Army Service in the All-Volunteer Era*

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Abstract

Since the beginning of the all-volunteer era, millions of young Americans have chosen to enlist in the military. These volunteers disproportionately come from disadvantaged backgrounds, and while some aspects of military service are likely to be beneficial, exposure to violence and other elements of service could worsen outcomes. This paper links the universe of Army applicants between 1990 and 2011 to their federal tax records and other administrative data and uses two eligibility thresholds in the Armed Forces Qualifying Test (AFQT) in a regression discontinuity design to estimate the effects of Army enlistment on earnings and related outcomes. In the 19 years following application, Army service increases average annual earnings by over $4,000 at both cutoffs. However, whether service increases long-run earnings varies significantly by race. Black servicemembers experience annual gains of $5,500 to $15,000 11-19 years after applying while White servicemembers do not experience significant changes. By providing Black servicemembers a stable and well-paying Army job and by opening doors to higher-paid post-service employment, the Army significantly closes the Black-White earnings gap in our sample.

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1 Introduction

At a time when upward social mobility is stagnating (Chetty et al., 2017) and economic opportunities continue to be starkly different by race (Bayer and Charles, 2018; Chetty et al., 2020), the United States Army has recruited millions of young Americans to serve "with promises of individual opportunity" (Bailey et al., 2009). Retired General Colin Powell has said that "the military [has] given African-Americans more equal opportunity than any other institution in American society" (Powell and Persico, 1996). Indeed, enlistment could increase opportunity and reduce racial inequality 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 (e.g., Barr, 2019; Breznitz, 2005; Wilson and Kizer, 1997). Yet, volunteer service also includes significant risks. 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 (e.g., Autor et al., 2016; Bingley et al., 2020; Loughran and Heaton, 2013).

Despite the role the modern Army might play in generating economic opportunity and reducing racial inequality for servicemembers, there is little causal evidence of the effects of service in the current all-volunteer era. We overcome the identification challenges inherent in disentangling enlistment decisions from other factors by exploiting discontinuities in Army hiring practices and find that enlistment increases cumulative earnings in the 19 years after application. However, the long-run effects of service differ by race. While enlistment only significantly increases earnings for White applicants in the short-run, we show that the Army is a vehicle of economic mobility for Black Americans—increasing long-run earnings, marriage, and homeownership without adverse employment effects.

In this paper, we estimate the effects of Army service for the universe of Active Duty Army applicants from 1990-2011, exploiting two Armed Forces Qualifying Test (AFQT) score cutoffs—at the 31st and 50th percentile of national math and verbal ability. Department of Defense (DoD) policy requires 96% of recruits to have an AFQT score of 31 or higher and 60% of recruits to have an AFQT of 50 or higher. As a result, the Army rarely accepts applicants with AFQT scores below 31, seldom accepts GED recipients with scores below 50, and often requires applicants to score 50 or higher to receive enlistment bonuses. Consequently, using applicants' first AFQT scores on file, we find that crossing the 31 and 50 AFQT cutoffs increases the probability of enlistment by 10 and 6 percentage points, respectively.

We leverage these AFQT cutoffs to estimate the effect of enlistment on earnings and related outcomes. We link the universe of Active Duty Army applicants to earnings, employment, disability, education, and other administrative records from the Internal Revenue Service (IRS), National Student Clearinghouse (NSC), Social Security Administration (SSA), and Department of Veterans Affairs (VA). We find that enlisting in the Army increases average annual earnings by over \$4,000 at both cutoffs in the 19 years following application. The effects of service vary over time, with the largest effects in the first 4 years and smaller effects 5-10 years after application. In the long-term, 11-19 years after application, we estimate a statistically insignificant \$2,200 increase in annual earnings at the lower AFQT cutoff and a marginally significant \$4,100 increase at the higher cutoff. Short-run employment increases at both cutoffs, but enlistment has no long-run effect on employment at either cutoff. Consistent with generous veteran education and disability benefits, we also find that the Army significantly increases both college attendance and disability compensation at both cutoffs.[1] Conversely, we find little effect on mortality, and if anything, a slight negative effect.

Our overall earnings estimates mask substantial heterogeneity by race. Enlisting in the Army increases Black applicants' annual earnings by \$5,500 at the 31 AFQT cutoff and by \$15,000 at the 50 AFQT cutoff 11-19 years after application. Meanwhile, White applicants experience statistically insignificant earnings losses of approximately \$3,000 at the 31 cutoff and insignificant gains of around \$4,000 at the 50 cutoff. Compared to their counterfactual earnings trajectories in our sample, Army service closes nearly all of the Black-White earnings gap. Black applicants tend to come from families with lower incomes and from counties with worse economic conditions than White applicants, which could help explain our findings. Indeed, we find some evidence that the Army is more beneficial for those with lower observable proxies of initial economic opportunity, independent of race. Yet, racial differences in the long-run effects of Army service persist even after accounting for pre-application characteristics, suggesting that Army service is distinctly beneficial for Black applicants.

We explore potential mechanisms for the greater long-term benefits of Army service for Black relative to White servicemembers. We find that differences in exposure to combat, disability receipt, and post-service educational attainment explain only a small fraction of divergent returns to service by race. However, we do find that Black servicemembers serve for longer and benefit disproportionately from access to a stable and well-paying military job. While the Army tends to be a relatively well-paying job for all servicemembers (Asch et al., 2010), Black servicemembers—who we find would have earned less than White servicemembers in the absence of enlistment—particularly benefit from an Army pay structure that pays Black and White soldiers equally.[2] Nevertheless, generous back-of-the-envelope calculations accounting for differences in Army retention and pay (along with combat deployments, disability receipt, and post-service education) still

[1] Army service increases the probability of attending college within 19 years of application by 14.7 and 20.2 percentage points at the low and high cutoff, respectively. Army also service increases disability compensation receipt – defined as any Veterans Affairs Disability Compensation (VADC), Social Security Disability Insurance (SSDI), or Supplemental Security Income (SSI) – by approximately 25 percentage points in the 19 years after application. When we restrict our analysis to significant, and typically work-limiting, disability—defined as any SSI, SSDI, or VADC with a combined disability rating of 100% or individual unemployability designation—we find that enlistment leads to about a 4 percentage point increase at both cutoffs.

[2] Army pay is strictly a function of military rank and years of service that commanders cannot adjust for specific soldiers. Consequently, Black and White servicemembers are compensated at comparable rates.

leave approximately $6,000-$12,000 of the Black-White gap to be explained. As a result, Black servicemembers necessarily experience larger increases in long-run post-service earnings. Indeed, we find that Black servicemembers are more likely to be employed in high-paying industries 19 years after enlisting. They are also more likely to be employed in the public sector. These patterns are less evident for White servicemembers. Although the precise elements of Army service that are most beneficial relative to civilian counterfactuals are unclear, potential explanations include increased human capital not captured by educational differences, access to networks, or credentialing effects that diminish racial discrimination (De Tray, 1982; Kleykamp, 2009). Overall, through both a stable and well-paying job and opening doors to higher-paid employment, Army service offers many Black Americans a path toward upward mobility.

Related Literature. The Department of Defense is the largest employer in the United States and affects the lives of many Americans — approximately one in thirteen American adults and one in seven men have served in the military (Census, 2018). Yet, existing causal studies of military service are primarily identified using conscription lotteries, which the U.S. ended in 1973 (e.g. Angrist, 1990; Angrist et al., 2011; Bingley et al., 2020; Card and Cardoso, 2012).[3] The effects of enlistment on those who choose to serve in today's all-volunteer force may differ both due to its voluntary nature and due to changes in military compensation, benefits, and the nature of combat. While several studies examine the consequences of all-volunteer service in the modern era by comparing veterans to non-veterans (e.g. Kleykamp, 2013; Makridis and Hirsch, 2019; Teachman and Tedrow, 2007) or by comparing applicants who enlist to those who do not (e.g. Angrist, 1998; Loughran et al., 2011; Martorell et al., 2014), these studies vary considerably in their estimates and may not account for important differences between those who select into service and those who do not. Indeed, we find in our data that ordinary least squares estimates of service on earnings among applicants are significantly larger than our corresponding regression discontinuity estimates. Our strategy identifies the causal effects of military service in the modern, all-volunteer era under less restrictive assumptions and among recent applicant cohorts on the margin of enlistment — a disadvantaged population of broader policy interest and the relevant population for assessing the consequences of expanding or contracting today's military.

Our extensive collection of administrative data enables us to estimate the direct, causal effect of modern-day service not just on earnings and employment, but also on several additional outcomes of broader policy interest including educational attainment, mortality, and disability compensation. In recent years and in the context of wars in Iraq and Afghanistan, G.I. Bill edu-

[3]One exception is Angrist (1998). In addition to comparing the earnings of applicants who enlist to the earnings of applicants who do not enlist after controlling for observable characteristics, the paper uses a second identification strategy that compares the earnings of low-scoring, unqualified applicants in the 1970s who, as a result of a misnormed ASVAB, were mistakenly allowed to enlist to the earnings of applicant cohorts in the early 1980s after this mistake was corrected. The results of the second identification strategy suggests post-service earnings losses of around $1,000 (in 2018 USD) for both white and nonwhite servicemembers.

cational benefits have expanded, VA Disability payments have increased, with VA programs now costing over $180 billion annually (Congressional Budget Office, 2018), and the risks associated with service may have changed. Our volunteer-era estimates provide policy-relevant updates to conscription-era studies of the effects of service on these outcomes (Angrist et al., 2010; Angrist and Chen, 2011; Autor et al., 2011; Bedard and Deschênes, 2006; Bound and Turner, 2002; Dobkin and Shabani, 2009; Johnston et al., 2016). Our direct estimates also provide important context for studies that examine the impacts of specific policy changes to the G.I. Bill and VA Disability Compensation (VADC) (Autor et al., 2016; Barr, 2015, 2019; Barr et al., 2021). Lastly, we are also able to study the impact of service on potential measures of well-being like homeownership and marriage, which help provide a broader view of whether the Army acts as a vehicle of upward mobility.

