The Effects of Army Service in the All-Volunteer Era

PRELIMINARY DRAFT, PLEASE DO NOT CITE OR CIRCULATE

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March 2020

Abstract

The United States has relied exclusively on volunteers to serve in the military since July 1, 1973. Volunteers tend to come from lower income households, yet we know relatively little about whether enlistment improves their prospects. This paper links the universe of Army applicants between 1990 and 2011 to IRS data and exploits eligibility thresholds at the lower (31) and middle (50) part of the Armed Forces Qualifying Test (AFQT) distribution in a regression discontinuity design to estimate the long-term, dynamic effects of Army enlistment on earnings, employment, education, disability, and geographic mobility. We show that Army service increases cumulative earnings in the 15 years following Army application at both cutoffs. We also find that Army service increases college attendance, disability compensation, and marriage rates, with no cumulative effects on employment. Further, we find striking heterogeneity by race, with black servicemembers experiencing large long-term earnings gains.

*All opinions expressed in this manuscript are those of the authors and do not represent the opinions of the United States Military Academy, Department of Defense, United States Army, or Department of Treasury. We thank Bill Evans, Kevin Lang, David Lyle, Brendan Price, and seminar participants at Boston University, the University of Maryland, Vassar College, and West Point for helpful comments. Greenberg: Department of Social Sciences, United States Military Academy, kyle.greenberg@westpoint.edu. B109 Lincoln Hall, West Point, NY 10996. Gudgeon: Department of Social Sciences, United States Military Academy, matthew.gudgeon@westpoint.edu. B107 Lincoln Hall, West Point, NY 10996. Isen: Office of Tax Analysis, U.S. Department of the Treasury, adam.isen@treasury.gov 1500 Pennsylvania Ave., NW Washington, DC 20220. Miller: Office of Tax Analysis, U.S. Department of the Treasury, corbin.miller@treasury.gov 1500 Pennsylvania Ave., NW Washington, DC 20220. Patterson: United States Military Academy, IZA, and CESifo, richard.patterson@westpoint.edu. B117 Lincoln Hall, West Point, NY 10996.

1 Introduction

> *"I wish that there were other activities in our society and in our nation that were as open as the military is to upward mobility... I wish that corporate America, I wish the trade unions around the nation would show the same level of openness and opportunity to minorities that the military has."*
>
> – Colin Powell

Approximately one in thirteen American adults and one in seven men have served in the United States military, with the Army being the largest service branch. Additionally, the military is the largest employer in the United States and the combined annual budgets of the Department of Defense (DoD) and Department of Veterans Affairs (VA) make up roughly five percent of U.S. GDP.[1] In spite of the significant role the military plays in the current U.S. economy, most rigorous estimates of the effects of military service rely on drafts which ended in the U.S. in 1973.[2] Since the advent of the all-volunteer era in 1973, significant changes in who enlists, their outside options, and their experiences in service, warfare, and veteran programs are likely to alter the effects of military service. What little is known about the effects of modern voluntary service on income and well-being is based on comparisons of enlistees to non-enlistees.[3]

Moreover, current Military applicants confront different economic prospects than prior generations. In the U.S., wage inequality is rising (Piketty et al., 2017), upward social mobility is stagnating (Chetty et al., 2017a), economic opportunities for men without college degrees are decreasing (Autor and Wasserman, 2013), opportunities are starkly different by race (Chetty et al., 2018), and employment prospects are diverging across geographical regions (Austin et al., 2018). Compared to the broader U.S. population, we find that those who join the Army are more likely to be male, black, come from low-income communities, and have no more than a high-school degree. Enlistment could increase opportunity by providing a stable source of income with generous education, tax, and health benefits, as well as opportunities to develop new skills, build networks, and out-migrate.[4] However, volunteer service also includes considerable risk. The Army separates young people from their communities when many of their peers are attending school or developing professional skills, exposes enlistees to violence, injury, and trauma, and is associated with high rates of disability receipt—all channels that could worsen labor market outcomes.[5]

In this paper, we investigate how voluntary enlistment in the U.S. Army affects long-run earnings and related outcomes using a fuzzy regression discontinuity design. We merge the universe of Active Duty Army applicants from 1990-2011 to earnings, employment, education, disability,

[1] Sources: 2018 Census American Community Survey; Department of Defense and Department of Veterans Affairs.
[2] See, for example, Angrist (1990); Angrist and Chen (2011); Angrist et al. (2011).
[3] For example, Angrist (1998), Kleykamp (2013), Martorell et al. (2014), and Teachman and Tedrow (2007).
[4] Examples include, Barr (2019); Wilson and Kizer (1997); Breznitz (2005).
[5] See, for example, Bingley et al. (2020); Loughran and Heaton (2013); Autor et al. (2016).

and marriage measures from IRS, National Student Clearinghouse (NSC), Social Security Administration (SSA), and VA administrative records. Using variation in enlistment rates created by two eligibility cutoffs in the Armed Forces Qualifying Tests (AFQT), we estimate the effect of Army service on economic outcomes. We find that Army service significantly increases overall earnings, education, disability compensation, and marriage rates.

Our identification strategy uses a Department of Defense (DoD) policy that requires 96% of recruits to have an AFQT score of 31 or higher and 60% of recruits have to an AFQT of 50 or higher.[6] As a result, the Army rarely accepts any applicants with AFQT scores below 31, seldom accepts GED recipients with AFQT scores below 50, and often requires applicants to score 50 or higher to receive enlistment bonuses. Because applicants are allowed to retake tests after a one to six month waiting period, we use an applicant's first AFQT score on file in our analysis.[7] Despite the possibility of retakes, crossing the 31 and 50 AFQT cutoffs on the first attempt increases the probability of service by 11.0 and 5.6 percentage points, respectively.[8]

Using the AFQT cutoffs in a fuzzy regression discontinuity design, we find Army service increases cumulative earnings by $73,753 at the lower ability (AFQT=31) cutoff and by $121,936 at the higher ability (AFQT=50) cutoff in the 15 years following application. The effects of service vary over time, with the largest effects in the first 4 years following application, averaging $10,000-12,000 a year. Effect sizes diminish but remain positive and frequently significant in each of the 5-10 years after application, averaging $2,142 at the lower cutoff and $3,318 at the higher cutoff. In the long-term, 11-19 years after application, point estimates drop further at the low cutoff, averaging $1,345 and not typically statistically significant, while they increase at the higher cutoff, averaging $3,875 and reaching $4,366 16-19 years out. In the short-run, employment effects are large and positive at both cutoffs. In the long-run, employment effects are close to zero at the high cutoff and are negative and significant at the low cutoff, averaging -3.3% 7-19 years out.

The effects of Army service on education and disability help contextualize these earnings and employment dynamics. Consistent with generous post-service education benefits, Army service increases the probability of attending college within 15 years of application by 6.5 and 17.0 percentage points at the low and high cutoff, respectively. In the short-run service decreases the likelihood of attending college, but within 5 years service increases attendance in every following year at both cutoffs, which more than offsets the short-run effect. Increased college attendance will exert a drag on earnings and employment in the medium run, but should increase long-term

[6] An applicant's AFQT score is derived from four of the 11 required Armed Services Vocational Aptitude Battery (ASVAB) tests. Each applicant's AFQT score is scaled to a percentile rank (1-99) matched to a nationally representative sample of 18-23 year olds.

[7] Applicants must wait one month to retake tests the first and second time and six months to retake each subsequent test.

[8] While we define our endogenous variable as enlistment in "any military service", in practice we identify the effects of Active Duty Army service. 80% of enlistees in our Army applicant sample join the Active Duty Army and crossing either threshold only modestly reduces the probability of enlistment in non-Army military service by 0.5 percentage points.

earnings (Oreopoulous and Petronijevic, 2013).

While increased education is expected to improve long-term earnings, we also find evidence of earnings-reducing consequences of service. In particular, Army service increases disability compensation receipt – defined as any Veteran's Affairs Disability Compensation (VADC), Social Security Disability Insurance (SSDI), or Supplemental Security Income (SSI) – by 15 percentage points at the low cutoff and 12.5 percentage points at the higher cutoff 5-19 years after application. These large estimates are driven by VADC, which is not work-limiting. When we restrict our analysis to work-limiting disability—defined as any SSI, SSDI, or VA Individual Unemployability designation—we find a 2-4 percentage point increase in work-limiting disability at the 30 cutoff and find no effect at the 50 cutoff. These differences could explain the smaller long-term effects on earnings at the lower cutoff.

In spite of the contrasting effects of Army service on education and disability, our overall evidence is consistent with the Army generally improving the well-being of the average enlistee. Furthermore, we find little discernible evidence of heterogeneous long-term earnings effects by cohort or by sex. We do however, find striking differences by race. Between 11-19 years after application, enlistment increases black applicant earnings by 19% at the 31 AFQT cutoff and 34% at the 50 AFQT cutoff ($5,253 and $10,837). Meanwhile, white applicants experience earnings losses of $3,449 at the 31 cutoff and smaller gains of $3,814 at the 50 cutoff. At the 31 cutoff, these differences are also reflected in employment and in work-limiting disability, with whites experiences employment reductions and increases in work limiting disability, while estimates on both of these dimensions are closer to zero for blacks.

The disproportionate benefits of Army service for black applicants is notable given that there is a gap between white and black economic prospects (e.g. Akee et al., 2017; Bayard et al., 1999), even after controlling for parental income (Chetty et al., 2018). One explanation for why black applicants particularly benefit from Army service is that their outside options are systematically worse (Chetty and Hendren, 2018). To explore whether racial differences in the effects of Army service are acting as a proxy for differences in economic opportunity, we are conducting ongoing work to estimate whether the effects of Army service vary by applicants' parental income as well as the economic conditions in their home states and counties.

Contributions. A number of studies use variation in draft lotteries to identify plausibly causal effects of conscripted military service, including Angrist (1990) and Angrist et al. (2011) in the United Sates, Card and Cardoso (2012) in Portugal, and Bingley et al. (2020) in Denmark.[9] How-

[9] Angrist (1990) and Angrist et al. (2011) find that in the United States, conscription during the Vietnam War decreased the earnings of white men by 10-15% in the first twenty years following conscription, but had no significant effects on earnings 20-40 years after conscription. In Portugal, Card and Cardoso (2012) find that conscription had no overall effects on wages but positive effects on individuals with little education whereas in Denmark, Bingley et al. (2020) find conscription has an lifetime earnings penalty of $23,000 among high-ability men and no impact on the earnings of low-ability men.

ever, the results of these studies may have limited application to modern volunteer services because people who select into Army service, including women, are different than those who are conscripted, the experiences of military service—such as compensation, medical care, educational benefits, and the nature of combat—have changed, and the economic opportunities for potential servicemembers have shifted. Our study addresses these changes by estimating the effects of Army service in the all-volunteer era.

Several studies examine the effects of *volunteer* service by comparing the outcomes of servicemembers to civilians, while controlling for observable characteristics.[10] A key innovation relative to these other volunteer-era studies is our regression discontinuity approach, which allows us to identify causal effects of service with less restrictive identification assumptions. This innovation appears to matter: we find that estimating the effects of military service using ordinary least squares (OLS) in our sample—a similar approach to prior volunteer-era studies—generates much larger earnings estimates than our regression discontinuity estimates. Furthermore, our RD approach identifies a local average treatment effect (LATE) for marginal Army applicants – a population that is particularly relevant for understanding policies to expand or contract the size of the military. Moreover, these marginal applicants are part of a disadvantaged population of broader policy interest.

Given the sex, education, race, and economic backgrounds of Army applicants (See Lutz, 2008; Kleykamp, 2006), applicants may face poor economic prospects (e.g. Akee et al., 2017; Chetty et al., 2018; Heckman et al., 2000; Tüzemen, 2018). Our estimates suggest that the Army may be an important U.S. institution in improving economic outcomes, particularly for young black men. Like Angrist (1998) and Card and Cardoso (2012), we find that military service improves prospects for black and less educated young men. However, we find substantially larger positive effects of service on non-whites than Angrist (1998).

One way the Army increases mobility is by expanding access to college through the G.I. Bill. A large literature finds that increased education improves earnings and mobility.[11] Prior studies use cross-cohort comparisons (Bound and Turner, 2002) or changes in G.I. Bill generosity (Barr, 2015, 2019) to estimate the effect of Army service and the GI-Bill on college attendance.[12] By using variation in enlistment, we provide a more direct measure of the total effect of Army service on post-secondary education.

Voluntary Army service could potentially *reduce* economic mobility by exposing servicemembers to trauma, disability, and death. Specifically, combat and training incidents could worsen labor market outcomes by negatively affecting human capital and health or by leading servicemembers reduce their labor supply in response to receiving VA or SSA disability compensation

[10] For example, see: Angrist (1998); Cohen et al. (1995); Kleykamp (2013); Martorell et al. (2014); Teachman and Tedrow (2007)

[11] For example, see Chetty et al. (2017b); Dale and Krueger (2002); Deming and Dynarski (2009); Zimmerman (2014).

[12] Angrist and Chen (2011) study the long-term effects of the Vietnam-era GI bill.

(Autor et al., 2016). Given the changes in combat and generosity of disability compensation over time, our volunteer-era estimates provide an important update to conscription-era studies of the effects of service on mortality (e.g. Bedard and Deschênes, 2006; Dobkin and Shabani, 2009; Johnston et al., 2016) and disability compensation (e.g. Angrist et al., 2010; Autor et al., 2011).

Finally, our study informs the effect of immense public spending on military personnel. Furthermore, veterans and servicemembers receive more than $22 billion annually in tax benefits and expenditures (Office of Tax Analysis). Recent studies have debated whether servicemembers are properly compensated for their service (e.g. Asch et al., 2010; Asch and Warner, 2001; Carrell and West, 2005). By accounting for the long-run effects of service on earnings, employment, education, and disability, our study informs military compensation, benefit and tax policies.

Roadmap. The remainder of our paper is structured in the following way: Section 2 explains our institutional background, Section 3 describes our data and sample, Section 4 describes our empirical approach, Section 5 describes our primary estimates of Army service on earnings and income, Section 6 explores how the Army affects various populations, and Section 7 concludes.

2 Background: Application, Service, and Post-Service Experiences

The Application Process. Those interested in enlisting in the U.S. Army must first visit their local U.S. Army recruiting office. After determining that a potential recruit meets basic age, citizenship, and background requirements, a recruiter will typically schedule a two day appointment for the applicant at one of 65 Military Entrance and Processing Stations (MEPS). All applicants take the Armed Services Vocational Aptitude Battery (ASVAB) during their first day at the MEPS while the second day consists predominately of physical tests, medical examinations, and a meeting with an enlistment counselor. Four of the 11 tests within the ASVAB contribute to an applicants raw Armed Forces Qualification Test (AFQT) score, which is then converted to a scaled AFQT score that represents the percentile-rank (1-99) of an applicants arithmetic and verbal reasoning skills relative to a nationally representative sample of 18-23 year olds (DoD, 2004).

Law prohibits non-high school graduates with AFQT scores below 31 from enlisting in any branch of the military. The Department of Defense further requires that at least 60 percent of recruits have AFQT scores of 50 or higher, and that no more than 4 percent of recruits have AFQT scores below 31 (DoD, 2004). To meet DoD requirements, the U.S. Army rarely accepts any applicants with AFQT scores below 31, typically does not accept GED recipients with AFQT scores below 50, and often limits enlistment bonuses to applicants with AFQT scores of 50 or higher.[13] These regulations create discontinuities in the probability of Army service based on applicants' first AFQT scores at scores of 31 and 50 (see Panel A of Figure 1).

[13]Source: Paragraph R-5.b of U.S. Army Recruiting Command Regulation 601-96, 2012, and DoD (2004).

While applicants are unlikely to be able manipulate their first AFQT score around the cutoff, they can retake the ASVAB. Applicants with low AFQT scores may retake the ASVAB one month after their initial examination and can retest again one month after the initial retest. Applicants who wish to retest a third time must wait an additional six months before doing so.[14]

The final step an applicant takes during the two-day appointment at a MEPS is meeting with an enlistment counselor. This counselor discusses which military occupational specialities (MOS) or job the applicant is eligible for, contract duration (typically 3-6 years), and available enlistment bonuses. Occupation eligibility is often determined by performance on job-specific groupings of ASVAB tests—groupings that differ from the four that compose the AFQT.[15] Eligibility for enlistment bonuses often depends on scoring at least 50 on the AFQT: the average enlistment bonus for servicemembers with a final AFQT score of 50 is $3,780 compared to just $1,620 for servicemembers who have an AFQT score of 49.[16] If an eligible applicant decides to accept a contract with the Army, the applicant then signs an enlistment agreement. Those who sign an enlistment agreement typically report to Army basic training within one year.

Characteristics of Army Service. Approximately 40% of applicants who enlist choose traditional combat occupations (e.g. infantryman or combat engineer) while others work as mechanics, truck drivers, medical laboratory specialists, and a variety of other non-combat occupations. The modal enlistee serves for a single enlistment term of 3-4 years, but roughly 25% of soldiers do not complete their initial term of service and 10-15% ultimately serve for 10 years or longer.

All enlistees experience a variety of employment benefits including tax-free housing payments (discussed further in section 3.3), access to tuition assistance and student-loan repayment programs, subsidized childcare, free personal and family healthcare, free dental care, and subsidized family dental coverage. However, Army service also carries considerable risk for many soldiers. In the years we study, around 50% of active duty Army enlistees deployed to a combat zone (e.g. Iraq or Afghanistan), with most deployed soldiers typically serving 9-15 months in combat during their initial enlistment term. Department of Defense casualty records indicate that 0.2% of enlistees are killed in action, about 2% are wounded in action, and 0.15% suffer serious or very serious wounds.

Veterans' Experiences. After leaving service, veterans are eligible for a wide range of bene-

[14]Prior to July 2004, the DoD scaled AFQT scores against young adults selected to participate in the 1980 Profile of American Youth (PAY). After July 1st, 2004, the DoD scaled AFQT scores according to the 1997 PAY. The U.S. Army did not adjust AFQT qualification requirements following the 2004 re-norming.

[15]If occupational eligibility scores coincided with enlistment eligibility scores, we would not be able to disentangle the effects of enlistment from the effects of within-military career placement. Fortunately, the structure of the ASVAB eliminates this potential confound.

[16]The average bonus amount among enlistees receiving bonuses is just under $10,000. Dollar amounts of offered bonuses vary depending on the recruiting needs of the Army, contract length, college credit accumulation, civilian skills (e.g. x-ray tech certification, Arabic translator), and occupation (MOS). See Asch et al. (2010) for additional details on enlistment bonuses.

fits, most notably education benefits, disability compensation, and access to free or subsidized healthcare.[17] Depending on the years of service and timing of college attendance, most veterans in our sample are either eligible for Montgomery GI Bill or Post-9/11 GI Bill education benefits. Early application cohorts in our sample are predominantly eligible for the Montgomery GI Bill whereas later application cohorts are likely to be eligible for the much more generous Post-9/11 GI Bill.[18] Compared to the Montgomery GI Bill, the Post-9/11 GI Bill reduced eligibility requirements,[19] increased maximum tuition reimbursements,[20] and introduced generous book and housing stipends.[21]

Veterans can also apply for direct monetary compensation for injuries sustained or aggravated during their time in service through the Veterans Affairs Disability Compensation (VADC) program. The Army assists soldiers with VADC applications before they transition out. Screening for VADC is less stringent than the screening for the two other major Federal disability programs: Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). Most importantly, VADC is not work-limiting or means tested: veterans can apply for and receive disability compensation regardless of their current employment or earnings status.[22]

In 2017, the VA paid disability compensation totalling $73 billion to 4.5 million veterans, slightly more than what was spent on medical care for the 6 million veteran patients.[23] Many of the most common disabilities among recent veterans are consistent with physical overuse injuries. According to the 2018 VA Annual Benefits Report, the three most common service-connected disabilities among Gulf War Era veterans are tinnitus (ringing in the ear), limitation of flexion (knees), and lumbosacral or cervical strain (back pain), while PTSD was the fifth most common. Veterans eligible for VADC receive monthly payments ranging from $140 per month to $3,500 per month depending on their degree of service-connected disability.[24]

Beyond direct monetary payments to veterans with service-connected disabilities, lifetime subsidized or free health care through the Department of Veterans Affairs could further ameliorate any negative effects of Army service. Most veterans are eligible for VA health care. The main eligibility requirement is to have served 2 years and received an honorable discharge. Once en-

[17] This paragraph draws on information provided by the Department of Veterans Affairs.

[18] The Post-9/11 GI Bill covers soldiers who served after 9/11/2001 and did not use their education benefits prior to 2008.

[19] Eligibility for the Montgomery GI Bill required a $100 monthly withholding of paychecks for the duration of an initial enlistment term, and successfully completion of an initial enlistment term. The Post-9/11 Bill has no withholding requirements and allows partial (40%-90%) benefits to those serving less than a full term.

[20] In 2008, the Montgomery GI Bill paid up to $1,321 per month for up to 36 months of tuition. In 2008, the Post-9/11 GI Bill funded 100% of tuition and fees up for veterans attending public schools as in-state students and paid for up to the most expensive in-state public cost for students attending private or out-of-state public schools.

[21] The yearly stipend for books and supplies is $1000 and the housing benefit can vary between $1000 and $3000 a month, depending on a veteran's location.

[22] One important exception to this is Individual Unemployability (IU) status. Veterans approved for IU status receive the highest possible amount of monthly VADC payments, but are not permitted to participate in gainful employment.

[23] Source: https://www.cbo.gov/system/files/2018-12/54881-VA_Spending_Paths_0.pdf.

[24] See https://www.va.gov/disability/compensation-rates/veteran-rates for details.

rolled, the VA categorizes veterans into different priority groups according to their income level and service-connected disability rating, with the highest priority groups receiving free care and lower priority groups receiving subsidized care with copays.[25]

3 Data and Sample

3.1 Data Sources

Our data come from a variety of administrative records. We combine Active Duty Army applicant records from the U.S. Military Entrance and Processing Command, or MEPCOM (1990-2011), with data from U.S. Army Administrative pay and service records (1989-2018), federal tax records (1999-2018), Social Security Administration disability compensation records (1998-2015), Department of Veteran's Affairs VA disability compensation records (1998-2015), and National Student Clearinghouse college education records (1999-2018).

3.2 Sample Construction

Our analysis sample is based on matching Army applicants for calendar years 1990 to 2011 to those who ever appear in Social Security records and applying several restrictions. First, our identification strategy requires applicant's initial AFQT score, so we restrict our analysis to people with an AFQT score recorded for their earliest application to the Armed Services. Second, to create bandwidths of equal length on both sides of each AFQT discontinuity, we limit to those with AFQT scores from 12 to 68. Third, we exclude applicants with prior service (approximately 6% of applicants). Fourth, we exclude the approximately 7% of applicants who took their ASVAB in high school as part of the ASVAB Career Exploration Program.[26] Fifth, we restrict our analysis to applicants we are able to match to Social Security records.[27] After these restrictions, our sample consists of 1,775,108 applicants.

For each individual in our Army-Treasury matched sample, we link tax records from 1999-2018 (e.g. employer-filed W-2 forms) for up to two years prior to and 19 years after application (-2,19). As a result, we are missing at least one early year of tax data for those who apply prior to 2001 and at least one later year of tax data for individuals who apply after 2000.[28] While it is

[25] See https://www.va.gov/health-care/eligibility/priority-groups/ for more details.

[26] Students who sit for the ASVAB Career Exploration Program have the option to apply to the military, but are not obligated to do so. We omit applications derived from these tests because we find evidence that applicants among these students may have decided to apply to the Army based on their scores.

[27] We try to match as many records as possible while limiting erroneous matches. Most records (96.1%) uniquely match on SSN and date of birth (DOB), and we supplement these with those that match exactly on some combination of SSN, DOB, and name where one of the three is allowed to be "close" to arrive at a final match rate of 98.9%. Here we consider matches "close" if they are within a few characters to allow for the possibility of misspellings and transcription error (where certain numbers may have been flipped such as month and day in DOB).

[28] Panel A of Figure A.2 shows the number of observations in our sample by years after application. The 31 AFQT

possible to look at tax outcomes beyond 19 years for individuals who apply to the Army prior to 1999, we restrict our analysis to 19 years post application because those who serve are eligible for a generous Army retirement pension at 20 years of service, which complicates the interpretation of wage and employment outcomes for 20 and more years post application.

3.3 Outcomes

Individual Earnings. Our primary outcome is individual earnings. We observe wages and earnings from two sources: (1) employer-provided W-2 wage and tax statements and (2) Army administrative pay records on non-taxable housing and deployment allowances. Our primary measure of individual earnings, which is available beginning in 1999, combines Medicare wages reported on Form W-2,[29] non-taxable deployment/foreign assignment payments (Hardship Duty Pay, Imminent Danger Pay, Hazardous Duty Pay, and Family Separation Allowances),[30] direct payments for food (Basic Allowance for Subsistence or BAS),[31] and Army housing allowances (Basic Allowance for Housing or BAH). We assume those with no W-2 wage earnings have zero wage earnings. All wages are adjusted to 2018 levels using the Urban Consumer Price Index (CPI-U) and we winsorize wages at the 99th percentile for the highest percentile of earners within each year.

We include non-taxable deployment/foreign assignment payments and BAS payments in our primary measure of earnings because these payments are direct compensation for Army-related work, not reported on the W-2, and those who do not join the military are very unlikely to receive similar tax-free payments. The sum of these payments can vary between $0 and $1,234 dollars a month, depending on the year, location, and nature of assignment. Additionally we include BAH in our primary earnings measure because it represents a significant fraction of servicemembers' compensation and there are relatively few situations where housing benefits provided to civilian employees are tax exempt.[32] All servicemembers are either provided with housing and utilities

Cutoff includes applicants with initial AFQT scores between 12 and 49 while the 50 AFQT Cutoff includes applicants with an initial AFQT score between 31 and 68.

[29] Servicemembers are exempted from paying income taxes on basic pay income received while deployed but are still required to pay medicare taxes. Therefore, we use medicare wages to account for basic income while servicemembers are deployed.

[30] Hardship Duty Pay is paid to servicemembers who are assigned to locations with living conditions that are substantially worse than in the continental United States. Imminent Danger Pay (IDP) is paid to servicmembers who serve in an area that is designated as an IDP area due to dangerous conditions. Hazardous Duty Pay is paid to servicemembers in jobs with high-risk duties such as parachute duty or flight duty. Family Separation Allowance (FSA) is paid to servicemembers who have dependents and are assigned to a location where paid relocation of family members is not authorized.

[31] BAS is a non-taxable payment to servicemembers that is meant to offset the cost of food. In 2019, BAS payments to enlisted servicemembers were $369 per month.

