,085	28,050	18,449

Robust standard errors in *Italic*
*** p<0.01, ** p<0.05, * p<0.1
The models include controls for percentile of AFQT score, age, education and race/ethnicity.

Appendix: Matching IPEDS and NSC Data

NSC data only contains information on whether, in a given year, a student was enrolled or obtained a degree from a given institution, together with the start date for the coverage of such institution by the NSC. We supplemented it with data from the Integrated Postsecondary Education Data System (IPEDS) at the National Center for Education Statistics in order to be able to identify additional characteristics of the institutions. For example, whether the highest degree granted is at the associate or bachelor's level (which defines our 2-year and 4-year degree attainment outcomes)[1].

In order to match the records, we took the following steps:

1. we matched the 8-digit U.S. Department of Education's Office of Postsecondary Education (OPE) ID in the NSC data to the 8-digit OPE ID in the IPEDS data; this was done on a year by year basis, so if a given institution had no OPE ID in the IPEDS data in a given year we left the start date missing at this stage (because we noticed cases of OPE IDs changing over time within the same Unit ID, i.e. the unique identifier for institutions in IPEDS).

2. For those OPE IDs that we could not match at the 8-digit level, we matched the 6-digit OPE ID in the NSC data to the 6-digit OPE ID in the IPEDS data. The 6-digit OPE ID excludes the last 2 digits, which identify the branch. The same criterion as above was retained, so if a

[1] IPEDS data downloaded from IPEDS Data Center (http://nces.ed.gov/ipeds/datacenter/Default.aspx) on 5/23/2012. Publicly released, revised data were used. We cannot observe the level of the degree for the individual student, only the level of the institution that granted it: therefore, our 2(4)-year degree outcome in fact corresponds to obtaining a degree from an institution granting at most 2(4)-year degrees.

given institution had no 6-digit OPE ID in the IPEDS data in a given year we left the start date missing at this stage.

3. With these two steps we could not match 255 of the 3,381 381 OPE ID's in the NSC data. After a case-by-case check, it turned out that in 3 cases the OPE ID in the IPEDS data did not correspond to the OPE ID in the NSC data, but the institution name did, so we manually changed those OPE IDs. We then checked one by one the 252 remaining institutions, and it turned out that 241 of them are located in foreign countries, so the fact that we can't match them to IPEDS is not a problem as we aim to compute U.S. coverage. For the remaining 11 we could find no records neither with the National Student Loan Data System[2] nor with of the Federal Student Aid Office of the U.S. Department of Education[3].

4. Furthermore, we made sure that the start date we assigned was unique within IPEDS institution (Unit ID), because the enrollments are counted at the Unit ID level. In fact, there are instances in which either the last 2 digits or the entire first 6 digits of the OPE ID are not constant within Unit ID over time. In these cases, we applied the most conservative criterion of assigning the latest start date.

5. Finally, we made sure that if a given institution was matched to the NSC data in at least one year (i.e. I had its start date for at least one year), the start date would be assigned to all years. This is because the start date of NSC coverage should be constant within institution over time (it should be so in principle; and at step 4 it was imposed to be so by construction for the cases that presented problems).

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[2] http://www.nslds.ed.gov/nslds_SA/defaultmanagement/cohortdata.cfm (as of 5/26/2012).

[3] http://federalstudentaid.ed.gov/datacenter/library/FY2008CDR.xls (as of 5/26/2012).

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Essay 3: Home-Country Academic Quality, Time Spent in the U.S., and the Math Achievement of Immigrant High School Students[29]

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Abstract

The process of adaptation of immigrant children to their host country has been shown to be empirically consistent with a segmented assimilation hypothesis, whereby the background and context of reception affect success in the receiving country. However, significant differences based on nativity remain unexplained in the literature. This study investigates whether home-country academic quality is a relevant dimension of segmentation, by examining a sample of 9th grade immigrants in U.S. high schools and their academic outcomes. The scores of different international assessments in mathematics are our measure home-country academic quality, and some options are discussed to deal with their typically weaker coverage of developing countries. The analyses show that home-country academic quality is a positive and significant predictor of current math achievement, with this relationship decreasing over time spent in the U.S. These findings are robust to a number of specifications, and do not hold when other country-level

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rankings of development other than international education assessments are used, suggesting that the underlying construct of home-country academic quality has a distinctive role. The policy implications stemming from these results are simple but powerful: the marginal return to providing education to immigrant students is increasing in the size of the initial gap of preparation, suggesting that it is suboptimal to neglect diversity in academic backgrounds.

1. Background and Motivation

The "challenge confronting immigrant children" (Zhou, 1997) has been acknowledged in the academic and policy literature since the 1990s, following the major and diverse waves of migration to the U.S. in the 1980s (McDonnell et al., 1993) and 1990s (Fry, 2007). Sociology has traditionally thought of immigrant adaptation from an assimilation perspective, whereby a one-directional, straight convergence of newcomers results in their acculturation irrespective of their different backgrounds, which are gradually surrendered in favor of the common culture of the host society. This framework also postulates that immigrants enter the social dynamics of the receiving country in a uniform way (Zhou, 1997).

This hypothesis has been gradually called into question upon the observation that inter-ethnic differences among immigrants tended to persist over time, and that in fact "second generation decline" could occur – such that "the children of poor, especially dark-skinned, immigrants "will either not be asked, or will be reluctant, to work at immigrant wages and hours as their parents did but will lack job opportunities, skills and connections to do better"" (Gans, 1992, pp. 173-174) as cited in (Zhou, 1997, p. 979). In this context, the theory of segmented assimilation (Portes and Zhou, 1993) acknowledges the gradient of different paths that immigrants experience, depending on the context of reception, and possibly leading to both upward and

downward trajectories. In light of these theoretical frameworks, this study seeks to assess the role of both time (prominent according the traditional, straight-line assimilation hypothesis) and pre-migration academic quality (as a possible determinant of segmented assimilation) with respect to the mathematics achievement of immigrant students. In order to do so, it exploits variation both in the academic quality of sending countries and in the timing of migration, which results in immigrant students being exposed for different durations to their home-country versus their receiving country school systems, and experiencing potentially very steep learning curves in the receiving country depending on the relative quality of academic preparation received before moving.

Educational outcomes are a typical measure of the adaptation progress of immigrant youth (Zhou, 1997). It is recognized that home-country experiences influence the attainment of immigrant children in American schools (Portes and Rumbaut, 2001, p.234); in particular, Hirschman explicitly mentions high levels of pre-migration human capital (such as schooling attained in the home country) among some immigrant groups as one of the factors that places them on an upward path of social mobility after migrating to the U.S. (2001, p.319) . However, in the literature, country-of-origin-specific variation in academic achievement is typically left unexplained by a variety of observed characteristics, as noted in the review of Glick and White (2004, p.275). For example, Portes and Rumbaut report that, on average and conditional on family socioeconomic status, family composition and types of acculturation, children of Mexicans and Haitians tend to do worse, and children of Chinese, Koreans and Vietnamese tend to do better (2001, p.242). Incorporating the role of time, Hirschman (2001) notes how longer presence in the U.S. has a positive association with the rate of school enrollment of immigrant youth from Mexico, El Salvador, Guatemala, South America and the former Soviet Union, but a

negative one for Germans and other Europeans; for other countries (Puerto Rico, Cuba, and Dominican Republic), the enrollment rate does not vary with the timing of migration.

All these results are consistent with the segmented assimilation theory, yet all, to some extent, fail at explaining what exactly is causing the segmentation, at best hinting at possible factors. This study aims at filling part of this gap by explicitly considering the schooling experiences received in one's home country as shaping their assimilation process in American schools. If pre- migration, country-specific academic quality was a dimension along which segmentation occurs in the assimilation process, we would expect to see immigrants from countries with higher average academic achievement do better in the U.S., ceteris paribus. Within this framework, we also expect the role of time spent in the U.S. to vary across countries of origin: more recent immigrants, who have been exposed for longer to their home-country educational system, should show some advantage (disadvantage) if their home-country schools grant better (worse) preparation than the average. This is the main innovation introduced by this study: looking at home-country academic quality as a dimension of segmentation implies investigating its role not only independently but also jointly with that of time spent in the U.S. In order to model this interdependencies, we adapt the analytical framework that Hanushek and Woessman (2012) employed to study the effect of home-country international test scores on earnings: while their work compared immigrants to the U.S. who were either educated entirely in their country of origin to those entirely educated in the United States, as if time spent in the U.S. was a binary variable, our work will examine it as a continuous one (Section 6 provides more details on the analytical framework as adapted from Hanushek and Woessman (2012)).

Among the various measures of educational attainment, this study examines knowledge and skills in math. The importance of mathematics and reading skills has been shown by virtue of

their relationship to subsequent labor market outcomes (Farkas 1996, as cited in Glick and White, 2003). However, while reading bears the influence of the transition from the native language (most likely) to English as the primary language of instruction for immigrants with some exposure to home-country schooling, mathematics skills should be less affected, as they are built off of a universal language. Since the aim of this study is investigating the roles of time in the U.S. and home-country academic quality, focusing on math skills avoids some of the potentially confounding factors related to language and, in principle, relies on a logical sequence of learning between pre- and post-migration years that is not as linear in the case of reading. Different choices of the outcome measures have led to different results in the academic literature on the educational attainment of immigrants. For the purposes of this study, the use of a standardized test as the dependent variable, besides bearing the obvious advantage of being perfectly comparable across schools, is preferable to grades, which can be influenced by other aspects than knowledge and skills (such as behavior, teacher perceptions, etc.) (Portes and Rumbaut, 2001, p.243). In fact, the analyses of Portes and Rumbaut show that, for example, Laotians and Cambodians are at the bottom of the Stanford math and reading tests scores distributions, while are among the top in GPA rankings (2001, p.236).

2. Research Questions

In light of the theoretical frameworks, existing empirical results and hypotheses outlined above, the three research questions that this study seeks to address are:

1. What is the relationship between pre-migration academic quality and math achievement in the U.S.? Is pre-migration academic quality a possible explanation for the residual "nativity" variation found in the literature?

2. What is the relationship between time spent in the U.S. and math achievement in the U.S.?
3. Does the relationship between time spent in the U.S. and math achievement vary by pre-migration academic quality?

With a view to offering a contribution to the explanation of segmented assimilation, this study explicitly considers the average academic achievement in the sending countries as a proxy for a country-of-origin-specific component of human capital formation. For simplicity, we call this construct "home-country academic quality". The idea is to capture the cross-country variation in the quality of academic environments to which the students are exposed before migrating to the U.S.: this introduces an interesting comparison not only between different sending countries themselves, but also between the different sending countries and the receiving country. Therefore, in order to indicate that this construct is not about the school-specific quality that immigrant student experience nor the school-specific preparation that they receive, we employ the expression "home-country academic quality" and its acronym HCAQ throughout the study. In order to answer the first research question, we measure the countrywide average level of mathematics achievement with the national mean scores of different international assessments.

With respect to the second research question, Portes and Rumbaut (2001) suggest that time could have both a positive (through acculturation) and a negative effect (as more recent immigrants are usually more driven). The assimilation perspective would predict a positive effect of time. Although it has already been investigated extensively, this research question is instrumental to the key contribution that this study seeks to make.

In fact, a positive answer to third research question would be consistent with an assimilation framework segmented on the basis of the educational input received before migration. A

negative one, on the other hand, would support a straight-line assimilation perspective. Our research hypothesis is that, holding timing of migration constant, the role of time spent in the U.S. is decreasingly important for immigrant students the more solid the academic background of their home country; this conversely implies that the relationship between country-specific academic background and math achievement in the U.S. is more important for more recent immigrants.

3. Data

Student-level data: HSLS

This study uses a sample of 9th grade immigrants in U.S. high schools from the High School Longitudinal Study of 2009 (HSLS:09) by the National Center for Education Statistics (NCES). The HSLS:09 is composed of a randomly selected sample of fall-term 9th graders in public and private high schools with both a 9th and an 11th grade (Ingels et al., 2011, p.6). It comprises a student survey and math assessment, as well as surveys of parents, teachers and principals. The mathematics assessment of algebraic reasoning consists of a computer-based test of student achievement in algebra, covering six domains of algebraic content (the language of algebra, proportional relationships and change, linear equations, inequalities, and functions, nonlinear equations, inequalities, and functions, systems of equations, sequences and recursive relationships) and four algebraic processes (demonstrating algebraic skills, using representations of algebraic ideas, performing algebraic reasoning, solving algebraic problems) (Ingels et al., 2011, pp.23-24).

The dependent variable for the analyses is the standardized theta score of the math assessment. Scoring of the assessment is based on Item Response Theory (IRT), a methodology that accounts not only for the number of correct answers, but also for the intrinsic difficulty of each question. The theta score estimates a student's ability based on IRT, and tends to be more normally distributed than other measures (such as estimated number-right scores), therefore it is recommended for use in correlational analyses and multivariate models (Ingels et al., 2011, p.28). The standardized score is a transformation of the theta score, rescaled to a mean of 50 and standard deviation of 10 (Ingels et al., 2011, p.29): it ranges from 24.02 to 82.19 in our sample.