Finally, to our knowledge, our study is the first to confirm the view held by many prominent figures, including Colin Powell, that an all-volunteer military can be a vehicle for opportunity for minority populations in the United States.[4] This paper suggests that the U.S. military, which disproportionately employs Black Americans (currently more than 200,000 (DoD, 2020)), could have an important role in reducing racial disparities in economic opportunity, contributing to a growing literature on differences in income mobility by race (e.g. Akee et al., 2017; Bhattacharya and Mazumder, 2011; Chetty et al., 2020; Mazumder, 2014). Our finding that Army service increases earnings of Black applicants by $5,500 to $15,000 is comparable to the effect of moving a young child to a lower-poverty neighborhood as estimated from the Moving to Opportunity Project (Chetty et al., 2016), but contrasts with related experimental evidence that suggests moving to lower-poverty areas has little impact on outcomes for older children and adults (Chetty et al., 2016; Kling et al., 2007; Ludwig et al., 2013).[5] At least among Black servicemembers, long-term earnings gains also contrast with findings from much of the literature on the impact of U.S. government sponsored training and other active labor market programs (e.g., Card et al. (2018), Greenberg et al. (2003)), though are comparable to estimates from recent sectoral specific job training programs (Katz et al., 2020).

Roadmap. The remainder of our paper is structured as follows: Section 2 explains our institutional background, Section 3 describes our data and sample, Section 4 describes our empirical approach, Section 5 presents the effects of Army service on earnings and related outcomes for all applicants, and, separately, for Black and White applicants, Section 6 explores potential explanations for the Black-White gaps in the effects of service, and Section 7 concludes.

[4] Angrist (1998) finds smaller post-service effects for nonwhite servicemembers, ranging from losses of $1,000 to gains of $2,000 (in 2018 USD) depending on the specification. Estimates from US conscription lotteries generally find small or insignificant results for nonwhite servicemembers (e.g., Angrist, 1990; Angrist and Chen, 2011).

[5] On the former point, $5,500 is 20% of mean earnings for Black applicants near the 31 AFQT cutoff. $15,000 is 50% of mean earnings for Black applicants near the 50 AFQT cutoff. Chetty et al. (2016) find that children from families who take a voucher to move to a lower-poverty neighborhood before the child is 13 years old have an annual income that is $3,477 (31%) higher than the average earnings of the control group ($11,270) in their mid-twenties.

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 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 10 tests within the ASVAB contribute to an applicant's 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 applicant's 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 no more than 4 percent of recruits have AFQT scores below 31 and that at least 60 percent of recruits have AFQT scores of 50 (DoD, 2004). To meet DoD requirements, the U.S. Army rarely accepts 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 (DoD, 2004; U.S. Army Recruiting Command Regulation 601-96, 2012). These regulations create discontinuities in the probability of Army service based on applicants' first AFQT scores of 31 and 50 (see panel (a) of Figure 1).

While applicants are unlikely to be able to 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.

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 specialties (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 the 10 ASVAB tests—groupings that differ from the four that compose the AFQT, which eliminates any confound from effects of within-military career placement. 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, including those with enlistment bonuses of zero, is $3,780 compared to just $1,620 for servicemembers who have an AFQT score of 49.

Characteristics of Army Service. Approximately 40% of applicants who enlist choose traditional combat occupations (e.g., infantry or combat engineer) while others work as logistical specialists, personnel clerks, mechanics, 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 receive a variety of employment benefits. These include access to tuition assistance and student loan repayment programs, subsidized childcare, free personal and family healthcare, free dental care, and subsidized family dental coverage.

Army service also carries considerable risk for many soldiers. In the years we study, around half 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 and about 2% are wounded in action.

Veterans' Experiences. After leaving service, veterans are eligible for a wide range of benefits, most notably education benefits, disability compensation, and access to free or subsidized healthcare. Most veterans in our sample are eligible for 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 more generous Post-9/11 GI Bill, which covers soldiers who served after 9/11/2001 and did not use their education benefits prior to 2008. Compared to the Montgomery GI Bill, the Post-9/11 GI Bill expanded eligibility, increased maximum tuition reimbursements, and introduced generous book and housing stipends.[6]

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 leave the service. Relative to the two other major Federal disability programs—Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI)—VADC provides compensation for a much broader range of conditions. Importantly, VADC is generally not work-limiting or means-tested: veterans can apply for and receive disability compensation regardless of their current employment or earnings status.[7] Many of the most common disabilities among recent veterans are consistent with physical overuse injuries. According to the 2019 VA Annual Benefits Report, the three most common service-connected disabilities among Gulf War Era veterans are tinnitus (ringing in the ear), hearing loss, and limitation of flexion (knees), while PTSD was the fourth most common. Veterans eligible for VADC receive monthly payments ranging from $140 per month to $3,500 per month

[6]Eligibility for the Montgomery GI Bill required a $100 monthly withholding of paychecks for the duration of an initial enlistment term, and successful 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. 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. 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. These stipends do not factor into our earnings estimates.

[7]One exception to this is VADC 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.

depending on their degree of service-connected disability.

Beyond direct monetary payments to veterans with service-connected disabilities, lifetime subsidized or free health care through the Department of Veterans Affairs could help offset 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 enrolled, 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.[8]

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 (1990-2018), federal tax records (1999-2018), Social Security Administration disability insurance records (1999-2015), Department of Veterans Affairs VA disability compensation records (1999-2018), and National Student Clearinghouse college education records (1999-2018).

3.2 Sample Construction

Our analysis sample consists of Army applicants in calendar years 1990 to 2011 who can be matched to Social Security records, with a few sample restrictions. First, we exclude applicants with prior military service (approximately 6% of applicants). Second, we exclude the approximately 7% of applicants who took their ASVAB in high school as part of the ASVAB Career Exploration Program.[9] Third, we restrict our analysis to 98.9% of the remaining applicants who we are able to match to Social Security records (see Section A.1 of the Data Appendix). After these restrictions, our sample consists of 2,594,896 applicants. Much of our analysis is further limited to the 1,775,059 applicants with AFQT scores close to our two cutoffs (between 12 and 68).

For each individual in our analysis matched sample, we link tax records from 1999-2018 (e.g., employer-filed W-2 forms) for up to one year prior to and 19 years after application. While it is 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

[8] See https://www.va.gov/health-care/eligibility/priority-groups/, accessed August 2021, for more details.

[9] 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.

a generous Army retirement pension at 20 years of service, which complicates the interpretation of wage and employment outcomes for 20 or more years after application.

3.3 Outcomes

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 allowances. Our baseline measure of individual earnings, available beginning in 1999, combines wages reported on Form W-2 and, consistent with Loughran et al. (2011), compensation from the military that ordinarily would be included as wages on the W-2 in the civilian sector but is nontaxable due to a special tax exclusion provided to servicemembers. The military pay included in our earnings outcome consists of Army housing allowances (Basic Allowance for Housing or BAH), direct payments for food (Basic Allowance for Subsistence BAS), and deployment/foreign assignment payments (Hardship Duty Pay, Imminent Danger Pay, Hazardous Duty Pay, and Family Separation Allowances).[10] 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. Section A.2 of the Data Appendix provides a detailed description of how we construct our earnings outcome and contains more information on military pays and allowances.

In addition to examining the effects of Army service on earnings, we explore the effects of the Army on employment, education, disability, and mortality. We consider a person employed in a specific year if she receives a W-2 with positive wages in the same year. Additionally, since higher education institutions that receive federal financial aid are required to file a 1098-T on behalf of each student they enroll, we identify an individual as having attended college if her college submits a Form 1098-T on her behalf. We supplement our education outcomes with associate and bachelor degree completion data from the National Student Clearinghouse (NSC). We also combine disability compensation records from the Department of Veterans Affairs — VA Disability Compensation (VADC) — and the Social Security Administration — Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) — to measure disability compensation. 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. Additionally, we examine proxies for independent contracting, homeownership, and marriage from 1099-MISC, 1098, and 1040 forms.[11]

[10]We do not include employees benefits, such as health coverage, retirement contributions, or GI-Bill tuition and related housing allowances, some of which are common across both military and civilian sectors (though GI benefits can be many times as generous as any civilian employer-provided tuition assistance, which cannot be more than $5,250 for it to be excluded from taxes).

[11]Our measure of independent contracting income is the total amount of non-employee compensation from all 1099-MISC forms, and we winsorize values at the 99th percentile in each year. Homeownership is defined as having a Form 1098 (Mortgage Interest Statement). A Form 1098 is filed for an individual by a mortgage issuer whenever interest above $600 is paid on a home mortgage. Finally, we categorize individuals as married in a given year if they filed a

3.4 Descriptive Statistics

Table 1 presents summary statistics for all U.S. Army applicants between 1990 and 2011. Overall, applicants are young (20.7 years), mostly male (78%), and most have not attended college (93%). Relative to a nationally representative sample of 17-23 year-olds from the 2000 Current Population Survey (CPS), applicants are more likely to be Black (21% vs. 15%) and less likely to be Hispanic (11% vs. 15%). Applicants are also more likely to come from disadvantaged counties in terms of household incomes, employment, and Chetty and Hendren (2018) measures of inter-generational mobility.[12] Moreover, we show in Figure A.1 that applicants come from families with about 15% lower median family income than a comparable, national random sample.[13]

Since the bulk of our estimates will be identified by applicants near the two AFQT cutoffs, we define our analysis sample to be the subset of applicants with initial AFQT scores between 12 and 68. 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 of 4.8 years.

4 Estimating Framework

4.1 Empirical Approach

Our empirical strategy takes advantage of Army 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. The discrete jumps in the probability that applicants enlist of 10.0 percentage points at an AFQT score of 31 and 6.0 percentage points at an AFQT score of 50 (see Table A.1) underlie the first stage of our fuzzy regression discontinuity (RD) identification strategy.[14]

In our fuzzy RD design, the lower-ability (AFQT$\geq$31) and higher-ability (AFQT$\geq$50) cut scores act as instruments for our endogenous variable: an indicator variable for applicants who ever en-

1040 with a status of married filing jointly or married filing separately that year.