[32] Employer-provided housing is typically taxable unless each of the following conditions are met: (1) It is furnished on the employer's business premises, (2) it is provided for the convenience of the employer and not for the benefit of the employee (i.e. there is a substantial business reason for the employee to live on company premises, and (3) employer-provided housing is a condition of employment (employees cannot elect to live off business premises).

free of charge (commensurate with their rank and dependent status) or a tax-exempt housing allowance of approximately equivalent value. We include the value of this BAH for each servicemember.[33]

Altogether, we find that the tax-exempt payments outlined above account for 17-25% of Army servicemembers compensation. While civilians are unlikely to receive tax-exempt payments, those who are Active Duty servicemembers in the other branches of the military (i.e. Navy, Air Force, Marines) are likely to receive comparable payments. Therefore, we adjust the income of those identified as likely to be Active Duty in other services by the employer identification number (EIN) on their W-2. Specifically, we calculate the fraction of earnings that come from non-taxable benefits among Active-duty Army servicemembers by application cohort and year. We then inflate the earnings of servicemembers in other branches by this fraction.

Although incorporating tax-free military compensation into our earnings estimates likely improves the accuracy of our estimates, we admittedly do not account for all forms of military or civilian pay and benefits. Specifically, we do not incorporate any self-employment earnings, tax-exempt civilian payments, or any payments not reported by employers to the IRS in our estimates of individual earnings. Furthermore, our individual earnings measure does not account for a variety of potentially tax-exempt benefits such as health coverage, tuition payments, or retirement contributions. To the extent that our estimates differentially miss non-W-2 civilian earnings (e.g. self-employment earnings) our estimates are likely to overstate differences between military and civilian earnings. To the extent that the military provides more generous non-taxed benefits (e.g. health and education benefits), our estimates may understate differences between military and civilian earnings.

Employment, Education, Disability Compensation, and Mortality In addition to examining the effects of Army service on earnings and income, we explore the effects of the Army on employment, education, disability, and mortality. If a person receives a W-2 with positive Medicare W-2 wages within a year, we consider them employed in that year. Additionally, if a college submits Form 1098-T on behalf of an individual,[34] we identify that individual as having attended college in the year of filing. We also supplement our education outcomes with associate and bachelor degree completion data from the National Student Clearinghouse (NSC).We also combine disability compensation records from Veterans Affairs—VA Disability Compensation (VADC)—and the Social Security Administration—Social Security Disability Insurance (SSDI) and Supplemental Security

source: https://www.irs.gov/publications/p15b accessed 9/19/2019. One specific exception to these rules is that housing benefits provided to clergy members are typically tax exempt. source: https://www.irs.gov/taxtopics/tc417, accessed 9/19/2019.

[33] Most servicemembers, including all servicemembers with dependents, are eligible to select BAH compensation instead of government provided housing. In situations where the individual resides in government provided housing, we assign them the BAH they would have received given their location, rank, and dependent status.

[34] Higher education institutions that receive federal financial aid are required to file a 1098-T on behalf of each student they enroll.

Income (SSI)—into four measures of disability compensation. Finally, we identify an individual as deceased if they have a date of death recorded in the SSA death file in a current or prior year.

Additional Outcomes: Independent Contracting and Marriage. To supplement our analyses of earnings and employment, we also estimate the effects of Army service on independent contracting as measured by 1099-Misc.[35] Our measure of 1099-Misc income takes the total amount on these information-returns and like earnings, expresses them in 2018 dollars, applies $0 if missing, and winsorizes values at the 99th percentile in each year. Finally, we categorize people as married in a given year if they filed a 1040 with a status of married filing jointly or married filing separately that year. Unlike other outcomes, which are based on information returns, 1040s are filed by individuals, hence identifying a marriage is contingent on filing a 1040. Therefore we complement our marriage analysis by examining the effects of service on 1040 filing.

3.4 Descriptive Statistics

Table 1 presents summary statistics for all U.S. Army applicants between 1990 and 2011 and the subset of applicants in our analysis sample (AFQT 12-68). Overall, applicants are young (20.7 years), mostly male (78%), and most have not attended college (94%). Relative to a nationally representative sample of 17-23 year-olds, applicants are more likely to be black (21% vs. 15%), and less likely to be Hispanic (11% vs. 22%).[36] In Panel (b) of Table 1 we find that applicants are more likely to come from disadvantaged counties than advantaged ones in terms of household income, poverty rate, single parent share, employment, and Chetty and Hendren (2018) measures of intergenerational mobility. Specifically, we construct population-weighted national terciles for each of these county-level measures and then compute the share of our applicant sample coming from each national tercile. For example, 37% of applicants come from counties in the bottom tercile of median household income, whereas only 26% of applicants come from counties in the top tercile of median household income.

Compared to the population of applicants, those in our analysis sample's initial AFQT range have lower average AFQT scores (42 vs. 52), are more likely to be black (26% vs 21%), and are less likely to have attended college (4% vs 7%). Applicants from our sample who do serve in the Army (47%) serve for an average 4.8 years.

[35] The most common use of the 1099-Misc is filing work for an independent contractor. Any employer of an independent contractor earning more than $600 is required to file a 1099-misc.

[36] We derive statistics on the general population of 17-23 year-olds from the 2018 American Community Survey.

4 Estimating Framework

4.1 Empirical Approach

Our empirical strategy takes advantage of the cutoffs in the Armed Forces Qualifying Test at scores of 31 and 50, as outlined in section 2. Panel A of Figure 1 graphically depicts the relationship between servicemembers' first AFQT score on record and the probability of enlistment.

In our fuzzy RD design, the lower-ability (AFQT≥31) and higher-ability (AFQT≥50) cut scores act as instruments for our endogenous variable: an indicator variable for applicants who ever enlist in the U.S. military. While we define our endogenous variable as enlistment in any military service, the vast majority of enlistees in our sample joined the Active Duty Army and crossing cutoff has only modest effects on enlistment in non-Active Duty Army service.[37] Specifically, our reduced form estimating equation is:

$$\text{Reduced Form:} \qquad y_{ij} = f(AFQT_i) + \beta(AFQT_i \geq CUT) + \mathbf{X}_i'\gamma + \epsilon_{ij} \tag{1}$$

And we recover the point estimates of military service on individual outcomes using the following two stage least squares (2SLS) model:

$$\text{First Stage:} \qquad Enlist_i = f(AFQT_i) + \beta_1(AFQT_i \geq CUT) + \mathbf{X}_i'\gamma_1 + \nu_i \tag{2}$$

$$\text{Second Stage:} \qquad y_{ij} = f(AFQT_i) + \beta_2 Enlist_i + \mathbf{X}_i'\gamma_1 + \epsilon_{ij} \tag{3}$$

$Enlist_i$ is an indicator for any military service. y_{ij} is an outcome for individual i, j years since Army application, such as earnings, income, employment, marriage status, or mortality. $f(AFQT_i)$ is either a parametric or non-parametric function of an applicant's first AFQT score on record. In these equations, $CUT = 31$ when we estimate effects at the 31 cutoff and $CUT = 50$ when we estimate effects at the 50 cutoff. $AFQT_i \geq CUT$ is an indicator for an individual's first AFQT score on record being at or above the 31 cutoff or the 50 AFQT cutoff. We estimate effects around each cutoff separately. Additionally, X_i is a vector of pre-application characteristics which always includes application calendar and fiscal year fixed effects and additional controls when mentioned, and ϵ_{ij} is an idiosyncratic error term.

In our primary specifications, $f(AFQT_i)$ is a non-parametric local linear function with a bandwidth of $h = 19$ and a triangular kernel.[38] We allow the slope of this linear function to differ on

[37] Over 80% of enlistees in our sample joined the Active Duty Army while 10% enlist in the Army Reserves or the Army National Guard and another 10% enlist in the Navy, Air Force, Marines, or Coast Guard. Crossing either cutoff reduces the probability of enlistment in a non-Army military service by roughly 0.5 percentage points. Relatedly, crossing the 31 threshold increases the probability of enlisting in the Army Reserves or Army National Guard by approximately 1 percentage point, but crossing the 50 threshold decreases the probability of enlisting in the Guard or Reserves by 0.5 percentage points. The other services use different, higher minimum eligibility AFQT cutoffs.

[38] The functions of our triangular kernels are $K(AFQT_I) = (1 - |\frac{AFQT_i - 30.5}{19}|)$ if $AFQT_i \in [12, 49]$ and $K(AFQT_I) =$

each side of the cutoff. A bandwidth of $h = 19$ is the maximum symmetric bandwidth for each cutoff. Given our relatively linear outcome variables this choice increases power without biasing estimates, something we verify in robustness checks to alternative bandwidths.[39] Additionally, we estimate a variety of alternative specifications that vary functional form (e.g. local quadratic), bandwidth (e.g. 9, 15), and inclusion of demographic controls (e.g. age, sex, and education). Robust standard errors are reported and depicted in all cases.

The parameter of interest is β_2, which identifies the local average treatment effects (LATE) of military service among individuals who were near one of the AFQT cutoffs *and* were induced to serve or not serve in the military based on their position relative to their cutoffs. Thus, our estimates identify the effect of military service among those who would have chosen to serve had they been eligible to join the Army based on their first AFQT score but would have not ultimately served if their first AFQT was below an eligibility cutoff. Because an offer of an enlistment must be offered an accepted, our estimates are identified among applicants for whom their application is marginal in the Army's view (i.e. offer of enlistment or bonus is only made conditional on being above the cutoff score) and for whom serving in the Army is a marginal proposition (e.g. the applicant does not study and retake the ASVAB until a satisfactory AFQT score is realized). Additionally, for those who are margin of being eligible for an enlistment bonus (e.g. many applicants with a high school degree and an initial AFQT score near 50), our estimates are driven by people who would have elected to join the Army if they were eligible for enlistment bonuses based on their first AFQT score, but would not join the Army if they were ineligible for bonuses in their first AFQT scores. These applicants are likely to be nearly indifferent between serving in the Army and their outside option, have high discount rates, or both.[40]

4.2 Validity of the Discontinuity Design

A threat to ours, and any, regression discontinuity design is the possibility of precise manipulation of the running variable around the threshold, as discussed in McCrary (2008) and Frandsen (2017). While applicants are unlikely to be able to precisely manipulate their AFQT scores around a cutoff (most exams are computerized adaptive tests), the ability to retest until qualifying for an enlistment or bonus offer is potentially problematic. To address this potential issue, we use an applicant's first AFQT score on record.[41]

$(1 - |\frac{AFQT_i - 49.5}{19}|)$ if $AFQT_i \in [31, 68]$.

[39] This choice does, however, complicate statistical comparisons across the 31 and 50 cutoff since we use the observations between 31 and 50 in both estimates.

[40] Alternatively, they might have present-biased preferences (Laibson, 1997).

[41] MEPCOM only records a servicemembers' most recent three ASVAB attempts and AFQT test scores. Thirteen percent of applicants in our sample retook the test at least once while another 2 percent retook the exam two or more times. For the 2% of applicants in our sample with three recorded scores, we are unable to determine whether their first score on record is their first attempt. Note, however, that applicants who wish to take the exam a fourth time must wait at least 6 months between their third and fourth attempt, which reduces the likelihood of this behavior.

To visually inspect for manipulation of the running variable around either threshold, Figure 2 displays two histograms of AFQT scores derived from applicants in our sample. We report AFQT scores from 1990–June 2004 (Panel (a)) and July 2004–2011 (Panel (b)) separately because the Department of Defense re-normalized scores in July 2004, leading to a shift in the distribution of AFQT scores (Segall, 2004). Notably, there is significant bunching at certain AFQT scores in both Panels. Bunching occur at points where multiple raw AFQT scores correspond to a single AFQT scale score (Mayberry and Hiatt, 1992; Segall, 2004). We do not have access to applicants' raw initial AFQT scores. Importantly, there does not appear to be bunching at scores adjacent to the thresholds of 31 and 50, suggesting applicants are unlikely to be manipulating their scores around the cutoff. By way of comparison, Figure A.1 plots the distribution of each applicants *most recent* AFQT score. These histograms reveal a strong effort on the part of many applicants to achieve scores to the right both thresholds and clearly indicate that an applicants *most recent* AFQT score does not provide a valid running variable in an RD design.

Additionally, we examine potential manipulation across the low- and high-score discontinuities by testing for balance in observable characteristics across the cutoffs. Specifically, we examine balance in characteristics such as race, education, and sex reported in the Army application, as well as IRS administrative records for employment, college attendance, and earnings in the year prior to application.[42] Panels (c) through (f) of Figure 2 plot averages for certain baseline characteristics by AFQT score, with additional covariate balance plots located in Figures A.3–A.4. At least visually, there does not appear to be any substantial manipulation across either cutoff along observable dimensions. However, we complement these figures with Table A.2, which reports estimates of equation 1 in both local linear and local quadratic specifications, where the dependent variables are the baseline characteristics.

Table A.2 does reveal some imbalanced estimates of baseline characteristics under the local linear specification, especially at the 31 cutoff. The estimates in Column (1) suggest a lack of balance on race, baseline education, and any 1040 filing in the year prior to application. Some of this could occur if AFQT scores exhibit a non-linear relationship with certain baseline characteristics. Consistent with this, in Column 2 of Table A.2 only one variable—whether an applicant has attended some college or more—varies at the 5% level across the 31 cutoff under the quadratic specification. Columns (3) and (4) of Table A.2 reveal little evidence of manipulation around the 50 cutoff, with three statistically significant point estimates under the linear specification that have insignificant, and substantially smaller, corresponding point estimates under the quadratic specification.

On the whole, we feel confident the results in Table A.2, especially when combined with the lack of observable manipulation in the AFQT histograms, argue against the possibility of systematic sorting around either threshold. Nevertheless, we will show that results from our baseline specification are robust to alternative functional forms (including the balanced quadratic specifi-

[42] Pre-application IRS records are available for the 2000-2011 applicant cohorts.

cation mentioned above), alternative bandwidths, and the inclusion of additional controls.

5 Effects of Army Service on Earnings and Income

In this section we present our main, dynamic estimates of the effects of Army enlistment on earnings and employment and address key robustness concerns. Our earnings results capture the average effect of enlistment over time. This average effect operates through disparate and potentially contrasting channels, such as education and disability. In order to contextualize the effects of enlistment on earnings, we thus move rapidly into presenting the effects of enlistment on education and on mortality and disability outcomes. We then present additional outcomes before concluding with a comprehensive discussion of results.

5.1 Effects of Army Service on Earnings

Figure 3 shows the relationship between earnings and first AFQT 1, 5, 10, and 15 years after application.[43] Earnings increase at both cutoffs in each of these years and the increases are statistically significant at both cutoffs 1 and 5 years after application (Panels (a) and (b)) and significant at the lower cutoff 10 years after application (Panel (c)). The size of the jump at each cutoff corresponds to our reduced-form estimates and we scale by our first-stage to obtain our 2SLS RD estimates. For example, one year after application, enlistment increases earnings by $11,411 at the 31 AFQT cutoff and $12,380 at the 50 AFQT cutoff (see Table 2). Table 2 provides additional snapshots of how the causal effect of enlistment on earnings varies over time. However, Figure 4 provides the most comprehensive picture of military service's dynamic effects.

Figure 4 Panel (a) plots the 2SLS RD coefficient estimates for *each* year from -2 to 19 years after application.[44] The dashed black line plots the estimates at the 31 AFQT cutoff, while the solid gray line plots the estimates at the 50 AFQT cutoff. These estimates are based on an unbalanced panel of application cohorts, a discussion we return to in Section 6 where we look at heterogeneity by application cohort.[45] The effect of enlistment on earnings is relatively stable over the first 3 years after application, with the average effect 1-3 years being $10,950 and $11,813 at the low and high cutoff respectively. This is consistent with the fact that most enlisted soldiers sign up for at least 3 years of service.

Turning to the medium-term effects of enlistment, we see that earnings of enlistees become closer to those of non-enlistees 5-15 years out. Nevertheless, the enlistment coefficient remains

[43] Appendix Figures A.15–A.16 show these plots for every year post-service from -2 to 19.

[44] Every estimate underlying Figures 4–6 is reported in Appendix Table A.3.

[45] We are restricted by IRS data starting in 1999 and ending in 2018. Thus, estimates are based off of later application cohorts in the short-run (e.g. 1999-2011 in year 0), most cohorts in the medium run (e.g. 1990-2009 in year 9), and earlier cohorts in the long-term, (e.g. 1990-1999 at year 19). See Appendix Figure A.2 Panel (a).

positive, economically meaningful, and statistically significant in many years of these years, averaging $1,840 at the low cutoff and $3,393 at the higher cutoff. 13 years after application, we no longer detect statistically significant effects at the low cutoff (the 16-19 year average estimate is $1,181). While not statistically distinguishable from the 31 cutoff, the long-term effect at the 50 cutoff is higher (the 16-19 year average is $4,366) and statistically significantly different from 0 in each of these years with the exception of 16.

The cumulative effect of enlistment on earnings in this 20-year window is clearly positive. The sum of the estimated coefficients over the first 15 years is $61,378 at the lower AFQT cutoff and $90,236 at the higher cutoff. To be more precise, in Table 3 Panel (a) columns (4) and (8) we restrict our sample to the 1999-2003 application cohorts for whom we observe earnings in each year after application from 0 to 15 and estimate the effect of enlistment on cumulative earnings over the first 15 years following application. We obtain positive and significant cumulative effects. Enlistment increases total earnings over the next 15 years by $75,573 at the 31 cutoff and by $121,936 at the 50 cutoff. These are large differences relative to the mean of total earnings: earnings are 21% higher than the mean at the 31 cutoff and 31% higher than the mean at the 50 cutoff. While a significant fraction of earnings benefits can be attributed to gains in the first 5 years after application (see columns (1) and (5)), columns (2)-(3) and (6)-(7) make it clear that earnings gains are persistently positive and non-negligible 6-10 and 11-15 years out. Years 6 through 15 account for over $21,000 in gains at the 31 cutoff and over $28,000 at the second.[46]

Comparison to OLS. Notably, OLS estimates of equation 3, reported in Table 2 and displayed graphically in Appendix Figure A.5, are substantially larger than corresponding 2SLS RD estimates. At the 31 cutoff, OLS estimates 5-19 years after application are all statistically larger than 2SLS estimates and imply that applicants who enlist earn between $7,000 and $9,500 more per year than applicants who do not enlist, even after controlling for AFQT score. OLS at the 50 cutoff are also larger than corresponding 2SLS estimates, albeit not always statistically significantly so. Given these the differences between our RD and OLS estimates, it is likely that applicants positively select into military service on unobservable dimensions.[47]

Robustness of Earnings Estimates. Before proceeding to additional outcomes, we pause to discuss the robustness of these earnings results. In Appendix Figure A.7, we probe robustness to the inclusion of demographic controls, to alternative functional forms, and to alternative bandwidths. Panel (a) shows that estimates at the 31-point cutoff are insensitive to the inclusion of controls

[46] Table 3 aims to maximize power by reporting the estimates for each time range on the largest possible sample of cohorts. As an alternative, Appendix Table A.4 restricts to the 99-03 cohort in all columns. Longer-term earnings gains account for a higher share of total earnings gains. We discuss heterogeneity by application cohort in more detail in Section 6.

[47] Although, it is possible that the causal effect of Army service among compliers in our analysis might differ from the average treatment effect on all applicants. Even if so, the LATE we estimate captures a policy relevant parameter in that a natural way to limit/expand the size of the army involves greater/lesser selectivity on AFQT scores.

for gender, race (black and hispanic dummies), age, education at time of application (still in high school, GED, high school diploma, some college, college graduate), and dummies for home of record state. Panel (b) reveals some sensitivity to the inclusion of controls in the longer-term effect estimates at the 50-point cutoff, but the estimates with and without controls remain statistically indistinguishable from one another and our key takeaways are unchanged. Panels (c) and (d) use two alternative function forms to capture the underlying relationship with AFQT: a linear specification without the triangular kernel, and a quadratic specification (also without the kernel). In each case, as in our baseline, the relationship is allowed to differ on each side of the cutoff. These specifications are statistically indistinguishable from one another. Finally, Panels (e) and (f) plot estimates using narrower bandwidths of 15 and 9 AFQT points. The presence of two AFQT cutoffs restricts us from expanding our window beyond our baseline of 19 AFQT points. The narrower bandwidths slightly increase our standard errors, but reassuringly do little to our coefficient estimates.

Relevance of Tax-Free Army Benefits. One contribution of our study is including tax-free military-specific compensation that is not captured in Medicare W-2 wages—most importantly the housing allowance—in our causal estimates of Army service. These tax-free benefits are a major part of Army compensation and its relative attractiveness, thus omitting them would lead to underestimating the effects of Army service. However, we readily acknowledge that our inclusion of these benefits may bias our estimates in favor of enlistment if alternative civilian opportunities also offer non-taxable, and hence unobservable to us, benefits (e.g. clergy housing benefits).

In Appendix Figure A.6, we show how our estimates differ when we exclude non-reported Army compensation from our earnings measure and simply estimate the effects of Army service on reported Medicare W-2 wages. These results reveal that accounting for Army pay that is not reported to the IRS more than doubles the estimates of the effects of service on earnings in the initial 3-4 years. Accounting for non-taxable Army benefits increases our long-run estimates by close to $1500 per-year at the 31 cutoff and $2500 per-year at the 50 cutoff. These longer-term differences are consistent with the fact that around 10% of enlistees stay in the Army for at least 15 years (see also Figure 1 Panel (b)). Army-specific compensation is thus an important factor in accounting for the effects of Army service on earnings. Given the rarity of employment opportunities with non-taxable benefits such as housing among this sample, we believe our baseline results in Figure 4 yield the most accurate picture of the true effect of enlistment on earnings.

5.2 The Effects of Army Service on Employment, Education, Disability Compensation, and Other Outcomes

In order to better understand our estimated effects of enlistment on earnings and their dynamics, we explore several potential underlying channels. Army service is a complex 'treatment', with

potential implications for both earning-enhancing vehicles like employment and education and earning-reducing channels like disability and mortality. Moreover, the implications of channels like greater post-service education for earnings are necessarily dynamic, likely reducing medium-term earnings and increasing longer-term earnings. We turn to these additional outcomes now.

The Effects of Army Service on Employment. Figure 4 Panel (b) plots 2SLS RD estimates of enlistment on employment, defined as having a positive W-2. In the first 1-3 years after application, enlistment increases employment by an average of 5.5 percentage points at the lower cutoff and 8.9 percentage points at the higher cutoff. Mean employment rates over this period are 91-93%.

Over time, the positive employment effects of enlistment diminish. 7-19 years after application, the effect of enlistment on employment is indistinguishable from 0 at the 50 cutoff (it averages 1.8 percentage points). By year 7 at the 31 cutoff, we start to observe negative and statistically significant employment effects. On average, we observe a 3.3 percentage point reduction in employment for years 7-19 and a 3.8 percentage point reduction for years 15-19. As we will see next, these longer-term negative employment effects are in large part explained by soldiers attending college.

Post Secondary Attendance. Figure 5 Panel (a) plots the effect of enlistment on post-secondary attendance, defined as having a 1098-T in the given year. In the first 1-3 years after application, enlistment decreases post-secondary attendance by 6.9 percentage points at the 31 cutoff and 5.5 percentage points at the higher cutoff. Mean post-secondary attendance rates over this period are 15-17%. In the short-term, enlistment is an alternative to attending college for some applicants.

In contrast, in later years enlistment increases college attendance. 6 years after application, enlistees are 11.0 and 8.5 percentage points more likely to attend a post-secondary institution at the first and second cutoff respectively. This is more than inter-temporal substitution: the effect of enlistment on post-secondary attendance remains positive all the way out to 18 years after application, averaging 5.7 and 7.7 percentage points for years 5-19. Cumulative estimates of the effect of enlistment on ever attending post-secondary (0-15 years) for the 1999-2003 cohorts are shown in Panel (c) of Table 3. Overall, enlistment increase college attendance by 6.5 percentage points (10% relative to the mean) at the lower cutoff and 17 percentage points (24% relative to the mean) at the higher cutoff. As we will see in Section 6, these effects are even larger for later application cohorts, consistent with increasing GI bill generosity over time.

In Appendix Figure A.8, we incorporate data on degree and college types as well as degree completion from the National Student Clearinghouse.[48] While coverage is imperfect, particularly in early years, this data allows us explore effects of enlistment on institution type and graduation outcomes.[49] Appendix Figure A.8 Panel (a) plots the effect of enlistment on post-secondary

[48]NSC data are not linked to IRS data and NSC outcomes are estimated on a slightly different and larger sample that includes the approximately 1% of applicants that we could not link to any IRS records.

[49]While NSC collects data from some colleges before 1990, many colleges phase into the data over time, particularly

attendance in the NSC data, which reassuringly mirrors 1098-T estimates. Meanwhile Panel (b) plots the effect of enlistment on graduation (associates degree or higher), showing positive long-term effects. Appendix Table A.5 shows that Army service increases cumulative attendance and graduation. Enlistment increases attendance at both public and private 2- and 4-year colleges.[50] We find increased attendance at both moderately or more selective institutions and minimally- or non-selective colleges, but effects are particularly large for less selective institutions. While enlistment increases attendance at both AFQT cutoffs, overall effects are larger at the 50 cutoff and driven by differentially higher 4-year non-profit attendance. Last, we find that service increases degree completion. Enlistment increases any degree completion by 4.4 p.p. at the low cutoff and 7.6 p.p. at the high cutoff. More than half of this is explained by increased bachelor (or higher) degree completion (2.3p.p and 4.2p.p).

Overall, these post-secondary attendance dynamics would be expected to limit earnings gains from enlistment and reduce employment rates in the medium-term while improving long-term earnings and employment.[51]

Indeed, in Figure 5 Panel (b), we revisit our employment results, this time constructing an indicator for whether a person is employed (any positive W-2) *or* enrolled in post-secondary school (any 1098-T). When taking into account education, we now see that service in the first 1-3 years has slightly smaller but still positive effects on being employed or in college. Relative to a mean of 92.7% (94.4%), enlistment increases employment/attendance rates by 0.044 (0.071) at the 31 (50) cutoff. In other words, enlistment is not simply a short-run substitute for education: it increases overall time spent employed or in school. Once we account for post-secondary attendance, the long-term effects of enlistment on time spent employed or in school at the low cutoff are not statistically significantly different from 0. 7-19 years after application, enlistment decreases the employment/attendance rate by an average of 1.3 percentage points at the 31 cutoff. Meanwhile, it increases employment/attendance by an average of 4.5 percentage points at the 50 cutoff.

Given the clear increase in post-secondary attendance, earnings effects with a medium-term dip and a long-term climb, are not surprising. While this description perhaps fits the higher cutoff, earnings and employment results at the lower cutoff show little sign of any long-term uptick. Even after accounting for post-secondary the point estimates on long-term employment at this cutoff are still negative. In light of our education results, this is somewhat surprising and may point to offsetting, harmful effects of Army service among some soldiers, the subject we turn to next.