The other key measure from HSLS is time spent in the U.S. Since the aim of this variable is to capture the length of exposure to the U.S. school system, we derive a measure of the number of years in the U.S. based on the number of grades taken in the U.S. We use information on the grade at which the student was placed in the U.S. upon arrival, provided by a question asked in the parents' interview: "In what grade was [your 9th-grader] placed when [he/she] started school in the United States?"[30]. The answer options were pre-kindergarten, kindergarten, and 1st grade through 9th grade. We define number of years in the U.S. as 9 minus the grade at which the student is placed in the U.S. upon arrival: for example, if a student is placed in 9th grade in the U.S., he is defined to have attended 0 grades in the U.S., while if a student is placed in 1st grade, he is defined to have attended 8. We also define a variable to capture grade repetition.

The sample for this study is defined as those students born outside the U.S., with a completed parent interview, student 9th grade math test score (even if imputed, and we define an indicator for that), and information on the country of origin and on the timing of migration. It includes

[30] Question wording was customized based on the sample member's gender; question wording was also customized such that the sample member's name appeared in place of "your 9th-grader".

students that we have identified as born in Puerto Rico (those classified as born in "Puerto Rico or another U.S. territory" and identifying their Hispanic/Latino/Latina origin as Puerto Rican). This yields a total sample size of 1,189. With respect to the total number of students identified as born outside the U.S., with a completed parent interview, and information on the country of origin and on the timing of migration (1,263), 74 observations are lost due to missing student 9th grade math test score (even after imputation performed in the HSLS:09 data file): the prevalence of missing student 9th grade math test score in this sample (5.86%) is higher than in the sample of students with parent interview as a whole (566 out of 16,995 cases, or 3.33%), but still low enough to raise any concern about bias.

Country-level data: PISA and TIMSS

We alternatively measure home-country academic quality by three international proxies of student achievement: 2009 PISA mathematics score, TIMSS mathematics 4th grade score and TIMSS mathematics 8th grade score. The 2009 OECD's Programme for International Student Assessment (PISA) is an international survey of 15-year-old students in 65 countries. We employ the country-level mean performance on the mathematics scale, which provides a profile of knowledge and skills on the subject (OECD, 2012). The average country-level mean score in 2009 was 496, with a maximum of 600 for Shanghai-China and a minimum of 331 for Kyrgyzstan, and with the U.S. standing below the mean at 487 (OECD, 2010). Data on PISA was obtained from the World Bank Education Statistics. The TIMSS and PIRLS International Study Center, Lynch School of Education, Boston College, and the International Association for the Evaluation of Educational Achievement (IEA) conduct the Trends in International Mathematics and Science Study (TIMSS) every 4 years. It is administered to students both in 4th

and 8th grades. Since the participating countries vary over time, we employ the most recent value for each country to have ever participated in one of the most recent assessments (1995, 1999, 2003 and 2007). Similarly to PISA, the measure is a country-level average scaled score for mathematics: in 2007, the mean was 500, with Qatar displaying the lowest result (307), and Chinese Taipei the highest one (598); the U.S. was above the mean at 508. Data for the TIMSS was obtained from the National Center for Education Statistics (Provasnik, Gonzales and Miller, 2009).

Practical aspects motivate the choice of average international test scores as a proxy for home-country academic quality, and its implications deserve some consideration. The treatment that this study seeks to examine is the schooling input that each student received in the country of origin. Therefore, an ideal measure of it would be based on an individual-level account of the schooling experience in the home country. Absent the possibility of observing it, the average of these international assessments is the best available measure. If students attended the typical school in their home country, these measures would be quite accurate. In practice, this treatment as captured by country-level averages can be conceptualized as a measure of the baseline home-country academic quality to which all students, with some variation around it, are exposed.

The potential existence of patterns in this variation, when used to explain 9th grade math test scores of immigrant students, may introduce selection bias. In particular, there is consensus in the relevant academic literature that immigrants are not randomly selected (for example, Borjas, 1987). In the context of this study, if students coming from countries with higher home-country academic quality were more positively selected than students coming from countries with lower home-country academic quality, a bias would be introduced towards finding positive effects of home-country academic quality, because a spurious positive correlation would be introduced

between home-country academic quality and student math test scores (which would tend to be higher for the more positively selected students). In fact, research has shown that all immigrant groups are positively selected (Feliciano, 2005). However, the degree of positive selection (measured as the difference in educational attainment between migrating and non-migrating citizens of a given country) varies across countries as a positive function of distance and a negative function of average educational attainment (Feliciano, 2005). Therefore, if average educational attainment is positively correlated with our measure of home-country academic quality, this evidence suggests that immigrants from countries with better home-country academic quality should not be *per se* more positively selected than immigrants from countries with poorer home-country academic quality. As a result, the choice of using average measures of home-country academic quality should not bias the results towards finding positive effects.

There are a total of 112 countries (including Puerto Rico) from which immigrants in the sample come from, but we do not observe international test scores for all of them. In particular, PISA covers 57% of the immigrants, TIMSS 4th grade covers 36% and TIMSS 8th grade covers 42%.

For both PISA and TIMSS, we assign Hong Kong's scores to China, which does not participate in either international assessment. This assumes that the home-country academic quality of Chinese immigrants is comparable to that of their peers from Hong Kong. Indirectly, this can be tested by virtue of comparing the educational attainment by immigrants from China and Hong Kong to the U.S. Using the IPUMS-CPS (King et al., 2010), which includes data from 50 years (1962-2011) of the March Current Population Survey (CPS), Table 17 provides some evidence that, among immigrants aged 30 and above, Chinese and Hong Kongers display similar educational attainments in terms of high school diplomas; even though the distribution of

educational attainment among Chinese immigrants tends to be to the left of that of Hong Kongers, Chinese immigrants are as likely as Hong Kongers to have a master's degree, and significantly more likely to have a Ph.D.

Even if this assumption was unrealistic, it would bias the results against finding an effect (attenuation bias). Assigning a higher score than the true one to China would introduce an artificial negative correlation between home-country academic quality and achievement in the U.S.: since the hypothesis is that such correlation is positive, if the true of China score was lower the expectation would be that Chinese students on average will perform poorly on the math test. Therefore, the relationship between home-country academic quality and math achievement would be drawn towards zero and not artificially inflated by this decision to overcome an inherent data limitation.

For the remaining countries without international mathematics assessments, we have to resort to imputation.

Table 17: Educational attainment of immigrants from China and Hong Kong to the U.S.

	% Educational Attainment	
	China	Hong Kong
None or preschool	2.6	0.4
	(2.4-3.0)	(0.2-0.9)
Grades 1, 2, 3, or 4	2.6	0.7
	(2.4-3.0)	(0.4-1.3)
Grades 5 or 6	5.2	3.6
	(4.8-5.7)	(2.8-4.8)
Grades 7 or 8	4	2.1
	(3.7-4.4)	(1.5-3.0)
Grade 9	2.7	1
	(2.4-3.1)	(0.6-1.7)
Grade 10	1.8	0.6
	(1.6-2.1)	(0.3-1.2)
Grade 11	0.8	0.9
	(0.7-1.0)	(0.5-1.5)
12th grade, no diploma	2.2	1.5
	(1.9-2.5)	(1.0-2.3)
High school diploma or equivalent	23.3	24.2
	(22.4-24.1)	(22.0-26.6)
Some college but no degree	5.1	10.8
	(4.7-5.5)	(9.3-12.6)
Associate's degree, occupational/vocational program	1.6	2.5
	(1.4-1.9)	(1.8-3.5)
Associate's degree, academic program	2.7	3.8
	(2.4-3.1)	(2.9-4.9)
Bachelor's degree	19.5	27.7
	(18.8-20.3)	(25.4-30.2)
Master's degree	15	14.8
	(14.3-15.7)	(13.1-16.8)
Professional school degree	2.4	1.6
	(2.2-2.8)	(1.1-2.4)
Doctorate degree	8.2	3.6
	(7.7-8.8)	(2.7-4.7)
N	*10128*	*1374*

Note: 95% confidence intervals in parentheses

4. Imputation

In general, missing data constitutes a twofold problem: as it lowers sample size, it reduces the efficiency (i.e. precision) of the estimates; on the other hand, it can yield biased estimates of the coefficients of interest. In the particular case of this study, because we are interested in estimating the relationships between math achievement, time in the U.S. and academic background on a sample of immigrant students in U.S. high schools, excluding students from those countries that do not take part in international assessments of mathematics skills would result in an unnatural restriction of the sample. Specifically, it would limit this study to students from countries that tend to be developed (especially in the case of PISA, whose core sample is represented by OECD countries), excluding a very policy-relevant segment of students who, coming from more disadvantaged backgrounds, need to work hardest to catch-up with their U.S.-born peers and potentially need more targeted support. This section introduces the theoretical underpinnings to imputation in the context of this study, motivates the choice of multiple imputation by chained equations as the imputation method and describes the choice of covariates for the imputation models.

There are three assumptions that can be made about missing data. Missing completely at random (MCAR) assumes that the probability of a missing value for a given observation is completely unrelated to the other characteristics of such observation. Missing at random (MAR), instead, supposes that the probability of a missing value for a given observation is randomly distributed conditional on the observables (i.e. independent of the unobservables). Finally, missing not at random (MNAR) is the most problematic case, since the probability of a missing

value for a given observation is correlated with the unobservables even after conditioning on the observables.

Various methods for dealing with missing values have been employed in the literature. As outlined above, simply deleting missing cases introduces bias unless the MCAR assumption holds, but even in the case of MCAR, list-wise deletion harms efficiency. Missing data can also be imputed by a number of variants of mean imputation, ranging from a straight replacement with the mean value, to the use of hot deck imputation (assigning a value from another observation with the same covariates), up to regression methods, which impute a fitted value predicted from a model. All these mean-based methods suffer from too little variation, because they are taking a single, albeit motivated guess at each missing value, which ultimately means reduced uncertainty with respect to what would have been the "true" full dataset (unobserved). These mean-based strategies therefore result in a downward bias in the standard errors of the estimates.

One of the more recent innovate developments in imputation approaches is multiple imputation. The idea behind multiple imputation (MI) stems exactly as a solution to these issues: creating multiple sets of imputed values to introduce some uncertainty in the process. Multiple imputation hinges on the MAR assumption. This assumption seems appropriate in the context of this study when considering that the absence of a given country from international mathematics assessments can be explained by a number of observables such as measures of income, health and quantity of schooling, but once conditioning on those variables the probability of a missing value should be exogenous to student-level math achievement in the U.S.

Different methods have been developed within the multiple imputation framework. Multivariate normal models assume that all variables are normally distributed. Multiple

imputation by chained equations, on the other hand, does not require such assumption because each variable with missing values is imputed with its own model (Royston and White, 2011), therefore according to its distribution. Borrowing from the work of Azur et al. (2011) the chained equations process can be described in 4 steps:

1. "A simple imputation, such as imputing the mean, is performed for every missing value in the dataset. These mean imputations can be thought of as "place holders."
2. The "place holder" mean imputations for one variable ("var") are set back to missing.
3. The observed values from the variable "var" in Step 2 are regressed on the other variables in the imputation model, which may or may not consist of all of the variables in the dataset. In other words, "var" is the dependent variable in a regression model and all the other variables are independent variables in the regression model. […]
4. The missing values for "var" are then replaced with predictions (imputations) from the regression model. When "var" is subsequently used as an independent variable in the regression models for other variables, both the observed and these imputed values will be used.

Steps 2–4 are then repeated for each variable that has missing data. […]

Steps 2 through 4 are repeated for a number of cycles, with the imputations being updated at each cycle. The number of cycles to be performed can be specified by the researcher. At the end of these cycles the final imputations are retained, resulting in one imputed dataset. […] The idea is that by the end of the cycles the distribution of the parameters governing the imputations (e.g., the coefficients in the regression models) should have converged in the sense of becoming stable. This will, for example, avoid dependence on the order in which the variables are imputed."

In order to impute values for the countries that did not participate in international assessments, we perform multiple imputation by chained equations at the country level. This seems the appropriate multiple imputation strategy in this case, given the skewness in the distribution of countries along the scale of international mathematics assessments, which represents a clear departure from a normal shape. We create 50 imputed datasets, since the literature recommends a number of imputations close to the percentage of missing data (Royston and White, 2011), each with 10 cycles, which is the default set by Royston (2004), and subsequently take the average following Rubin's rules (Schafer, 1999). In Stata, the program `ice` was written and subsequently updated by Royston (2005a; Royston, 2005b; Royston, 2007; Royston, 2009).

We implement four different imputation strategies:

1. joint TIMSS and PISA imputation with country-level covariates;
2. separate TIMSS and PISA imputation with country-level covariates;
3. for Mexico only, TIMSS imputation based on data on % correct answers;
4. imputation based only on country-level test scores.