[12] For example, 37.9% of applicants come from the lowest population-weighted national tercile of 1990 county median household income, 37.3% come from the lowest national tercile of 2000 county employment rates, and 37.8% come from the lowest national tercile of Chetty and Hendren county-level inter-generational mobility estimates.

[13] We note that we are only able to match applicants who were aged 16 or younger in 1996 to their family incomes, so unlike the other summary statistics above which apply to all applicants (AFQT 1-99) who applied between 1990 and 2011, family income comparisons represent applicants from cohorts in the late 90s and the 00s. The notes in Figure A.1 provide additional details.

[14] Additionally, Panel (b) of Figure 1 shows the estimated dynamic reduced-form effects of scoring at or above 31 and 50 AFQT cutoffs on military service. While scoring above a threshold increases the probability of military service in every year after taking the ASVAB, these effects are most pronounced in the first few years after application. This reflects the fact that the most common length of service is one term (typically three to four years) and only a small fraction of those who join the military remain in the military for their full career.

list in the U.S. military. While we define our endogenous variable as enlistment in any military service, the vast majority of enlistees in our Army applicant sample joined the Active Duty Army. Crossing either cutoff has only modest effects on enlistment in non-Active Duty Army service, so our estimates can be reasonably interpreted as the effects of Active Duty Army service.[15] Specifically, our reduced form estimating equation is:

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

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

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

$$\text{Second Stage:} \qquad y_i = f(AFQT_i) + \beta_2 Enlist_i + \mathbf{X}_i'\gamma_2 + \epsilon_i \tag{3}$$

$Enlist_i$ is an indicator for any military service. y_i is an outcome such as earnings 10 years after application. $f(AFQT_i)$ is a 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 or 50 AFQT cutoff. We estimate effects around each cutoff separately. Additionally, X_i is a vector of pre-application characteristics, which always includes quarter-by-year of application fixed effects and additional controls when mentioned, and ϵ_i is an idiosyncratic error term.

In our primary specifications, $f(AFQT_i)$ is a quadratic function of AFQT with a bandwidth of 19 and a rectangular kernel. We allow this function to differ on each side of the cutoff. A bandwidth of 19 is the maximum symmetric bandwidth for each cutoff. Given the smoothness in our outcome variable, this choice increases power without biasing estimates, something we verify in robustness checks to alternative bandwidths. Additionally, we estimate a variety of alternative specifications that vary functional form (e.g., linear, linear with triangular kernel, quadratic with triangular kernel), bandwidth (e.g., 3, 4, ..., 19), and inclusion of demographic controls (e.g., age, sex, race, education, and home state). Robust standard errors are reported in all cases.

The parameter of interest is β_2, which identifies the local average treatment effects (LATE) of

[15] We are only permitted access to data on Active Duty Army applicants, but some of these individuals eventually join other services, which have higher minimum eligibility thresholds. Over 80% of enlistees in our sample joined the Active Duty Army, 10% joined the Active Duty Navy, Air Force, Marines, or Coast Guard, and 10% joined the Army Reserves or the Army National Guard. Crossing the 31 threshold decreases the probability of enlistment in a non-Army service by 0.5 percentage points and increases the probability of enlisting the Army Reserves or Army National Guard by 0.8 percentage points. Crossing the 50 threshold reduces the probability of enlisting in a non-Army service by 0.7 percentage points and reduces the probability of enlisting in the Army Reserves or Army National Guard by 0.9 percentage points.

military service among individuals who were near the applicable AFQT cutoff *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 whose decision to serve was contingent on whether their first AFQT score was above or below an eligibility cutoff. Because an offer of enlistment must be offered and accepted, our estimates are identified among applicants for whom their application is marginal in the Army's view (i.e. an 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 successively retake the ASVAB until a satisfactory AFQT score is realized or is only induced to serve because of being eligible for a bonus).

4.2 Validity of the Discontinuity Design

A threat to our 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.[16]

To visually inspect the running variable for manipulation 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 occurs at points where multiple raw AFQT scores correspond to a single AFQT percentile score (Mayberry and Hiatt, 1992; Segall, 2004). Unlike their percentile scores, applicants' raw initial AFQT scores are not recorded in their files. 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.[17] In comparison, Figure A.2 plots the distribution of each applicant's *most recent* AFQT score. These histograms reveal a strong effort on the part

[16] MEPCOM 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 attempts, which reduces the likelihood of this behavior. Any issues introduced by this data limitation will likely be reflected by imbalance in exogenous characteristics.

[17] As noted above, outside of retesting, it should not be possible to manipulate AFQT scores. Formally testing for manipulation of AFQT scores around the cutoffs using the methods described in McCrary (2008) or Frandsen (2017) is not appropriate in our setting because they assume continuity or local smoothness in the running variable. Instead we estimate Equation 1 on data collapsed to the first AFQT score level where the outcome is the number of applicants per AFQT score. The result of these tests do not indicate a significant discontinuity in the density at either cutoff (p-values of 0.87 at the 31 cutoff and 0.24 at the 50 cutoff).

of some applicants to achieve scores to the right of both thresholds and clearly indicate that an applicant's *most recent* AFQT score does not provide a valid running variable in an RD design.

Additionally, we examine potential manipulation across both discontinuities by testing for balance in observable characteristics across the cutoffs. Specifically, we examine balance in characteristics such as race, education, age, 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.[18] Panels (c) through (f) of Figure 2 plot averages for certain baseline characteristics by AFQT score, with additional covariate balance plots located in Figure A.3. There does not appear to be any substantial imbalance across either cutoff.

We complement these figures with Table 2, which reports estimates of Equation 1 where the dependent variables are the baseline characteristics. Table 2 confirms the balance in covariates across AFQT cutoffs shown in Figure 2. Among the 28 comparisons of covariates across low and high AFQT cutoffs, only one comparison at the low AFQT cutoff–whether an applicant attended at least some college–is significant at the 5% level (with two at the 10% level). Furthermore, joint tests of significance at both the low and high cutoffs (p-values of 0.32 and 0.90, respectively) are consistent with balance. Altogether, the results in Table 2, especially when combined with the lack of observable manipulation in the AFQT histograms, argue against the possibility of systematic sorting around either threshold.

5 Effects of Army Service

We begin this section by presenting the effects of enlistment on earnings and employment for all Army applicants between 1990-2011. We also report effects of service on additional mediators of earnings, including education, mortality, and disability. These overall estimates mask substantial heterogeneity by race. In Section 5.2, we show that Black enlistees experience differentially large long-run increases in earnings, gains that are also reflected in increases in homeownership and marriage. In contrast, Army service produces insignificant, small, and sometimes negative, long-run earnings effects for White applicants. These racial differences in the effects of service on earnings are not explained by differences in effects on education or disability. We end this section by demonstrating robustness of earnings estimates and then turn to a broader assessment of the potential channels for the Black-White gap in effects of Army service in Section 6.

[18]IRS outcomes in the year prior to application are available for the 2000-2011 applicant cohorts.

5.1 Effects of Army Service on Labor Market Outcomes

5.1.1 Earnings and Employment

We begin by exploring the relationship between earnings and first AFQT score in Figure 3. Panels (a)-(d) show that earnings increase discontinuously at both AFQT cutoffs 1, 5, 10, and 19 years after application.[19] The size of the jump at each threshold — the reduced form — divided by the corresponding first-stage in Table A.1 yields a 2SLS estimate of the effects of Army service. We estimate these 2SLS RD effects of enlistment (β_2 in Equation 3) on earnings in each year relative to application separately for each cutoff and plot the resulting coefficient estimates in panel (a) of Figure 4. Each point along the dashed black line [solid gray line] corresponds to the 2SLS estimate of Army service on earnings in the stated year relative to application at the 31 AFQT cutoff [50 AFQT cutoff].[20] At the 31 AFQT cutoff, Army service has large positive effects on annual earnings of approximately $11,000 in the first three years after application, positive effects of approximately $3,000 between 5-14 years after application, and smaller and statistically insignificant positive effects of approximately $2,000 15-19 years after application. Relative to the 31 AFQT cutoff, the effects at the 50 AFQT cutoff are broadly similar in magnitude between 1-14 years after application and are approximately $3,000 larger 15-19 years after application (effects that are significant but not statistically distinguishable from those at the lower cutoff).[21]

Panel (b) of Figure 4 plots 2SLS RD estimates of enlistment on employment, defined as having positive W-2 Medicare wages. In the first 1-3 years after application, enlistment increases employment by an average of 6.9 percentage points at the lower cutoff and 6.4 percentage points at the higher cutoff. In the medium- and long-run, the positive effects of Army service on employment dissipate at both cutoffs and are not distinguishable from zero.

Even though we find that Army service has no long-run employment effects and generally positive earnings effects, it is possible that these average effects mask diverging outcomes for those who are helped and harmed by Army service. In particular, the large potential risks (disruption, injury, death, etc.) and rewards (training, healthcare, education) could increase *both* the probability of being at the top and the probability of being at the bottom of the earnings distribution. However, in Figures A.7-A.8 we assess how service affects one's place in a nationally representative

[19] Figure A.4 plots the relationship between AFQT scores and earnings for every year -1,0,...,19 relative to application.

[20] Every estimate underlying Figure 4 is reported in Table A.2. Given that IRS data starts in 1999 and ends in 2018, these estimates are based on an unbalanced panel of application cohorts. In particular, estimates are based on later application cohorts in the short-run (e.g., 1999-2011 in year 0), almost all 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 Figure A.5 panel (a)). We explore heterogeneity by application cohort later in this Section.