Mortality and Disability Compensation. Reduced human capital and health due to injuries or

around the early 2000s. Given the dynamic patters of our estimated enlistment effects, this changing coverage could bias our estimates. The strong similarity of the effects of enlistment on post-secondary attendance in the NSC and 1098-T data provide some reassurance that any such bias is limited.

[50] Over 98% of 2-year college attendance in our sample comes from 2 year public colleges.

[51] Our earnings outcome does not capture the housing allowance associated with the Post-9/11 GI Bill. Its inclusion would increase earnings estimates in Figure 2.

death from combat exposure and training could be exerting a drag on employment and earnings and offsetting any long-term gains from increased education.[52] Indeed, the large number of veterans receiving disability could be reflective of the negative health consequences of recent military service (see for e.g. Stiglitz and Bilmes, 2008). Moreover, disability compensation *in and of itself* could reduce earnings and labor force participation through income effects, the work-limiting aspects of Individual Unemployability, or through interactions with SSDI.[53] We probe these outcomes here.[54]

In Table 4 we use our RD design to estimate the effect of service on mortality within 1, 3, 5, 10, and 15 years of application. Estimates are generally noisy. There is some indication that service is protective in the early years at the lower cutoff, perhaps because Army employment reduces risky behavior in the short-run or health care availability helps limit mortality. Over time, point estimates at the 31 cutoff suggest that service increases mortality, with the point estimates for mortality within 15 years turning positive, but statistically indistinguishable from 0. At the 50 cutoff point estimates hint at increased short-term risk and reduced long-term risk, but we are wary to draw any conclusions given the noise. The 15-year means are less than 1.9 percent. We turn next to disability receipt and compensation.

Figure 6 reports estimates of the effect of enlistment on annual disability compensation payments. Our measure of disability compensation sums Veterans Affairs Disability Compensation (VADC) payments from the Department of Veterans Affairs, Social Security Disability Insurance (SSDI) payments from the Social Security Administration, and Supplemental Security Income (SSI) payments from the SSA.[55] Panel (b) shows that between 5 and 19 years after service, enlistment increases disability receipt by an average of 15.4 percentage points at the 31 cutoff and 12.5 percentage points at the 50 cutoff. These estimates are relatively stable and more than 3 times as large as the estimated impact of Vietnam-era military service on disability receipt (Angrist et al., 2010).[56] Meanwhile, the effect of enlistment on disability compensation reaches around $1,500 six years out and steadily increases to $3,200 at the 31 cutoff while fluctuating between $1,300 and $2,600 at the 50 cutoff 7-19 years after application (see Panel (a)). Mean disability compensation

[52] Recall that deaths show up in earnings as zeros, since the deceased would have matched to the master SSN file but would not have any Medicare W-2 earnings.

[53] Consistent with these points, Autor et al. (2016) find that VADC receipt among Vietnam-era veterans causes one-in-five recipients to leave the labor force and increases SSDI receipt by 12 percentage points. Coile et al. (2019) documents a similar interaction between VADC and SSDI among Gulf Era Veterans.

[54] We note, of course, that this is not an exhaustive list of potentially harmful effects of service, just as education is not an exhaustive list of beneficial effects of service. For example, reduced human capital due to time away from the traditional labor force may be an important factor. Alternatively, it is also possible that soldiers are unable to realize gains from their additional education. Nevertheless, mortality and disability, like education, are first order channels oft mentioned in related literature that also have the advantage of being observable and measurable.

[55] These data are not linked to IRS data and hence are estimated on a slightly different and larger sample that includes the approximately 1% of applicants that we could not link to any IRS records. We only have SSDI and SSI data from 1999 through 2015, so we only report disability outcomes from the same years.

[56] Table 4 of Angrist et al. (2010) reports that the causal effect of Vietnam-era military service on disability receipt was 4 percentage points by 2000, nearly three decades after most Vietnam-era veterans concluded their service.

and receipt by year can be seen in Appendix Figure A.2. These are large effects, especially relative to our estimates of the effects of enlistment on earnings in Figure 4.

Relative to the effects of Army service on receiving any disability compensation, we find that service has much smaller effects on the receipt of disability compensation that is explicitly work limiting, which includes veteran and civilian SSDI and SSI receipt as well as veteran Individual Unemployability VADC status. Panel (c) shows a 2-4 percentage point increase in work-limiting disability at the 31 cutoff (about one-fifth of the effect on any disability receipt) and no observable effect at the 50 cutoff. Much of this is driven by SSI/SSDI (Panel (d)) and SSDI in particular, which is consistent with Autor et al. (2016)'s findings that VADC receipt increases SSDI receipt. Appendix Figure A.9 breaks down the effects on disability receipt by type of disability.

Overall, while non-IU VADC explains the majority of our disability receipt results (see Panels (c) and (d) of Appendix Figure A.9), we do find an increase in work-limiting disability compensation at the lower cutoff. This almost certainly exerts a drag on baseline earnings (which do not include disability compensation) and employment and helps contextualize our baseline estimates. Higher overall disability compensation at both cutoffs may also be exerting a drag on employment and earnings through health or income effects, but if so these effects are evidently not large enough to negate our positive average employment and earnings effects.[57]

Additional Outcomes: Independent Contracting, 1040 Filing, and Marriage. Before discussing how these baseline results fit together, we briefly examine some additional outcomes.

Figure 7 Panel (a) shows 1099-Misc Income (any 1099-Misc follows a similar pattern). The most common use of the form is to report earnings for work as an independent contractor. Service lowers 1099-Misc income in the first 10 years by an average of 83$ and 199$ at the lower and higher cutoff respectively off of means of around $300. However, in the long-run service has a positive effect on 1099-Misc at the lower cutoff (averaging $250 15-19 years out) and little effect at the higher cutoff.

Panel (b) shows that enlistment has small positive short-term effects on filing a 1040 (3.0 and 3.5 percentage points off of 1-3 year means of 67.0 and 69.5 at the 31 and 50 cutoff, respectively). It has no statistically significant longer term effects in later years at the 31 cutoff. Long-term estimates of enlistment on filing a 1040 at the 50 cutoff are noisier, bouncing between 0 and 5 percentage points and averaging 3 percentage points, but also rarely significant. Mean 1040 filing rates 5-19 years out are 79.6 and 82.1 around the 31 and 50 cutoff, respectively.

We designate a person as married if his or her 1040 filing status is married (either filing jointly or separately). We note upfront that this indicator will be 0 if you do not file a 1040, and hence is contingent on filing (unlike the outcomes up to this point, which were based on information returns). However, because enlistment has relatively small effects on filing, the bias introduced by filing effects is likely to be limited. Figure 7 Panel (c) plots our estimates of the effects of Army

[57] Indeed, the extent to which these disability numbers reflect worsened health outcomes is unclear (Angrist et al., 2010).

service on this 1040-based marriage indicator. In the first 3 years after application, enlistment increases marriage rates by 18.0 and 17.2 percentage points at the first and second cutoff, respectively. Relative to the means of 17.0 and 20.6% over this period, these are large effects. Moreover, they cannot be explained by filing effects. While marriage does not necessarily benefit individuals, a significant body of research suggests that marriage and happiness are strongly positively correlated (e.g. Kahneman et al., 1999; Stack and Eshleman, 1998; Diener et al., 1999) and that people become more happy when they marry (e.g Stutzer and Frey, 2006; Clark et al., 2008).

These short-term effects diminish over time, but enlistment nevertheless appears to cause a persistent increase in the probability of marriage, averaging 8.6 percentage points in years 5-15 at the lower cutoff and 11.1 percentage points at the higher cutoff. These long-term effects cannot be explained by 1040 filing effects, which average -1 and 3 percentage points over this period. While the Army does incentivize marriage with financial benefits such as increased housing allowances, it is striking that there are such large effects on marriage long after most servicemembers have left the Army.

5.3 Discussion

We now return to a holistic assessment of results thus far. In the short-run, enlistment increases employment rates and earnings by around $10,000 at both thresholds. Non-enlistees are more likely to attend college and less likely to be employed during this time.

While many enlistees leave the service, some stay and make a career out of the Army. As enlistees leave the service, they attend school. Over 15 years, they are more likely to have attended college than had they not enlisted. Delayed college attendance, potentially coupled with missing years of traditional labor force participation, should exert a drag on medium-term earnings gains and employment rates. Presuming veterans are able to translate additional education into gains, the overall increased rates of education should translate into long-term earnings and employment gains.

Consistent with college attendance, we see lower medium-term earnings and effects of $1,500-3,000. At the higher cutoff, consistent with returns to education and perhaps to service experience, earnings effects begin to rebound from a low of $1,253 9 years out to over $5,000 19 years out. Meanwhile, at the lower cutoff, we see little evidence of an education-led rebound, despite positive education effects. At both 9 and 19 years out earnings effects are very close to $1,500. Employment rates, which were unchanged and insignificantly positive at the 50 cutoff, fall in the long-run at the 31 cutoff by 3-5 percentage points.

The absence of large education-driven long-term earnings gains at the lower cutoff is likely explained by disability-induced reductions in employment.[58] Work limiting disability increases

[58] It could also be that service-members at this cutoff are less able to realize the gains from education, but given the disability results we favor an offsetting positive and negative narrative.

by around 4 percentage points in the long-run at the low cutoff with no discernible effect at the higher cutoff. The large increase in total disability compensation at both cutoffs may also be limiting long-term employment and earnings gains.

Overall, enlistment comes with large cumulative earnings gains 0-15 years out (over 20% increases at both cutoffs). These gains are accompanied by permanent increases in post-secondary attendance and marriage. This evidence is consistent with the Army generally improving the well-being of the average enlistee. We turn next to dissecting this average: does enlistment help some more than others? How does this depend on one's experience in the army and on one's outside opportunities?

6 The Effects of Army Service Among Different Populations

The effects of army service would differ across demographics if diverse groups experience the army differently or if they would have had different counter-factual experiences. In this section, we examine whether the effects of enlistment are heterogeneous with respect to application cohort, gender, and race.

The Effects of Army Service on Different Application Cohorts. Our first sample split examines whether the earnings effects of enlistment differ significantly for applicants in the 1990s as compared to applicants in the 2000s. The nature of combat and deployment experiences have changed dramatically since the start of the global war on terrorism in 2001. For example, pre-2001 38% of Army enlistees deployed, while post-2001 63% did. In response, pay and incentives as well as Post-9/11 education benefits have also adapted. The applicant pool itself may also have changed. Moreover, this sample split speaks directly to how to interpret our dynamic results, which are based on an unbalanced panel of application cohorts.

Figure 8 compares the effect of enlistment at the 31 cutoff for 1990-2000 applicants to 2001-2011 applicants. We prioritize the 31 cutoff for display purposes and because we have more power. Appendix A.10 contains all the estimates at the 50 cutoff. Panel (a) compares the effect of enlistment on earnings across cohorts. The short-term effect of enlistment on earnings is substantially larger for the 2001-2011 cohorts. This is predominately because military base pay and housing allowances were higher for the 2001-2011 cohorts than for the 1990-2000 cohorts. We do not detect differences in the longer term effects of service across these cutoffs.[59] Panels (b)-(f) compare the effect of enlistment on employment, post-secondary attendance, employed or attending post-secondary, and on disability receipt. The most apparent differences are for post-secondary attendance and disability receipt. Later cohorts post-secondary attendance effects are larger for the 2001-2011 cohorts at the 31 cutoff, consistent with the large expansion in education benefits

[59] Long-term Point estimates at the 50 cutoff do appear lower and closer to zero for later cohorts, but they are noisy and statistically indistinguishable.

brought on by the Post-9/11 GI Bill (Barr, 2015). Disability receipt has also increased dramatically for the more recent cohorts, although effects on work-limiting disability are not statistically distinguishable. These patterns across cohorts once again highlight the potential offsetting benefits and costs of Army service: compared to early cohorts, later Army cohorts take advantage of larger education benefits, have higher rates of disability receipt, and experience similar long-run earnings trajectories.[60]

The Effects of Army Service By Gender. Next we explore whether the Army differentially benefits men and women. Outside of correlational studies, we are not aware of other papers that estimate the causal effects of military service in the U.S. for women. Prior to 2016, women were precluded from serving in combat occupations such as infantry and armor, but all women were eligible for deployment to combat zones (and 33% of female Army enlistees in our cohorts did so) and many served in combat units.[61] While the possibility of less exposure to trauma and injury potentially benefits women relative to men, women may be at a disadvantage due to gender imbalance in the military.[62]

In Figure 9 we explore whether the Army has differential dynamic effects on earnings for male and female applicants at the 31 cutoff (Appendix Figure A.11 shows estimates at the 50 cutoff). We obtain relatively similar estimates of the effects of enlistment on earnings for both men and women at the 31 cutoff. Medium and long-term point estimates appear larger for women at the 50 cutoff, albeit not significantly so. In Panels (b)–(f), we see relatively little evidence of different employment, post-secondary attendance, and disability by gender at the 31 cutoff.[63] Post-secondary attendance and disability are occasionally higher for women at the 50 cutoff.

Race and the Effects of Army Service. Although only 13.4% of the United States Population is black or African American,[64] 25.6% of applicants in our sample and 23.1% of enlistees are black.[65] Thus the Army has the potential to disproportionately affect the outcomes of young black Americans. Prior work by Angrist (1998) suggests that the Army may produce modest earnings gains among black servicemembers while potentially harming white servicemembers.

In Figures 10 and A.12 we examine whether the effects of Army service vary by whether an individual is black or white. We find positive effects of Army service on earnings for black appli-

[60] Patterns in earnings at the 50 AFQT cutoff across cohorts are also consistent with offsetting education benefits and disability costs. At this cutoff, later cohorts do not attend college at higher rates but do receive higher levels of disability compensation. This may explain why later cohorts do not see the long-run earnings gains experienced by earlier cohorts.

[61] Reference: https://www.defense.gov/Newsroom/News/Article/Article/632536/carter-opens-all-military-occupations-positions-to-women accessed 10/21/2019.

[62] For example, research suggests that the presence of female mentors and role-models matter for women's educational and professional success (e.g. Beaman et al., 2012; Carrell et al., 2010).

[63] Appendix Figure A.14 shows marriage heterogeneity.

[64] Source: https://www.census.gov/quickfacts/fact/table/US/RHI225218qf-headnote-a accessed 10/18/2019.

[65] In the window around the 31 cutoff, 30% of applicants are black and 48% are white. In the window around the 50 cutoff, 22% are black and 60% are white.

cants in both the short and long run, with magnitudes substantially larger than those reported in Angrist (1998). For black applicants at both cutoffs, we find large positive effects of Army service on earnings in the short-, medium-, and long-run. In the first three years after application, enlistment increases earnings by an average of $12,527 at the low cutoff and $12,731 at the high cutoff. 4-10 years after application, black enlistees experience average earnings increases of $3,836 and $7,082 at the 31 and 50 cutoff, respectively. 11-19 years after application, they experience gains of $5,253 and $10,837. These long-term earnings gains are large, corresponding to 19 and 34% of mean earnings over the same period.[66] The long-term earnings effects of roughly $5000 per year among black applicants at the 31 cutoff accounts for roughly 20 percent of the raw black-white wage gap among 30-39 year old American men.[67]

The effects of Army service among black applicants contrast starkly with the effects among white applicants, particularly at the 31 cutoff. At the lower ability cutoff, white applicants realize smaller earning gains in the short run (1-3 years post application), experience no earnings gains in the medium run (4-10 years post application), and actually experience earnings losses averaging $3,449 in the long run (11-19 years). These earning losses are a clear indication that service has offsetting, negative effects for at least some enlistees. Differences are less stark at the higher cutoff (Panel (b)), with whites experiencing average medium-term earnings gains of $5,308 and longer-term gains of $3,814. While not significantly different, these gains are nevertheless substantially smaller than the comparable earnings gains for blacks.[68]

The divergent experiences of blacks and whites are also reflected in employment rates. Figure 10 Panel (b) shows that blacks do not experience reductions in employment as a result of service at either cutoff, while whites at the lower cutoff experience large employment reductions. This occurs despite similar post-secondary attendance patterns (Panel (c)). Panel (d) and (e) reveal that while disability receipt is similar overall, the effect of service on work limiting disability is lower for blacks at the 31 cutoff. This is consistent with differential combat exposure for blacks and whites.

Several theories could explain the large differences in estimated earnings effects by race. First, black servicemembers may have different Army experiences. For example, they may be less likely to experience combat. Carter et al. (2017) find that only 24% of black enlisted servicemembers serve in a combat arms branch of the Army (e.g. infantry, field artillery, and special forces) compared to 50% of white enlisted servicemembers. As a result, Carter et al. find that black servicemembers are less likely than white servicemembers to be injured during service. Second, blacks

[66] By way of comparison, matching estimates reported in Angrist (1998) suggest that military service among black applicants from 1978 through 1982 was associated with a short-term earnings increase $4,000 and a long-term earnings increase of roughly $1,500 (in $2018).

[67] The 2018 American Community Survey shows that black men between 30 and 39 years old earn $32,000 on average while white men of the same age earn $58,000 on average.

[68] Results for Hispanics at the 31 cutoff, available upon request, are noisy due to the small sample size but fall between those for blacks and whites.

may be more likely to benefit from the credentials that come with Army service and post-Army college attendance. There is a large body of evidence suggesting racial discrimination in the labor market.[69] If the credentials of Army service and college attendance attenuate racial biases in the labor market, then Army service may differentially benefit black applicants in the long run. Finally, even if the Army experience and its returns are similar across race, a black person's outside options may be worse. For example, Black Americans face worse economic prospects even after controlling for parental income and tend to come from places with limited economic opportunity (Chetty et al., 2018; Chetty and Hendren, 2018). As such, it is unclear whether and to what extent the earning heterogeneity by race is reflective of lower opportunities for upward mobility among blacks.

In ongoing work, we are investigating whether the Army appears to help those with lower opportunity generally, regardless of race. Exploiting the fact that many of our applicants were dependents on their tax returns in years prior to application, we are using the IRS data to construct parental income as a proxy for opportunity and we are also exploring geography-based metrics. For now, consistent with Colin Powell's positive outlook on the role of the military in providing "openness and opportunity to minorities", we can conclude that the Army appears to provide black Americans with a valuable opportunity for upward economic mobility.

7 Conclusion

In this paper, we use a fuzzy regression discontinuity (RD) approach to investigate the causal effects of enlisting in the U.S. Army. We find that Army service significantly increases aggregate earnings, post-secondary attendance, disability compensation, and marriage at each cutoff. The positive effects of Army service on earnings are largest in the first three to five years after application but persist in the medium-run. At the lower ability cutoff, these positive effects on earnings largely dissipate by 13 years post-application. This is not the case at the 50 cutoff, where we estimate positive long-run earnings effects. Consistent with these differences, we find that service increases education at both cutoffs but increases work-limiting disability receipt only at the lower cutoff. The dynamic effects of service differ significantly by race. Army service has large and persistent positive effects on earnings for black applicants, with black enlistees experiencing long-run (11-19 years post application) earnings gains of 19% and 34% at the 31 and 50 AFQT cutoffs, respectively. In contrast, whites experience long-run earnings and employment losses at the 31 cutoff and small long-run earnings gains at the 50 cutoff.

Over the last several decades, income inequality in the United States has been rising (Piketty et al., 2017) and the prospects for young males with limited education have been declining (Autor

[69] See Lang and Lehmann (2012) for a review. Furthermore, more recent evidence suggests that racial discrimination increases when employers can observe race but not other applicant information such as criminal records (Agan and Starr, 2017; Doleac and Hansen, 2016).

and Wasserman, 2013). Mobility prospects have been particularly dire for black men (Chetty et al., 2018) and those from disadvantaged locations (Austin et al., 2018; Chetty and Hendren, 2018). As such, applicants from the Army—who, relative to the broader American population, are more likely to be male, black, high-school educated or less, and come from disadvantaged locations—are an important population. Our estimates suggest that the Army can be a critical institution for improving the life outcomes for a group of people facing limited economic prospects.

Overall, our results suggest that comprehensive government programs, such as military service, can have a meaningful positive impact on disadvantaged populations. More generally, they suggest that polices that deliver some of the positive aspects of military service to the broader population are likely to help at risk individuals. For example, more active hiring among disadvantaged populations, expanded health care coverage, or broad education benefits resembling a GI Bill for displaced workers, would likely benefit the target population.

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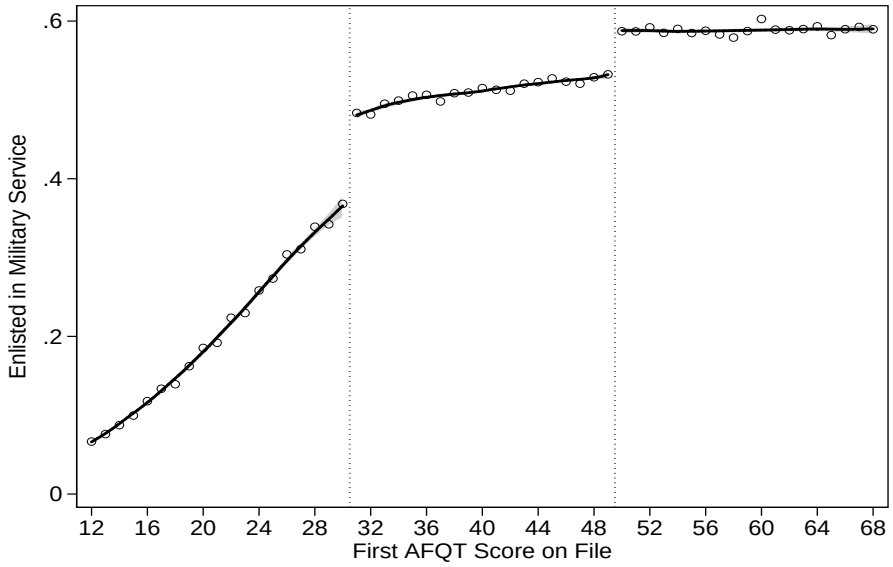
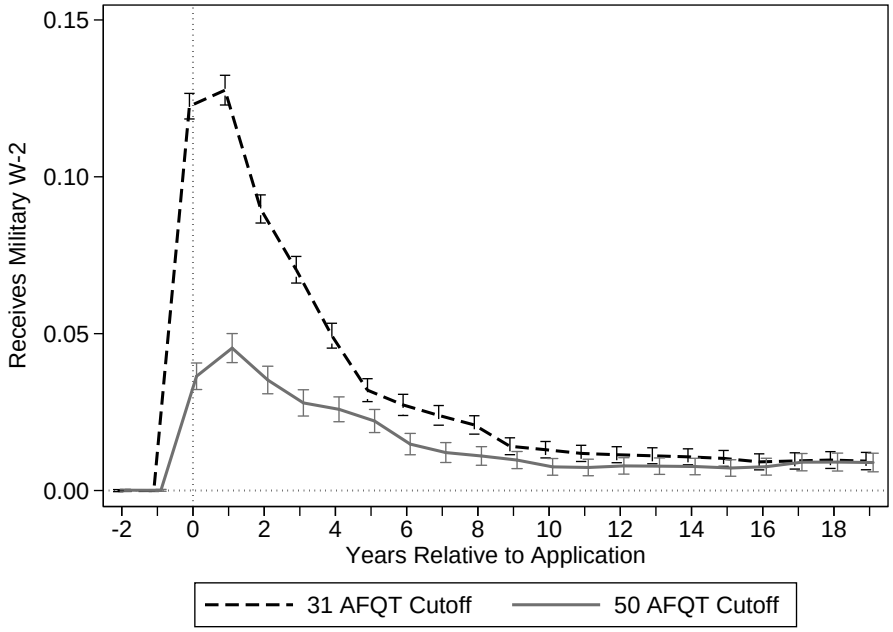
Figures

Figure 1: AFQT scores and Military Service

(a) First Stage: AFQT score and Military Service

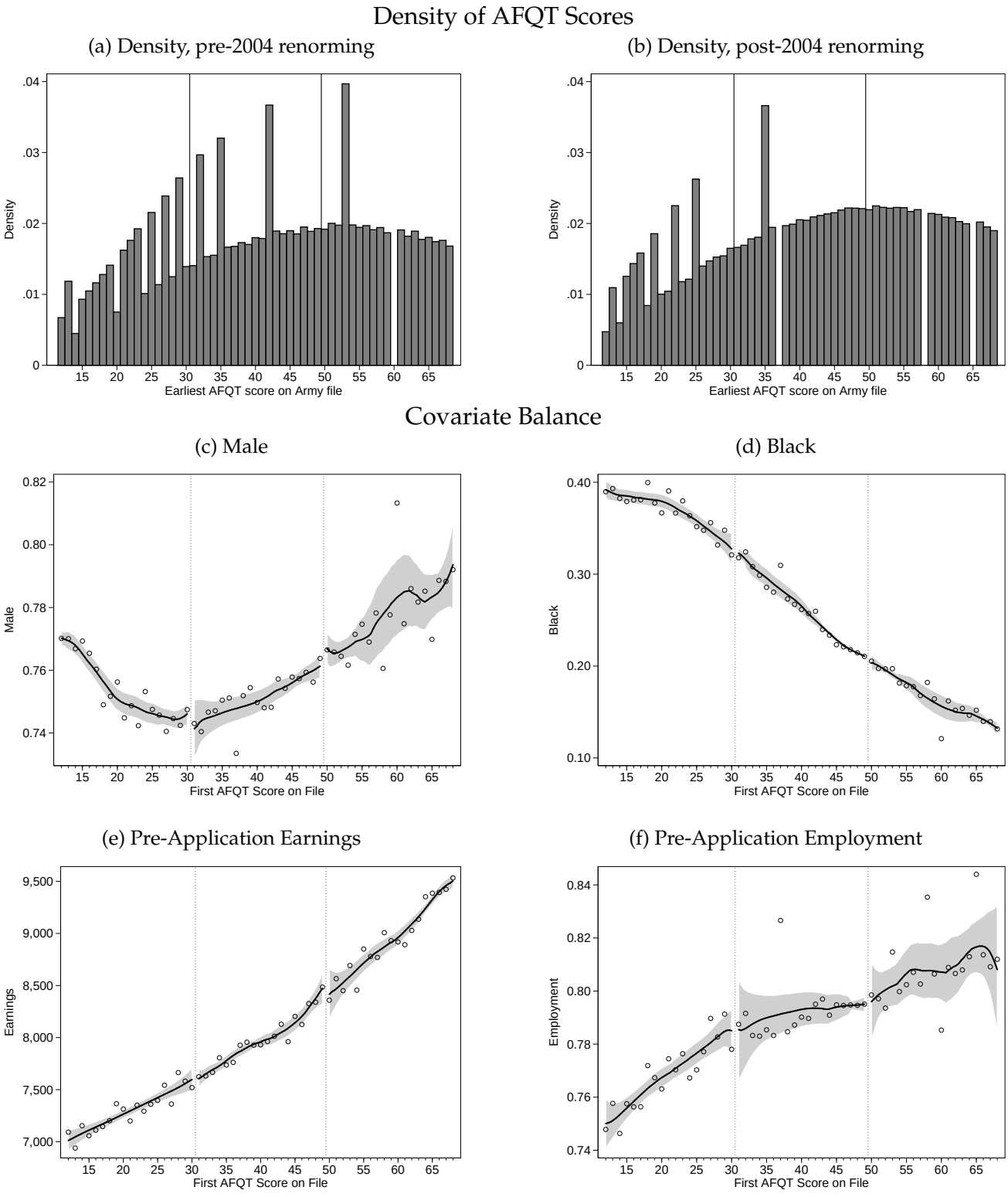


(b) Reduced Form: In Military, by Years Since Application



Notes: Panel (a) shows our first stage: it plots the probability of military service as recorded in the MEPCOM Army applicant data data against applicants' earliest AFQT score on file. The two RD cutoffs at AFQT scores of 31 and 50 are indicated by dashed vertical lines. We see a clear discontinuity in the probability of enlistment at both cutoffs. Panel (b) plots reduced form RD estimates of having a Military W-2 separately for each of the two RD thresholds indicated in Panel (a). Each point on the dashed black line (solid gray line) corresponds to a separate reduced form RD estimate of the effect of crossing the 31 (50) threshold on having a military W-2 in the given number of years after the application calendar year. 95% confidence intervals are indicated.