Correlation coefficients between scores imputed with the first two strategies range between 0.97 and 0.99. We choose the "joint imputation" as our preferred strategy among the two, as it is based on a larger information set. In one case the imputed value for the PISA score would have been below zero, and we reassign it to the observed minimum among positive values (imputed and not imputed). In the first two models, we use covariates that are known to be correlated with the underlying metric we are trying to capture (country-level school quality). Country-level

development has historically been shown to have a positive effect on educational outcomes, both at the individual (Baker, Goesling and LeTendre, 2002) and at the country level (Chiu and Khoo, 2005; Lee and Barro, 2001). Another well-known positive determinant of country-level educational outcomes are enrollment ratios, as for example in Kyriacou (1991). A sizeable literature has also devoted attention to the role of health as enhancer of schooling outcomes, as documented in the earlier work of Behrman (1996) and more recently confirmed through the use of instrumental variables (Ding et al., 2009). The importance of health for education is particularly prominent in developing countries, which represent the largest part of the subset of countries for which imputation is needed: as comprehensively reviewed in Vogl (2012), intra-generationally, educational outcomes are affected by childhood health and life expectancy impacts investments in education; inter-generationally, parental health affects children's educational outcomes. Finally, economist James Poterba has studied the role of demographic structures on the allocation of resources to education, which is negatively associated with the prevalence of elderly population (Poterba, 1997).

Specifically, we include the following variables in order to measure the constructs identified in the literature:

- Percent of Population Below Age 15;
- Life Expectancy at Birth;
- Infant Mortality Rate (per 1,000 live births);
- Total Fertility Rate;
- Gross National Income (GNI) at Purchasing Power Parity per Capita;
- Deaths due to NonCommunicable Diseases (NCDs);

- School enrollment, primary (% net).

The first six variables are taken from the Population Reference Bureau's World Population Data Sheet[31], while the last one comes from the World Bank Indicators[32].

For Mexico only, we have data available on the percentage of correct answers from the 1995 TIMSS, but not on the overall country-level score on the TIMSS scale (Backhoff and Solano, 2003). Mexico participated in the 1995 TIMSS but the Mexican Government decided to withdraw its participation from the study after the tests had been administered and graded. As a result, the IEA destroyed all data and never released Mexico's mean score on the TIMSS scale, while the Mexican Government retained the original tests and later on released the information on the percentage of correct answers. In order to test the robustness of our preferred imputation method, we exploit this information using multiple imputation based on the percentage of correct answers and TIMSS score of all the other countries that participated in the 1995 TIMSS. This Mexico-specific imputation method yields comparable results to the other two imputation methods outlined above: for TIMSS math 4th grade, Mexico ranks between Iran and Portugal irrespective of the imputation method; for TIMSS math 8th grade, Mexico ranks between Iran and Turkey across imputation methods 1 and 3, but with method 2 would rank slightly higher (between Thailand and Lebanon).

The fourth imputation strategy is applicable only to countries that have at least one test score available, as it is based on test scores only. Correlation coefficients between scores imputed with the first and fourth strategies range between 0.93 and 0.96. This fourth method also yields

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[31] http://www.prb.org/Publications/Datasheets/2012/world-population-data-sheet.aspx (as of 8/2/2012).

[32] http://data.worldbank.org/indicator (as of 8/2/2012).

comparable results to the Mexico-specific procedure: both with the third and with the fourth imputation strategies, for TIMSS math 4th grade Mexico ranks between Iran and Portugal, and for TIMSS math 8th grade it ranks between Iran and Turkey.

Our preferred measure of country-level school quality is PISA, both because it has a higher coverage of the sample and because all the countries participated in the most recent assessment (2009, except for Macedonia), which is not the case for TIMSS.

5. Descriptive Analyses

Table 18 presents a description of the analytic sample for this study. Many of the countries of origin of the students in the sample are not listed individually, but collapsed into regions. This is due to reporting constraints from the National Center for Education Statistics: cells with N < 20 cannot be shown in order to prevent disclosure of study participants' identity. The collapsing of countries into regions is for descriptive purposes only, and the country-specific scores will be used in the full multivariate analysis. For descriptive purposes, the collapsing enhances statistical precision by boosting the cell-level sample size for the estimation of means. Appendix 1 describes in detail how all countries of origin with less than 20 students are grouped into regions.

While not reflecting the full variation in the sample, Table 18 summarizes some of the key sources of comparison in this study. When grouping by regions, an average of country-level values weighted by the degree of representativeness in the sample is reported in columns (a), (b), (c), (e), (f) and (g).

The largest "sending" countries are Mexico, China, India and the Philippines. The high positive correlation between the three international test scores is immediately reflected: countries

that tend to do well in one assessment tend to do well in all of them, and vice versa. Also, there is a tendency to migrating with younger kids from some countries: Germany, and Southern and Western Europe tend to "send" students with lower likelihood of having attended any school in their home countries (and consequently students from those countries display the lowest average number of years of schooling before migrating). Other countries display an opposite pattern: students coming from America, South-Central Asia and Africa tend to arrive later in the U.S. These tendencies might be related to different migration motives and circumstances, making it more or less likely for parents to be able choose the timing of migration in order to minimize the disruption of the children's learning path.

Column (g) reports the theta score of the 9th grade math assessment, instead of the standardized theta score described above (which will be used in the regression analyses). Since the theta score ranges between -2.58 and 3.03, it provides a more immediate way of visually identifying countries (or regions) from which students tend to perform better or worse than their peers. Relative to fall 2009 9th graders, students coming from countries that offer academic quality above the U.S. (as measured by the international test scores) achieve higher math scores on average. The opposite also holds for a number of cases but there are notable exceptions, among which India stands out for a very good performance from a relatively low country-level starting point; selection of the pool of migrants might play a major role.

Interestingly, there is some suggestive evidence that the relationship between home-country academic quality and learning achievement in the U.S. might be stronger the longer time the student has spent in the home-country schooling system. For example, among countries offering better academic quality than the U.S., South Korea seems to provide a more significant legacy on the math tests scores of its migrants than Germany: South Korean students spend on average 5

times as long as their German peers in their home country's schools. Towards the bottom of the distribution, Pakistani students (with an average of 1.1 years in their home country's schools) do not seem to suffer from poorer academic quality as much as their fellows from the Caribbean (with an average of 2.4 years in their home country's schools), although the latter benefit from a significantly better academic environment before migration on average. The regression analyses introduced in the next section will try to explore these patterns in a more systematic way.

Table 18. Sample statistics.

Country of Origin (ranked by 2009 PISA Score)	(a) PISA Grade	(b) TIMSS 8th Grade	(c) TIMSS 4th Grade	(d) Total % of Sample	(e) % Attend School Outside of U.S.	(f) Average # of Years Attending School Outside of U.S.	(g) 9th Grade Math Assessment	(h) n
China	555	607	572	8.83%	28.57%	1.4	0.996	105
South Korea (Republic of Korea)	546	581	597	5.55%	34.85%	2.0	0.998	66
Japan	529	568	570	2.35%	28.57%	1.4	1.007	28
Canada	527	506	531	2.02%	20.83%	0.8	1.010	24
Germany	513	525	502	2.52%	6.67%	0.4	0.236	30
Northern Europe	496	533 [#]	510 [#]	1.67%	39.99%	1.9	0.478	20
United States	487	529	508	NA	NA	NA	0.136	14,392
Southern and Western Europe	473 [#]	489 [#]	487 [#]	3.26%	17.95%	0.6	0.574	39
Eastern Europe	450 [#]	503 [#]	482 [#]	2.69%	46.87%	1.4	0.357	32
Puerto Rico	437 [#]	439 [#]	458 [#]	3.78%	60.00%	2.7	-0.413	45
South Eastern, Eastern Asia and Oceania	422 [#]	446 [#]	448 [#]	3.69%	52.27%	2.6	0.336	44
Mexico	419	436 [#]	421 [#]	21.53%	35.55%	1.4	-0.339	256
Western Asia	410 [#]	397 [#]	406 [#]	2.18%	30.77%	1.5	0.394	26
Viet Nam (Vietnam)	407 [#]	419 [#]	426 [#]	1.85%	36.36%	1.8	0.557	22
Colombia	381	355	380	2.19%	42.31%	1.5	0.304	26
South America	367 [#]	367 [#]	393 [#]	4.61%	63.64%	2.4	0.459	55
Caribbean	362 [#]	359 [#]	382 [#]	4.88%	55.17%	2.4	-0.290	58
Central and Northern America	341 [#]	338 [#]	365 [#]	3.36%	67.50%	2.7	-0.212	40
India	326 [#]	314 [#]	330 [#]	8.33%	39.39%	1.5	0.983	99
Philippines	313 [#]	358	378	6.14%	58.90%	2.4	0.370	73
South-Central Asia	280 [#]	242 [#]	296 [#]	2.43%	62.07%	3.5	0.216	29
Pakistan	206 [#]	170 [#]	227 [#]	1.93%	34.78%	1.1	-0.019	23
Africa	140 [#]	152 [#]	191 [#]	4.09%	65.30%	2.6	-0.366	49
Total (immigrants)				100.00%	42.05%	1.8	0.274	1,189

[#] Indicates that the country of origin did not participate in the test; the reported score is imputed.
For regions, it indicates that at least one country in the region did not participate in the test and had the score imputed.

6. Regression Analyses

In order to model the three research questions that this study seeks to address, we borrow from the framework which Hanushek and Woessman (2012) used to estimate the effect of home-country international test scores on earnings of immigrants to the U.S. Specifically, they designed a difference-in-differences approach, in which the effect of home-country test scores is identified by comparing earnings of immigrants from countries with varying levels of test scores both in the "treatment group" (home-country-educated immigrants) and the "control group" (U.S.-educated immigrants), and then the difference is taken of the differences between the two groups. Hanushek and Woessman only consider individuals who have been educated either completely at home or completely in the U.S., because they need a clear distinction between treatment and control for their research design. On the contrary, since we are interested in the trajectories of immigrants, we modify this approach in order to exploit the information on the "dose" (i.e. the length) of exposure to home-country education in order to study whether the intensity of the "response" (i.e. the relationship between home-country academic quality and 9th grade math test scores) varies based on it. Adapting this framework, we estimate a basic model of the following form:

$$MathScore_{ic} = \propto + \beta YearsinUS_{ic} + \gamma AcademicQuality_c + \delta YearsinUS_{ic} * AcademicQuality_c + \boldsymbol{\theta}' \boldsymbol{X_i} + \varepsilon_i,$$

where i indexes students, c indexes home countries, and $\boldsymbol{X}$ is a vector of covariates, through Ordinary Least Squares with heteroskedasticity-robust standard errors.

X contains gender, age, race, whether the student is bilingual, whether the student has repeated grades, parental education, parental income[33], family situation (whether the student lives with both biological parents, a single biological parent, a biological parent and a step parent, or other), parents' expectations on the student's education, student's expectations on own education, whether the student attends school in an urban, suburban, or rural location, whether the student attends a private school, whether the student attends school in a State with favorable tuition and financial aid state policies for undocumented immigrants[34], and a series of indicator variables for imputed or missing covariates[35]. The choice of the covariates has been informed by the literature review. Various studies have highlighted the importance of bilingualism (Portes and Rumbaut, 2001; Glick and White, 2003; Zhou, 1997), urban location (Portes and Rumbaut, 2001; Hirschman, 2001), parental socioeconomic status (Portes and Rumbaut, 2001; Hirschman, 2001; Fry, 2007; Glick and White, 2003; Glick and White, 2004), age (Portes and Rumbaut, 2001), gender(Portes and Rumbaut, 2001), educational expectations (Portes and Rumbaut, 2001; Glick and White, 2004), family structure (Portes and Rumbaut, 2001; Hirschman, 2001; Fry, 2007; Glick and White, 2003; Zhou, 1997) and grade repetition (Glick and White, 2004).

$\hat{\beta}$ captures the relationship between years in the U.S. an mathematics achievement: we expect that, holding everything else constant, having been in the U.S. for a longer time is positively associated with learning.

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[33] In logarithmic scale, continuous variable obtained assigning the mid-point of the corresponding bracket.

[34] See Appendix 1 for additional detail on this variable.

[35] Missing: bilingual, ever repeated a grade, grade of placement after migration, country of origin. Imputed (imputations already processed in the HSLS:09): 9th grade math test score, race, parental education, living situation, parental income, student's expectations, parents' expectations.

$\hat{\gamma}$ describes the relationship between home-country academic quality and mathematics achievement: we expect that, ceteris paribus, coming from a country with a better average academic quality in mathematics is positively associated with performance on the mathematics test.

Finally, $\hat{\delta}$ is the key parameter of interest: it represents an estimate of the derivative of the association between years in the U.S. and 9th grade math test score with respect to home-country average international mathematics test score; in other words, $\hat{\delta} = 0$ is the null hypothesis that allows me to test whether a straight-line assimilation (failure to reject the null hypothesis) or a segmented one (rejection of the null) is the theoretical framework that best fits the data. A priori, we expect a negative sign for this coefficient: the number of years in the U.S. should matter less for students coming from countries with better academic quality, or, analogously, the influence of the home-country academic quality should diminish as the student spends longer time in the U.S.

One limitation of this model stems from the simple observation that schools vary considerably in their quality within countries. This model does not capture this variation, neither in the home countries, as it only focuses on country-level means, nor in the schools that receive immigrant students in the U.S. By focusing on the average of home-country academic achievement in mathematics, this study seeks to offer a potential explanation to some of the unexplained country-level variation observed in young immigrants' educational attainment. While within-country variation in pre-migration academic quality is important for the outcome that we examine, it will be at least in part related to the family socioeconomic status that we

control for. Ultimately, the data limits our possibility of controlling for finer measures of pre-migration educational inputs.