[21] To examine other forms of earnings, Figure A.6 panel (a) shows independent contractor earnings (any independent contractor earnings follows a similar pattern). Service has no effect on contractor earnings in the first 10 years at the lower cutoff and a small negative effect at the higher cutoff. In the long run, service has a positive effect on contractor earnings at the lower cutoff (averaging around $300 15-19 years out) and little effect at the higher cutoff. Overall, independent contractor earnings would add at most a small bump to earnings at the 31 cutoff.

earnings distribution and do not find evidence of this kind of dispersion.[22]

To better understand how enlistment impacts cumulative earnings and employment, Figure 5 and Table 3 report cumulative overall outcomes (0-19 years after application) and cumulative long-term outcomes (11-19 years after application). We measure an individual's cumulative outcome as her average annual earnings or employment rate over the specified years after application (e.g., 0-19 or 11-19), thus making our cumulative estimates comparable to the year-by-year estimates in Figures 3–4. Because we have a limited number of earnings records for each applicant—tax record availability begins in 1999 and ends in 2018—we weigh each individual by the number of years they can be observed in each of our aggregate estimates. Panel (a) of Figure 5 shows that average annual earnings 0-19 years after application increase at both cutoffs. The corresponding 2SLS RD estimates exceed \$4,000 per year (see panel (a) of Table 3).[23]

Estimates of cumulative earnings 11-19 years after application suggest that Army service increases long-run earnings at both cutoffs, although long-run cumulative estimates are typically smaller and less precise than overall cumulative estimates. As reported in column (2) of Table 3, Army service increases annual earnings at the 31 AFQT cutoff by \$2,000 in panel (a) (statistically insignificant) or, when using log earnings, by approximately 16% in panel (b) (significant at the 10% level). At the higher AFQT cutoff, service increases long-run earnings by over \$4,000 annually or 17% in panel (b) (both significant at the 10% level). While we observe positive effects of Army service on earnings at both cutoffs, panel (c) of Table 3 shows that enlistment affects neither overall nor long-run average employment at either AFQT cutoff.

5.1.2 Education, Mortality, and Disability

We turn next to the effects of Army service on education, mortality, and disability. These outcomes are particularly relevant to Army service given servicemembers' exposure to conflict and access to unique veterans' education and disability benefits. Important in their own right, these outcomes directly impact earnings potential and help contextualize our earnings estimates.

Post Secondary Attendance. Panel (a) of Figure 6 plots the effect of enlistment on post-secondary attendance, defined as having a 1098-T in the given year. In the short term, enlistment is a substitute for education with enlistment decreasing post-secondary attendance by 8.2 percentage points at the 31 cutoff and 9.1 percentage points at the higher cutoff. In the long run, however, the Army significantly increases the probability of attending college at both cutoffs. Together, these educa-

[22]These figures take a random sample of U.S. earners and construct within gender, birth cohort, and tax year earnings percentiles, conditional on positive earnings. We merge these percentile cutoffs to our applicant data on gender, birth cohort, and tax year. Then, we estimate the effect of service on having zero earnings and, separately, earnings in each of the four quartiles (0-25,25-50,50-75,75-100) of positive national earnings.

[23]Table A.3 shows that OLS estimates are systematically larger than our 2SLS RD estimates, even with fixed effects for AFQT. This is consistent with applicants positively selecting into military service on unobservable dimensions or the average effect of Army service on earnings being larger than the LATEs measured at the 31 and 50 AFQT cutoffs.

tion patterns explain part of the estimated short-run earnings effect as well as some of its medium-run dip, when soldiers are disproportionately likely to be enrolled in college. Most importantly, in Panel (a) Table 5, we find that Army service increases the overall probability of ever attending college by 14.7 percentage points at the 31 AFQT cutoff and 20.2 percentage points at the 50 AFQT cutoff. However, in Table A.4, which examines attendance in National Student Clearinghouse records for 1999-2011 application cohorts, we find that a vast majority of affected applicants are attending minimally selective institutions.[24] Additionally, this increased attendance only translates into significant increases in associate's and bachelor's degree attainment at the 31 cutoff (3.8 percentage points and 3.4 percentage point increases, respectively), and even at the 31 cutoff, the effects on degree attainment are much smaller than effects on enrollment . Using estimates of the returns to Associates degrees from Jepsen et al. (2014) and bachelor's degrees from Ashworth and Ransom (2019), these increases in degree attainment would predict an increase in average earnings of about $650 at the 31 cutoff.[25] Furthermore, recent evidence from Barr et al. (2021) suggests that the returns to college attendance among recent veterans may be substantially lower than has been measured in other contexts, perhaps driven by disproportionate attendance at low value-added institutions.

Mortality. Given the potential combat and training risk faced by servicemembers, in Table 4 we estimate the effect of service on mortality within 1, 3, 5, 10, 15, and 19 years of application. While mortality at a young age is a rare event and leads to noisy estimates relative to their means, we do not find evidence that the Army significantly increases mortality at any point after application at either cutoff. At the 31 cutoff there is some evidence that the Army service may reduce mortality in the first few years after application, but the estimates become more imprecisely estimated and statistically indistinguishable from 0 in later years. At the 50 cutoff point estimates hint at reduced long-term risk, but we are wary to draw any conclusions at either cutoff given the lack of precise effects. Overall, our findings in Table 4 suggest that mortality is not significantly affected by Army service and, therefore, unlikely to be a meaningful driver of our observed earnings and employment effects.

Disability. While we do not find effects of Army service on mortality, service could still affect disability and disability compensation, especially given the presence of veteran-specific disability

[24]Some higher education institutions do not issue Form 1098-T for students who do not pay tuition, which makes use of the National Student Clearninghouse data a useful robustness check. Additionally, while not shown, defining attendance as receiving a 1098-T, pell grants, or GI bill tuition assistance to produce a more comprehensive measure of attendance produces extremely similar results as just using Form 1098-T, with a negative effect on receiving pell grants offset by a positive effect on GI benefits.

[25]Jepsen et al. (2014) find that an associate's degree is worth about $6,000 per year for men and $9,200 per year for women. Applying these numbers to our sample, an associate's degree is worth approximately $6,700 per year. Therefore, the increase in associate's degrees of 3.8 percentage points accounts for $254 dollars. Ashworth and Ransom (2019) estimate a college graduation wage premium of approximately 45%. Given average earnings of $24,830 around the 31 cutoff, we would expect an increase in earnings of approximately $11,175 for degrees. Therefore, the 3.4 percentage point increase in bachelor's degrees accounts for a $380 increase in average earnings.

benefits. Increases in disability could lower earnings potential. 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 (Autor et al., 2016; Coile et al., 2019). Panel (b) of Figure 6 reports estimates of the effect of enlistment on our measure of annual disability compensation: the sum of VADC, SSDI, and SSI payments.[26] The effect of enlistment on disability compensation is nearly identical at both cutoffs, reaching $2,000 per year by seven years out and steadily increasing to over $3,000 in later years.[27]

On the extensive margin, enlistment increases disability receipt by an average of 17 percentage points at the 31 cutoff and 15 percentage points at the 50 cutoff 5-19 years after application (see panel (a) of Figure A.9).[28] When we examine whether individuals ever receive disability at any point, panel (b) of Table 5 shows that Army service increases the probability of receiving disability compensation by 25 and 26 percentage points at the 31 and 50 cutoffs, respectively. Importantly, however, we find much smaller effects on the receipt of compensation for significant disability, which we define as receipt of SSDI, SSI, or a VA determination that a veteran is 100% disabled or is eligible for Individual Unemployability status. Panel (b) of Figure A.9 shows that while point estimates are usually positive at both cutoffs, these estimates are generally below 3 percentage points and only occasionally statistically significant.[29]

Large effects on disability compensation could reflect negative health consequences of military service (Stiglitz and Bilmes, 2008) but could also be driven by differences in screening and compensation between veteran and civilian disability programs (Angrist et al., 2010). To this point, non-work-limiting VADC that is exclusively available to veterans explains the majority of our disability receipt results. Nonetheless, whether through health effects, income effects, or both, increased disability compensation is likely to exert a drag on employment and earnings (which do not include disability compensation). For example, Autor et al. (2016) estimate that each additional dollar of VADC has a marginal propensity to reduce earnings by $0.26, which would imply that long-term increases of $3000 in disability payments might reduce average earnings by roughly $800.

Thus far, estimates from our full sample suggest Army service increases earnings in the short run and has smaller, but still positive, effects 11 to 19 years after application. In the long run, any potential returns to increased educational attainment may be offset by reductions in earnings

[26] Data use agreements prevent us from linking VA, SSA, and NSC data to IRS data. Thus, estimates from these outcomes are from 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. To keep our samples consistent across outcomes, we extrapolate to years 2016-2018 using 2015 values, adjusting for inflation.

[27] Consistent with benefit expansions in recent years, these effects are larger for later cohorts (see Section 5.3).

[28] Notably, these estimates, while reflective of a different population and treatment effect, are more than 3 times as large as the estimated impact of Vietnam-era military service on disability receipt (Angrist et al., 2010).

[29] Panel (b) of Table 5 indicates that Army service increases the probability of ever receiving compensation for a significant disability by 4 percentage points at both cutoffs. Subsequent panels of Figure A.9 show some evidence that enlistment increases SSI or SSDI receipt more than 5 years after application.

due to disability, although other factors are likely contributing to the effects of Army service. Indeed, no discussion of potential channels will be complete without an understanding of the stark heterogeneity in the effects of Army service by race, which we turn to now.

5.2 Racial Differences in the Effects of Military Service

Research indicates that Black Americans tend to grow up in places with limited economic opportunity, face worse economic prospects than other Americans, and are especially vulnerable to entering the labor market during a recession (for recent evidence see, e.g., (Chetty and Hendren, 2018; Chetty et al., 2020; Schwandt and Von Wachter, 2019)). We begin this subsection by showing evidence that racial disparities in economic opportunities and prospects extend to Army applicants in our sample. We then demonstrate that Army service especially benefits Black servicemembers and helps to close earnings gaps. Next, we investigate whether our results can be explained by factors that are correlated with race such as prior education, county economic conditions, parental earnings, or other applicant characteristics.