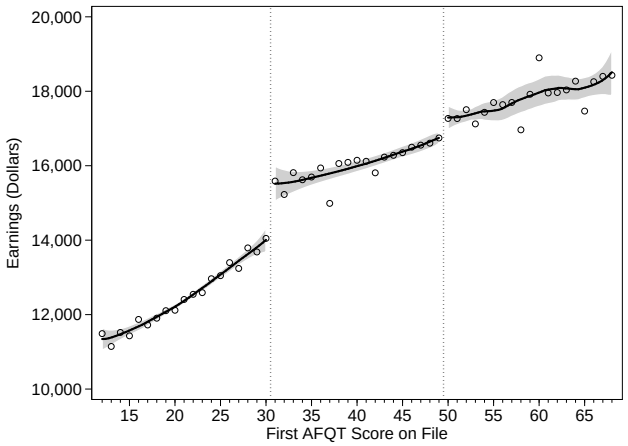
Figure 2: Validity Checks



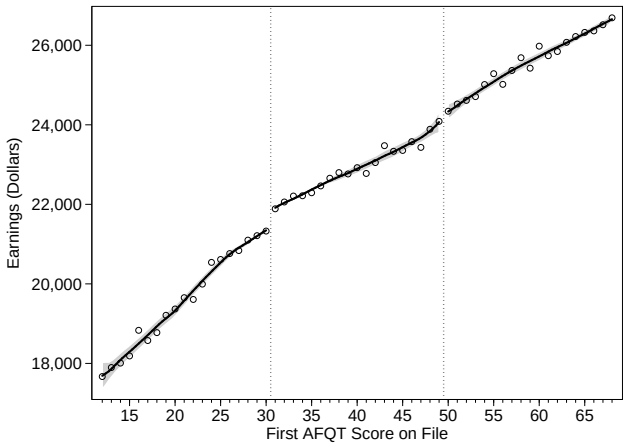
Notes: Panels (a) and (b) show the distribution of earliest AFQT scores on record before and after the July 2004 ASVAB re-norming, respectively. Panels (c)-(f) show covariate balance on selected observables. Panel (c) plots fraction of applicants that are male, panel (d) plots the fraction of black applicants, panel (e) shows balance on our baseline earnings in the year prior to application, and panel (f) shows balance on pre-application employment (any positive W-2). Appendix Figure A.3 contains additional covariate reduced form plots and Appendix Table A.2 shows the corresponding balance check regressions.

Figure 3: Reduced Form Plots: Earnings, 1, 5, 10, and 15 Years Post Application

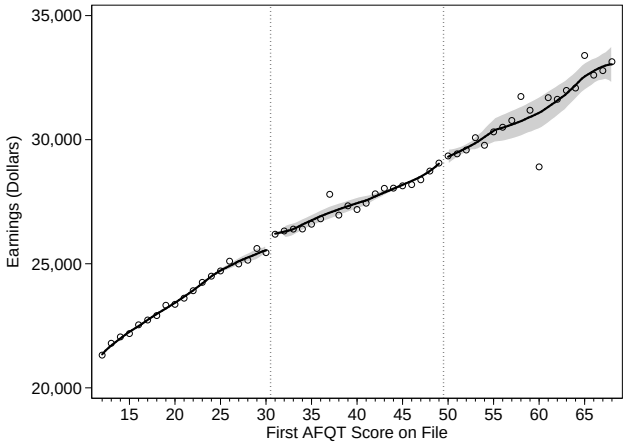
(a) 1 Year Post Application



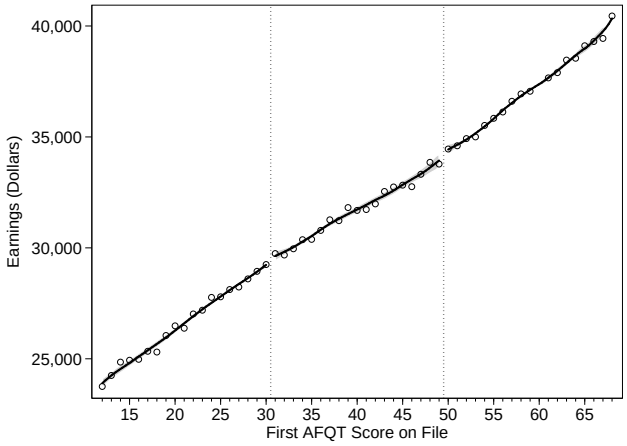
(b) 5 Years Post Application



(c) 10 Years Post Application



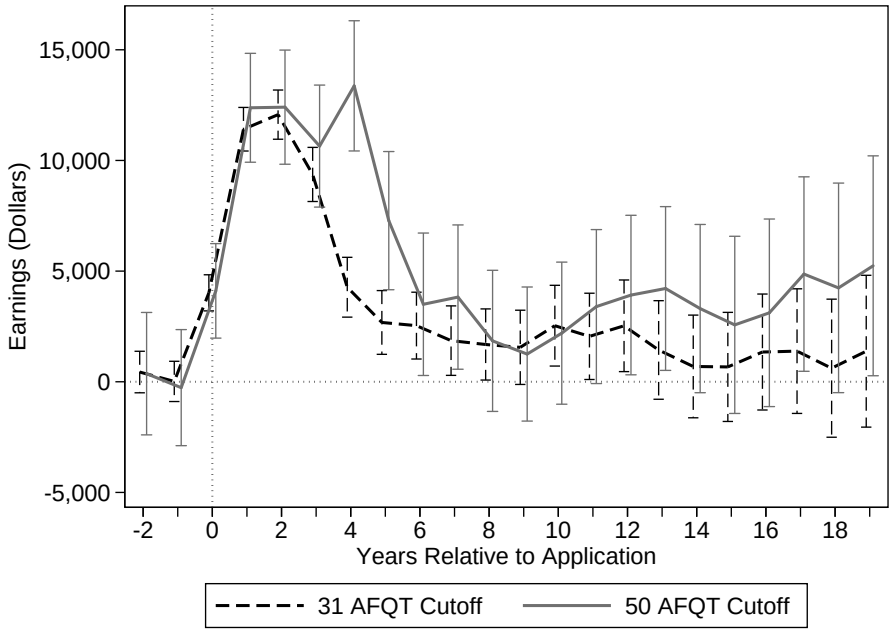
(d) 15 Years Post Application



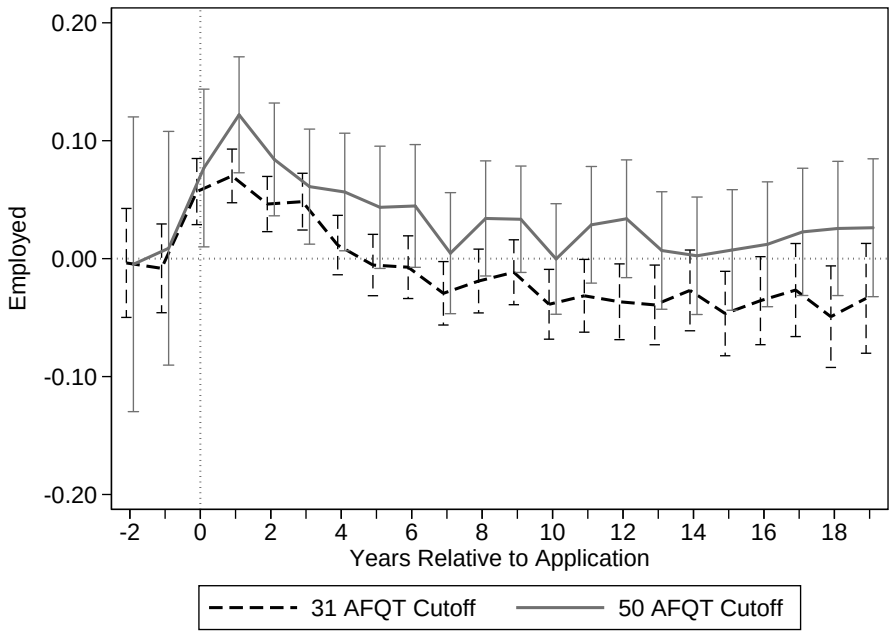
Notes: This figure plots our baseline earnings outcome 1,5,10, and 15 years after application as a function of the earliest AFQT score on file. Appendix Figures A.15–A.16 contain the reduced form plots for *all* years -2 to 19. Figure 4 panel (a) plots corresponding 2SLS RD estimates of the effect of enlistment on earnings for all years -2 to 19 since application.

Figure 4: Effects of Enlistment on Earnings and Employment (2SLS RD Estimates)

(a) Effects of Enlistment on Earnings



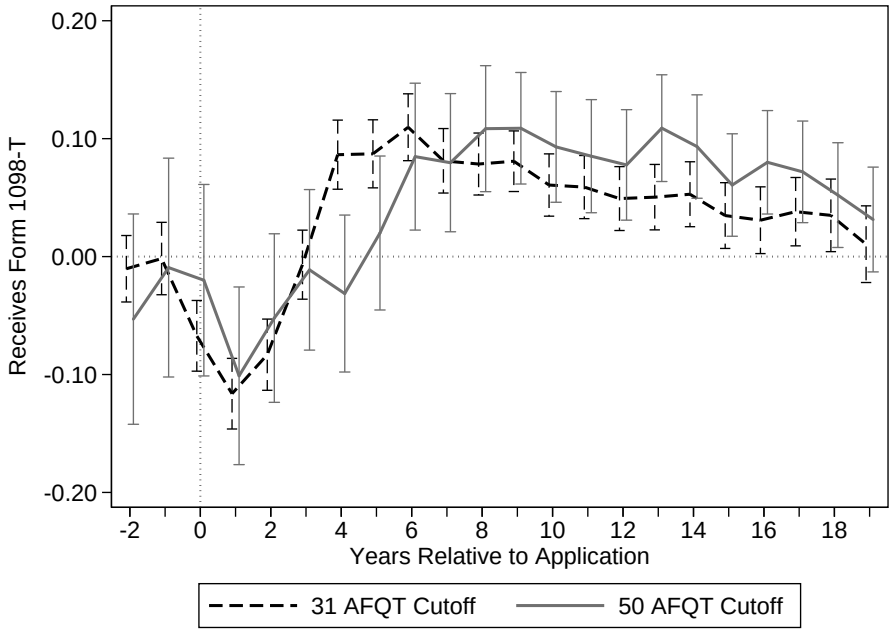
(b) Effects of Enlistment on Employment



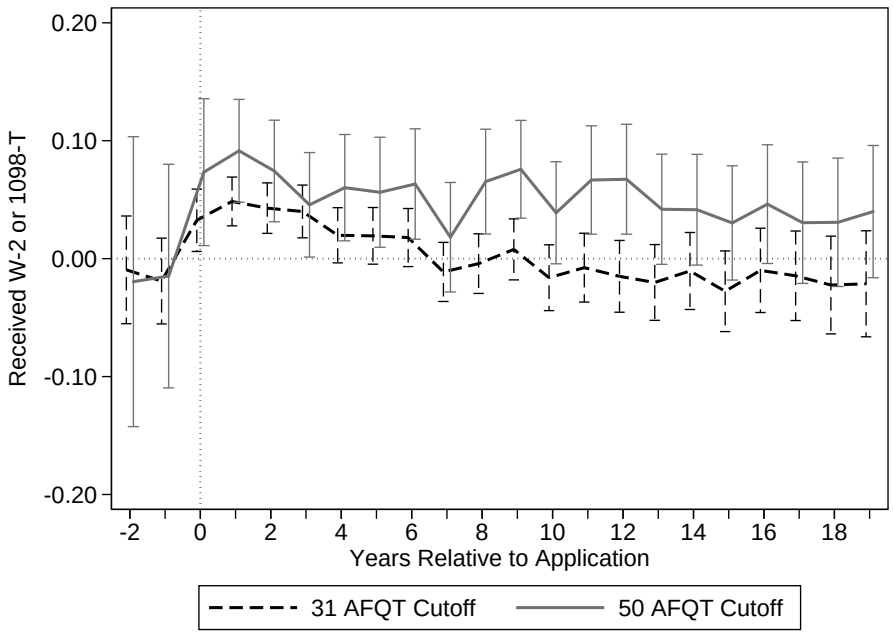
Notes: This figure plots separate 2SLS Regression Discontinuity (RD) estimates of the effect of enlistment on earnings (panel (a)) and employment (panel (b)) at each AFQT cutoff (Equation 3) for every year relative to application, beginning 2 years before application and ending 19 years after application. The dashed black line plots coefficient estimates and 95% confidence intervals for each year at the 31 AFQT cutoff, while the solid gray line does so at the 50 AFQT cutoff. Baseline earnings consist of Medicare W-2 wages plus non-taxable deployment payments and allowances as outlined in Section 3. We assign 0 earnings to those with no W-2 wage earnings. Earnings are adjusted to 2018 dollars and winsorized at the 99th percentile within each year. Employment is defined as having any positive Medicare W-2 earnings in the given year.

Figure 5: Effects of Enlistment on Education and Related Outcomes (2SLS RD Estimates)

(a) Post-Secondary Attendance (Any 1098-T filed)



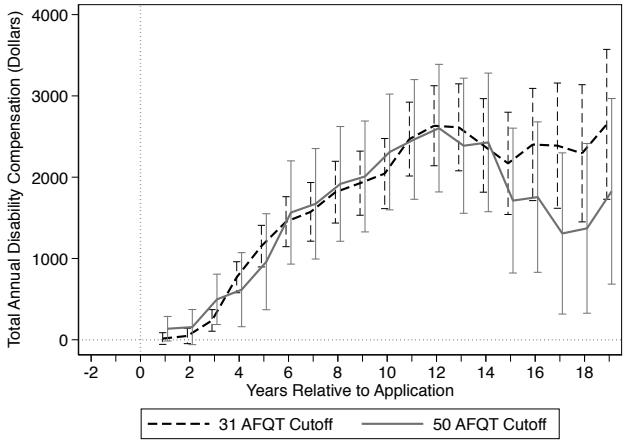
(b) Employed or Attending Post-Secondary



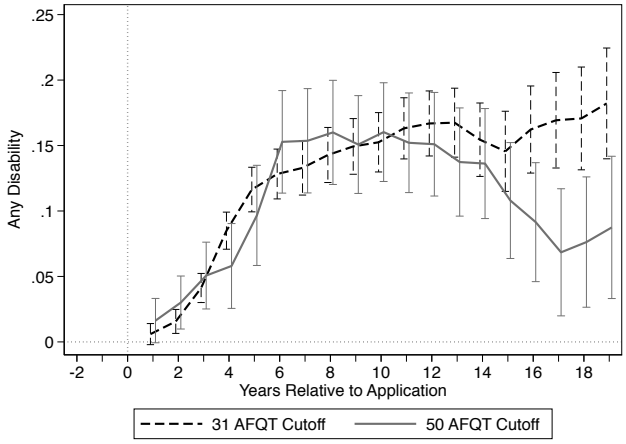
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on post-secondary education enrollment. Panel (a) plots coefficient estimates and 95% confidence intervals for post-secondary attendance in the given year, defined as having a 1098-T on record. Panel (b) plots estimates for an indicator that equals one if the person is employed (any positive Medicare wages on W-2) or attending post-secondary school (any 1098-T) in the given year. See the notes to Figure 4 for additional details.

Figure 6: Effects of Enlistment on Disability Compensation and Receipt (2SLS RD Estimates)

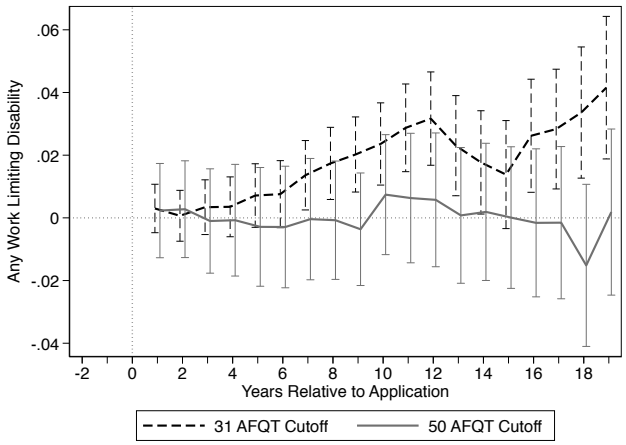
(a) Total Disability Comp. (VADC+SSI+SSDI)



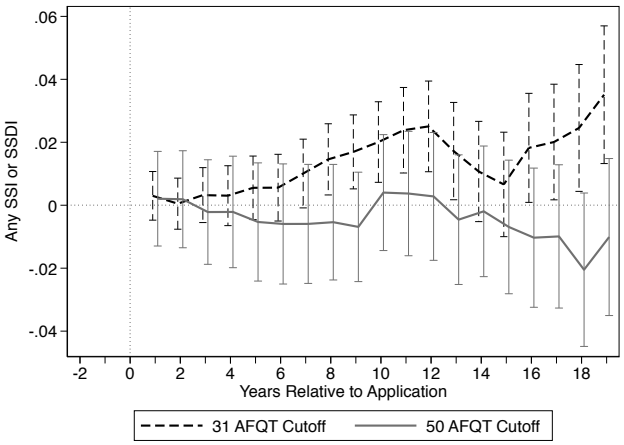
(b) Any Disability Comp. (any VADC, SSI, or SSDI)



(c) Any Work-Limiting Disability

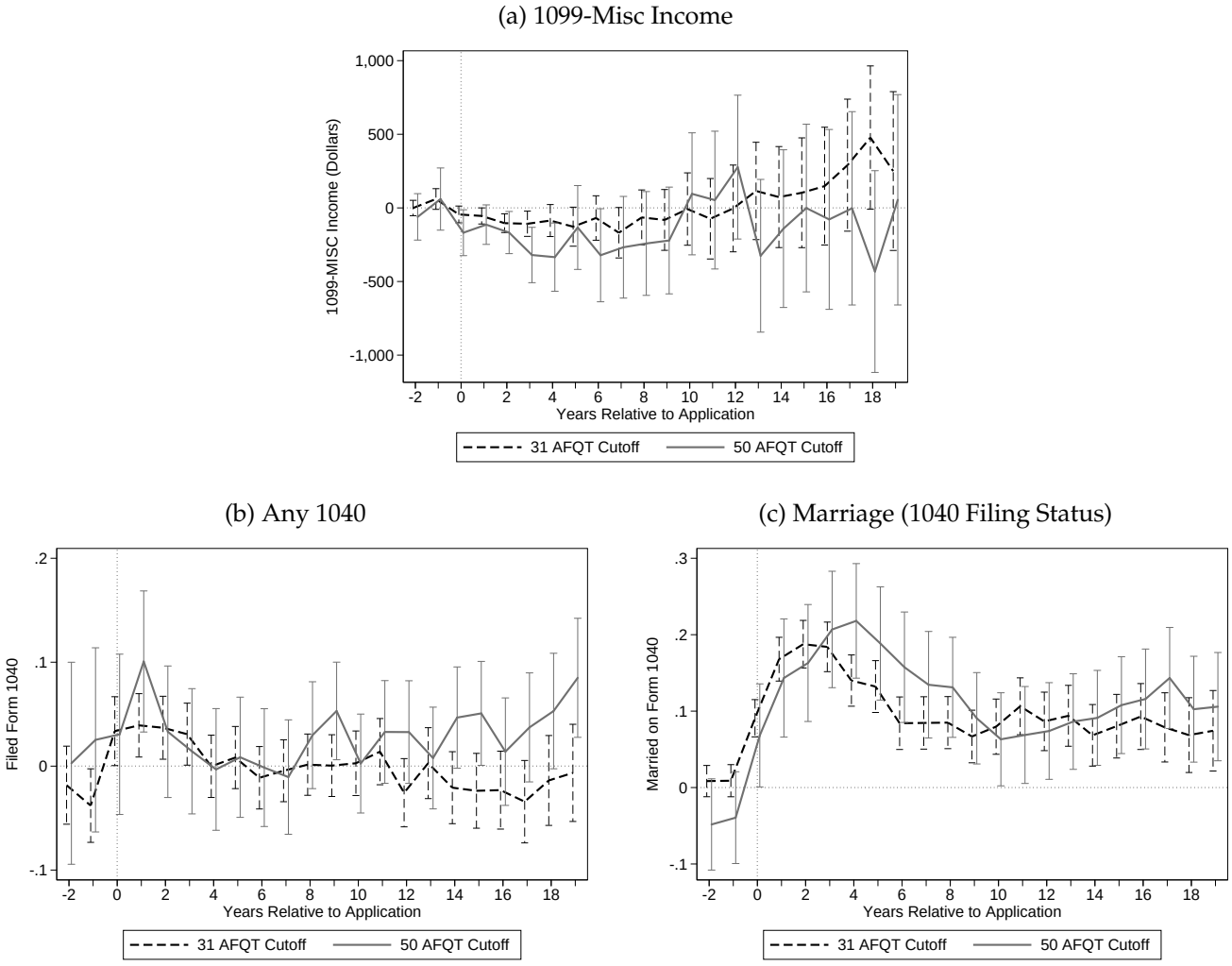


(d) Any SSI or SSDI



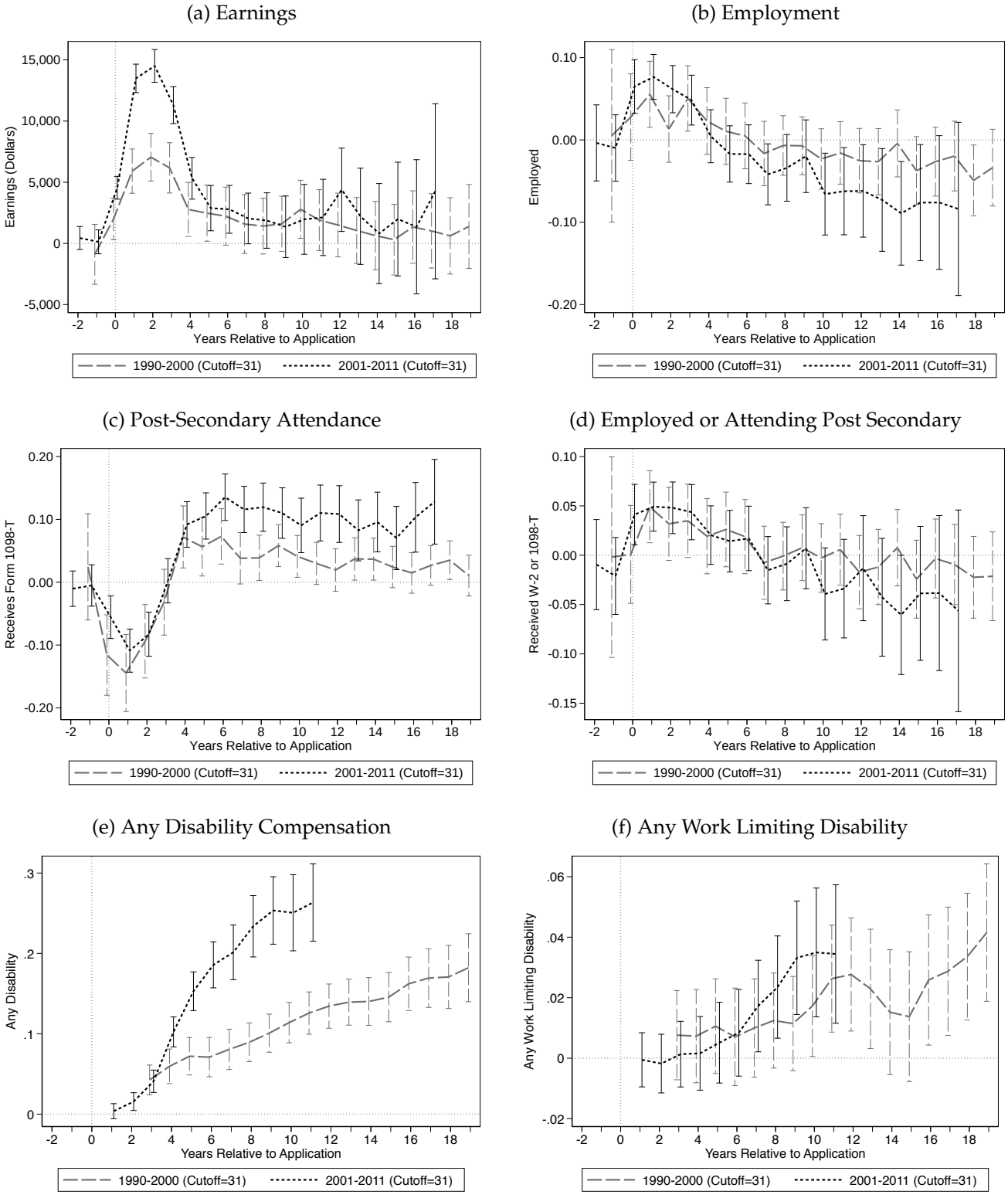
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on disability compensation and receipt. Panel (a) plots coefficient estimates and 95% confidence intervals for total annual disability compensation (2018 dollars), defined as the sum of Veterans Affairs Disability Compensation (VADC), Social Security Disability Insurance (SSDI), and Supplemental Security Income (SSI). We have all these data for the years 1999-2015. Panel (b) plots 2SLS RD estimates where the outcome is an indicator for receiving any of the above types of disability. Panel (c) plots estimates where the outcome is an indicator for receiving work-limiting disability, defined as any SSI, SSDI, or having a Veterans Affairs' Individual Unemployability (VADC-IU) designation. Panel (d) plots estimates where the outcome is an indicator for receiving any SSI or SSDI. See the notes to Figure 4 for additional details.