The same variation in school quality will play a role in the receiving country. The context of reception for immigrant students in the U.S. is largely determined by what school they attend. While again the general quality of instruction will be in part determined by family socioeconomic status, even within similar standards different schools might be attaining very different outcomes in terms of welcoming immigrant students. In our model, some rough measure of State-level variation in policies towards the education of immigrants is included, but we are unable to capture the precise situation of individual schools. However, this would matter only if unobserved variation in U.S. school quality was correlated with our key explanatory variables, conditional on the covariates. For example, some high-SES individuals from a given country with school level l might decide to migrate only when they are sure that their child will be placed in a high quality school in the U.S. if their child is a very good student at home (thereby shifting the distribution if their migration times to the right); other high-SES individuals from a country with the same school level l might decide to migrate whenever it is most convenient for them if their child is a poor student and they believe that, irrespective of the school quality she is assigned to, she cannot do worse. In this case, there would be an unobserved negative correlation between time in the U.S. and 9th grade math test score which would make me underestimate the positive magnitude of $\hat{\beta}$, i.e. if anything it would make me less likely to fail to reject the null hypothesis of $\hat{\beta} = 0$. This kind of mechanism might be much more infrequent among low-SES immigrants, who most likely have no control over the timing of their choice. Furthermore, conditional on SES, controlling for educational expectations should absorb some of the remaining unobserved variation in the quality of the school attended in the U.S.

We estimate this model on three different samples:

1. the full sample of immigrants (n = 1189), assigning the score imputed with method 1 to countries without an observed score (method 3 for Mexico);
2. the "intermediate" sample of immigrants (the sample of immigrants from countries with at least one observed test score, n = 798), assigning the score imputed with method 4 to countries without an observed score;
3. the restricted sample of immigrants (the sample of immigrants from countries with the respective test score available, i.e. the models run with PISA scores only include immigrants from countries that have an observed PISA score, n varies).

We create a "standardized" version of the international test scores, so that everything is expressed in terms of deviations from the U.S. values. We subtract the U.S. score from each country's score, then divide by the "standard deviation" from the U.S. score (i.e. the square root of the mean of the squared deviations from the U.S. score). This "standard deviation" is computed on the whole sample of countries (i.e. imputation method 1 is used) in order to obtain a unique measure that can be used both in the models that we run on the full sample and in those that we run restricting to immigrants from countries for which we observe original test scores.

7. Results

Table 19. Summary of main results by choice of international assessment and sample.

Proxy for home-country academic quality		Full Sample (no interaction)	Full Sample (interaction)	Intermediate sample (no interaction)	Intermediate sample (interaction)	Restricted sample (no interaction)	Restricted sample (interaction)
PISA	*# years in US*	0.310 ***	0.091	0.224 *	0.133	0.166	0.131
	home-country academic quality	3.163 ***	5.715 ***	1.783 ***	2.968 ***	4.530	6.692 ***
	interaction		-0.418 ***		-0.200		-0.346
	N	*1189*		*798*		*683*	
TIMSS 4	*# years in US*	0.309 ***	0.039	0.221 *	0.094	0.208	0.160
	home-country academic quality	2.626 ***	5.261 ***	1.567 ***	3.013 ***	4.140	6.781 ***
	interaction		-0.433 ***		-0.241		-0.455
	N	*1189*		*798*		*430*	
TIMSS 8	*# years in US*	0.316 ***	0.071	0.222 *	0.114	0.253	0.215
	home-country academic quality	2.866 ***	5.527 ***	1.869 ***	3.249 ***	3.951	7.128 ***
	interaction		-0.435 ***		-0.229 *		-0.528 *
	N	*1189*		*798*		*495*	

*Notes: *** indicates p-value < 0.01 and * indicates p-value < 0.1;*

Home-country academic quality expressed in SD from the U.S.

All models control for: gender, age, race, whether the student is bilingual, whether the student has repeated grades, parents' education and income , family situation, parents' and student's expectations on the student's education, whether the student attends school in an urban, suburban, or rural location, whether the student attends a private school, whether the student attends school in a State with favorable tuition and financial aid state policies for undocumented immigrants, and indicator variables for imputed or missing covariates.

Table 19 summarizes the results of the main regression analyses for the key variables of interest. Detailed results are presented in Table 23-Table 25: main results – TIMSS 8th grade. Results are for most part insensitive to the choice of proxy for home-country academic quality. In the full sample, an additional year of school in the U.S. is associated with roughly 0.03 standard deviations (the standardized theta math score has mean 50 and standard deviation 10) higher 9th grade math test scores. Home-country academic quality is positively associated with the 9th grade math test score: in particular, coming from a country one standard deviation above the U.S. in PISA (175 points in the full sample) is positively associated with a roughly 0.32 standard deviations higher math score. One such country does not exist in the sample, since the U.S. is above the middle of the distribution. However, the same gap in PISA scores can be traced, for example, between Indonesia and South Korea. Another example is Singapore, which

scores 75 points above the U.S. (equivalent to 0.43 standard deviations in the full sample): therefore, Singaporean students have a “baseline” advantage of 0.136 standard deviations in the math test. On the other end of the spectrum, India has an imputed PISA value one standard deviation below the U.S. (311), so its students would suffer from a baseline disadvantage of 0.32 standard deviations in the math test when coming to the U.S.

These results do not account for the segmentation of assimilation, i.e. for the potential for a segmented effect of time: the interaction terms indicate that the effect of time spent in the U.S. varies with home-country academic quality, and is stronger the poorer the latter is. In other words, the effect of home-country academic quality decreases the longer time immigrant youth spend in U.S. schools. An extra year spent in the U.S. reduces the effect of home-country academic quality by roughly 0.04 standard deviations. As a result, students from countries with better home-country school quality than the U.S. enjoy a positive effect of their country’s academic background which decreases in the time spent in the U.S.; on the contrary, students from countries with better home-country school quality than the U.S. enjoy a negative effect of their country’s academic background which decreases (in absolute terms) in the time spent in the U.S.

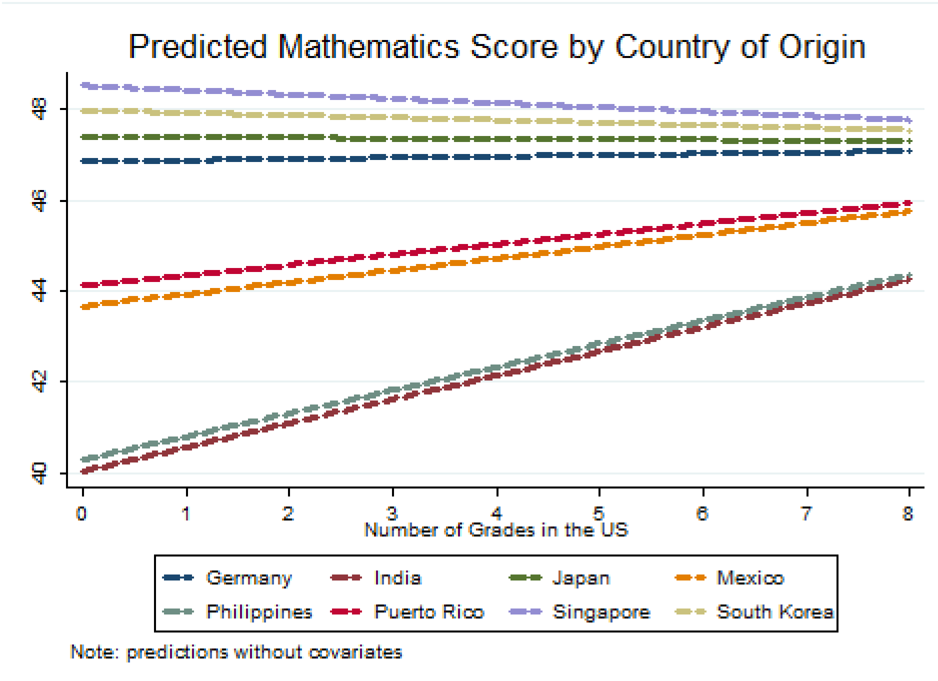
The magnitude of these results can be compared to findings in the literature on the effects of other dimensions that are typically thought of as influencing academic performance. Thanks to a natural experiment in the UK, the impact of an additional year of parents’ schooling on test scores has been estimated at 0.1 standard deviations (Dickson, Gregg and Robinson, 2013); by virtue of exploiting expansions in the Earned Income Tax Credit in the U.S., a $1,000 increase in family income has been shown to raise combined math and reading test scores by 0.06 standard deviations (Dahl and Lochner, 2012); finally, an extra day of school has been related to a 0.0125

standard deviations increase in academic performance in California elementary schools (Jez and Wassmer, 2013). This evidence suggests that, in terms of the effect on academic performance, the difference in home-country academic quality between the U.S. and India, or between South Korea and Indonesia, (per se, i.e. without interaction with time spent in the U.S.) is roughly comparable to 26 extra days of school in a year, an increase of more than $5,000 in family income, or an increase of more than 3 years in parental education.

As the sample size is restricted, some of the coefficients on the key variables of interest tend to lose significance. However, their sign and order of magnitude tend to be confirmed. This is consistent with the expected reduction in statistical power, but also with the reduced variation in international test scores once the sample is progressively restricted towards a set of countries that are predicted to score higher than those which do not participate in one or more international assessments. Notwithstanding these limitations, in the models with TIMSS 8^{th} grade as a proxy the coefficient on the interaction term retains some significance.

To illustrate the relationship more clearly, we plot predicted values of the 9^{th} grade math test scores of immigrants coming from different countries relative to their U.S.-born peers using the parameters from the PISA test score models.

Figure 18. Learning trajectories of immigrant students relative to the U.S.-born peers.



In Figure 18, the different intercepts are determined by the gradient of standard deviations of home-country academic quality with respect to the U.S. In particular, the 4 lines at the top represent students from countries (Singapore, South Korea, Japan and Germany), whose PISA math scores are above the U.S.; conversely, the 4 lines at the bottom represent students from countries (Puerto Rico, Mexico, Philippines and India) whose PISA math scores are below the U.S. The slopes are jointly determined by the coefficient on the number of years and the coefficient on the interaction term, yielding a country-specific trajectory given the different average performances in PISA. Students from countries that start with a high performance relative to the U.S. tend to stay on an upper trajectory; however, they display some sign of downward convergence towards their native-born peers. As they progress along the line of integration in the U.S. school system, they tend to lose a bit of the advantage accumulated in their home country, but their marginal decline is much smaller than the gains displayed by

students coming from less competitive academic backgrounds. Students from Mexico, the Philippines, Puerto Rico and India, display a significant convergence in achievement as a function of time, and such convergence is stronger the larger the initial gap with their U.S. peers.

Overall, these findings support the hypothesis that time spent in the U.S. and home-country academic quality are positively associated with post-migration educational outcomes. Furthermore, the strength of the legacy of home-country academic quality is mediated by time spent in the U.S.

8. Robustness Checks

To test the validity of the findings, a series of robustness checks were performed. These are aimed at eliciting the sensitivity of results to the imputation process and its variants, the functional form of the relationship between years spent in the U.S. and 9th grade math score, and the choice of the proxy for home-country academic quality.

One concern with the imputation process for this study is that the country-level covariates used to impute the proxy for school quality are endogenous to the regression model; therefore the results obtained including observations with imputed values could be spurious. More precisely, the imputed proxy could be showing a statistically significant effect not because the construct of home-country academic quality itself matters for the learning trajectories of immigrant youth, but rather because the underlying country-level measures have themselves significant explanatory power for the observed variation.

In order to address this concern, we run the models on the full and "intermediate" samples including as controls all the variables used in the imputation process. If the country-level covariates, instead of the school quality metric, were responsible for the observed statistical

relationships, the coefficients involving home-country academic quality ($\hat{\gamma}$ and $\hat{\delta}$) should lose significance in favor of the newly introduced parameters. On the contrary, as Table 20 shows, the coefficient on the interaction term retains significance in the full sample and, with respect to the main set of results (Table 19) gains marginal significance in the intermediate one for the PISA and TIMSS 4th grade definitions, suggesting that the mechanism we are trying to test appears robust to the choice of imputing variables. The magnitudes of the estimates remain very similar to the main specifications. Detailed results of this robustness check are presented in Table 26-Table 28.

Table 20. Summary of results when including all variables used in the imputation.

Proxy for home-country academic quality		Full Sample *(no interaction)*	Full Sample *(interaction)*	Intermediate sample *(no interaction)*	Intermediate sample *(interaction)*
PISA	*# years in US*	0.290 ***	0.069	0.255 *	0.144
	home-country academic quality	2.542	5.296 ***	2.835 **	4.161 ***
	interaction		-0.422 ***		-0.233 *
	N	*1189*		*798*	
TIMSS 4	*# years in US*	0.283 ***	0.003	0.251 *	0.102
	home-country academic quality	-0.724	2.077	0.337	1.937
	interaction		-0.446 ***		-0.276 *
	N	*1189*		*798*	
TIMSS 8	*# years in US*	0.286 ***	0.030	0.247 *	0.119
	home-country academic quality	1.199	4.057 ***	1.604	3.197 **
	interaction		-0.449 ***		-0.264 *
	N	*1189*		*798*	

*Notes: *** indicates p-value < 0.01, ** indicates p-value < 0.05 and * indicates p-value < 0.1;*
Home-country academic quality expressed in SD from the U.S.
In addition to the covariates listed in Table 4, these models control for all the variables used in the imputation process. education and income , family situation, parents' and student's expectations on the student's education, whether the student attends school in an urban, suburban, or rural location, whether the student attends a private school, whether the student attends school in a State with favorable tuition and financial aid state policies for undocumented immigrants, and indicator variables for imputed or missing covariates.