5.2.1 Differences in Opportunity by Race

Black applicants come from households with lower earnings, apply from counties with worse economic conditions, and have lower counterfactual earnings trajectories than similar White applicants. Figure A.10 shows that the median family income for Black applicants at age 16 is only 55% as large as the median family income for White applicants ($34,780 for Black applicants, $64,240 for White applicants), with AFQT scores explaining only a fraction of this family income gap. These differences also hold for compliers: Table A.5 shows that Black compliers at both cutoffs grew up in households with lower parental income than White compliers. Similarly, Table A.5 shows that Black applicants (and compliers) in our sample tend to come from counties with higher poverty rates, higher shares of single parents, higher population densities, and lower employment rates than White applicants (and compliers).[30]

Finally, following the method described in Abadie (2002), Figure 7 reports estimates of counterfactual economic trajectories for Black and White compliers in our sample.[31] We find that, in the absence of military service, Black compliers at both cutoffs would have earned roughly $8000-$12,000 less per year than White compliers at the same AFQT cutoff 11-19 years after application. To put this into context, $10,000 is about 40% of the unconditional (i.e. not adjusted for AFQT)

[30]More generally, the table shows that the characteristics of black compliers are generally similar to the characteristics of all black applicants near their respective cutoffs, and we see similar patterns when we compare white compliers to white applicants near their respective cutoffs.

[31]We estimate potential outcomes for untreated compliers (i.e., average outcomes for compliers who do not enlist) by estimating our 2SLS model (Equation 3) with $-y_i(1 - Enlist_i)$ as the dependent variable. Formally, this recovers the expected value of earnings among compliers who do not enlist.

Black-White average earnings gap among 3039 year-old men.[32] These differences in counterfactual trajectories for compliers extend to other important outcomes as well (see Figure A.11). For example, non-enlisting Black compliers also have about 18-30 percentage point lower marriage rates and 15-20 percentage point lower homeownership rates. We now examine how Army service differentially impacts economic outcomes for Black and White applicants.

5.2.2 Earnings and Employment

In Figure 8, we show the dynamic 2SLS RD effects of enlistment on earnings for Black and White applicants. In panel (a), we show that the effects of Army service differ by race at the 31 cutoff, with the Army having more positive effects for Black applicants in each year following application. In the first 3 years following application, the effects of service are positive for both groups but significantly larger for Black applicants. Between 4-10 years after application, the Army increases Black applicant earnings by close to $4,000 a year but has no effect on White applicant earnings. In the long run (11-19 years after application), the effects of Army service grow for Black applicants and average over $5,000 a year whereas the Army decreases earnings of White applicants by over $3,000 a year. At the 50 AFQT cutoff, panel (b) of Figure 8 shows less evidence of initial differences in the effects of Army service, but the effects begin to diverge over time. In the 11-19 years after application, the Army increases earnings for Black applicants by an average of almost $15,000 but by only around $4,000 for White applicants.[33] Divergent effects of Army service for Black and White applicants are also reflected in employment rates. Panels (c) and (d) of Figure 8 show that Black applicants do not experience reductions in employment as a result of service at either cutoff, while White applicants at the lower cutoff experience large employment reductions.

When we plot cumulative overall (0-19 years) and long-run (11-19 years) earnings by AFQT score separately by race in panels (a)-(d) of Figure 9, the large increases in earnings at both cutoffs for Black applicants stand in contrast to the much smaller changes in earnings observed for White applicants. In panels (a) and (b) of Table 6, our estimates indicate that enlistment increases the overall average earnings of Black applicants by $6,000 and $12,000 dollars per year at the low and high AFQT cutoff, respectively. In contrast, enlistment does not significantly increase the earnings of White applicants at the lower AFQT cutoff and only increases average earnings by $4,000 at the higher AFQT cutoff. In the long run, enlistment has persistent positive effects on earnings for Black applicants at both cutoffs of around $5,500 and $15,000 per year at the low and high cutoff, respectively. Long-run estimates for White applicants are indistinguishable from zero but suggest that Army service may reduce earnings at the lower cutoff by around $3,000 and increase earnings

[32] The 2018 American Community Survey (ACS) reveals that average annual earnings for 30-39 year-old Black men is $31,900 while the average annual earnings for 30-39 year-old White men is $57,600 (Census, 2018).

[33] We would like to be able to explore outcomes for other racial and ethnic groups but power issues limit our capacity to do so. For example, 95% confidence intervals for earnings effects among Hispanic applicants, who are the next largest racial/ethnic group in our sample, span over $30,000 at the 31 cutoff and $46,000 at 50 cutoff.

by around $4,000 at the higher cutoff.

Altogether, our long-run cumulative results point to a Black-White gap in the effects of long-run Army service on earnings of $8,500-$11,000 per year and these differences by race are statistically significant at both cutoffs. This heterogeneity is notable for two reasons. First, while our long-run estimates of earnings effects for White applicants at the lower cutoff are consistent with findings from Angrist (1998), our results for Black applicants suggest that Army service in the All-Volunteer Era substantially increase long-term earnings for at least some individuals, a contrast to Angrist (1998). Second, the $8,500-$11,000 difference in estimated earnings effects is sufficiently large enough to close the entire counter-factual earnings gap among untreated compliers over the same period at the higher cutoff and to close 90% of the gap at the lower cutoff (Figure 7).

Table 6 panels (c) and (d) show that differential effects of enlistment on employment can account for some of Black-White gap at the lower AFQT cutoff, but not at the higher cutoff. At the lower AFQT cutoff, Army service has a positive, but insignificant, effect on average employment for Black applicants of 2.6 percentage points and a statistically significant negative effect on employment for White applicants of 4.2 percentage points. In the long run, the Black-White employment gap becomes larger as the effects of enlistment on employment for White applicants become more negative. In contrast, at the higher cutoff, we observe no differences in the effect of enlistment on employment, which we estimate to be around 5 percentage points for both races.

We also explore how the effects of enlistment vary when we partition our sample by both race and gender (Figure A.12). Although sample sizes are generally too small to make firm conclusions, the effects of Army service on earnings trajectories for Black women appear similar to Black men at both the 31 and the 50 cutoff. White women also appear to have similar point estimates to White men at the 31 cutoff and higher (though not statistically significant) long-run earnings estimates at the 50 cutoff.

5.2.3 Are the Heterogeneous Effects of Service by Race Reflected in Other Dimensions of Opportunity?

The stark differences in the long-run effects of Army service by race raise the question of whether and to what extent these differences are reflected in other observable correlates of opportunity Two exercises suggest that the differences in the effects of Army service by race are not easily explained by other pre-application characteristics.

First, in Table 8, we implement a propensity score reweighting approach that reweights Black applicants along observable characteristics to more closely resemble those of White applicants. Specifically, we rerun our 2SLS estimates for Black applicants using inverse probability weights constructed from a logit regression of Black and White applicants in which the dependent variable is a dummy for being a White applicant. The logit regression is estimated using applicant age, gender, initial education, county-of-application characteristics—including measures of poverty, employment, median income, and single-parent household shares—and when possible, applicants' childhood household information from 1040 filings such as family income and whether they lived in a single-parent household. Table 8 panels (a) and (b) show that reweighting Black applicants along this rich set of characteristics barely moves our 2SLS estimates.

Second, in Table 8 panels (c) and (d), we estimate a model that allows for the effects of Army service to differ by both race and a pre-application proxy for economic disadvantage. Using the same set of covariates as above, we construct an economic 'disadvantage index', defined as the additive inverse of predicted average earnings 11-19 years out, in standard deviations. We construct predicted earnings using applicants just to the left of each threshold (i.e. those with an AFQT score of 30 or 49) and a leave-one-out procedure to avoid introducing endogenous stratification in finite samples (Abadie et al., 2018). For all Black and White applicants in each RD window, we then estimate a 2SLS model that instruments for Enlist $\times$ Black and Enlist $\times$ Disadvantage with $\mathbb{1}(AFQT \geq CUT)\times$Black and $\mathbb{1}(AFQT \geq CUT)\times$ Disadvantage. Columns (2) and (4) show that while individuals with lower predicted earnings do appear to benefit somewhat more from Army service (statistically significantly at the higher cutoff but not the lower), the inclusion of the index and its interaction with Enlistment does very little to, and has a smaller magnitude than, the estimated differential effect of Army service for Black servicemembers. While our measure of disadvantage captures relevant aspects of economic opportunity—those with higher measured disadvantage do appear to benefit more than those with higher measured advantage—the covariates we use in these two exercises cannot capture all facets of economic opportunity. Nevertheless, it appears that the differences in the effects of Army service by race are not captured by available covariates.

5.2.4 Racial Differences in Education, Disability, and Other Outcomes

Here we investigate whether the large Black-White differences in effects of Army service on earnings are reflected in other outcomes. In particular, we examine whether racial differences in the effects of service on education or disability could contribute to the disparity in earnings estimates.

Post Secondary Attendance. One potential explanation for the differential earnings effect by race is a differential effect on college outcomes such as college attendance, college quality, and graduation by race. In Panel (a) of Figures 10 and A.13, we explore differences by race in the effects of Army service on college attendance, as measured by Form 1098-T tax records. These figures suggest that enlistment has similar dynamic effects on college attendance for Black and White applicants at both cutoffs. In panels (a) and (b) of Table 7, we examine aggregate education outcomes, including graduation and attendance, separately by race. We find that the overall effects on attendance are similar by race. Additionally, we are unable to detect differences in graduation rates across race at either cutoff, though point estimates are higher for Black applicants. Lastly, in Table A.6 we find that much of the increase in attendance among Black applicants at the 31 cutoff appears to be driven by attendance at for-profit colleges, institutions with a poor record of delivering financial benefits for students (Cellini and Turner, 2019). Altogether, our findings suggest that postsecondary education is only likely to be a minor factor in the Black-White gap in the effects of service, a point that we return to in more detail in Section 6.

Disability Compensation. Another possible contributor to the Black-White gap in the returns to Army service is a differential effect of service on disability compensation. In panel (b) of Figure 10 we examine the dynamic effects of service on total disability compensation at the 31 cutoff by race and find that the effects of enlistment for Black and White applicants are indistinguishable. At the 50 cutoff, panel (b) of Figure A.13 shows that enlistment increases disability compensation by a greater amount for White applicants relative to Black applicants in the medium run (5-10 years), but that this difference largely dissipates in the long run. In terms of significant disability, panel (c) in Figure 10 and A.13 shows that effects on significant disability tend to be somewhat larger for white applicants in the medium- and long-run, although these differences are never statistically significant. Table 7 panels (c) and (d) largely confirm that there are, at most, minimal differences in the effect of service on disability receipt and compensation by race.[34] Although unlikely to be a primary contribution, in Section 6 we discuss the extent to which differences in disability receipt could contribute to the differential effect of service on earnings by race.