Figure 7: Effects of Enlistment on Other Outcomes (2SLS RD Estimates)



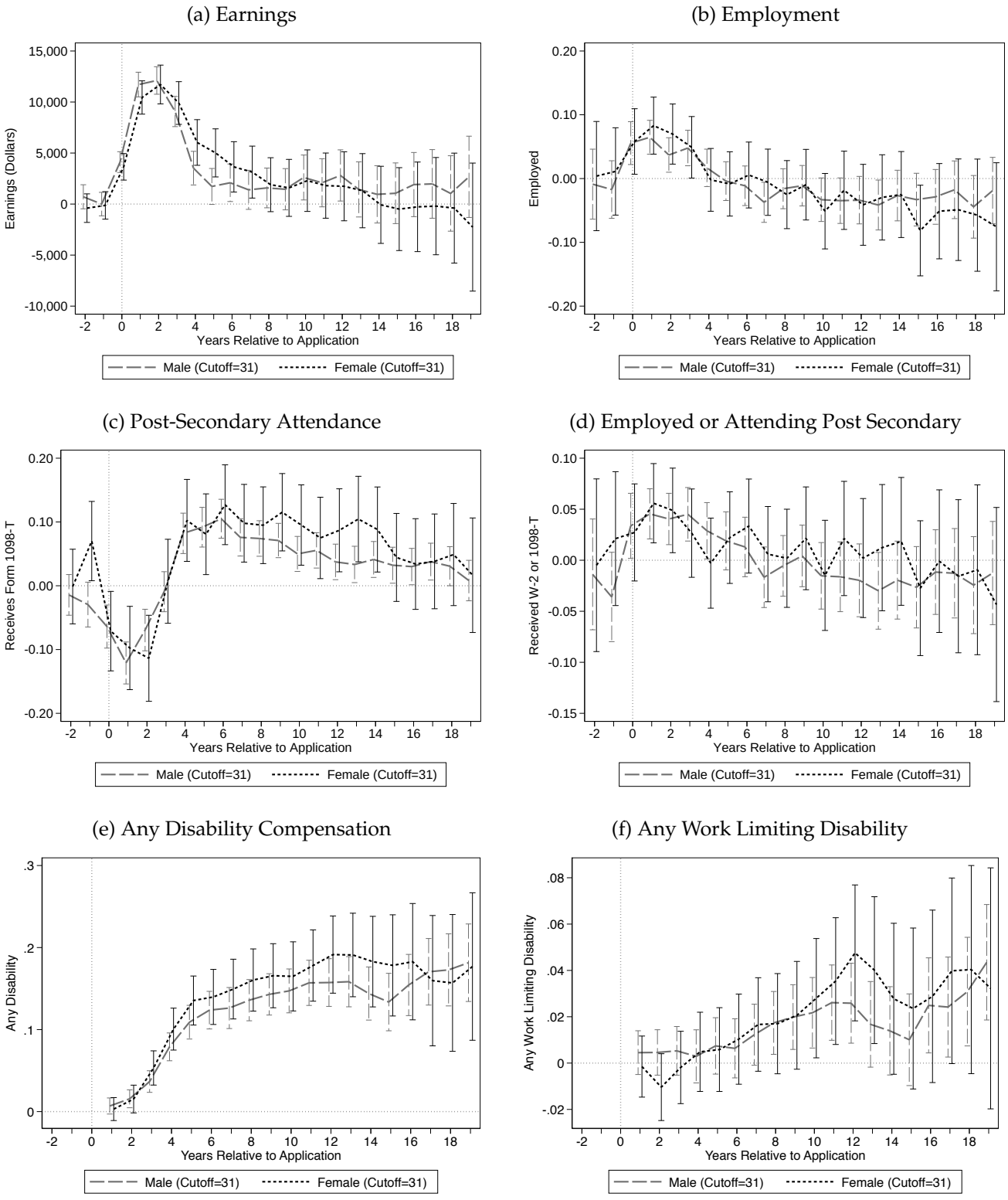
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on total miscellaneous income, filing a 1040, and marriage. Panel (a) shows estimates of the effect of enlistment on total 1099-MISC income from information-returns in $2018 (individuals without a 1099-MISC have this set to 0). Panel (b) shows estimates of the effect of enlistment on 1040 filing at each cutoff. Panel (c) shows estimates of the effect of enlistment on marriage, defined as being in a married filing status on your 1040 in the given year. This is equal to 0 for non-filers. See the notes to Figure 4 for additional details.

Figure 8: Heterogeneity by Application Cohort (31 AFQT Cutoff)



Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by application cohort. Throughout, we compare estimates for the 1990-2000 application cohorts (the dashed gray line) to those for the 2001-2011 cohorts (the dotted black line) at the 31 AFQT cutoff. Appendix Figure A.10 contains the plots at the 50 cutoff. Panel (a) compares earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

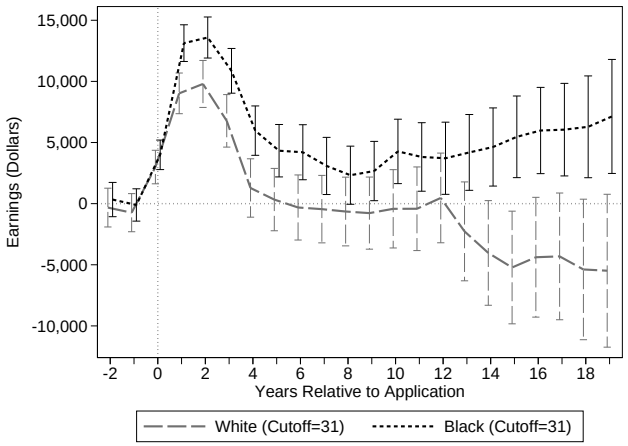
Figure 9: Heterogeneity by Gender (31 AFQT Cutoff)



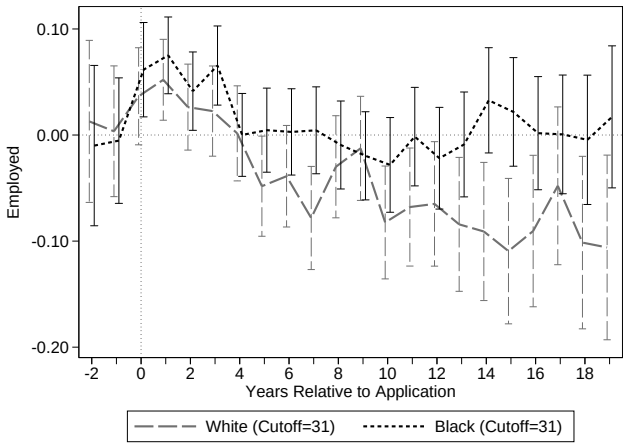
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by gender. Throughout, we compare estimates for men (the dashed gray line) to those for women (the dotted black line) at the 31 AFQT cutoff. Appendix Figure A.11 contains the plots at the 50 cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

Figure 10: Heterogeneity by Race (31 AFQT Cutoff)

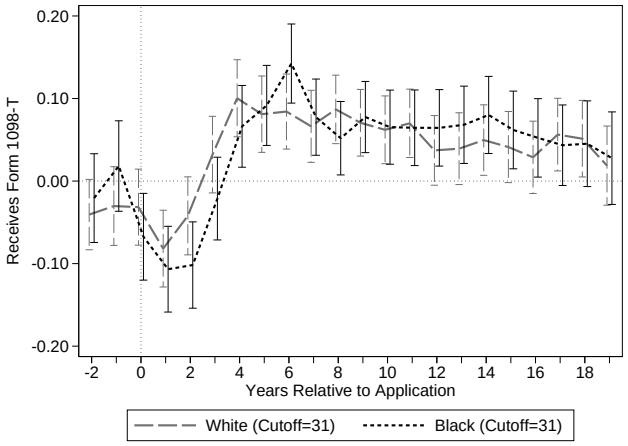
(a) Earnings



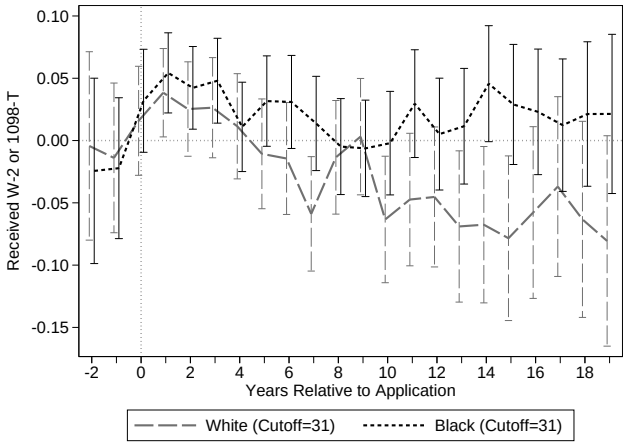
(b) Employment



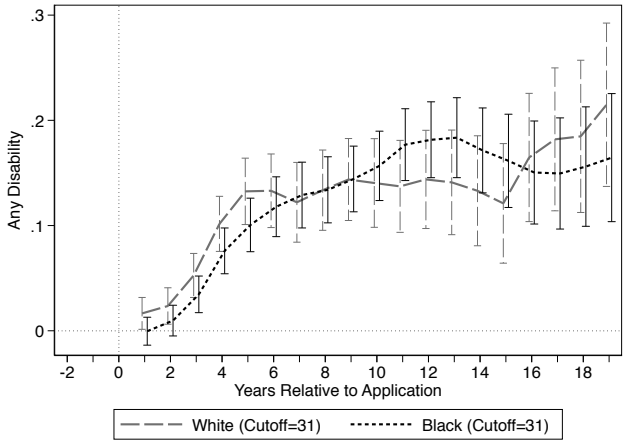
(c) Post-Secondary Attendance



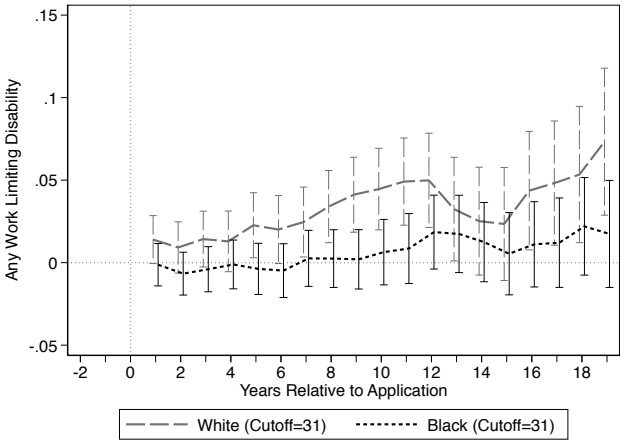
(d) Employed or Attending Post Secondary



(e) Any Disability Compensation



(f) Any Work Limiting Disability



Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by race. Throughout, we compare estimates for white applicants (the dashed grey line) to those for black applicants (the dotted black line) at the 31 AFQT cutoff. Appendix Figure A.12 contains the plots at the 50 cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

Tables

Table 1: Summary Statistics

	All Applicants	Analysis Sample	Enlisted (Analysis Sample)	Did Not Enlist (Analysis Sample)
	(1)	**(2)**	**(3)**	**(4)**
	Panel (a): Means At Time of Application			
Enlisted	0.483	0.465	1.000	0.000
Years Served	2.326	2.243	4.826	0.000
Age	20.693	20.508	20.213	20.764
First AFQT on File	52.002	42.028	46.462	38.179
Male	0.779	0.760	0.809	0.717
White (Non-Hispanic)	0.604	0.548	0.580	0.521
Black (Non-Hispanic)	0.212	0.256	0.231	0.278
Hispanic	0.108	0.123	0.123	0.123
No High School Diploma	0.142	0.166	0.149	0.180
High School Diploma	0.536	0.531	0.559	0.506
Some College+	0.070	0.042	0.045	0.039
Still In High School	0.251	0.262	0.247	0.274
N	2,594,896	1,775,108	824,897	950,211

	Panel (b): County-Level Comparison to U.S. Population (All Applicants)				
	Median H.H. Income	Poverty Rate	Single Parent	Employment Rate*	Inter-Gen Mobility
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Bottom Tercile (U.S.)	0.370	0.249	0.293	0.364	0.361
Middle Tercile (U.S.)	0.342	0.371	0.371	0.330	0.330
Top Tercile (U.S.)	0.263	0.355	0.310	0.282	0.265
Missing County Data	0.025	0.025	0.026	0.024	0.044
N	2,594,989	2,594,989	2,594,989	2,594,989	2,594,989

Notes: Panel (a) summarizes covariate means from Army applicant data (MEPCOM data) at the time of first application. Column (1) describes characteristics for all applicants (AFQT scores between 1 and 99) from 1990-2011, where Columns (2), (3), and (4) report characteristics from our analysis sample: those with earliest AFQT scores on record between 12 and 68. The education categories are mutually exclusive: still in High School refers to those still enrolled in high school at the time of application; No High School Diploma refers to those no longer in high school with a GED, credential near completion, or less than high school completion; High School Diploma refers those who have completed a high school diploma but not attended college; Some College+ includes any one who has attended at least one semester of college. Panel (B) links application county to 1990 (and in the case of employment 2000) county-level data. We compare the all applicant sample's counties to those of the U.S. by constructing representative, population weighted U.S. terciles of 1990 median household income, poverty rate, single parent household rate, employment rate (2000), and inter-generational mobility rates from Chetty et al. (2018).

Table 2: 2SLS and OLS Earnings Estimates By Years Since Application

	1 Year Before	1 Year After	3 Year After	5 Year After	7 Year After	9 Year After	11 Year After	13 Year After	15 Year After	17 Year After	19 Year After
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
					Panel (a): 2SLS 31 AFQT Cutoff						
Enlist (2SLS)	17	11,411***	9,368***	2,681***	1,860**	1,556*	2,052**	1,437	671	1,385	1,381
	(464)	(502)	(624)	(735)	(801)	(856)	(994)	(1,136)	(1,257)	(1,437)	(1,750)
					Panel (b): OLS 31 AFQT Cutoff						
Enlist (OLS)	4	7,861***	11,787***	8,948***	7,723***	7,161***	7,576***	8,050***	8,608***	9,001***	9,499***
	(29)	(30)	(36)	(41)	(45)	(48)	(55)	(63)	(70)	(79)	(92)
Number of Observations	555,286	671,070	793,037	894,892	1,000,427	1,064,711	969,081	882,000	800,809	699,583	582,309
Dep. Var. Mean	7,722	14,683	20,053	21,689	23,724	25,362	26,762	28,292	29,767	30,954	32,110
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
					Panel (c): 2SLS 50 AFQT Cutoff						
Enlist (2SLS)	-264	12,380***	10,649***	7,281***	3,827**	1,253	3,397*	4,215**	2,572	4,867**	5,240**
	(1,338)	(1,255)	(1,406)	(1,593)	(1,663)	(1,545)	(1,775)	(1,888)	(2,042)	(2,241)	(2,535)
					Panel (d): OLS 50 AFQT Cutoff						
Enlist (OLS)	53**	8,702***	11,895***	8,361***	6,751***	6,181***	6,680***	7,249***	7,913***	8,400***	9,021***
	(26)	(26)	(32)	(37)	(40)	(44)	(50)	(57)	(65)	(74)	(85)
Number of Observations	658,666	787,748	925,594	1,037,158	1,155,868	1,222,565	1,109,460	1,013,570	918,715	787,209	652,445
Dep. Var. Mean	8,438	16,884	22,891	24,156	26,227	28,259	30,220	32,275	34,217	35,835	37,442

Notes: This table reports 2SLS RD and OLS estimates of the effect of enlistment on earnings. For display purposes, we omit estimates in even years since application. Panel (a) shows 2SLS estimates at the 31 cutoff (which correspond to points on the dashed black line in Figure 4 panel (a)). Panel (b) reports OLS estimates of equation 3, where the outcome is earnings and $f(AFQT_i)$ is replaced with AFQT fixed effects, from the sample of applicants in the RD window around the 31 cutoff. Panel (c) shows the 2SLS estimates at the 50 cutoff (which correspond to to points on the solid gray line in Figure 4 panel (a)). Panel (d) reports OLS estimates of equation 3, where the outcome is earnings and $f(AFQT_i)$ is replaced with AFQT fixed effects, from the sample of applicants in the RD window around the 50 cutoff. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 3: Cumulative 2SLS RD Estimates

	(1)	(2)	(3)	**(4)**	(5)	(6)	(7)	**(8)**
	31 AFQT Cutoff				50 AFQT Cutoff			
Relative Years	0-5	6-10	11-15	**0-15**	0-5	6-10	11-15	**0-15**
				Panel (a): Cumulative Earnings				
Enlist	48,009***	13,978***	7,342	**73,573*****	75,414***	12,152	16,737**	**121,936*****
	(3,045)	(4,288)	(5,207)	**(16,239)**	(7,961)	(8,080)	(8,507)	**(33,113)**
Dep. Var. Mean	105,107	119,153	140,651	**348,552**	120,112	132,761	160,334	**397,169**
				Panel (b): Cumulative Years of Employment				
Enlist	0.246***	-0.075	-0.172***	**-0.029**	0.578***	0.074	0.068	**0.587**
	(0.05)	(0.06)	(0.07)	**(0.23)**	(0.12)	(0.11)	(0.10)	**(0.41)**
Dep. Var. Mean	5.3	4.2	4.0	**13.4**	5.4	4.2	4.0	**13.6**
				Panel (c): Attended Any Post-Secondary in Given Time Window				
Enlist	0.011	0.131***	0.088***	**0.065****	-0.011	0.137***	0.197***	**0.170*****
	(0.02)	(0.02)	(0.02)	**(0.03)**	(0.06)	(0.04)	(0.03)	**(0.06)**
Dep. Var. Mean	0.438	0.343	0.276	**0.627**	0.486	0.407	0.324	**0.695**
Application Cohorts	99-11	93-08	90-03	**99-03**	99-11	93-08	90-03	**99-03**
Individuals	612,247	827,941	838,698	**275,461**	721,660	950,220	966,232	**329,264**

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *cumulative* earnings, employment and educational attendance. Columns (1)-(4) estimate cumulative effects at the 31 AFQT cutoff, while columns (5)-(8) do so at the 50 cutoff. Each column looks at cumulative outcomes over a different time horizon: 0-5 years since application, 6-10 years since, 11-15 years since, and finally 0-15 years since. In each column, we restrict to the largest possible subsample of application cohorts for whom we have data spanning the relevant time horizon. As an alternative, Appendix Table A.4 restricts to the 1999-2003 cohorts in all columns as these are the cohorts we observe in all years 0-15 following application. We estimate the effect of enlistment on total earnings in panel (a), on total years of employment in panel (b), and on ever attending a post-secondary institution within the given time window in panel (c). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: 2SLS RD Cumulative Mortality Estimates By Years Since Application

	Died w/in 1 Year **(1)**	Died w/in 3 Years **(2)**	Died w/in 5 Years **(3)**	Died w/in 10 Years **(4)**	Died w/in 15 Years **(5)**
	Panel (a): 31 AFQT Cutoff				
Enlist	-0.00158	-0.00590***	-0.00202	-0.00297	0.00448
	(0.00131)	(0.00214)	(0.00272)	(0.00443)	(0.00612)
Number of Observations	1,137,580	1,137,580	1,137,580	1,016,628	800,795
Dep. Var. Mean	0.00131	0.00336	0.00566	0.01245	0.01858
	Panel (b): 50 AFQT Cutoff				
Enlist	-0.00012	0.00573	0.00435	-0.00480	-0.00883
	(0.00257)	(0.00411)	(0.00519)	(0.00739)	(0.00898)
Number of Observations	1,311,097	1,311,097	1,311,097	1,163,935	918,701
Dep. Var. Mean	0.00132	0.00352	0.00595	0.01285	0.01890

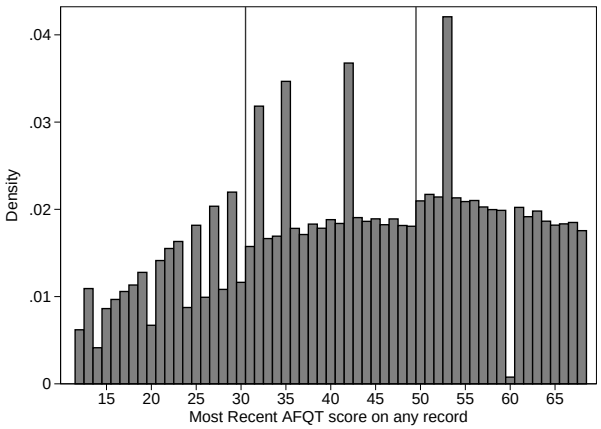
Notes: This table reports 2SLS RD estimates of enlistment on cumulative mortality. The IRS stores death dates (from the SSA Death Master File) and hence no additional matching beyond that described in Section 3 is required. Less than 20 applicants have death dates prior to application and we drop these. Our outcome, an indicator for died within x years after application, equals 1 if the relevant tax year is greater than or equal to the applicant's death year. Panel (a) shows 2SLS RD estimates at the 31 cutoff while Panel (b) shows 2SLS RD estimates at the 50 cutoff. Columns (1)-(5) show the effect of enlistment on deaths within 1, 3, 5, 10, and 15 years respectively. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Online Appendix

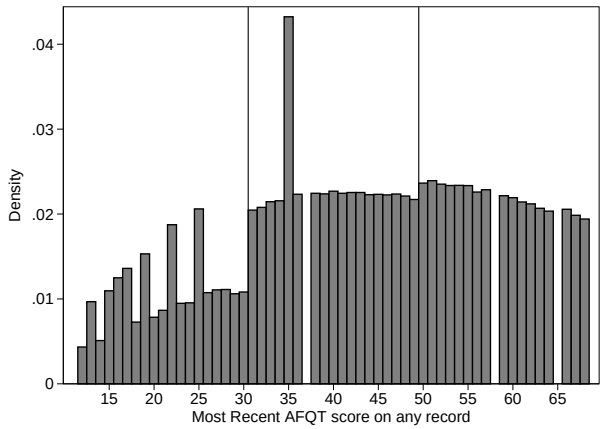
A Appendix Figures

Figure A.1: Density of *Final* AFQT Scores

(a) Last AFQT Density, pre-2004 renorming



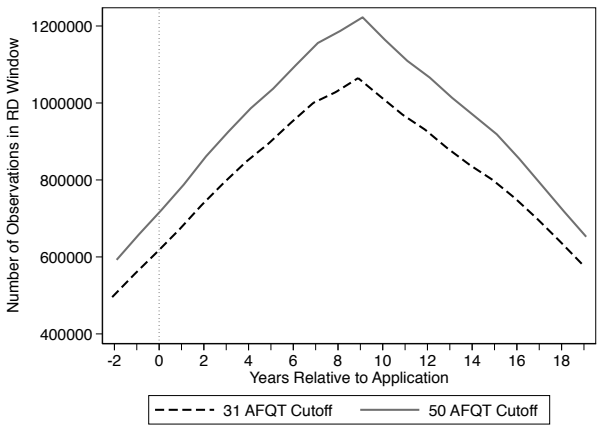
(b) Last AFQT Density, post-2004 renorming



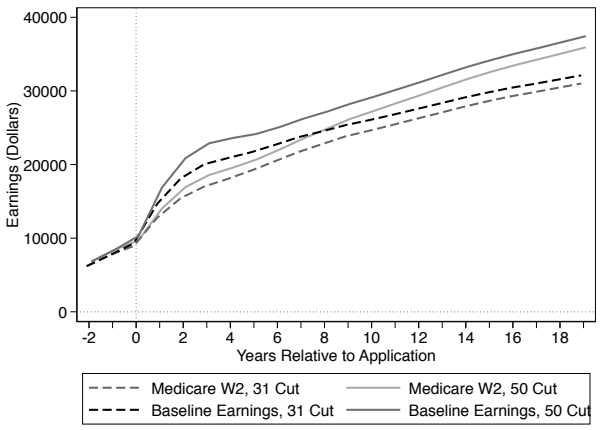
Notes: Panels (a) and (b) show the distribution of final AFQT scores on record before and after the April 2004 ASVAB re-norming, respectively. In contrast to Figure 2, the distribution of final AFQT scores exhibits bunching at both cutoffs.