In order to check the sensitivity of results to the functional form of the relationship between years spent in the U.S. and 9th grade math score, we employ a logarithmic functional form for the relationship between number of years in the U.S. and math assessment (Table 29-Table 31). The natural logarithm transformation has the effect of making the difference between years smaller as the number of years increases. In other words, as opposed to a linear functional form, this

transformation models the relationship in a way that the average marginal effect declines in the number of years, i.e. $\frac{\partial MathScore_{ic}}{\partial YearsinUS_{ic}} = \hat{\beta} > 0$ and $\frac{\partial^2 MathScore_{ic}}{\partial^2 YearsinUS_{ic}} = \frac{\partial \hat{\beta}}{\partial YearsinUS_{ic}} < 0$.

The results are qualitatively in line with the main models.

We also try using the Human Development Index (HDI) instead of the proxies for home-country academic quality. The HDI is a summary measure of development including health, schooling and income, and the data used comes from the 2011 Human Development Report (UNDP). This robustness check serves two purposes. First, to employ a metric that does not need imputation since it is readily available for all of the countries in the sample. The only exception is Puerto Rico, for which we construct the HDI computing the single components: life expectancy at birth in 2011 from the Population Reference Bureau[36], mean years of schooling from the American Community Survey's 1-year estimates for 2011[37], and Gross National Income (GNI) per capita from UN Stats[38]. Second, to test whether a measure of an alternative construct, presumably highly correlated to our original measures of home-country academic quality, would show similar statistical patterns in a sort of placebo falsification test: as it turns out, the interaction term in the full sample is not statistically significant; the same holds when employing non-income HDI, where only the health and education components of the index are considered (Table 32). This indicates that the underlying construct that international test scores and our imputation process capture, which we have termed "home-country academic quality", and not a "generic" measure of development, is relevant for explaining the very specific variation in 9th grade math scores following migration.

[36] http://www.prb.org/DataFinder/Topic/Rankings.aspx?ind=6 (as of 9/10/2013).

[37] http://factfinder2.census.gov/faces/nav/jsf/pages/searchresults.xhtml?refresh=t (as of 9/10/2013).

[38] http://unstats.un.org/unsd/snaama/dnllist.asp (as of 9/10/2013).

Finally, in order to test the sensitivity of results to the choice of imputation method, we use imputation method 1 (instead of method 4) on the intermediate sample. Table 29-Table 31 show that the results are robust to the choice of the imputation method.

9. Policy Context and Relevance of the Findings

The findings summarized by Figure 18 directly relate to two specific policies recently implemented in the U.S. First, the "so called" pathways to citizenship are a policy choice that enters into tension with the scarcity of public resources and the perception that net social welfare losses are generated by spending money on the education of immigrants. In this context, the Deferred Action for Childhood Arrivals (DACA), which was launched in mid-2012 by the U.S. Government, introduced relief from deportation and the possibility for work authorization for undocumented immigrants who arrived in the U.S. before age 16. The design of this policy has opened up new opportunities and challenges for education: it has placed new demands on schools, both administrative and substantial, as they are called upon providing records of enrollment or graduation and advice to students seeking to benefit from the policy, and most importantly upon educating these new young members of the American society; on the other hand, it could enhance immigrant students' motivation to complete high school and ability to pursue higher education, as many States have formalized DACA status as a requirement to benefit from in-State tuition at State institutions.

The second important new policy that is related to the findings of this study is the Common Core State Standards (CCSS), a States-led initiative to set common standards for math and English language arts in order for learning to be more uniform across the U.S. The phenomenon of migration poses very practical challenges to the achievement of the CCSS policy objectives,

although at the same time there exists an opportunity to learn from immigrant students' adaptation, an "extreme version" of the heterogeneity that each teacher faces in a given classroom, to understand how to optimally allocate scarce resources in order to achieve uniform targets in the presence of very variegated initial conditions.

The results of this study can provide some interesting implications for the opportunities and challenges highlighted in the policy context discussed above. We have learnt that assimilation is segmented: home-country academic quality is a significant predictor of the high school achievement of immigrants: there exists great heterogeneity and this study has identified one important source of it. By virtue of examining trajectories that stem from this fact, our results have shown that even students with the poorest home-country academic quality can, in principle, eventually catch up, since the marginal productivity of time spent in the U.S. is highest for students with lowest HCAQ. Some practical insights directly stem from these simple conclusions, as from the study of immigrant children we can learn how to deal with heterogeneity in education in general. A first, obvious implication is that there cannot be a one-size-fits-all approach – the descriptive analyses in Section 5 have shown that great minds do not always think alike, but also differently. Secondly, the results of this study offer some indications that homogeneous sorting of students can be both complicated (as there exist many different dimensions of heterogeneity) and encounter the risk of disproportionately disadvantaging the students with the poorest backgrounds. On the contrary, heterogeneous groups can be assigned heterogeneous tasks: in order to make sure that all students are pushed to their limits, while the difficulties can and should be different, the complexity (i.e. the level of thinking) should be the same. In other words, in the context of a heterogeneous group, teaching to the average does not maximize aggregate learning. When examining the trajectories of immigrant students, we have

found that the aim should be both to bring the bottom up to speed and not to lose the top. In this regard, language, culture, prior knowledge and experiences are assets that teachers can leverage upon in order to maximize cross-fertilization and overall results[39].

10. Conclusion

The analyses presented in this study help shedding some light on the three research questions that originally motivated it.

Pre-migration academic quality, as measured with all limitations related to the use of international assessments, is a significant predictor of math achievement in the U.S., and therefore a candidate to bridging, at least partially, the gap that literature in the field of migration has residually attributed to the country of origin per se. The role of schooling in the home country has a relevant legacy effect on performance in the receiving country. This has important policy implications for the way receiving countries manage the integration of immigrants in their school systems. First of all, it implies that there is no one-size-fits-all approach that will maximize the learning of the whole student body, be they native or born abroad. Secondly, it suggests that there are important cross-fertilization effects that would get lost in any strategy for the composition of classes involving some degree of sorting by geographical background. The gains of students coming from more disadvantaged country-level academic backgrounds are presumably at least in part attributable to the drive these pupils experience towards achieving the level of their peers; the instruction and learning support that they receive might initially be at a

[39] These insights, however, should not be interpreted as an indication of whether a centralized approach such as that of CCSS may or may not be efficient in the case of the U.S. Other examples exist of leaner sets of common standards and greater autonomy left to local districts (e.g. Finland), and it is not the aim of this study to evaluate which model is most effective.

higher level than what they experience before, but in the medium run they "pull" them upwards. Losing such gains would reduce societal welfare not only in the present but also in the future, when new or future citizens struggle to find their way into the labor market.

Time spent in the U.S., as measured by the number of grades taken after arriving in the country, is positively associated with math achievement. Acculturation is at play, but the results show that assimilation is not happening on a straight line; rather, it is segmented along home-country academic quality, and the interaction term in our model is the key to identifying such segmentation. The analyses show that the legacy of home-country academic quality decreases over time, and it does so faster the further apart home-country academic quality is from the U.S. average: given the U.S. relative position in the international assessments, on net there appears more to gain from bottom-up trajectories (i.e. those of students moving from countries with poorer academic quality on average) than there is to lose from top-down ones (i.e. those of students moving from countries with better academic quality on average). Again, this sends a very strong signal for policy: even the students coming from the most-disadvantaged country-specific academic backgrounds can eventually catch up with their native peers, but allowing enough time is of the essence in decreasing the mediating role that educational input received before migration continues to exert on current learning. If on one hand the provision of education needs to be "patient" enough with students who need to catch up, as they eventually will, on the other hand educators are faced with an important challenge in making sure that the stock of learning that immigrants from better-performing school systems infuse into the receiving country does not get dispersed. Initiatives aimed at encouraging the students to share experiences, memories and facts from their respective countries will send the message that those rich

backgrounds are recognized and valuable not only to the individual students but also to their peers.

In fact, these analyses are only a first step along a relevant research agenda on the challenges and opportunities that the inflow of significant shares of students with varied academic backgrounds opens for the receiving country's school system. It would be interesting to study the effects of such variety on the performance of native students, although a robust research design would be needed in order to identify them. Perhaps more naturally following this study, one potential extension could consider the role of the academic quality in the receiving context. Such line of work could help address an intrinsic limitation of this study, which considers the whole U.S. as a single receiving entity, while there is great underlying variation in the quality of instruction that a student might face post-migration; at the same time, it could exploit such variation to explore in greater detail the relationship between the relative position of the sending and receiving countries' school systems, and its implications for the learning trajectories of young, migrating students. Future research could also consider investigating the role of specific curricular items that receive different attention across countries in shaping not only the assimilation dynamics at the heart of this study, but also the subsequent success of students on an increasingly global market for higher education.

At a time of public budget cuts this study hints at positive returns on investments in education to immigrant students, and shows how such returns can be an increasing function of the initial educational gap. A short-term approach that would naturally be discouraged by severe initial difficulties should be replaced by a medium-to-long-term vision with a view to the potential social benefits that this simple analytical model has begun to illustrate.

Appendix 1: Countries of Origin with Less than 20 Students

All countries of origin with less than 20 students are grouped into regions in the following fashion, based on the classification of countries by region of the world provided by the United Nations Population Division[40]:

Table 21. Regional groups of countries with less than 20 students in the sample.

	Africa	
	Burundi	
	Ethiopia	
	Kenya	
	Tanzania (United Republic of)	
	Zambia	
	Asia and Oceania	
South Eastern and Eastern Asia and Oceania	South-Central Asia	Western Asia
Hong Kong	Kazakhstan	Armenia
Taiwan	Bangladesh	Azerbaijan
Mongolia	Bhutan	Iraq
Indonesia	Iran	Israel
Laos	Nepal	Lebanon
Malaysia	Sri Lanka	Qatar
Myanmar (formerly Burma)		Saudi Arabia
Singapore		Syria (Syrian Arab Republic)
Thailand		Turkey
Australia		United Arab Emirates
New Zealand		
Fiji		
Marshall Islands		
Micronesia (Federated States of)		
Tonga		

[40] http://esa.un.org/unpd/wpp/Excel-Data/country-classification.pdf (as of 9/30/2013).

Europe		
Southern and Western Europe	Eastern Europe	Northern Europe
Albania	Belarus	Denmark
Bosnia and Herzegovina	Bulgaria	Finland
Croatia	Poland	Latvia
Greece	Romania	Lithuania
Italy	Russia (Russian Federation)	England
Montenegro	Ukraine	Wales
Portugal		Scotland
Slovenia		
Spain		
Macedonia		
Austria		
France		
Netherlands		
Switzerland		

America		
Central and Northern America	Caribbean	South America
Belize	Cuba	Argentina
Costa Rica	Dominican Republic	Bolivia
El Salvador	Haiti	Brazil
Guatemala	Jamaica	Chile
Honduras	Martinique	Ecuador
Nicaragua	Trinidad and Tobago	Guyana
Panama		Paraguay
Bermuda		Peru
		Uruguay
		Venezuela (Bolivarian Republic of)

Appendix 2: States with Favorable Tuition and Financial Aid State Policies for Undocumented Immigrants

Table 22: States with Favorable Tuition and Financial Aid State Policies for Undocumented Immigrants: 1997-2012

State	Legislation	Date Enacted	Policy Allows In-State Tuition	Policy Allows In-State Tuition + Finanical Aid
California	AB 540	Jan-02	X	
Illinois	HB 60	May-03	X	
Kansas	HB 2145	Jul-04	X	
Nebraska	LB 239	Jul-06	X	
New Mexico	SB 582	Apr-05		X
New York	SB 7784	Aug-03	X	
Texas	HB 1403	Jun-01		X
Utah	HB 144	Jul-02	X	
Washington	HB 1079	Jul-03	X	
Wisconsin	A75	Jun-09	X	

For the definition of the indicator variable capturing whether the student attends school in a State with favorable tuition and financial aid state policies for undocumented immigrants, the States listed in Table 22 were coded as 1.