Homeownership and marriage Before concluding this section, we briefly examine whether the effect of Army service differs by race for several additional outcomes including homeownership and marriage in panels (d) and (f) of Figures 10 and A.13.[35] Army service may increase homeownership through the VA loan guarantee program—a program that reduces the costs of homeownership by removing the requirement of a down payment or private mortgage insurance and potentially lowering interest rates and other costs—or through greater income stability and earnings gains, especially early on in one's career. To the extent that Black individuals in our sample face greater barriers to homeownership than White individuals (e.g. Charles and Hurst, 2002), we may find larger effects of service on homeownership among Black applicants. Indeed, in panel (d) of Figure 10 we find that enlistment increases long-run homeownership by approximately 15 percentage points for Black applicants at the 31 cutoff, but has no effect among White

[34]The effects of service on mortality could affect how differences in disability compensation are interpreted. However, in Table A.7 we do not find any evidence of Army service affecting mortality for Black or White applicants.

[35]The effects of service on homeownership and marriage for our whole sample can be found in Figure A.6. In panel (b) of Figure A.6, we find evidence of positive long-run effects of service on homeownership and at both cutoffs. In panel (d) we find large short-term effects of service on marriage at both cutoffs. The effects on marriage dissipate somewhat in the long run (particularly at the 31 cutoff) but remain positive.

applicants. Consequently, enlistment closes most of the homeownership gap among untreated Black and White compliers at this cutoff (Figure A.11). Similarly, in Panel (d) of A.13, we find that while the long-run effects of enlistment on homeownership are positive for both Black and White applicants at the 50 cutoff, they are larger for Black applicants (though statistically indistinguishable from White applicants).

In panel (f) of Figures 10 and A.13 we compare the effects of enlistment on marriage by race at the 31 and 50 cutoffs, respectively. At the 31 cutoff, we see large short-run effects on marriage for both Black and White applicants. However, in the long run, we find that enlistment only has positive effects on marriage for Black applicants– increasing their probability of being married by an average of 15 percentage points between 5-19 years after application (or half the counterfactual 11-19 gap in Figure A.11).[36] At the 50 cutoff the effects of enlistment on marriage are somewhat higher for Black applicants in the short-run and somewhat higher for White applicants in the long run but are not statistically distinguishable. 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 among Black applicants long after most servicemembers have left the Army.

Overall, Black enlistees experience large cumulative and long-term earnings gains. These gains are accompanied by permanent increases in homeownership and, at the lower cutoff, marriage. The estimated earnings effects for Black enlistees are statistically significantly higher than those of White enlistees at both cutoffs and are not likely explained by differences in the effects of service on education or disability compensation by race. While we will explore the causes of the Black-White gap in effects of Army service in greater detail in Section 6, we now pause to more thoroughly examine our earnings results.

5.3 Robustness of Earnings Estimates.

In Figures A.14-A.19, we probe the robustness of our estimated long-run effects of enlistment (11-19 years post application) to the inclusion of demographic controls, to alternative functional forms, and to alternative bandwidths. Figures A.14 and A.15 report robustness checks for estimates using the full sample while Figures A.16-A.19 show how patterns of robustness continue when we examine the effects of service among Black and White subgroups. Specifically, in each of these figures, panel (a) shows the robustness of our main quadratic results (no controls and bandwidth=19) to each alternative bandwidth (3-18) and inclusion of controls for gender, race (Black, Hispanic, and White dummies), age, education at the time of application (still in high school, GED, high school diploma, some college or college graduate), and dummies for home of record state. Panels (b), (c), and (d), similarly show robustness to bandwidths (3-19) and inclusion of

[36] One concern is that differential effects of enlistment on marriage by race are driven by the differential filing effects. However, we see an identical gap of 15 percentage points in effect size when we condition our estimates on those who file.

controls, but for different functions of AFQT including quadratic with triangular kernel (b), linear (c), and (d) linear with triangular kernel. Panel (a) of Figure A.14 (quadratic specifications at the 31 cutoff) shows a pattern that is common among nearly all of our robustness checks: inclusion of controls has little influence on the estimated effects of service, estimates are relatively stable but become more precise as bandwidths are increased, and wider bandwidth estimates tend to show more conservative effects than narrower bandwidth estimates. Panels (b), (c), and (d) of Figure A.14, show that our estimates are also robust to similar perturbations of quadratic with triangular kernel, linear, and linear with triangular kernel functional forms of AFQT.

Additionally, we show in Table A.8 sensitivity to alternative treatments of standard errors. We do not cluster our standard errors on our running variable in our main estimates (Kolesár and Rothe, 2018). However, when we do so in columns (2) and (7) of Table A.8, we find that our estimates become more precise for our whole sample at both cutoffs, Black applicants at the 31 cutoff, and White applicants at both cutoffs.[37] Furthermore, in columns (5) and (10) we show that our reduced-form estimates (Equation 1) are robust to a conservative approach of collapsing earnings by AFQT score (grouped means). Altogether, these estimates suggest the significance of our estimates is not driven by the construction of our standard errors.

Relevance of Tax-Free Army Compensation. As in Loughran et al. (2011), we include tax-free military-specific compensation that is not captured by W-2 wages (one benefit of our administrative data on military pay)—most importantly the housing allowance—in our causal estimates of Army service. While our baseline results in Figure 4 and Figure 8 that include these wages yield the most accurate measure of earnings, we examine robustness to their exclusion. Predictably, when we omit non-taxable military compensation in Figure A.20, we find smaller effects of Army service for the whole sample and for both Black and White applicants. However, even when these tax-free earnings are excluded, we still find large and significant positive long-run effects among Black applicants. Thus, while Army-specific compensation is an important factor in accounting for the effects of Army service on earnings, excluding this form of compensation does not qualitatively change our primary findings. Note also that our estimates use pre-tax earnings and therefore do not take into account the tax-advantaged benefit of a significant portion of military compensation.

Changing Outcomes Over Time. The dynamic earnings effects in Figures 4, 6, 8, and 10 are estimated on an unbalanced panel of applicant cohorts from 1990-2011 using earnings data from 1999-2018. Accordingly, each estimate from 0 to 19 years after application is identified off of at least 10 applicant cohorts (see Figure A.5), with earlier cohorts contributing disproportionately to the long-term estimates. Indeed, our 11-19 cumulative estimates for the full sample and for Black

[37] The estimated effect of service for Black applicants at the 50 cutoff becomes slightly less precise when clustering by AFQT score, but remains statistically significant at the 1% level (p-value = .002).

and White applicants look similar when we restrict to a balanced panel of 1990-1999 applicants (see Table A.9). While applicants in the 1990s and 2000s share much in common — each risked deployment and benefited from VA Disability and GI bill benefits — later cohorts experienced increases in the frequency of deployment following the advent of the global war on terror in 2001.[38] In response, pay, incentives, and benefits also adapted. The Post-9/11 GI Bill sizeably expanded tuition reimbursements, while VA Disability spending almost quadrupled between 2000 and 2018, in part due to increased compensation (Barr, 2015; VBA, 2000, 2020). Here we ask whether we see any differences in the effects of Army service for later cohorts. While we can estimate short- and medium-term effects for post-2001 cohorts with relative precision, we are naturally unable to identify long-term effects for the most recent cohorts.

Figure A.21 compares the effect of enlistment at the 31 cutoff for 1990-2000 applicants to 2001-2011 applicants while Figure A.22 does so for the 50 cutoff. Long-run effects for later cohorts are imprecisely estimated, consistent with the 17 year effect being estimated off of a single applicant cohort (2001). Nevertheless, standard errors on the short- and medium- run estimates allow for some conclusions. At the lower cutoff, earnings estimates are higher in the first years after application among more recent cohorts (2001-2011) relative to earlier cohorts (1990-2000), consistent with expansions in Army pay, but are otherwise broadly similar thereafter. Short-run earnings estimates are also higher at the 50 cutoff, but longer-term point estimates are more negative for recent cohorts, though particularly noisy and not statistically distinguishable. In line with the large expansion in education benefits brought on by the Post-9/11 GI Bill, post-secondary attendance effects among more recent cohorts are larger at the 31 cutoff, but not distinguishably different at the 50 cutoff. Consistent both with expanded VADC benefits and greater deployment, disability receipt and compensation increase noticeably for later cohorts.[39] Regarding a longer time horizon, our setting is not well suited to draw firm conclusions about the long-term earnings effects of Army service on individuals who enlisted in the mid- to late-2000s. Further research on such servicemembers is warranted, especially considering the high levels of disability receipt among recent veterans in our sample.

[38]We consider our setting to be informative of the effects of U.S. Army service during recent periods of war. Applicants who enlisted in the 1990s risked deployments to several conflicts, including the Persian Gulf War (1990-1991), operations in Somalia (1992-1994), the war in Bosnia and Hersogovenia (1992-1995), and the war in Kosovo (1998-1999). Consistent with this, and the large share of pre-2001 enlistees who continued serving after September 11th, 38% of Army enlistees in our sample who applied prior to 2001 deployed to a combat zone, compared to 63% of Army enlistees in our sample who applied in 2001 or later.

[39]We do not report early vs. late cohort comparisons within race due to these being even noisier than the overall estimates and ultimately inconclusive. Nevertheless, point estimates are consistent with later Black cohorts having higher short-run and similar medium and long-term earnings effects as compared to earlier Black cohorts.