Figure A.2: Sample Size and Mean Outcomes By Years Since Application

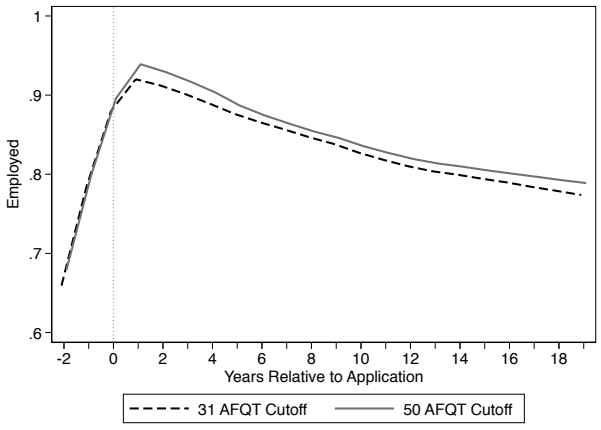
(a) Sample Size



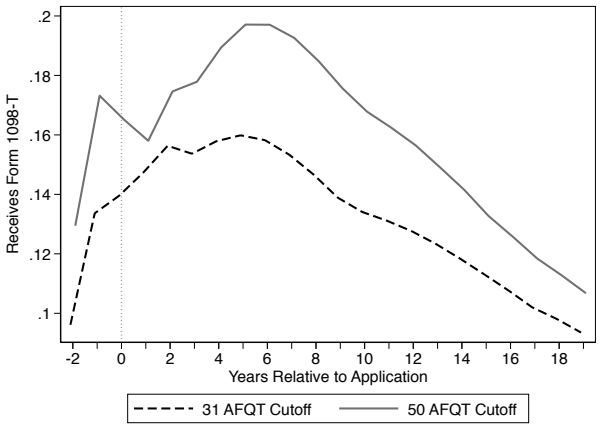
(b) Mean Earnings



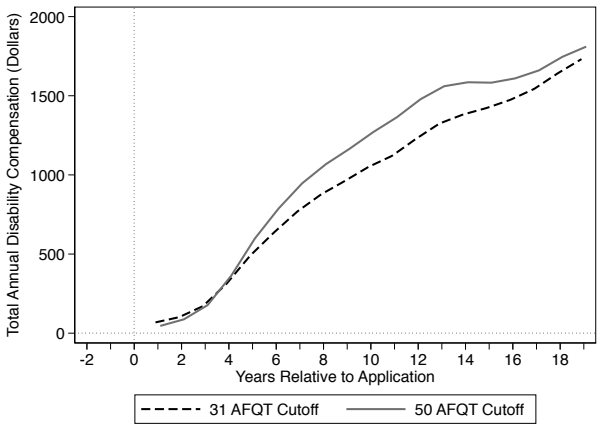
(c) Mean Employment



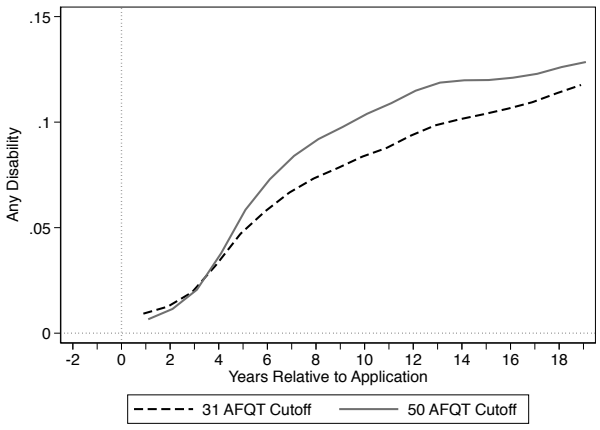
(d) Mean Post-Secondary Attendance



(e) Mean Disability Compensation

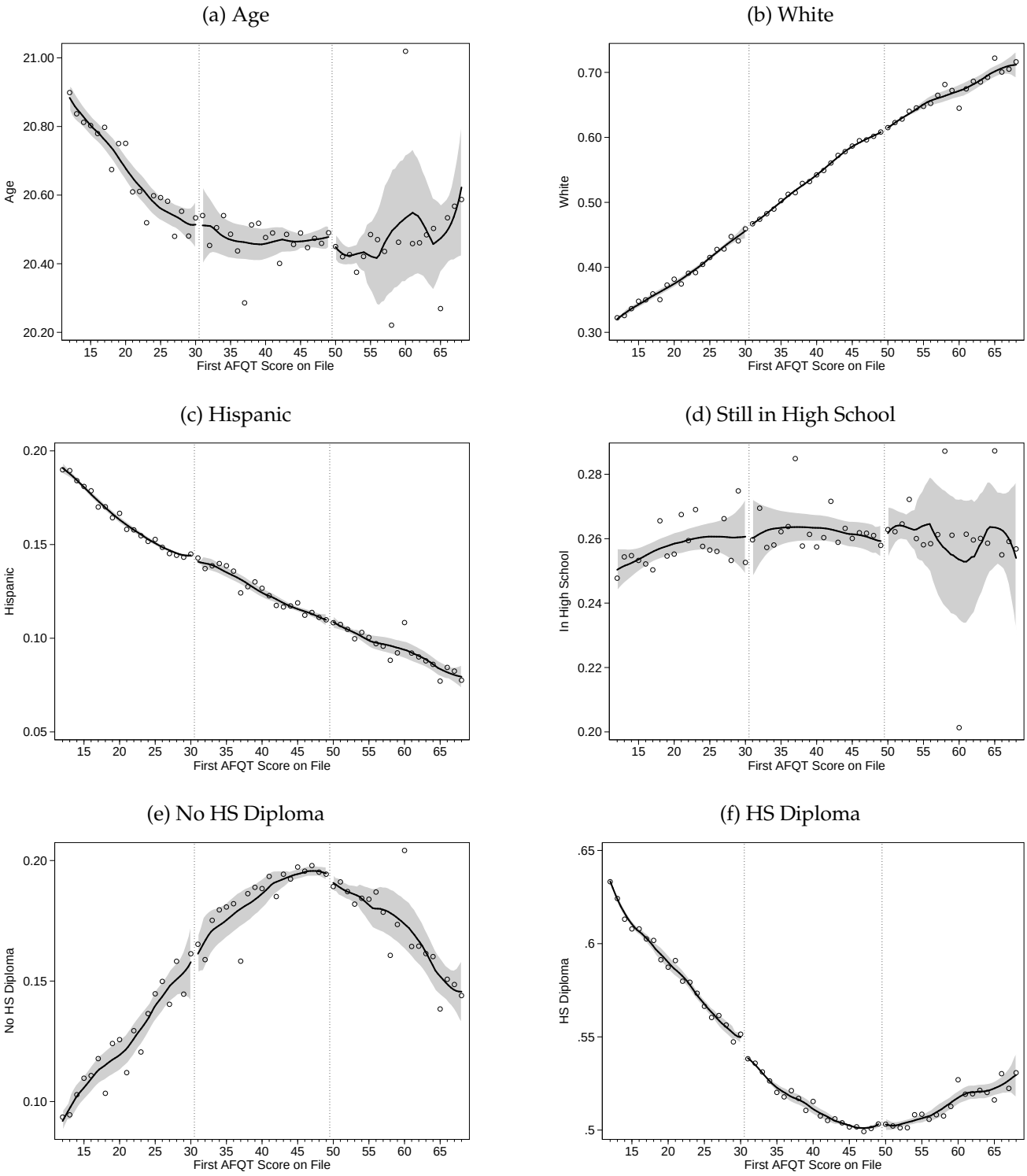


(f) Mean Disability Receipt



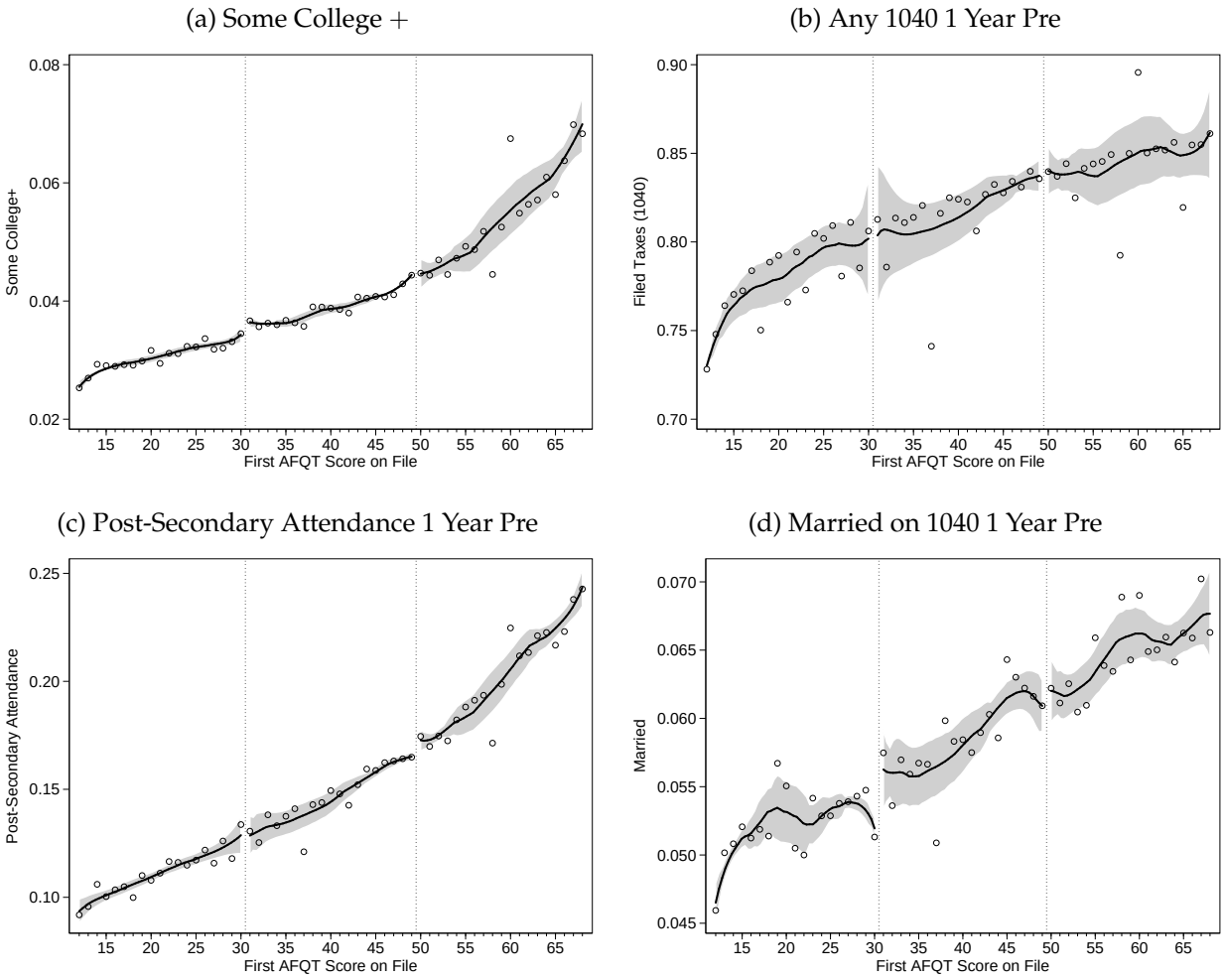
Notes: These figures plot our sample size, mean earnings (medicare W-2 + non-taxable bonuses and allowances in 2018 dollars), mean employment (any W-2), mean post-secondary attendance (any Form 1098-T), mean disability compensation (VADC+SSI+SSDI in 2018 dollars), and mean disability receipt (any VADC, SSI, or SSDI) by years since application. In panel (b) when we show mean earnings, we also show raw Medicare W-2 earnings in order to facilitate a comparison between this and our baseline earnings measure.

Figure A.3: Additional Covariate Balance Plots



Notes: Figures A.3– A.4 (along with Figure 2 panels (c)-(f)) plot the reduced form relationship between first AFQT on file and all the covariates/pre-application outcomes in Table A.2.

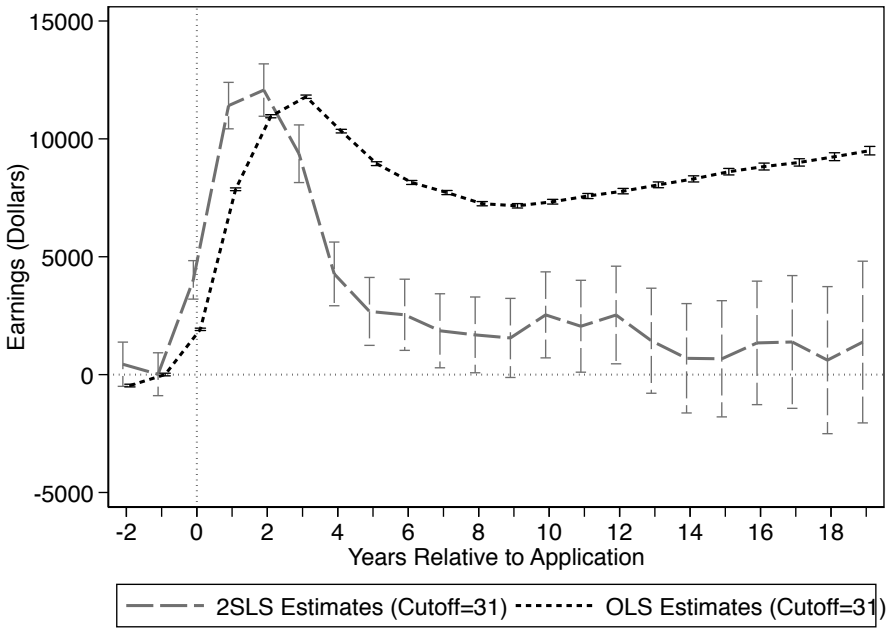
Figure A.4: Additional Covariate Balance Plots (Continued)



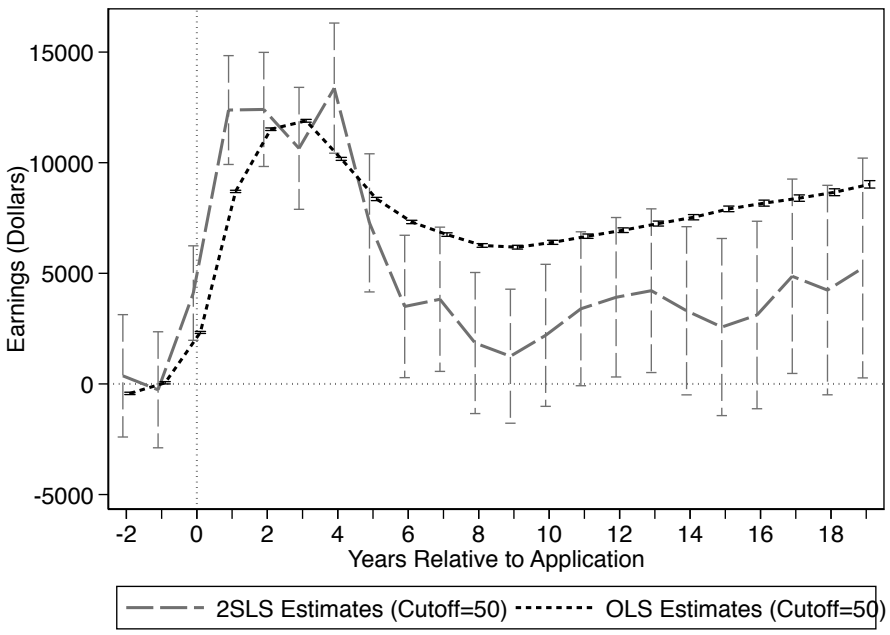
Notes: Figures A.3– A.4 (along with Figure 2 panels (c)-(f)) plot the reduced form relationship between first AFQT on file and all the covariates/pre-application outcomes in Table A.2.

Figure A.5: Comparison of 2SLS RD and OLS Estimates of Enlistment on Earnings

(a) 31 AFQT Cutoff

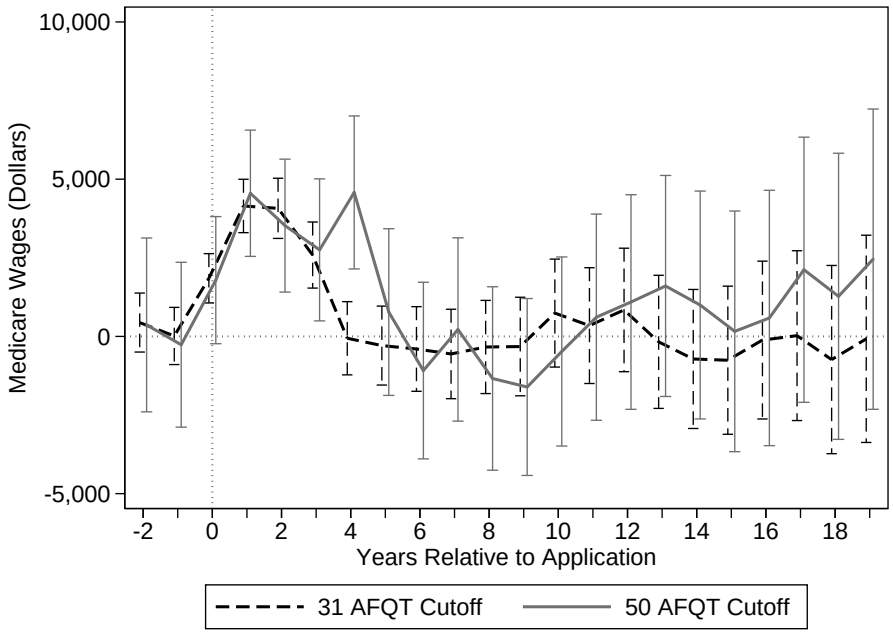


(b) 50 AFQT Cutoff



Notes: This figure compares OLS estimates of the effects of enlistment on earnings to our 2SLS RD estimates. The OLS estimates are constructed within the same RD window (and hence have the same sample size) and include fixed effects for every possible first AFQT score (in addition to fixed effects for years of application). Panel (a) compares OLS to 2SLS RD estimates in the window around the 31 cutoff. The dashed grey line plots the 2SLS coefficient estimates while the dotted black line plots the OLS estimates. 95% confidence intervals are shown. Panel (b) does the same around the 50 cutoff.

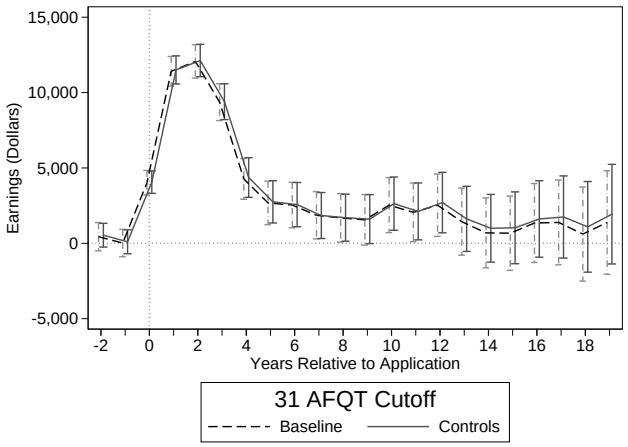
Figure A.6: Effects of Enlistment on Raw Medicare W-2 Earnings (2SLS RD Estimates)



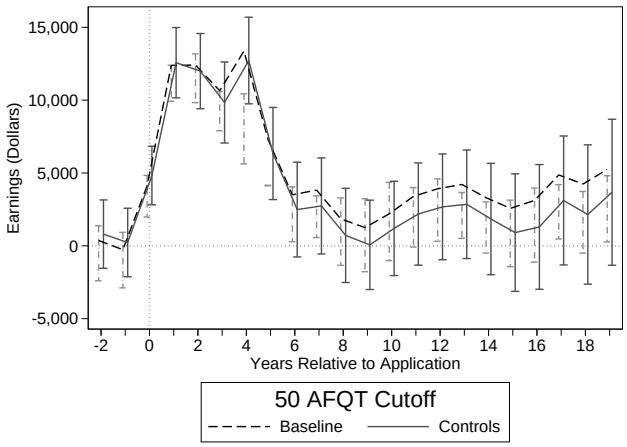
Notes: This figure plots 2SLS RD estimates of Equation 3 on raw Medicare W-2 earnings in years -2 to 19 after application. The dashed black line plots coefficient estimates and 95% confidence intervals for each year around the 31 AFQT cutoff, while the solid gray line does so around the 50 AFQT cutoff.

Figure A.7: Robustness of Earnings Estimates

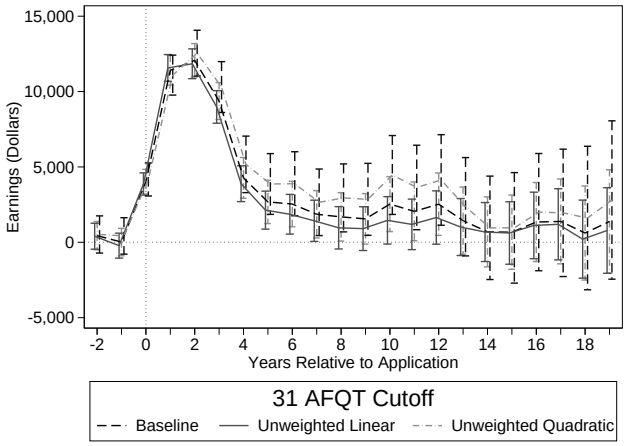
(a) Inclusion of Controls, 31 Cutoff



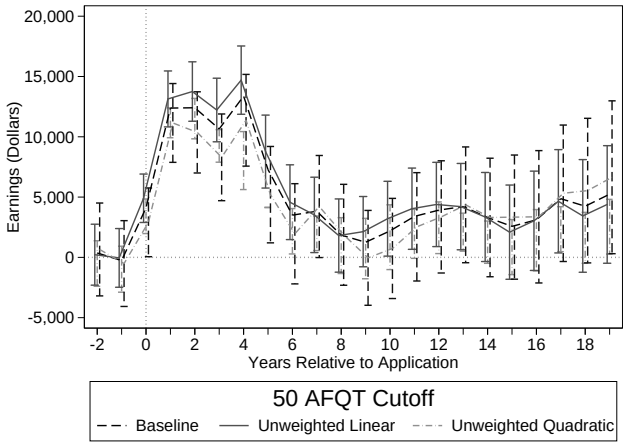
(b) Inclusion of Controls, 50 Cutoff



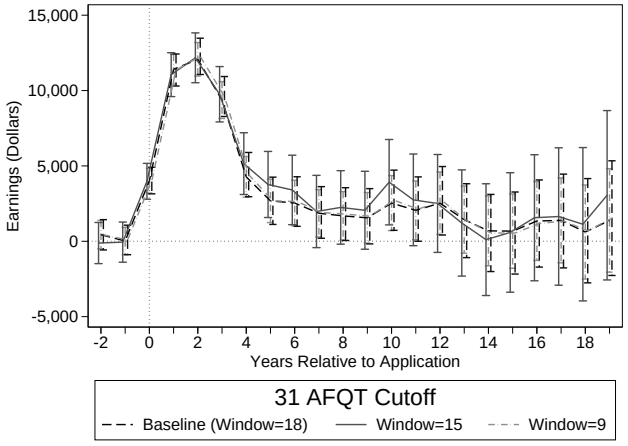
(c) Alternate Specifications, 31 Cutoff



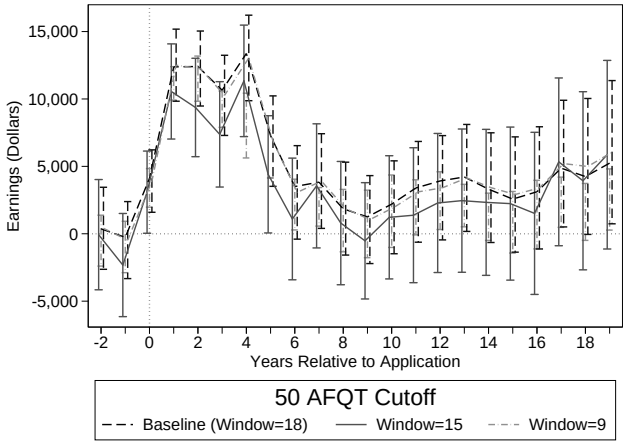
(d) Alternate Specifications, 50 Cutoff



(e) Alternate Bandwidth, 31 Cutoff



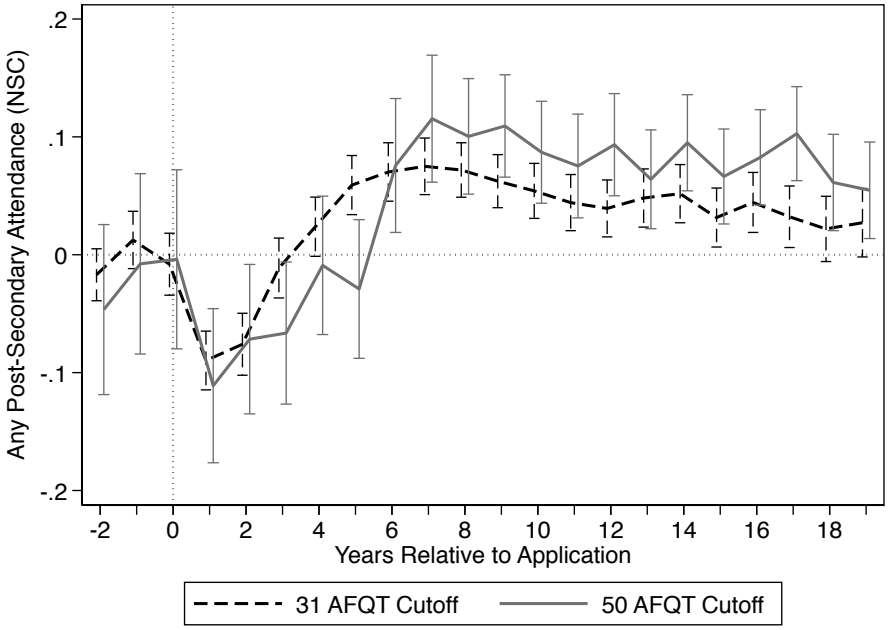
(f) Alternate Bandwidth, 50 Cutoff



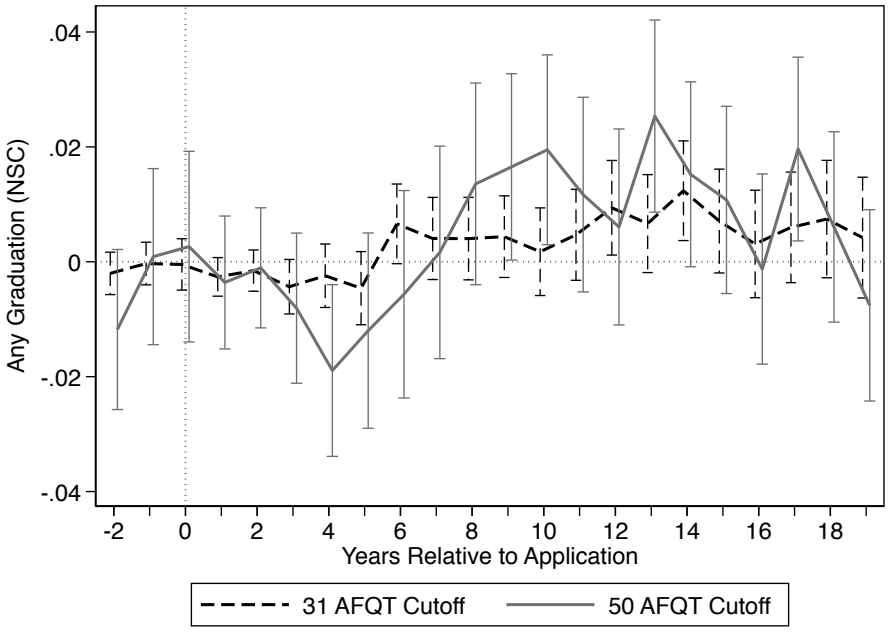
Notes: This figure demonstrates robustness of the earnings estimates in Figure 4 panel (a) to the inclusion of controls (in panels (a) and (b)), to a local linear (no kernel) specification and local quadratic (no kernel) specification (in panels (c) and (d)), and to smaller bandwidths (in panels (e) and (f)).