Appendix 3: Detailed Regression Results[41]

Table 23: main results - PISA

		Full Sample		Intermediate sample		Restricted sample	
		no interaction	*interaction*	*no interaction*	*interaction*	*no interaction*	*interaction*
Number of years in the US		0.310 ***	0.091	0.224 *	0.133	0.166	0.131
Home-country academic quality		3.163 ***	5.715 ***	1.783 ***	2.968 ***	4.530 ***	6.692 **
Interaction			-0.418 ***		-0.200		-0.346
Male		1.329 ***	1.243 **	1.128 *	1.057 *	1.550 **	1.519 **
Age		-0.344	-0.363	0.035	-0.007	0.029	-0.011
Race	black	-2.870 **	-2.643 *	-1.137	-0.975	-3.877	-3.775
(omitted: white)	hispanic	-2.118 **	-2.132 **	-1.928 **	-1.863 **	-1.426	-1.321
	asian	3.542 ***	3.458 ***	2.980 ***	3.065 ***	2.961 ***	2.987 ***
	other	-0.498	-0.650	0.665	0.569	-0.729	-0.667
Bilingual		1.867 ***	1.603 **	4.021 ***	3.860 ***	4.129 ***	3.959 ***
Ever repeated a grade		-3.032 ***	-3.103 ***	-3.159 ***	-3.111 ***	-3.339 ***	-3.328 ***
Missing ever repeated a grade		2.462	2.493	-1.271	-1.159	0.634	0.624
Parental education	less than high school	0.344	0.413	0.848	0.862	0.990	0.966
(omitted: high school)	associate's degree	0.816	0.814	2.213 **	2.288 **	2.274 **	2.300 **
	bachelor's degree	2.886 ***	2.979 ***	3.841 ***	3.901 ***	3.609 ***	3.640 ***
	advanced degree	3.738 ***	3.821 ***	5.416 ***	5.479 ***	4.801 ***	4.822 ***
Log parents' income		0.719 **	0.703 **	0.723 *	0.784 **	1.055 **	1.100 **
Student lives with	both biological parents	0.762	0.771	0.722	0.687	1.194	1.205
(omitted: a biological parent and a step parent)	a single biological parent	-0.675	-0.654	-0.393	-0.403	0.329	0.382
	other	1.326	1.147	1.379	1.218	2.329	2.119
Parents' education expectation	complete high school or less	-1.527	-1.526	-1.725	-1.658	-1.348	-1.202
(omitted: complete a bachelor's degree or start a master's degree)	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.581 ***	-3.599 ***	-3.218 **	-3.175 **	-2.255	-2.170
	complete a master's degree or start a Ph.D.	2.523 ***	2.525 ***	3.000 ***	3.001 ***	3.564 ***	3.604 ***
	complete a Ph.D/M.D/Law/other prof. degree	3.552 ***	3.550 ***	3.253 ***	3.288 ***	3.924 ***	3.997 ***
	don't know	-0.025	0.065	0.690	0.748	1.228	1.304
	missing	-2.616	-2.660	1.022	0.927	0.748	0.839

[41] Any variable not listed in a given table was omitted.

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.186 ***	-3.371 ***	-3.408 ***	-3.456 ***	-2.548 **	-2.537 **
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.630	-1.694	-1.550	-1.509	-0.966	-0.905
	complete a master's degree or start a Ph.D.	1.815 **	1.692 *	2.199 **	2.121 **	2.839 **	2.813 **
	complete a Ph.D/M.D/Law/other prof. degree	2.979 ***	2.853 ***	3.054 ***	3.005 ***	3.252 ***	3.257 ***
	don't know	-2.140 **	-2.228 **	-2.038 **	-2.142 **	-2.120 *	-2.175 **
	missing	-7.792 ***	-7.752 ***	-6.289	-6.427	-5.658	-5.724
School locale	city	1.493	1.572	2.317 **	2.302 **	1.053	1.150
	suburb	1.034	1.040	1.530	1.469	0.113	0.157
	town	(omitted)	(omitted)	(omitted)	(omitted)	-0.941	-0.829
	rural	1.547	1.405	1.814	1.690	(omitted)	(omitted)
Private school		-2.110 ***	-2.280 ***	-2.916 ***	-2.998 ***	-2.632 ***	-2.665 ***
Imputed math test score		-1.257	-1.247	-1.823 *	-1.853 *	-2.268 **	-2.259 **
Imputed race		-0.939	-0.704	-2.194	-1.959	-2.132	-1.681
Imputed parental education		-0.629	-0.727	-3.320 *	-3.363 *	-2.488	-2.523
Imputed living situation		4.613 **	4.914 **	-1.798	-2.776	-2.613	-3.240 *
Imputed parental income		-0.430	-0.282	-1.123	-1.070	-2.767	-2.748
Favorable tuition and financial aid state policies for undocumented immigrants		0.149	0.128	-0.419	-0.423	-1.047	-1.055
Constant		43.762 ***	45.993 ***	36.448 ***	37.165 ***	33.604 ***	33.893 ***
	N	1189		798		683	

Table 24: main results – TIMSS 4th grade

		Full Sample		Intermediate sample		Restricted sample	
		no interaction	*interaction*	*no interaction*	*interaction*	*no interaction*	*interaction*
Number of years in the US		0.309 ***	0.039	0.221 *	0.094	0.208	0.160
Home-country academic quality		2.626 ***	5.261 ***	1.567 ***	3.013 ***	4.140 ***	6.781 ***
Interaction		0.000 ***	-0.433 ***		-0.241		-0.455
Male		1.345 ***	1.256 **	1.131 *	1.054 *	1.168	1.039
Age		-0.376	-0.410	-0.051	-0.097	0.803	0.705
Race (omitted: white)	black	-4.405 ***	-4.256 ***	-1.458	-1.334	-4.809 *	-4.717
	hispanic	-2.053 **	-2.057 **	-1.989 **	-1.906 **	1.866	1.824
	asian	3.269 ***	3.196 ***	2.852 ***	2.934 ***	2.715 **	2.846 **
	other	-0.898	-1.033	0.249	0.182	0.979	0.735
Bilingual		1.670 **	1.383 **	3.758 ***	3.577 ***	4.138 ***	4.039 ***
Ever repeated a grade		-3.027 ***	-3.092 ***	-3.076 ***	-3.017 ***	-1.627	-1.530
Missing ever repeated a grade		2.898	2.952	-0.879	-0.826	0.000 ***	0.000 ***
Parental education (omitted: high school)	less than high school	0.304	0.388	0.756	0.766	-3.654	-3.363
	associate's degree	0.753	0.737	2.163 **	2.215 **	1.005	1.076
	bachelor's degree	2.810 ***	2.885 ***	3.670 ***	3.731 ***	1.691	1.777
	advanced degree	3.636 ***	3.706 ***	5.371 ***	5.427 ***	3.546 ***	3.618 ***
Log parents' income		0.753 **	0.734 **	0.758 *	0.826 **	0.974 *	1.060 *
Student lives with (omitted: a biological parent and a step parent)	both biological parents	0.685	0.685	0.743	0.698	0.264	1.934 *
	a single biological parent	-0.705	-0.681	-0.312	-0.317	-0.973	0.659
	other	1.314	1.077	1.391	1.174	0.000 ***	1.456
Parents' education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-1.542	-1.567	-1.661	-1.590	-4.519 **	-4.581 **
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.682 ***	-3.683 ***	-3.191 **	-3.098 **	-9.203 ***	-9.091 ***
	complete a master's degree or start a Ph.D.	2.601 ***	2.615 ***	3.193 ***	3.207 ***	1.641	1.622
	complete a Ph.D/M.D/Law/other prof. degree	3.539 ***	3.540 ***	3.346 ***	3.405 ***	3.752 ***	3.740 ***
	don't know	-0.049	0.032	0.734	0.796	-0.594	-0.496
	missing	-2.898	-3.017	0.916	0.829	-0.951	-1.922

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.178 ***	-3.291 ***	-3.344 ***	-3.349 ***	-2.349	-2.454
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.573	-1.565	-1.508	-1.439	0.943	0.911
	complete a master's degree or start a Ph.D.	1.835 **	1.756 *	2.281 **	2.222 **	2.867 **	2.681 *
	complete a Ph.D/M.D/Law/other prof. degree	3.033 ***	2.942 ***	3.230 ***	3.199 ***	3.414 **	3.264 **
	don't know	-2.130 **	-2.172 **	-1.966 **	-2.049 **	-0.514	-0.708
	missing	-7.791 ***	-7.693 **	-6.172	-6.383	-7.168	-7.749
School locale (omitted: town)	city	1.475	1.561	2.310 **	2.300 **	3.341 *	3.332 *
	suburb	1.080	1.095	1.538	1.481	2.546	2.482
	rural	1.557	1.409	1.812	1.688	3.410 *	3.253 *
Private school		-2.016 ***	-2.172 ***	-2.844 ***	-2.923 ***	-3.808 ***	-3.967 ***
Imputed math test score		-1.136	-1.097	-1.732 *	-1.747 *	-2.061	-1.993
Imputed race		0.160	0.341	-0.805	-0.651	0.647	0.308
Imputed parental education		-0.560	-0.565	-3.177 *	-3.205 *	2.604	2.588
Imputed living situation		4.465 **	4.673 **	-1.296	-2.465	-2.492	-3.489
Imputed parental income		-0.502	-0.391	-1.276	-1.240	1.227	1.394
Favorable tuition and financial aid state policies for undocumented immigrants		0.074	0.037	-0.541	-0.545	-0.014	0.003
Constant		44.300 ***	47.068 ***	37.551 ***	38.466 ***	23.902	23.234
	N	1189		798		430	

Table 25: main results – TIMSS 8th grade

		Full Sample		Intermediate sample		Restricted sample	
		no interaction	*interaction*	*no interaction*	*interaction*	*no interaction*	*interaction*
Number of years in the US		0.316 ***	0.071	0.222 *	0.114	0.253	0.215
Home-country academic quality		2.866 ***	5.527 ***	1.869 ***	3.249 ***	3.951 ***	7.128 ***
Interaction			-0.435 ***		-0.229 *		-0.528 *
Male		1.320 ***	1.242 **	1.108 *	1.052 *	1.313 *	1.191
Age		-0.405	-0.442	-0.061	-0.121	0.603	0.477
Race	black	-3.771 ***	-3.525 ***	-1.290	-1.133	-1.669	-1.495
(omitted: white)	hispanic	-1.903 **	-1.916 **	-1.573 *	-1.494	0.351	0.475
	asian	3.284 ***	3.186 ***	2.595 ***	2.659 ***	1.648 *	1.797 *
	other	-0.904	-1.027	0.196	0.160	-0.124	-0.223
Bilingual		1.743 **	1.441 **	3.870 ***	3.663 ***	4.386 ***	4.268 ***
Ever repeated a grade		-2.977 ***	-3.041 ***	-3.077 ***	-3.017 ***	-2.262	-1.978
Missing ever repeated a grade		2.774	2.783	-0.891	-0.857	0.560	0.413
Parental education	less than high school	0.401	0.478	0.803	0.816	-1.078	-0.764
(omitted: high school)	associate's degree	0.730	0.728	2.113 **	2.185 **	0.329	0.384
	bachelor's degree	2.796 ***	2.907 ***	3.662 ***	3.750 ***	1.624	1.727
	advanced degree	3.671 ***	3.756 ***	5.323 ***	5.379 ***	3.641 ***	3.682 ***
Log parents' income		0.731 **	0.720 **	0.726 *	0.800 **	0.925 *	1.041 **
Student lives with	both biological parents	0.748	0.765	0.702	0.681	1.317	1.231
(omitted: a biological parent	a single biological parent	-0.706	-0.660	-0.368	-0.346	0.214	0.121
and a step parent)	other	1.324	1.095	1.331	1.105	0.873	0.657
Parents' education expectation	complete high school or less	-1.499	-1.467	-1.688	-1.575	-4.232 **	-4.351 **
(omitted: complete a bachelor's degree or start a master's degree)	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.602 ***	-3.576 ***	-3.218 **	-3.112 **	-8.365 ***	-8.316 ***
	complete a master's degree or start a Ph.D.	2.568 ***	2.604 ***	3.045 ***	3.089 ***	2.529 **	2.541 **
	complete a Ph.D/M.D/Law/other prof. degree	3.573 ***	3.578 ***	3.308 ***	3.364 ***	3.997 ***	3.978 ***
	don't know	0.010	0.115	0.719	0.791	-0.192	-0.067
	missing	0.000 ***	-2.756	0.902	0.894	-1.868	-2.480

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.194	***	-3.342	***	-3.418	***	-3.432	***	-3.384	*	-3.407	*
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.606		-1.613		-1.549		-1.465		1.788		1.909	
	complete a master's degree or start a Ph.D.	1.810	*	1.719	*	2.209	**	2.147	**	2.170		2.084	
	complete a Ph.D/M.D/Law/other prof. degree	2.993	***	2.893	***	3.105	***	3.076	***	3.203	**	3.142	**
	don't know	-2.155	**	-2.232	**	-2.016	**	-2.122	**	-0.927		-1.104	
	missing	-7.882	***	-7.792	***	-6.294		-6.460		######	*	######	*
School locale (omitted: town)	city	1.495		1.572		2.339	**	2.303	**	2.359		2.240	
	suburb	1.059		1.050		1.554		1.464		1.819		1.652	
	rural	1.559		1.398		1.786		1.631		3.159	*	2.888	*
Private school		-2.127	***	-2.283	***	-2.916	***	-2.976	***	-3.381	***	-3.517	***
Imputed math test score		-1.188		-1.158		-1.776	*	-1.799	*	-2.553	*	-2.580	*
Imputed race		0.012		0.261		-0.964		-0.697		-1.867		-2.255	
Imputed parental education		-0.588		-0.661		-3.262	*	-3.312	*	-6.148		-6.171	
Imputed living situation		4.711	***	5.026	**	-1.290		-2.336		-1.483		-2.497	
Imputed parental income		-0.416		-0.282		-1.155		-1.103		0.878		1.027	
Favorable tuition and financial aid state policies for undocumented immigrants		0.128		0.102		-0.493		-0.495		0.120		0.147	
Constant		44.685	***	47.245	***	38.077	***	39.009	***	26.954	*	28.038	**
	N	1189				798				495			