6 Understanding Black-White Differences in the Effects of Service

In Section 5, we documented that Black and White applicants face different economic and household trajectories: in the absence of Army service, Black applicants earn approximately $12,000 less than White applicants 19 years after application at both cutoffs (see Figure 7). Army service closes these earnings gaps. While we find some evidence that the Army disproportionately benefits Black and White applicants from disadvantaged backgrounds in Table 8, we also find that observable measures of disadvantage—including county economic conditions, household earnings, and other applicant characteristics—explain little of the Black-White gap. These results further motivate investigation into mechanisms that may underlie the differences in the effects of service by race.

In this section, we explore whether differences in retention in the Army, combat exposure, Army occupation, disability compensation, educational attainment, or access to employment can explain the Black-White gap in the effects of Army service 19 years after application. We focus our estimates at the 19-year mark because a vast majority of our sample has left the military at this point, allowing us to best assess long-run racial differences in the effects of service.[40]

Differences in Retention. The Army may pay more than some servicemembers' outside options for several reasons, including accumulation of Army-specific human capital or compensation for risk and other disamenities (Asch et al., 2010). Given that the Black complier population experiences lower counter-factual earnings (Figure 7), we might expect Black servicemembers to stay in the Army longer than White servicemembers. In Figure A.23, we estimate the effect of enlistment on retention in the Army. While the vast majority of those who enlist have left to presumably better opportunities within 19 years of applying, we do find that Black servicemembers are more likely than White servicemembers to stay in the Army for the long-run. Specifically, Black servicemembers are 4.4 percentage points more likely to be serving in the military than White servicemembers at the 31 cutoff and 13.8 percentage points more likely to be serving at the 50 cutoff.[41]

Even though Black servicemembers stay in the Army longer than White servicemembers, and it is conceivable that the Army pays a wage premium, these facts alone cannot explain the Black-White gap in the effects of service. In order to explain the gaps without any (differential) increases in post-service earnings, the Army pay premium relative to one's outside opportunity would have to be impossibly large — $303,499 at the 31 cutoff and $123,415 at the 50 cutoff, numbers that far exceed total Army pay.[42] Fundamentally, the true size of any Army pay premium among those

[40] Table A.10 catalogs potential explanations for the Black-White gap in the effects of Army service and serves as a useful guide for this section.

[41] 7.6% of Black compliers at the 31 cutoff and 25.8% of Black compliers at the 50 cutoff are still serving 19 years after application. In contrast, only 3.1% of White compliers at the 31 cutoff and 12.0% of White compliers at 50 cutoff are serving 19 years after application.

[42] Those still in the military 19 years after application are paid around $70,000 on average, which is less than the Army-civilian pay gap would need to be to fully explain our results. The $303,499 and $123,415 values are obtained by

still in the Army at 19 years of service is unknown. Yet, even a number from the higher end of the literature — $33,000 as informed by Asch et al. (2014) — would only be able to explain $1,463 of the Black-White gap at the lower cutoff and $4,559 of the gap at the higher cutoff,[43] leaving over $10,000 to explain at both cutoffs.[44] As such, enlistment must differentially increase the post-service, non-Army pay of Black veterans. The rest of this section asks which aspects of service or the transition out of service increase earnings potential.

Combat, Deployment, and Disability. Differences in the risk and trauma soldiers face could potentially impact long-term earnings potential and explain the Black-White gap in the effects of service. White servicemembers tend to be significantly more likely to serve in a combat arms branch of the Army (e.g., infantry, armor, artillery, combat engineers, and special forces) than Black servicemembers. Table A.11 shows that this is also true among compliers in our specifications. Although soldiers in combat and non-combat occupations deploy to combat zones at comparable rates (Greenberg et al., 2021), those in combat occupations may face greater exposure to the harmful consequences of war (see e.g., Cesur et al., 2013; Cesur and Sabia, 2016; Chandra et al., 2011). In Table A.11, we find that Black servicemembers are, if anything, more likely to be deployed to a combat zone at both cutoffs. This is consistent with Black soldiers deploying to combat zones at similar rates to White soldiers but serving longer than White soldiers. Nevertheless, it remains possible that White servicemembers experience heightened risk while deployed. While we find no racial differences in being killed in action (Table A.11) or in disability compensation (Figures 10 and Figure A.13), White applicants do appear more likely to be wounded in action at the higher cutoff (5.7 percentage points) and are more likely to receive compensation for a significant disability at both cutoffs (3.4-4.2 percentage points), though these differences are not statistically significant. Even if we take these differences at face value, they are unlikely to explain much of the Black-White effect gap. For example, if we make the strong assumption that those compensated for significant disability are completely incapacitated and would have received sample-average wages ($32,139 at the 31 cutoff and $37,471 at the 50 cutoff) in the absence of their significant disability, this would explain $1,093 of the gap at the lower cutoff and $1,573 of the gap at the higher cutoff (see Table A.10; Maestas et al. (2013) find that SSDI receipt reduces employment by 28 percentage points and earnings by $4,400-$5,300).

dividing the Black-White differences in earnings effect sizes by the differences in retention at 19 years after application from Figure A.23. We note further that if we attempt to explain the positive earnings effects for Black applicants, rather than the Black-White gap, we recover similarly implausible premia for Army service.

[43] If we construct our estimates using individuals whose highest-paying employer (i.e. the employer from which the applicant earned the most in that year) is the military as opposed to having any military W-2, we get similar results. With this alternate construction, staying in military explains $1,701 at the lower cutoff and $5,692 at the higher cutoff.

[44] The $33,000 Army Pay premium is from the 11th Quadrennial Regular Military Compensation (QRMC) report, which estimated in 2009 that servicemembers are paid approximately $30,000 more than similarly qualified civilians 19 years after application. To get $33,000, we adjust for the fact that QRMC inflates Army wages by approximately 6% to reflect tax-advantaged earnings of servicemembers and then we adjust for inflation using the CPI-U.

Occupations and Human Capital. Given that Black servicemembers tend to choose different Army occupations than White servicemembers (Johnson et al., 2017), another possibility is that the specific occupations Black servicemembers hold generate skills that are more relevant to non-Army occupations than the types of occupations White servicemembers hold. Table A.11 confirms that Black compliers tend to choose different occupational fields than White compliers. For example, Infantry is the most over-represented occupational group among White compliers while Quarter Master (e.g., Unit Supply, Logistical, and Culinary Specialists) is the most over-represented occupational group among Black compliers. However, it does not appear that Black applicants are systematically selecting occupations with higher expected veteran earnings. Hahn et al. (2020) provide median earnings estimates at the occupational group level 10 years after leaving the Army. Applying these occupational-group level median wages to our estimated differences in occupational choice by race in Table A.11 suggests that differences in Army occupations are unlikely to explain much of the estimated Black-White gap. Specifically, based on the median earnings of chosen occupations, we would expect future earnings among Black veterans to be $810 lower at the 31 AFQT cutoff and $1,788 higher at the 50 AFQT cutoff (Table A.10).[45] Note, however, that these results do not rule out the possibility that Black applicants have larger long-term human capital benefits from a generic Army job (relative to their civilian counterfactual job opportunities) as compared to White applicants.

Educational Attainment. Differential human capital accumulation by race could also occur after service if there are differences in utilization or returns to the Army's educational benefits. Even though we find similar college attendance effects by race in Table 7, the long-term impact on earnings could be larger for Black veterans if they attend more selective institutions, or are more likely to graduate, than their White counterparts. While estimates from NSC data reported in Table A.6 show that the effects of service on attending a more selective institution (moderately selective or higher) do not differ by race, the Army may disproportionately increase degree completion among Black applicants. If we were to take these imprecisely estimated differences in degree completion in Table 7 seriously and use the estimates of returns to associate's degrees from Jepsen et al. (2014) and bachelor's degrees from Ashworth and Ransom (2019), overall differences in degree completion could explain roughly $200 at the low cutoff and $1400 at the high cutoff.[46] Further, these

[45]We perform this exercise for eight of the ten most common occupational groupings that we are able to clearly map to Hahn et al. (2020). The 8 groups include Infantry, Combat Engineering, Field Artillery, Armor, Human Resources, Medical Specialist, Supply Administration, and Vehicle Maintenance and represent more than 80% of our complier population for both Black and White servicemembers at both cutoffs. Black Servicemembers are over-represented in Human Resources, Medical Specialist, and Supply Administration. White servicemembers are over-represented in Infantry, Combat Engineering, Armor, and Vehicle Maintenance. At the 31 cutoff, a weighted average of median earnings among these 8 occupational groups 10 years after leaving the Army is $49,778 for the occupations Black servicemembers hold and $50,588 for the occupations White servicemembers hold. At the 50 cutoff, these values are $54,292 and $52,504 for the occupations of Black and White servicemembers, respectively.

[46]Jepsen et al. (2014) find that an Associate's degree is worth about $6,000 per year for men and $9,200 per year for women. Applying these numbers to our sample, an associate's degree is worth approximately $6,700 per year. We

modest differences in earnings due to education may be overstated due to potentially low overall returns to education for veterans and particularly low returns to Black veterans due to higher rates of attendance at for-profit colleges (see Table A.6).[47]

Access to Higher Paying Employment. Thus far, differences in Army retention, combat exposure, Army occupations, and educational attainment, are unlikely to explain more than $1,970 (or 14.6%) of the $13,451 Black-White gap in earnings at the AFQT=31 cutoff and $9,364 (or 54.9%) of the $17,052 gap at the AFQT=50 cutoff (see Table A.10). This leaves at least a $7,000-$12,000 gap to explain at each cutoff, suggesting that Army service improves the post-service civilian labor market earnings of Black veterans more than White veterans. Here we show that beyond providing a relatively stable and well-paying job, the Army increases the likelihood that Black servicemembers find employment in the public sector and in higher-paying industries in the years following their service.

Compared to the private sector, public sector jobs have historically had small differences in pay by race (Ehrenberg and Schwarz, 1986; Grodsky and Pager, 2001). The Army likely increases access to these public sector jobs through government networks and by enabling many veterans to declare veteran's preference in the application process.[48] Consistent with preferential treatment in the public sector, we find that Black applicants who serve are more likely to be employed in the public sector 19 years after application (Table A.12). Though not statistically distinguishable, this does not appear to be the case for White applicants.