Figure A.8: Effects of Enlistment on Educational Attendance and Graduation (NSC)

(a) Post-Secondary Attendance (NSC)

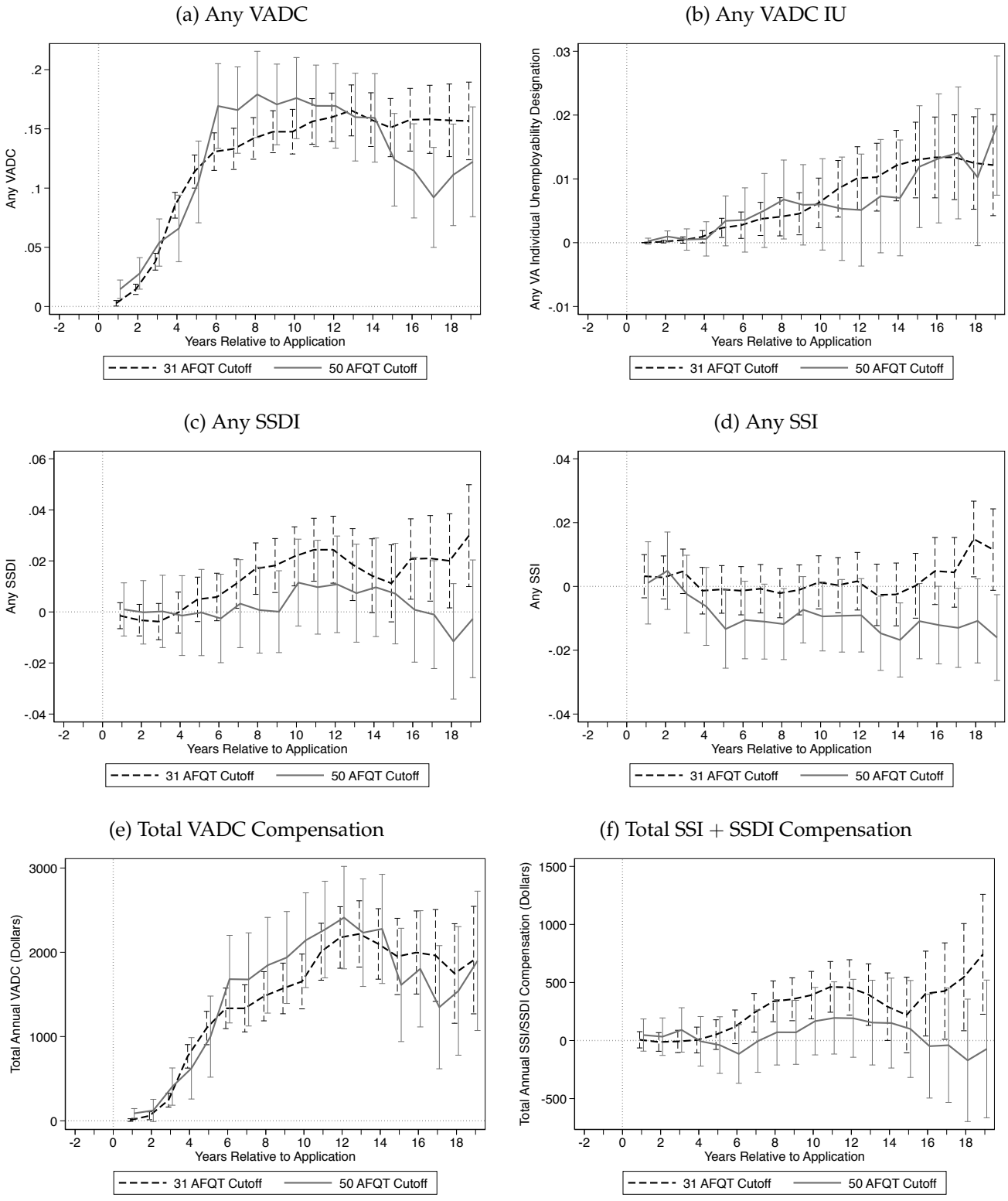


(b) Associates Degree or Higher Obtained (NSC)



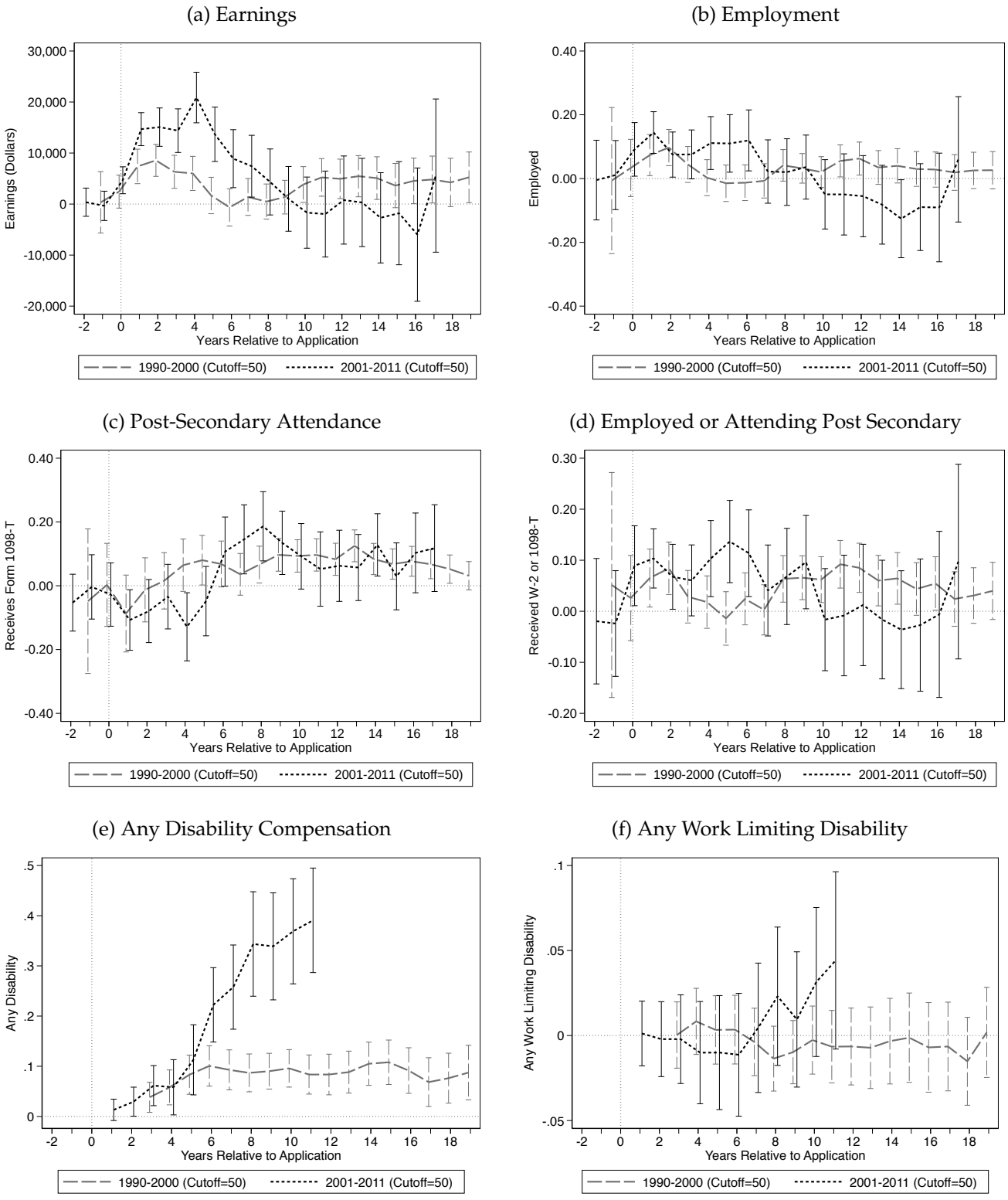
Notes: This figure reports 2SLS RD estimates of enlistment on post-secondary attendance and graduation (in the given year), where outcomes are derived from National Student Clearinghouse (NSC) data from 1999 forward. Table A.5 displays estimates of the effect on enlistment on cumulative educational outcomes using NSC data.

Figure A.9: Effects of Enlistment on Disability Receipt and Compensation By Type



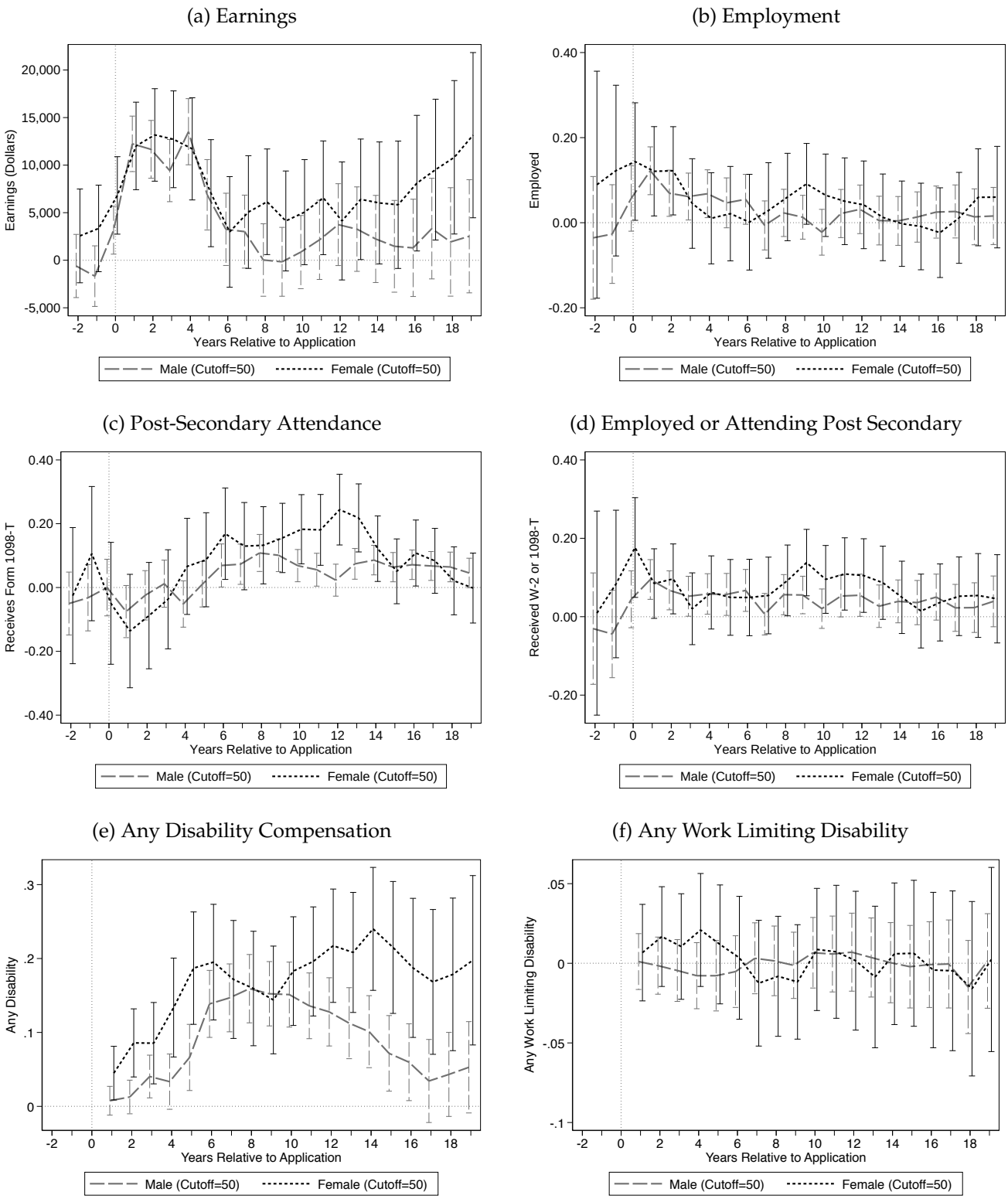
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on receiving any VA Disability Compensation in panel (a), on any VA Individual Unemployability (VADC IU) designation in panel (b), on any SSDI in panel (c), on any SSI in panel (d), on total VADC compensation in panel (e), and on total SSI + SSDI compensation in panel (f). The dashed black line plots coefficient estimates and 95% confidence intervals for each year around the 31 AFQT cutoff, while the solid gray line does so around the 50 AFQT cutoff.

Figure A.10: Heterogeneity by Application Cohort (50 AFQT Cutoff)



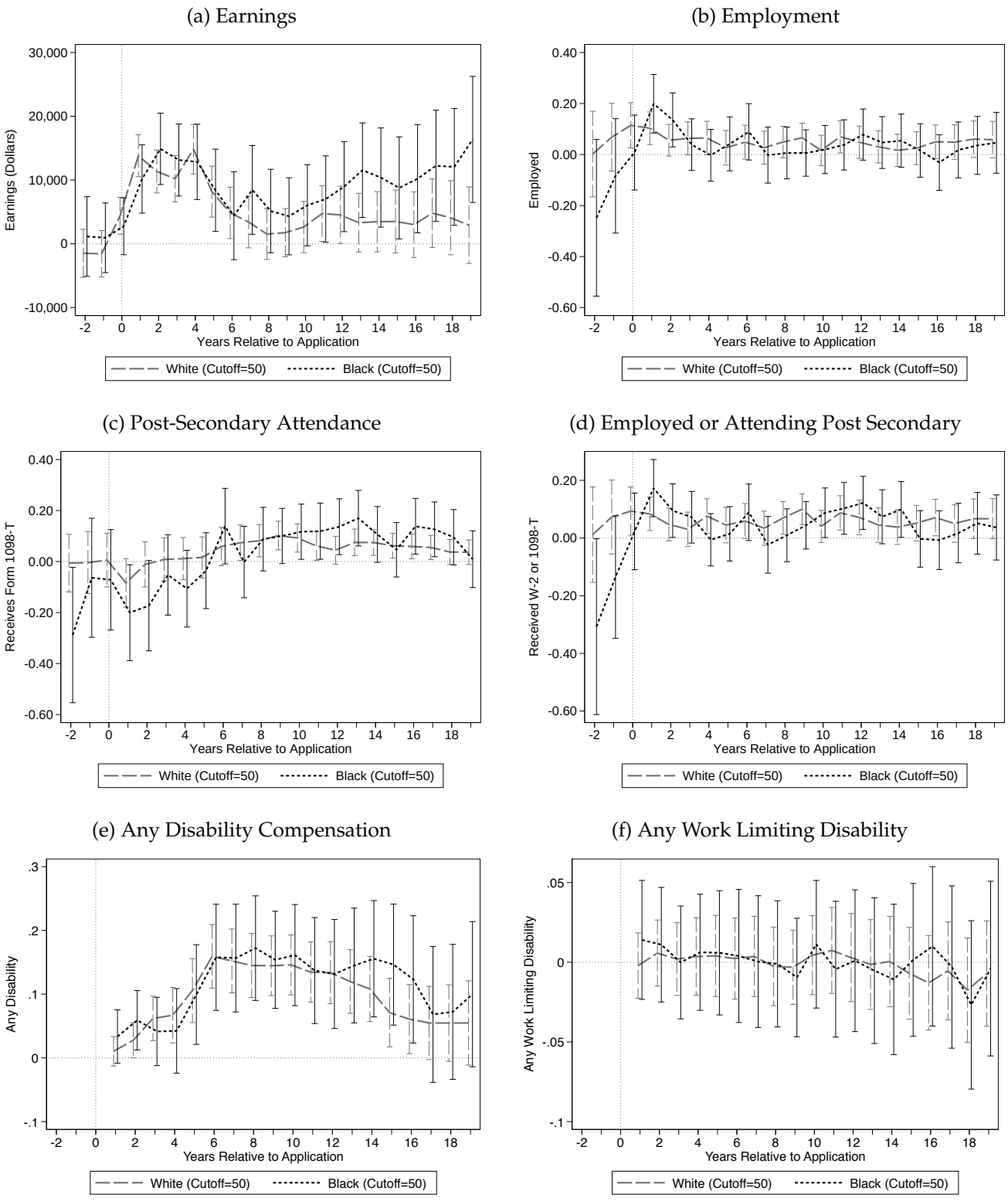
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by application cohort. Throughout, we compare estimates for the 1990-2000 application cohorts (the dashed gray line) to those for the 2001-2011 cohorts (the dotted black line) at the 50 AFQT cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

Figure A.11: Heterogeneity by Gender (50 AFQT Cutoff)



Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by gender. Throughout, we compare estimates for men (the dashed gray line) to those for women (the dotted black line) at the 50 AFQT cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

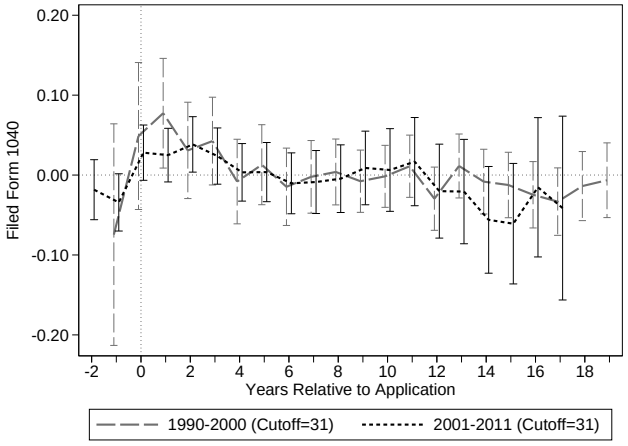
Figure A.12: Heterogeneity by Race (50 AFQT Cutoff)



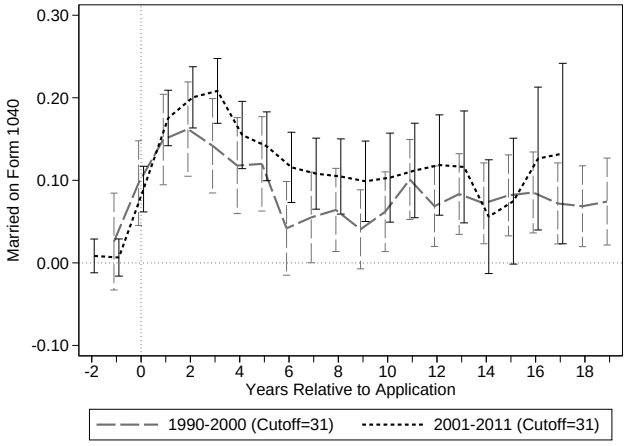
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by race. Throughout, we compare estimates for white applicants (the dashed grey line) to those for white applicant (the dotted black line) at the 50 AFQT cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares employment or post-secondary estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any work-limiting disability receipt estimates.

Figure A.13: Heterogeneity in 2SLS Marriage Estimates (31 AFQT Cutoff)

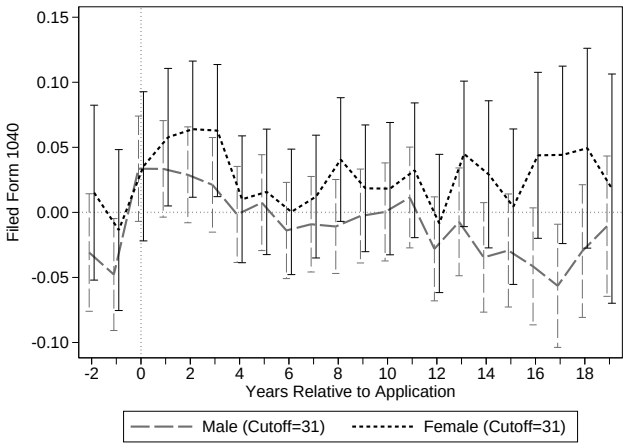
(a) Any 1040 by Cohort, 31 AFQT Cutoff



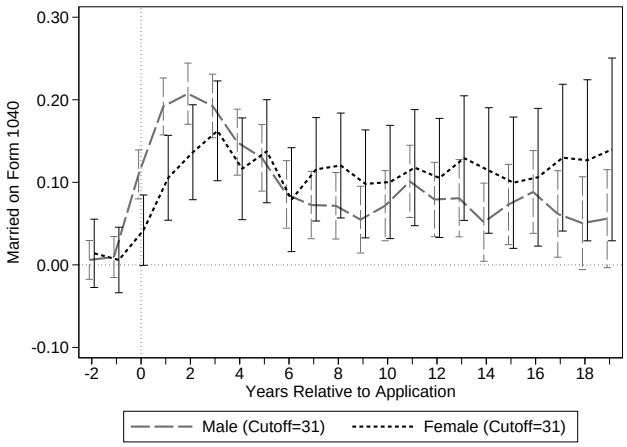
(b) Married by Cohort, 31 AFQT Cutoff



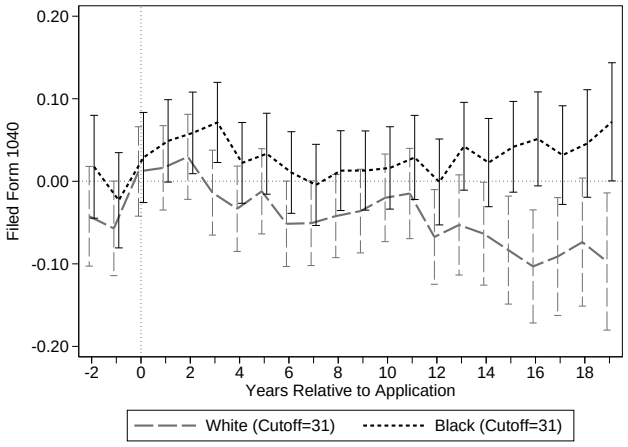
(c) Any 1040 by Gender, 31 AFQT Cutoff



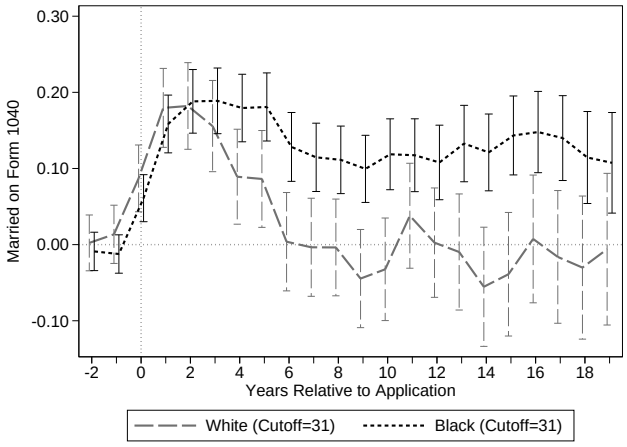
(d) Married by Gender, 31 AFQT Cutoff



(e) Any 1040 by Race, 31 AFQT Cutoff



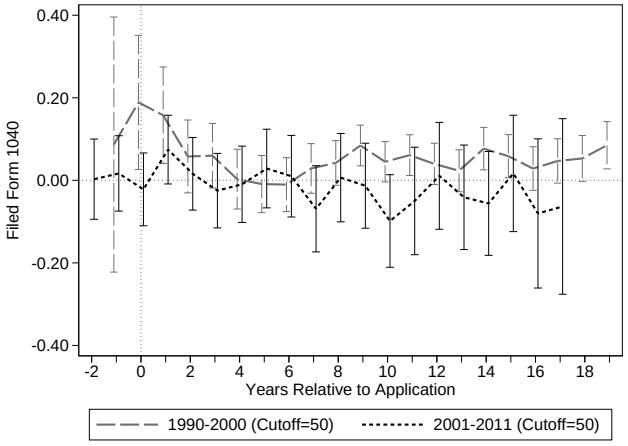
(f) Married By Race, 31 AFQT Cutoff



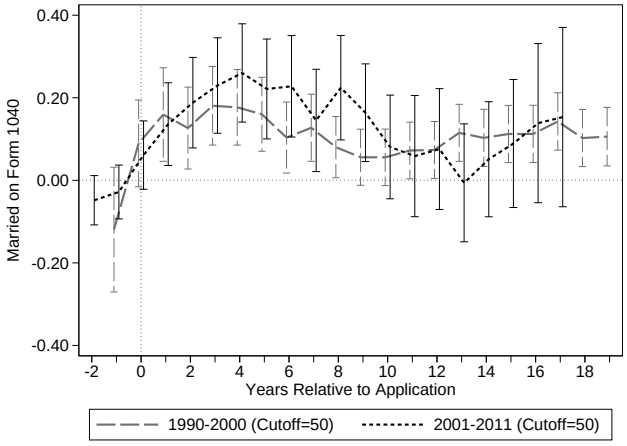
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on filing a 1040 and on marriage for different sub-sample splits at the 31 cutoff. Panel (a) and (b) compare these estimates across cohorts, panels (c) and (d) compare these estimates across gender, and panels (e) and (f) compare these estimates across race.