Table 26: robustness checks, inclusion of all country-level variables used in the imputation – PISA

		Full Sample				Intermediate sample			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.290	***	1.289	***	0.257	**	1.799	**
Home-country academic quality		0.015		0.031	**	0.033	**	0.054	***
Interaction				-0.003	***	-64.842	***	-0.003	*
Percent of population below age 15		-22.617		-23.338		0.932	***	-68.115	***
Life expectancy at birth		0.181		0.171		0.370	***	0.933	***
Infant mortality rate (per 1,000 live births)		-0.033		-0.039		3.366	*	0.367	***
Total fertility rate		-0.497		-0.370		0.000		3.668	*
GNI PPP per capita		0.000		0.000		-30.186	***	0.000	
Deaths due to NCD's		-19.041	***	-19.803	***	-0.405	**	-31.016	***
School enrollment, primary (% net)		0.004		0.001		1.127	*	-0.394	**
Male		1.274	**	1.195	**	-0.068		1.064	*
Age		-0.552		-0.580		-5.320	**	-0.142	
Race	black	-2.869	*	-2.652		-1.475		-5.225	**
(omitted: white)	hispanic	-1.788		-1.760		0.526		-1.341	
	asian	2.244	*	2.169	*	-1.451		0.558	
	other	-0.597		-0.763		4.399	***	-1.512	
Bilingual		2.516	***	2.258	***	0.000	***	4.163	***
Ever repeated a grade		-2.386	**	-2.483	***	-0.778		-2.842	***
Missing ever repeated a grade		1.913		1.979		1.622	*	-0.711	
Parental education	less than high school	0.910		0.986		2.096	**	1.619	*
(omitted: high school)	associate's degree	1.643	*	1.631	*	3.420	***	2.171	**
	bachelor's degree	3.150	***	3.216	***	5.363	***	3.464	***
	advanced degree	3.938	***	4.023	***	0.488		5.389	***
Log parents' income		0.849	**	0.805	**	-0.316		0.552	
Student lives with	both biological parents	0.387		0.409		-1.593		-0.063	
	a single biological parent	-0.841		-0.830		-1.279		-1.317	
	a biological and a step parent	(omitted)		(omitted)		(omitted)		-1.019	
	other	0.465		0.253		-1.637		(omitted)	

Parents' education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-1.383	-1.409	-3.280 **	-1.515
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.012 **	-3.016 **	2.831 ***	-3.211 **
	complete a master's degree or start a Ph.D.	2.324 ***	2.346 ***	3.076 ***	2.852 ***
	complete a Ph.D/M.D/Law/other prof. degree	3.342 ***	3.313 ***	0.564	3.144 ***
	don't know	0.330	0.370	0.239	0.629
	missing	-1.891	-2.018	-3.162 ***	0.233
Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.362 ***	-3.525 ***	-1.393	-3.184 ***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.195	-1.178	1.987 *	-1.317
	complete a master's degree or start a Ph.D.	2.045 **	1.918 **	3.074 ***	1.901 *
	complete a Ph.D/M.D/Law/other prof. degree	3.092 ***	3.016 ***	-2.038 **	3.037 ***
	don't know	-2.234 **	-2.297 **	-6.191	-2.141 **
	missing	-5.695 **	-5.605 *	2.107 *	-6.366
School locale	city	0.359	0.559	1.341	2.096 *
	suburb	-0.375	-0.244	(omitted)	1.255
	town	-1.408	-1.272	1.805	(omitted)
	rural	(omitted)	(omitted)	-2.743 ***	1.653
Private school		-2.464 ***	-2.579 ***	-1.483	-2.795 ***
Imputed math test score		-1.588 *	-1.566	-2.067	-1.478
Imputed race		-2.777	-2.410	-2.766	-1.637
Imputed parental education		-1.575	-1.606	-1.574	-2.790
Imputed living situation		4.319 **	4.639 *	-1.198	-2.876
Imputed parental income		-0.660	-0.488	(omitted)	-1.164
Imputed parents' expectations		(omitted)	(omitted)	-0.015	(omitted)
Favorable tuition and financial aid state policies for undocumented immigrants		0.026	0.025	0.000 ***	-0.027
Missing country of origin		(omitted)	(omitted)	21.603	(omitted)
Constant		48.093 ***	44.289 ***	36.448 ***	12.513
	N	1083		789	

Table 27: robustness checks, inclusion of all country-level variables used in the imputation – TIMSS 4th grade

		Full Sample				Intermediate sample			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.283	**	1.347	***	0.250	*	1.403	**
Home-country academic quality		-0.004		0.012		0.011		0.025	*
Interaction				-0.003	***			-0.002	*
Percent of population below age 15		-40.002	**	-41.235	***	-71.926	***	-75.331	***
Life expectancy at birth		0.240		0.243		0.971	***	0.966	***
Infant mortality rate (per 1,000 live births)		-0.040		-0.045		0.382	***	0.373	***
Total fertility rate		-0.002		0.096		3.360	*	3.689	*
GNI PPP per capita		0.000	*	0.000	*	0.000		0.000	
Deaths due to NCD's		-16.068	***	-16.991	***	-28.281	***	-28.849	***
School enrollment, primary (% net)		0.067		0.061		-0.327	*	-0.316	*
Male		1.192	**	1.106	**	1.083	*	1.012	*
Age		-0.502		-0.551		-0.038		-0.115	
Race	black	-2.148		-2.034		-4.991	**	-4.887	**
(omitted: white)	hispanic	-1.207		-1.187		-1.711		-1.599	
	asian	3.076	***	2.983	**	1.414		1.457	
	other	-0.065		-0.233		-0.829		-0.933	
Bilingual		2.394	***	2.104	***	4.192	***	3.955	***
Ever repeated a grade		-2.344	**	-2.428	**	-2.964	***	-2.886	***
Missing ever repeated a grade		1.741		1.831		-1.161		-1.077	
Parental education	less than high school	0.989		1.081		1.653	*	1.664	*
(omitted: high	associate's degree	1.688	*	1.663	*	2.242	**	2.318	**
school)	bachelor's degree	3.208	***	3.258	***	3.562	***	3.608	***
	advanced degree	4.020	***	4.088	***	5.527	***	5.540	***
Log parents' income		0.891	**	0.849	**	0.518		0.591	
Student lives with	both biological parents	0.373		0.384		-0.503		-0.260	
	a single biological parent	-0.742		-0.722		-1.645		-1.376	
	a biological and a step parent	(omitted)		(omitted)		-1.440		-1.180	
	other	0.483		0.192		(omitted)		(omitted)	

Parents' education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-1.355	-1.401	-1.612	-1.511
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.042 **	-3.035 **	-3.376 **	-3.266 **
	complete a master's degree or start a Ph.D.	2.372 ***	2.402 ***	2.845 ***	2.862 ***
	complete a Ph.D/M.D/Law/other prof. degree	3.355 ***	3.334 ***	3.055 ***	3.122 ***
	don't know	0.349	0.383	0.640	0.688
	missing	-1.921	-2.121	0.342	0.309
Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.278 ***	-3.371 ***	-3.133 ***	-3.149 ***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.099	-1.010	-1.531	-1.445
	complete a master's degree or start a Ph.D.	2.038 **	1.949 **	2.004 *	1.920 *
	complete a Ph.D/M.D/Law/other prof. degree	3.155 ***	3.107 ***	3.138 ***	3.104 ***
	don't know	-2.177 **	-2.195 **	-2.059 **	-2.154 **
	missing	-5.736 *	-5.581 *	-6.368	-6.636
School locale	city	0.364	0.581	2.145 *	2.137 *
	suburb	-0.386	-0.237	1.406	1.337
	town	-1.400	-1.249	(omitted)	(omitted)
	rural	(omitted)	(omitted)	1.893 *	1.757
Private school		-2.428 ***	-2.530 ***	-2.680 ***	-2.725 ***
Imputed math test score		-1.614 *	-1.552	-1.425	-1.424
Imputed race		-3.451	-3.131	-1.649	-1.457
Imputed parental education		-1.516	-1.460	-2.683	-2.701
Imputed living situation		4.255 **	4.484 **	-1.257	-2.686
Imputed parental income		-0.718	-0.582	-1.267	-1.237
Favorable tuition and financial aid state policies for undocumented immigrants		0.008	-0.015	-0.010	-0.031
Constant		45.774 ***	41.725 ***	20.923	14.540
	N	1083		789	

Table 28: robustness checks, inclusion of all country-level variables used in the imputation – TIMSS 8th grade

		Full Sample				Intermediate sample			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.286	**	1.535	***	0.246	*	1.587	**
Home-country academic quality		0.008		0.027	**	0.022	*	0.040	**
Interaction				-0.003	***			-0.003	*
Percent of population below age 15		-27.539	*	-28.968	*	-71.817	***	-75.001	***
Life expectancy at birth		0.215		0.218		0.940	***	0.943	***
Infant mortality rate (per 1,000 live births)		-0.026		-0.031		0.411	***	0.403	***
Total fertility rate		-0.501		-0.350		4.014	**	4.326	**
GNI PPP per capita		0.000		0.000		0.000		0.000	
Deaths due to NCD's		-17.535	***	-18.454	***	-27.157	***	-27.964	***
School enrollment, primary (% net)		0.039		0.038		-0.381	**	-0.371	**
Male		1.245	**	1.173	**	1.101	*	1.049	*
Age		-0.553		-0.607		-0.083		-0.169	
Race	black	-2.792	*	-2.613		-5.295	**	-5.198	**
(omitted: white)	hispanic	-1.527		-1.524		-1.249		-1.161	
	asian	2.369	**	2.227	*	0.863		0.869	
	other	-0.549		-0.721		-1.202		-1.294	
Bilingual		2.448	***	2.135	***	4.229	***	3.981	***
Ever repeated a grade		-2.345	**	-2.428	**	-2.949	***	-2.856	***
Missing ever repeated a grade		1.929		1.962		-0.881		-0.827	
Parental education	less than high school	0.904		0.989		1.604	*	1.620	*
(omitted: high	associate's degree	1.649	*	1.641	*	2.146	**	2.237	**
school)	bachelor's degree	3.160	***	3.250	***	3.449	***	3.519	***
	advanced degree	3.953	***	4.037	***	5.398	***	5.424	***
Log parents' income		0.866	**	0.829	**	0.500		0.574	
Student lives with	both biological parents	0.356		0.390		-0.433		-0.160	
	a single biological parent	-0.815		-0.775		-1.620		-1.317	
	a biological and a step parent	(omitted)		(omitted)		-1.348		-1.074	
	other	0.454		0.171		(omitted)		(omitted)	

Parents' education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-1.371	-1.358	-1.599	-1.478
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.026 **	-2.980 **	-3.356 **	-3.247 **
	complete a master's degree or start a Ph.D.	2.357 ***	2.418 ***	2.827 ***	2.871 ***
	complete a Ph.D/M.D/Law/other prof. degree	3.351 ***	3.336 ***	3.088 ***	3.151 ***
	don't know	0.356	0.412	0.611	0.668
	missing	-1.915	-1.999	0.274	0.273
Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.333 ***	-3.463 ***	-3.163 ***	-3.181 ***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.171	-1.095	-1.522	-1.418
	complete a master's degree or start a Ph.D.	2.057 **	1.954 **	1.989 *	1.907 *
	complete a Ph.D/M.D/Law/other prof. degree	3.131 ***	3.073 ***	3.085 ***	3.057 ***
	don't know	-2.211 **	-2.268 **	-2.052 **	-2.167 **
	missing	-5.681 *	-5.533 *	-6.403	-6.605
School locale	city	0.373	0.597	2.168 *	2.132 *
	suburb	-0.368	-0.227	1.438	1.334
	town	-1.403	-1.235	(omitted)	(omitted)
	rural	(omitted)	(omitted)	1.880 *	1.718
Private school		-2.453 ***	-2.555 ***	-2.727 ***	-2.756 ***
Imputed math test score		-1.583 *	-1.535	-1.423	-1.430
Imputed race		-2.532	-2.120	-0.676	-0.390
Imputed parental education		-1.571	-1.563	-2.746	-2.763
Imputed living situation		4.256 **	4.572 **	-1.359	-2.574
Imputed parental income		-0.678	-0.513	-1.254	-1.214
Favorable tuition and financial aid state policies for undocumented immigrants		0.006	-0.005	-0.023	-0.039
Constant		44.989 ***	39.247 **	20.973	13.198
	N	1083		789	

Table 29: robustness checks, log years and imputation method 1 instead of 4 – PISA