Figure A.14: Heterogeneity in 2SLS Marriage Estimates (50 AFQT Cutoff)

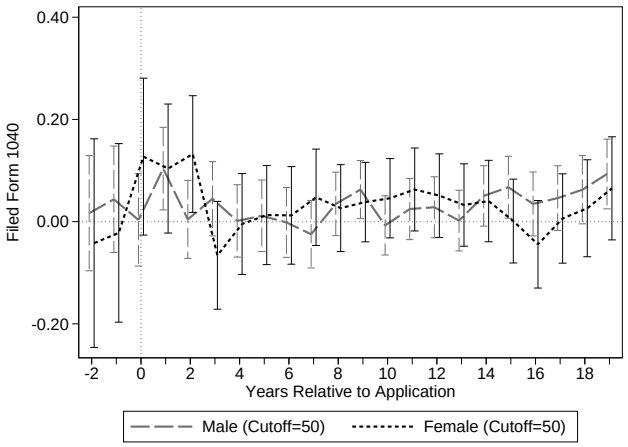
(a) Any 1040 by Cohort, 50 AFQT Cutoff



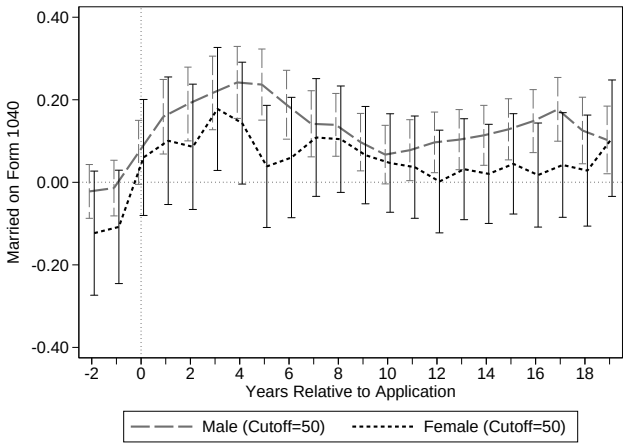
(b) Married by Cohort, 50 AFQT Cutoff



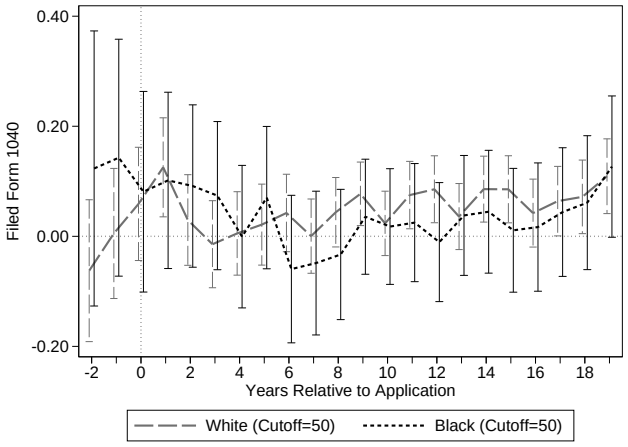
(c) Any 1040 by Gender, 50 AFQT Cutoff



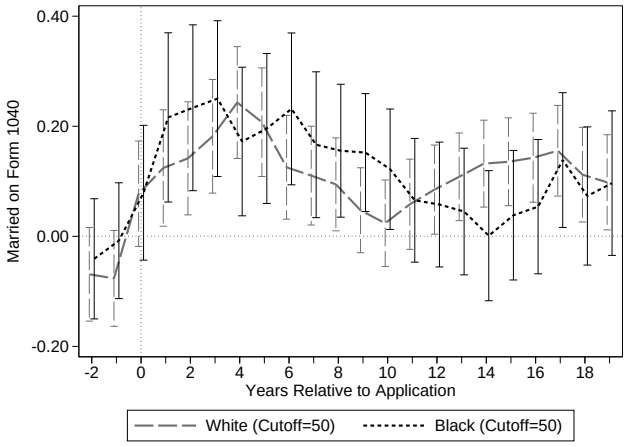
(d) Married by Gender, 50 AFQT Cutoff



(e) Any 1040 by Race, 50 AFQT Cutoff

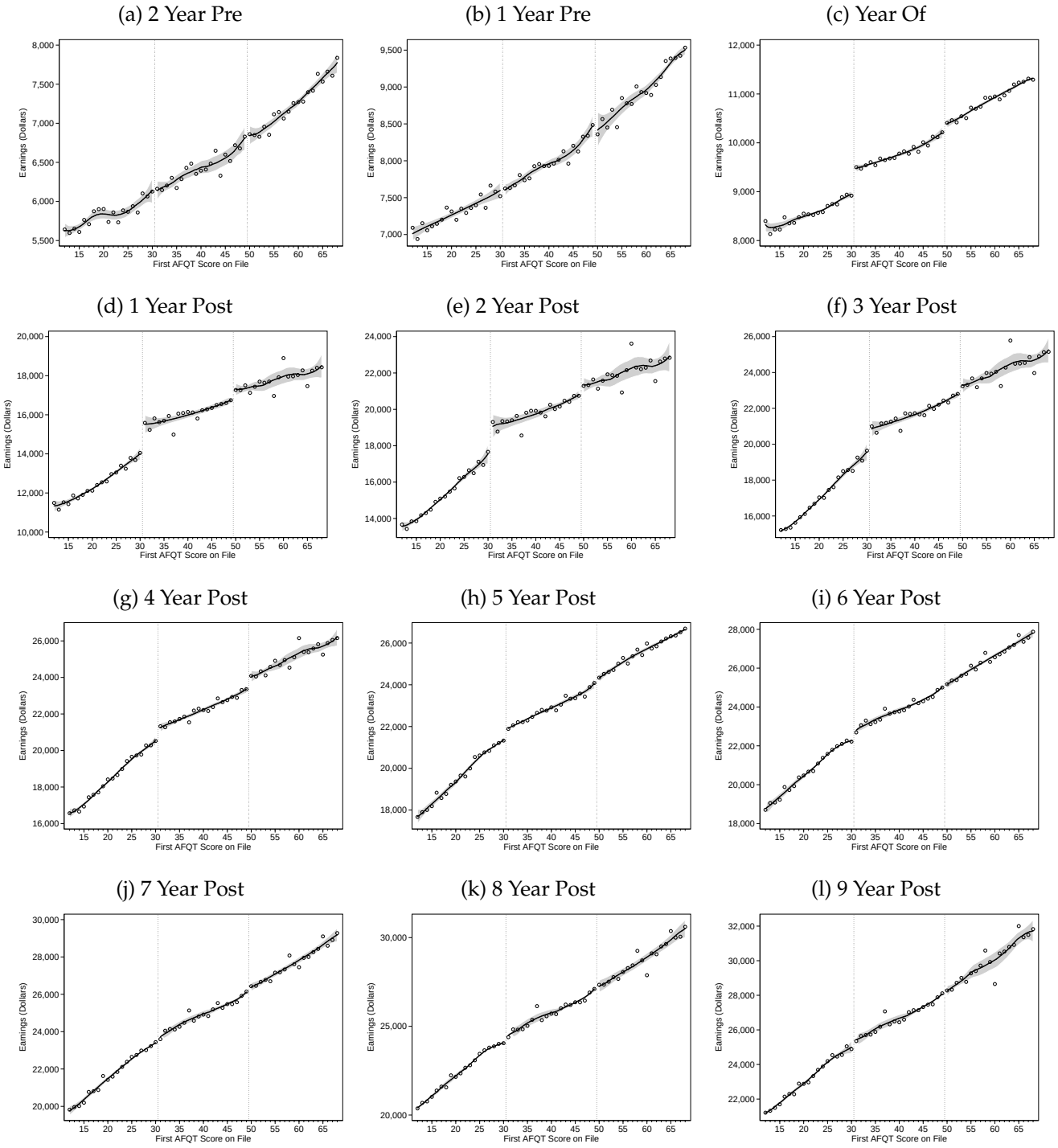


(f) Married By Race, 50 AFQT Cutoff



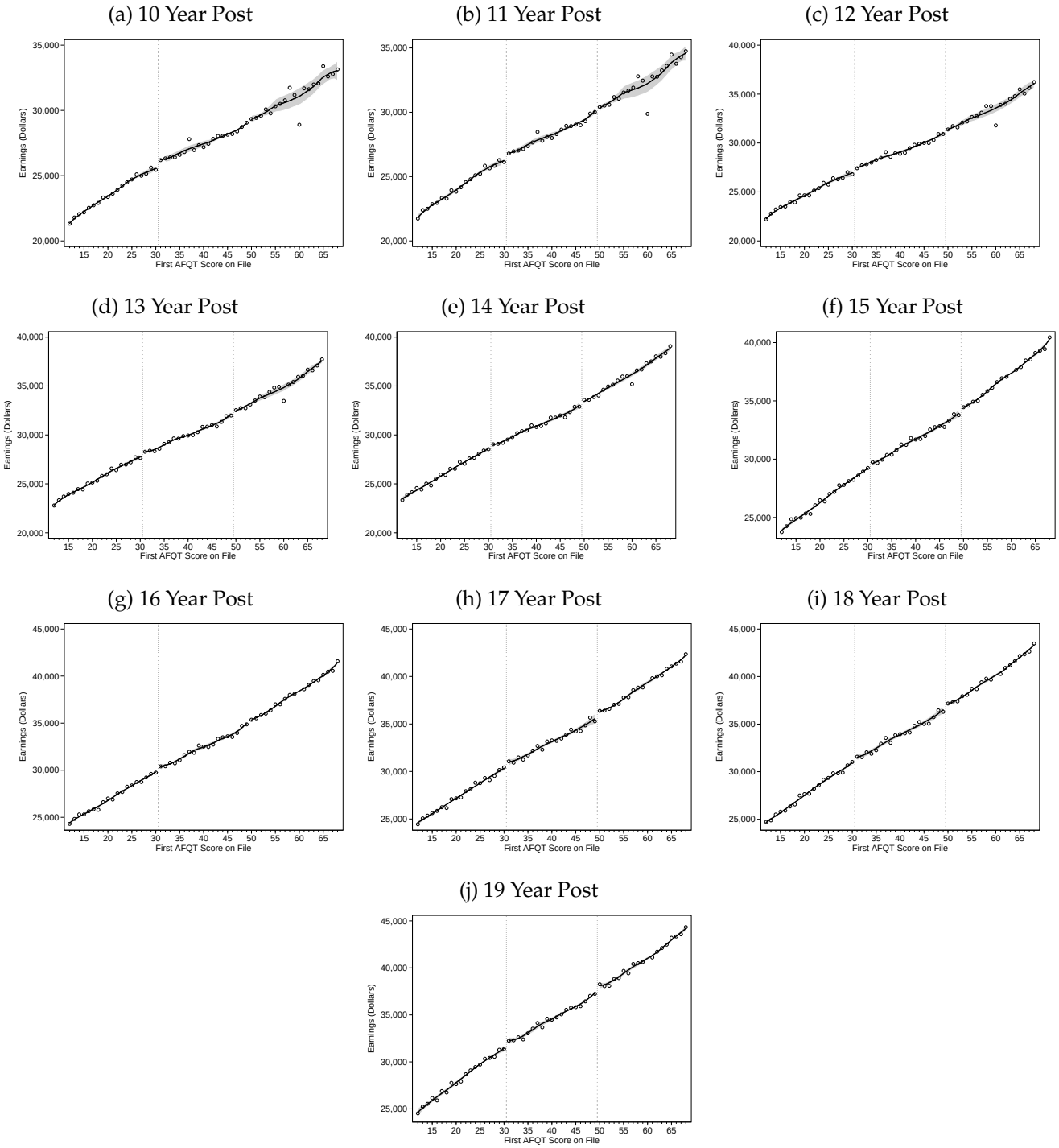
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on filing a 1040 and on marriage for different sub-sample splits at the 50 cutoff. Panel (a) and (b) compare these estimates across cohorts, panels (c) and (d) compare these estimates across gender, and panels (e) and (f) compare these estimates across race.

Figure A.15: Reduced Form Plots For Baseline Earnings Estimates (2 years pre-Application — 9 Years Post)



Notes: This figure plots our baseline earnings outcome -2 to 9 years after application as a function of the earliest AFQT score on file. Figure A.16 covers 10 to 19 years after application.

Figure A.16: Reduced Form Plots For Baseline Earnings Estimates (10—19 Years Post-Application)



Notes: This figure plots our baseline earnings outcome 10 to 19 years after application as a function of the earliest AFQT score on file.

Appendix Tables

Table A.1: First Stage: Enlistment (Reduced Form Estimates)

	Enlisted **(1)**	Enlisted **(2)**
	Cutoff: AFQT=31	Cutoff: AFQT=50
1(AFQT≥31)	0.110***	
	(0.002)	
1(AFQT≥50)		0.056***
		(0.002)
Individuals	1,137,595	1,311,111
F-stat	3231.791	935.801

Notes: This table reports estimates of equation (2), where the left hand side variable is an indicator for ever enlisting in the military. Thus, the table shows the first stage effect of crossing the 31 AFQT threshold (in column (1)) and of crossing the 50 AFQT threshold (in column (2)) on enlistment.

Table A.2: Covariate Balance (Reduced Form Estimates)

	(1)	(2)	(3)	(4)
	AFQT=31		AFQT=50	
	Linear	Quadratic	Linear	Quadratic
Age	0.021	0.004	-0.030***	-0.016
	(0.013)	(0.018)	(0.012)	(0.017)
Male	0.001	-0.003	0.003*	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
White	0.004**	-0.003	-0.002	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)
Black	-0.007***	0.004	0.000	0.000
	(0.002)	(0.003)	(0.001)	(0.002)
Hispanic	0.004***	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.002)
In High School	0.001	0.003	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
No HS Diploma	0.006***	-0.001	-0.003***	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)
HS Diploma	-0.008***	-0.005*	0.002	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Some College+	0.001*	0.002**	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Earnings (1 Year Pre)	2.057	51.222	-10.783	-21.971
	(57.125)	(79.363)	(54.578)	(76.027)
Employment (1 Year Pre)	-0.001	0.001	0.000	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Any 1040 (1 Year Pre)	-0.005**	-0.001	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Post-Secondary Attendance (1 Year Pre)	-0.000	0.000	-0.000	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Married on 1040 (1 Year Pre)	0.001	0.002	-0.002	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)
N (For Age through Some College+)	1,137,595	1,137,595	1,311,111	1,311,111
N (For 1 Year Pre Outcomes)	555,286	555,286	658,666	658,666

Notes: This table reports reduced-form RD estimates of equation (1) on covariates and pre-application outcomes at both thresholds. Columns (1) and (3) employ our baseline local linear regression with a triangular kernel. Columns (2) and (4) employ a local quadratic (no kernel) regression. The education categories are mutually exclusive, as described in the notes for Table 1.

Table A.3: 2SLS RD Estimates For Main Outcomes

Years Since App	Earnings	Earnings	Emp.	Emp.	Post-Sec.	Post-Sec.	Emp. or Post-Sec.	Emp. or Post-Sec.	Tot Dis.	Tot Dis.	Any Dis.	Any Dis.
AFQT Cutoff:	31	50	31	50	31	50	31	50	31	50	31	50
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
-2	440	366	-0.004	-0.005	-0.010	-0.053	-0.009	-0.020				
	(479)	(1411)	(0.024)	(0.064)	(0.014)	(0.045)	(0.023)	(0.063)				
	[495612]	[592472]	[495612]	[592472]	[495612]	[592472]	[495612]	[592472]				
-1	17	-264	-0.008	0.009	-0.002	-0.009	-0.019	-0.015				
	(464)	(1338)	(0.019)	(0.051)	(0.016)	(0.047)	(0.019)	(0.048)				
	[555286]	[658666]	[555286]	[658666]	[555286]	[658666]	[555286]	[658666]				
0	4019***	4108***	0.057***	0.077**	-0.067***	-0.020	0.033**	0.073**				
	(416)	(1089)	(0.014)	(0.034)	(0.015)	(0.041)	(0.013)	(0.032)				
	[612247]	[721660]	[612247]	[721660]	[612247]	[721660]	[612247]	[721660]				
1	11411***	12380***	0.070***	0.122***	-0.116***	-0.101***	0.049***	0.091***	15	136*	0.006	0.016*
	(502)	(1255)	(0.012)	(0.025)	(0.015)	(0.038)	(0.011)	(0.022)	(37)	(77)	(0.004)	(0.009)
	[671070]	[787748]	[671070]	[787748]	[671070]	[787748]	[671070]	[787748]	[681150]	[795268]	[681150]	[795268]
2	12070***	12409***	0.046***	0.084***	-0.083***	-0.052	0.043***	0.074***	47	156	0.016***	0.030***
	(567)	(1316)	(0.012)	(0.024)	(0.015)	(0.036)	(0.011)	(0.022)	(48)	(110)	(0.005)	(0.010)
	[734580]	[861418]	[734580]	[861418]	[734580]	[861418]	[734580]	[861418]	[746048]	[870086]	[746048]	[870086]
3	9368***	10649***	0.048***	0.061**	-0.007	-0.011	0.040***	0.046**	238***	497***	0.041***	0.051***
	(624)	(1406)	(0.012)	(0.025)	(0.015)	(0.035)	(0.011)	(0.023)	(68)	(158)	(0.006)	(0.013)
	[793037]	[925594]	[793037]	[925594]	[793037]	[925594]	[793037]	[925594]	[805690]	[935236]	[805690]	[935236]
4	4272***	13371***	0.012	0.057**	0.086***	-0.031	0.020*	0.060***	769***	616***	0.085***	0.058***
	(689)	(1501)	(0.013)	(0.025)	(0.015)	(0.034)	(0.012)	(0.023)	(97)	(232)	(0.007)	(0.017)
	[847770]	[986271]	[847770]	[986271]	[847770]	[986271]	[847770]	[986271]	[861441]	[996790]	[861441]	[996790]
5	2681***	7281***	-0.005	0.044*	0.087***	0.020	0.019	0.056**	1152***	960***	0.116***	0.097***
	(735)	(1593)	(0.013)	(0.026)	(0.015)	(0.033)	(0.012)	(0.024)	(130)	(301)	(0.009)	(0.020)
	[894892]	[1037158]	[894892]	[1037158]	[894892]	[1037158]	[894892]	[1037158]	[874310]	[1005909]	[874310]	[1005909]
6	2538***	3502**	-0.007	0.045*	0.110***	0.085***	0.018	0.063***	1453***	1566***	0.128***	0.153***
	(770)	(1642)	(0.014)	(0.027)	(0.014)	(0.032)	(0.013)	(0.024)	(157)	(324)	(0.010)	(0.020)
	[948893]	[1097382]	[948893]	[1097382]	[948893]	[1097382]	[948893]	[1097382]	[891356]	[1020780]	[891356]	[1020780]
7	1860**	3827**	-0.029**	0.005	0.081***	0.080***	-0.011	0.018	1573***	1673***	0.133***	0.154***
	(801)	(1663)	(0.014)	(0.026)	(0.014)	(0.030)	(0.013)	(0.024)	(184)	(347)	(0.011)	(0.020)
	[1000427]	[1155868]	[1000427]	[1155868]	[1000427]	[1155868]	[1000427]	[1155868]	[895763]	[1021534]	[895763]	[1021534]
8	1686**	1849	-0.019	0.034	0.078***	0.108***	-0.004	0.065***	1816***	1917***	0.143***	0.160***
	(820)	(1627)	(0.014)	(0.025)	(0.013)	(0.027)	(0.013)	(0.023)	(194)	(360)	(0.011)	(0.020)
	[1028269]	[1187171]	[1028269]	[1187171]	[1028269]	[1187171]	[1028269]	[1187171]	[912219]	[1042039]	[912219]	[1042039]
9	1556*	1253	-0.012	0.033	0.081***	0.109***	0.008	0.076***	1926***	2009***	0.149***	0.151***
	(856)	(1545)	(0.014)	(0.023)	(0.013)	(0.024)	(0.013)	(0.021)	(201)	(348)	(0.011)	(0.019)
	[1064711]	[1222565]	[1064711]	[1222565]	[1064711]	[1222565]	[1064711]	[1222565]	[949830]	[1082083]	[949830]	[1082083]
10	2535***	2196	-0.039**	-0.000	0.061***	0.093***	-0.016	0.039*	2045***	2310***	0.152***	0.160***
	(931)	(1638)	(0.015)	(0.024)	(0.013)	(0.024)	(0.014)	(0.022)	(220)	(363)	(0.012)	(0.019)
	[1016643]	[1163949]	[1016643]	[1163949]	[1016643]	[1163949]	[1016643]	[1163949]	[900658]	[1028985]	[900658]	[1028985]
11	2052**	3397*	-0.031**	0.029	0.059***	0.085***	-0.008	0.067***	2469***	2464***	0.163***	0.152***
	(994)	(1775)	(0.016)	(0.025)	(0.014)	(0.024)	(0.015)	(0.023)	(232)	(376)	(0.012)	(0.019)
	[969081]	[1109460]	[969081]	[1109460]	[969081]	[1109460]	[969081]	[1109460]	[856578]	[981145]	[856578]	[981145]
12	2529**	3918**	-0.037**	0.034	0.049***	0.078***	-0.015	0.067***	2633***	2603***	0.167***	0.151***
	(1056)	(1839)	(0.016)	(0.025)	(0.014)	(0.024)	(0.016)	(0.024)	(251)	(400)	(0.013)	(0.020)
	[930408]	[1066121]	[930408]	[1066121]	[930408]	[1066121]	[930408]	[1066121]	[818021]	[933088]	[818021]	[933088]
13	1437	4215**	-0.039**	0.007	0.050***	0.109***	-0.020	0.042*	2613***	2387***	0.167***	0.137***
	(1136)	(1888)	(0.017)	(0.025)	(0.014)	(0.023)	(0.016)	(0.024)	(273)	(424)	(0.013)	(0.021)
	[882000]	[1013570]	[882000]	[1013570]	[882000]	[1013570]	[882000]	[1013570]	[769635]	[869300]	[769635]	[869300]
14	693	3307*	-0.027	0.002	0.053***	0.093***	-0.010	0.041*	2391***	2428***	0.154***	0.136***
	(1183)	(1939)	(0.017)	(0.025)	(0.014)	(0.022)	(0.017)	(0.024)	(293)	(435)	(0.014)	(0.021)
	[838698]	[966232]	[838698]	[966232]	[838698]	[966232]	[838698]	[966232]	[715114]	[800376]	[715114]	[800376]
15	671	2572	-0.047**	0.007	0.035**	0.061***	-0.028	0.030	2170***	1712***	0.146***	0.108***
	(1257)	(2042)	(0.018)	(0.026)	(0.014)	(0.022)	(0.017)	(0.025)	(321)	(455)	(0.016)	(0.023)
	[800809]	[918715]	[800809]	[918715]	[800809]	[918715]	[800809]	[918715]	[656572]	[731114]	[656572]	[731114]
16	1345	3116	-0.036*	0.012	0.031**	0.080***	-0.010	0.046*	2402***	1755***	0.162***	0.091***
	(1337)	(2162)	(0.019)	(0.027)	(0.014)	(0.022)	(0.018)	(0.026)	(352)	(472)	(0.017)	(0.023)
	[753318]	[855548]	[753318]	[855548]	[753318]	[855548]	[753318]	[855548]	[596016]	[664251]	[596016]	[664251]
17	1385	4867**	-0.027	0.023	0.038**	0.072***	-0.015	0.030	2388***	1308***	0.169***	0.068***
	(1437)	(2241)	(0.020)	(0.028)	(0.015)	(0.022)	(0.019)	(0.026)	(393)	(506)	(0.019)	(0.025)
	[699583]	[787209]	[699583]	[787209]	[699583]	[787209]	[699583]	[787209]	[538151]	[600528]	[538151]	[600528]
18	614	4243*	-0.049**	0.026	0.035**	0.052**	-0.022	0.031	2294***	1371**	0.171***	0.076***
	(1591)	(2416)	(0.022)	(0.029)	(0.016)	(0.023)	(0.021)	(0.028)	(431)	(533)	(0.020)	(0.025)
	[641983]	[718639]	[641983]	[718639]	[641983]	[718639]	[641983]	[718639]	[478204]	[533504]	[478204]	[533504]
19	1381	5240**	-0.034	0.026	0.011	0.031	-0.021	0.040	2650***	1826***	0.182***	0.087***
	(1750)	(2535)	(0.024)	(0.030)	(0.017)	(0.023)	(0.023)	(0.029)	(470)	(582)	(0.022)	(0.028)
	[582309]	[652445]	[582309]	[652445]	[582309]	[652445]	[582309]	[652445]	[413306]	[458686]	[413306]	[458686]

Notes: This table contains the main estimates underlying Figures 4 — 6. The coefficient estimate for each year comes first, followed by the standard error in parentheses and the observation count in brackets. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.4: Cumulative 2SLS Estimates (1999-2003 Cohorts)

	(1)	(2)	(3)	**(4)**	(5)	(6)	(7)	**(8)**
	31 AFQT Cutoff				50 AFQT Cutoff			
Relative Years	0-5	6-10	11-15	**0-15**	0-5	6-10	11-15	**0-15**
				Panel (a): Cumulative Earnings				
Enlist	37,311***	22,551***	13,711*	**73,573*** **	68,505***	37,051***	16,380	**121,936*** **
	(4,297)	(6,301)	(7,871)	**(16,239)**	(8,450)	(12,949)	(16,630)	**(33,113)**
Dep. Var. Mean	102,086	115,430	131,036	**348,552**	115,998	130,638	150,532	**397,169**
				Panel (b): Cumulative Years of Employment				
Enlist	0.215***	-0.013	-0.230**	**-0.029**	0.479***	0.299*	-0.191	**0.587**
	(0.07)	(0.10)	(0.11)	**(0.23)**	(0.12)	(0.18)	(0.21)	**(0.41)**
Dep. Var. Mean	5.4	4.1	3.9	**13.4**	5.5	4.2	3.9	**13.6**
				Panel (c): Attended Any Post-Secondary in Given Time Window				
Enlist	-0.026	0.143***	0.129***	**0.065** **	0.036	0.166***	0.285***	**0.170*** **
	(0.03)	(0.03)	(0.03)	**(0.03)**	(0.06)	(0.06)	(0.06)	**(0.06)**
Dep. Var. Mean	0.418	0.369	0.307	**0.627**	0.479	0.429	0.353	**0.695**
Application Cohorts	99-03	99-03	99-03	**99-03**	99-03	99-03	99-03	**99-03**
Individuals	275,461	275,461	275,461	**275,461**	329,264	329,264	329,264	**329,264**

Notes: This table 2SLS RD estimates of the effect of enlistment on *cumulative* earnings, employment and educational attendance. Columns (1)-(4) estimate cumulative effects at the 31 AFQT cutoff, while columns (5)-(8) do so at the 50 cutoff. Each column looks at cumulative outcomes over a different time horizon: 0-5 years since application, 6-10 years since, 11-15 years since, and finally 0-15 years since. Throughout, we restrict to the 1999-2003 cohorts whom we observe in all years 0-15 following application. We estimate the effect of enlistment on total earnings in panel (a), on total years of employment in panel (b), and on ever attending a post-secondary institution within the given time window in panel (c). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.5: 2SLS RD Estimates For NSC Outcomes

Cutoff:	31	50	31	50
Cohorts:	All	All	99-03	99-03
Outcome as of:	by2019	by2019	0-15yrs	0-15yrs
	(1)	**(2)**	**(3)**	**(4)**
Attend Post-Secondary	0.076***	0.125***	0.067**	0.189***
	(0.018)	(0.032)	(0.032)	(0.060)
Attend 4-Yr College	0.085***	0.154***	0.115***	0.207***
	(0.016)	(0.031)	(0.028)	(0.059)
Attend 4-Yr Non-Profit (Pub. or Priv.)	0.050***	0.130***	0.068***	0.180***
	(0.014)	(0.029)	(0.025)	(0.055)
Attend 4-Yr For-Profit	0.066***	0.067***	0.063***	0.086**
	(0.011)	(0.022)	(0.021)	(0.042)
Attend 2-Yr College	0.049***	0.067**	0.030	0.129**
	(0.017)	(0.032)	(0.031)	(0.061)
Attend $\geq$ Mod. Selective	0.020*	0.048**	0.032	0.093**
	(0.011)	(0.024)	(0.020)	(0.046)
Attend $\leq$ Min. Selective	0.067***	0.113***	0.090***	0.195***
	(0.018)	(0.032)	(0.032)	(0.062)
Assoc. Degree or Higher	0.044***	0.076***	0.031	0.064
	(0.011)	(0.024)	(0.020)	(0.046)
Bach. Degree or Higher	0.023***	0.042**	0.020	0.007
	(0.009)	(0.020)	(0.015)	(0.037)
Individuals	1159354	1328772	279870	332560

Notes: Each row reports 2SLS RD estimates of the effect of enlistment on the stated outcome. In columns (1) and (2) the sample is all applicants between 1990 and 2011 and the outcomes are defined as of 2019. In columns (3) and (4) the sample is applicants between 1990 and 2003 and the outcomes are defined for 0-15 years after application. In columns (1) and (2) we use all NSC data available to us (coverage begins pre 1990 for some schools). In columns (3) and (4) we use NSC data from 1999 forward. Significance levels: $*$: 10% $**$: 5% $***$: 1%.