		Log years				Imputation method 1			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.404	***	0.141		0.224	*	0.133	
Home-country academic quality		3.231	***	4.080	***	3.551	***	5.912	***
Interaction				-0.634	***			-0.399	
Male		1.336	***	1.282	**	1.128	*	1.057	*
Age		-0.318		-0.283		0.035		-0.007	
Race	black	-3.064	**	-3.180	**	-1.137		-0.975	
(omitted: white)	hispanic	-2.158	**	-2.172	**	-1.928	**	-1.863	**
	asian	3.483	***	3.426	***	2.980	***	3.065	***
	other	-0.429		-0.610		0.665		0.569	
Bilingual		1.694	**	1.530	**	4.021	***	3.860	***
Ever repeated a grade		-2.954	***	-3.070	***	-3.159	***	-3.111	***
Missing ever repeated a grade		2.169		1.872		-1.271		-1.159	
Parental education	less than high school	0.406		0.458		0.848		0.862	
(omitted: high school)	associate's degree	0.832		0.862		2.213	**	2.288	**
	bachelor's degree	2.781	***	2.877	***	3.841	***	3.901	***
	advanced degree	3.688	***	3.766	***	5.416	***	5.479	***
Log parents' income		0.793	**	0.744	**	0.723	*	0.784	**
Student lives with	both biological parents	0.843		0.872		0.722		0.687	
(omitted: a biological parent and a step parent)	a single biological parent	-0.678		-0.623		-0.393		-0.403	
	other	1.605		1.438		1.379		1.218	
Parents' education expectation	complete high school or less	-1.642		-1.724		-1.725		-1.658	
(omitted: complete a bachelor's degree or start a master's degree)	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.504	***	-3.597	***	-3.218	**	-3.175	**
	complete a master's degree or start a Ph.D.	2.560	***	2.453	***	3.000	***	3.001	***
	complete a Ph.D/M.D/Law/other prof. degree	3.564	***	3.512	***	3.253	***	3.288	***
	don't know	-0.015		0.012		0.690		0.748	
	missing	-2.120		-1.815		1.022		0.927	

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.195	***	-3.322	***	-3.408	***	-3.456	***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.667		-1.746		-1.550		-1.509	
	complete a master's degree or start a Ph.D.	1.749	*	1.657	*	2.199	**	2.121	**
	complete a Ph.D/M.D/Law/other prof. degree	2.974	***	2.881	***	3.054	***	3.005	***
	don't know	-2.168	**	-2.277	**	-2.038	**	-2.142	**
	missing	-7.793	***	-7.736	***	-6.289		-6.427	
School locale (omitted: town)	city	1.544		1.645	*	2.317	**	2.302	**
	suburb	1.028		1.052		1.530		1.469	
	rural	1.635	*	1.546		1.814		1.690	
Private school		-1.985	***	-2.238	***	-2.916	***	-2.998	***
Imputed math test score		-1.340		-1.317		-1.823	*	-1.853	*
Imputed race		-1.344		-1.143		-2.194		-1.959	
Imputed parental education		-0.679		-0.741		-3.320	*	-3.363	*
Imputed living situation		4.019	**	4.123	**	-1.798		-2.776	
Imputed parental income		-0.509		-0.389		-1.123		-1.070	
Favorable tuition and financial aid state policies for undocumented immigrants		0.205		0.180		-0.419		-0.423	
Constant		44.080	***	44.749	***	36.448	***	37.165	***
	N	1189				798			

Table 30: robustness checks, log years and imputation method 1 instead of 4 – TIMSS 4th grade

		Log years				Imputation method 1			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.398	***	0.079		0.221	*	0.094	
Home-country academic quality		2.696	***	3.510	***	2.524	***	4.853	***
Interaction				-0.616	***			-0.389	
Male		1.353	***	1.295	**	1.131	*	1.054	*
Age		-0.352		-0.324		-0.051		-0.097	
Race	black	-4.615	***	-4.749	***	-1.458		-1.334	
(omitted: white)	hispanic	-2.086	**	-2.123	**	-1.989	**	-1.906	**
	asian	3.204	***	3.140	***	2.852	***	2.934	***
	other	-0.833		-0.989		0.249		0.182	
Bilingual		1.493	**	1.324	*	3.758	***	3.577	***
Ever repeated a grade		-2.947	***	-3.069	***	-3.076	***	-3.017	***
Missing ever repeated a grade		2.621		2.412		-0.879		-0.826	
Parental education	less than high school	0.365		0.421		0.756		0.766	
(omitted: high school)	associate's degree	0.766		0.786		2.163	**	2.215	**
	bachelor's degree	2.704	***	2.775	***	3.670	***	3.731	***
	advanced degree	3.583	***	3.643	***	5.371	***	5.427	***
Log parents' income		0.826	**	0.780	**	0.758	*	0.826	**
Student lives with	both biological parents	0.767		0.784		0.743		0.698	
(omitted: a biological parent and a step parent)	a single biological parent	-0.709		-0.657		-0.312		-0.317	
	other	1.583		1.365		1.391		1.174	
Parents' education expectation	complete high school or less	-1.657		-1.749		-1.661		-1.590	
(omitted: complete a bachelor's degree or start a master's degree)	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.606	***	-3.678	***	-3.191	**	-3.098	**
	complete a master's degree or start a Ph.D.	2.639	***	2.539	***	3.193	***	3.207	***
	complete a Ph.D/M.D/Law/other prof. degree	3.552	***	3.511	***	3.346	***	3.405	***
	don't know	-0.038		-0.005		0.734		0.796	
	missing	-2.412		-2.245		0.916		0.829	

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.189	***	-3.271	***	-3.344	***	-3.349	***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.609		-1.655		-1.508		-1.439	
	complete a master's degree or start a Ph.D.	1.771	*	1.706	*	2.281	**	2.222	**
	complete a Ph.D/M.D/Law/other prof. degree	3.032	***	2.951	***	3.230	***	3.199	***
	don't know	-2.160	**	-2.245	**	-1.966	**	-2.049	**
	missing	-7.794	***	-7.659	***	-6.172		-6.383	
School locale (omitted: town)	city	1.527		1.626	*	2.310	**	2.300	**
	suburb	1.075		1.095		1.538		1.481	
	rural	1.643	*	1.551		1.812		1.688	
Private school		-1.894	**	-2.141	***	-2.844	***	-2.923	***
Imputed math test score		-1.215		-1.176		-1.732	*	-1.747	*
Imputed race		-0.202		-0.094		-0.805		-0.651	
Imputed parental education		-0.611		-0.631		-3.177	*	-3.205	*
Imputed living situation		3.870	**	3.954	*	-1.296		-2.465	
Imputed parental income		-0.582		-0.468		-1.276		-1.240	
Favorable tuition and financial aid state policies for undocumented immigrants		0.128		0.098		-0.541		-0.545	
Constant		44.670	***	45.476	***	37.551	***	38.466	***
	N	1189				798			

Table 31: robustness checks, log years and imputation method 1 instead of 4 – TIMSS 8th grade

		Log years		Imputation method 1	
		no interaction	*interaction*	*no interaction*	*interaction*
Number of years in the US		0.404 ***	0.125	0.222 *	0.114
Home-country academic quality		2.922 ***	3.762 ***	3.229 ***	5.613 ***
Interaction			-0.627 ***		-0.396 *
Male		1.326 ***	1.276 **	1.108 *	1.052 *
Age		-0.381	-0.354	-0.061	-0.121
Race	black	-3.997 ***	-4.108 ***	-1.290	-1.133
(omitted: white)	hispanic	-1.941 **	-1.982 **	-1.573 *	-1.494
	asian	3.215 ***	3.125 ***	2.595 ***	2.659 ***
	other	-0.845	-1.004	0.196	0.160
Bilingual		1.557 **	1.375 **	3.870 ***	3.663 ***
Ever repeated a grade		-2.894 ***	-3.019 ***	-3.077 ***	-3.017 ***
Missing ever repeated a grade		2.489	2.142	-0.891	-0.857
Parental education	less than high school	0.467	0.521	0.803	0.816
(omitted: high school)	associate's degree	0.745	0.780	2.113 **	2.185 **
	bachelor's degree	2.686 ***	2.787 ***	3.662 ***	3.750 ***
	advanced degree	3.616 ***	3.675 ***	5.323 ***	5.379 ***
Log parents' income		0.808 **	0.764 **	0.726 *	0.800 **
Student lives with	both biological parents	0.833	0.857	0.702	0.681
(omitted: a biological parent and a step parent)	a single biological parent	-0.708	-0.646	-0.368	-0.346
	other	1.592	1.381	1.331	1.105
Parents' education expectation	complete high school or less	-1.615	-1.693	-1.688	-1.575
(omitted: complete a bachelor's degree or start a master's degree)	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.523 ***	-3.569 ***	-3.218 **	-3.112 **
	complete a master's degree or start a Ph.D.	2.605 ***	2.525 ***	3.045 ***	3.089 ***
	complete a Ph.D/M.D/Law/other prof. degree	3.586 ***	3.542 ***	3.308 ***	3.364 ***
	don't know	0.021	0.063	0.719	0.791
	missing	-2.262	-1.896	0.902	0.894

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.204	***	-3.320	***	-3.418	***	-3.432	***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.642		-1.712		-1.549		-1.465	
	complete a master's degree or start a Ph.D.	1.743	*	1.675	*	2.209	**	2.147	**
	complete a Ph.D/M.D/Law/other prof. degree	2.990	***	2.910	***	3.105	***	3.076	***
	don't know	-2.188	**	-2.303	***	-2.016	**	-2.122	**
	missing	-7.896	***	-7.794	***	-6.294		-6.460	
School locale (omitted: town)	city	1.546		1.650	*	2.339	**	2.303	**
	suburb	1.052		1.068		1.554		1.464	
	rural	1.645	*	1.554		1.786		1.631	
Private school		-2.003	***	-2.245	***	-2.916	***	-2.976	***
Imputed math test score		-1.269		-1.225		-1.776	*	-1.799	*
Imputed race		-0.362		-0.220		-0.964		-0.697	
Imputed parental education		-0.641		-0.690		-3.262	*	-3.312	*
Imputed living situation		4.104	**	4.212	**	-1.290		-2.336	
Imputed parental income		-0.498		-0.368		-1.155		-1.103	
Favorable tuition and financial aid state policies for undocumented immigrants		0.185		0.149		-0.493		-0.495	
Constant		45.051	***	45.797	***	38.077	***	39.009	***
	N	1189				798			

Table 32: robustness checks, HDI and non-income HDI instead of the measures of home-country academic quality

		HDI				nonincome HDI			
		no interaction		*interaction*		*no interaction*		*interaction*	
Number of years in the US		0.361	***	0.966	*	0.367	***	1.048	*
Home-country academic quality		11.820	***	17.058	***	11.118	***	16.705	***
Interaction				-0.854				-0.904	
Male		1.098	**	1.097	**	1.097	**	1.100	**
Age		-0.250		-0.241		-0.248		-0.238	
Race	black	-4.096	***	-4.055	***	-4.439	***	-4.391	***
(omitted: white)	hispanic	-2.382	***	-2.442	***	-2.507	***	-2.567	***
	asian	4.344	***	4.253	***	4.218	***	4.122	***
	other	-0.333		-0.390		-0.556		-0.600	
Bilingual		1.681	**	1.630	**	1.651	**	1.599	**
Ever repeated a grade		-2.921	***	-2.947	***	-2.937	***	-2.960	***
Missing ever repeated a grade		3.230		3.210		3.224		3.233	
Parental education (omitted: high school)	less than high school	0.554		0.601		0.618		0.668	
	associate's degree	0.783		0.798		0.750		0.772	
	bachelor's degree	2.856	***	2.909	***	2.785	***	2.845	***
	advanced degree	3.822	***	3.868	***	3.792	***	3.842	***
Log parents' income		0.831	**	0.803	**	0.864	**	0.835	**
Student lives with (omitted: a biological parent and a step parent)	both biological parents	0.673		0.692		0.723		0.751	
	a single biological parent	-0.493		-0.503		-0.443		-0.443	
	other	1.043		1.009		1.053		1.027	
Parents' education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-1.530		-1.576		-1.506		-1.541	
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-3.800	***	-3.814	***	-3.747	***	-3.752	***
	complete a master's degree or start a Ph.D.	2.390	***	2.391	***	2.424	***	2.427	***
	complete a Ph.D/M.D/Law/other prof. degree	3.186	***	3.167	***	3.233	***	3.211	***
	don't know	-0.180		-0.153		-0.184		-0.150	
	missing	-3.264		-3.262		-3.246		-3.240	

Student's education expectation (omitted: complete a bachelor's degree or start a master's degree)	complete high school or less	-3.152 ***	-3.213 ***	-3.119 ***	-3.183 ***
	start an associate's degree, complete an associate's degree or start a bachelor's degree	-1.895	-1.899	-1.886	-1.898
	complete a master's degree or start a Ph.D.	2.008 **	1.973 **	2.035 **	2.006 **
	complete a Ph.D/M.D/Law/other prof. degree	3.078 ***	3.035 ***	3.123 ***	3.084 ***
	don't know	-2.078 **	-2.102 **	-2.050 **	-2.080 **
	missing	-7.586 **	-7.555 **	-7.495 **	-7.434 **
School locale (omitted: rural)	city	0.084	0.152	0.075	0.145
	suburb	-0.367	-0.325	-0.374	-0.333
	town	-1.434	-1.400	-1.414	-1.385
Private school		-2.116 ***	-2.178 ***	-2.128 ***	-2.186 ***
Imputed math test score		-1.170	-1.142	-1.142	-1.113
Imputed race		-0.963	-0.962	-0.797	-0.807
Imputed parental education		-0.793	-0.833	-0.727	-0.781
Imputed living situation		5.506 ***	5.806 ***	5.382 ***	5.713 ***
Imputed parental income		-0.842	-0.789	-0.844	-0.797
Favorable tuition and financial aid state policies for undocumented immigrants		0.028	0.026	0.030	0.026
Constant		32.671 ***	29.217 ***	32.342 ***	28.372 ***
	N	1173		1173	

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