More generally, we also find that service increases the likelihood that Black applicants are eventually employed in higher-paying industries. We map each applicants highest-paying employer (i.e. the employer from which the applicant earned the most in that year) 19 years after application to its six digit NAICS industry code using the Employer Identification Number (EIN) on their W-2 form. We then assign each six-digit industry code to its average annual pay according to a 50 percent random sample of 32 to 44 year-old U.S. workers during the years we examine. The first two columns of Table A.13 reveal that Army service increases average industry pay among Black

retrieve our estimates for associate's degree by multiplying the Black-White differences in associate's degree attainment by this value. Ashworth and Ransom (2019) estimate a college graduation wage premium of approximately 45%. Given average earnings of $32,139 around the 31 cutoff and $37,471 at the 50 cutoff 19 years after application, we would expect an increase in earnings of approximately $14,463 and $16,862 at the 31 and 50 cutoffs respectively. We retrieve our estimates for bachelor's degree by multiplying the Black-White differences in degree attainment by these values.

[47] Barr et al. (2021) examine the expansion of education benefits in the Post 9-11 GI Bill and find that returns to college attendance among veterans are much lower than found in other settings. Cellini and Chaudhary (2014) find significantly lower returns to for-profit college attendance and Deming et al. (2016) find that employers prefer not to interview individuals with for-profit degrees.

[48] Army service allows most veterans in our sample to declare veteran's preference for public sector jobs: an entitlement that grants preferential treatment to veterans in the hiring decision. Eligible veterans in our sample for federal veteran's preference include those with a service-connected disability, and those who served before January 1992, or served between September 11, 2001 and August 31, 2010. Lewis and Pathak (2014) find that these veterans' preferences often extend to state and local government positions as well, with 46 states having systems that mirror the federal system and 4 states with even more generous veteran provisions.

applicants by \$7000-\$13,000 as compared to \$0-\$4,000 among White applicants, differences that are marginally statistically significant. We find in columns (3) and (4) a similar pattern when we exclude applicants who are not working and therefore were mechanically receiving a value of \$0. To explore whether these industry pay differences are driven by higher military retention among Black servicemembers, columns (5) and (6) then control for military service. While any such exercise that controls for endogenous variables is necessarily suggestive—though we find this exercise may not be subject to much bias—it appears to be the case that industry pay differences are driven by increases in average *civilian* industry pay for Black applicants.[49] While the Army may make it more likely that Black veterans find higher paying jobs within a given industry, it also appears to provide pathways to work in different, higher-paying industries than would otherwise have been the case.

Overall, through both a stable, well-paying job and by opening doors to future higher-paid employment, Army service offers many Black Americans a path towards upward mobility. The fact that the Army differentially helps Black Americans earn more after leaving service is not easily explained by differences in Army occupations, educational attainment, or disability compensation rates. Given the limited civilian opportunities for Black Americans both overall (Chetty et al., 2020) and in our sample (Figure 7), as well as documented racial discrimination in the labor market (Lang and Lehmann, 2012), several alternative explanations emerge — all worthy of further exploration. These include access to networks, increased geographic mobility, increased human capital not captured in occupational or educational differences (including the possibility that skills gained in Army service relative to their counterfactual experience are differentially more valuable for Black applicants), or an important credentialing effect that diminishes racial discrimination.[50]

7 Conclusion

In this paper, we exploit eligibility thresholds at the 31st and 50th percentile of the AFQT in a fuzzy regression discontinuity design to estimate the causal effects of voluntarily enlisting in the U.S. Army from 1990 through 2011. While we find that Army service increases cumulative earnings, post-secondary attendance, disability compensation, homeownership, and marriage at each cutoff, the long-run effects vary considerably by race. In contrast to White servicemembers— who do not experience statistically significant earnings gains at either cutoff 11-19 years after application— Black servicemembers see long-run earnings gains of \$5,500 and \$15,000 per year and at the 31 and

[49]Given that Black applicants are more likely to remain in Army service, controlling for military service would bias the difference in industry pay upwards (downwards) if those marginal Black applicants are negatively (positively) selected. We find no evidence that the marginal Black applicants that remain in the military are differentially selected as indicated from predicted earnings using their pre-application characteristics based off of regression models using applicants just below each cutoff. This suggests that controlling for service is not biasing our higher paying industry findings.

[50]For example, research has suggested that Black servicemembers may benefit more from veteran credentials in the private sector labor market than White servicemembers (De Tray, 1982; Kleykamp, 2009).

50 AFQT cutoffs, respectively. This gap in long-run earnings estimates for Black and White veterans, which does not appear to be driven by differences in exposure to combat, disability receipt, or educational attainment, is consistent with the Army generating access to better-paying jobs for Black veterans. While we cannot exactly identify which aspects of military service expand employment opportunities for Black veterans, future research, including audit studies, could explore whether access to networks (e.g Brown et al., 2016; Burks et al., 2015), increased human capital (not captured by educational differences), a credentialing effect that potentially diminishes racial discrimination (De Tray, 1982; Kleykamp, 2009), or other factors, drive these effects.

More broadly, over the last several decades, income inequality in the United States has been rising (Piketty et al., 2017), income mobility is slowing (Chetty et al., 2017), and the prospects for young males with limited education have been declining (Autor and Wasserman, 2013). Economic opportunities have been particularly dire for Black men (e.g., Bhattacharya and Mazumder, 2011; Akee et al., 2017; Chetty et al., 2020). Our estimates suggest that the Army can be a critical institution for improving economic mobility for Black Americans. While a large body of evidence finds that childhood environment and other pre-labor-market factors explain much of the Black-White income gap (Neal and Johnson, 1996; Altonji and Blank, 1999; Fryer, 2011; Lang and Lehmann, 2012; Chetty et al., 2020), Army service appears to offer at least one avenue during young adulthood for reducing this gap. Additionally, our results suggest that research into policies and programs that deliver some of the positive aspects of military service to the broader population—such as access to stable employment with health and education benefits—may shed light on additional approaches to help disadvantaged young adults.

Finally, to fully understand the impacts of military service, it is important to evaluate whether benefits to Black servicemembers extend to future generations. Recent evidence suggests policies designed to reduce the Black-White wage gap in a single generation might not persist through subsequent generations (Chetty et al., 2020). Yet, existing evidence on intergenerational effects of military service, while being limited to conscription-era lotteries and their associated negative shock, suggests the effects we detect may persist across generations (Goodman and Isen, 2020). Although current data limitations preclude us from investigating this, whether large earnings gains for Black veterans improve outcomes for their children should be the subject of future research.

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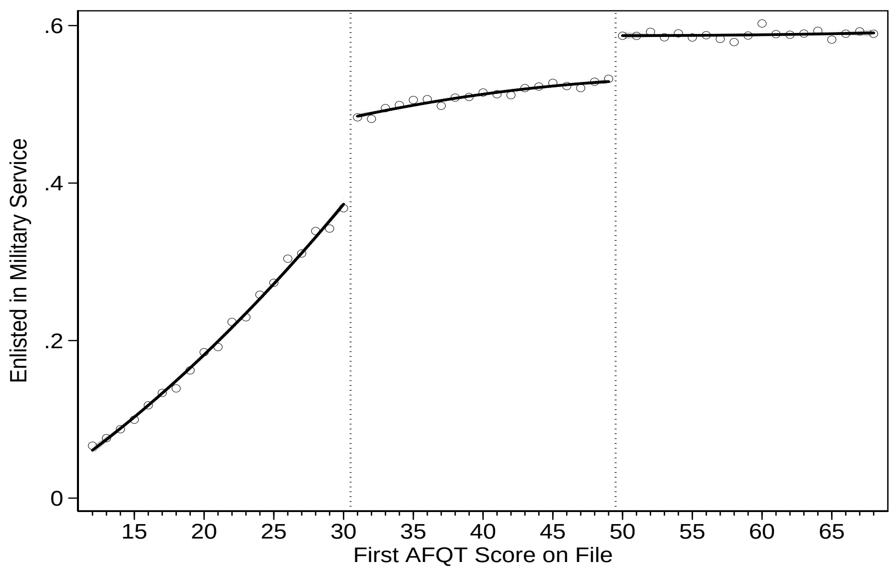
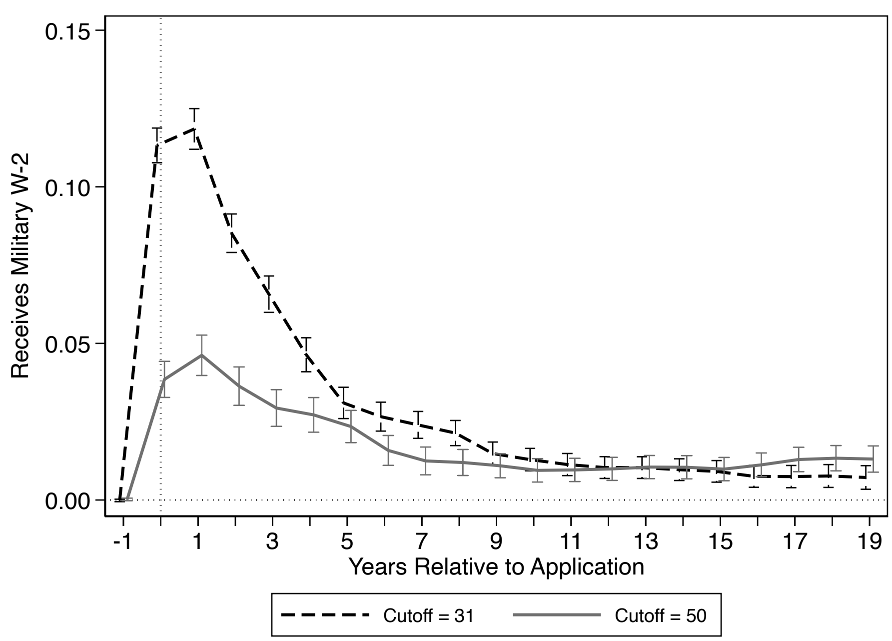
Figures

Figure 1: AFQT scores and Military Service

(a) First Stage: AFQT score and Military Service

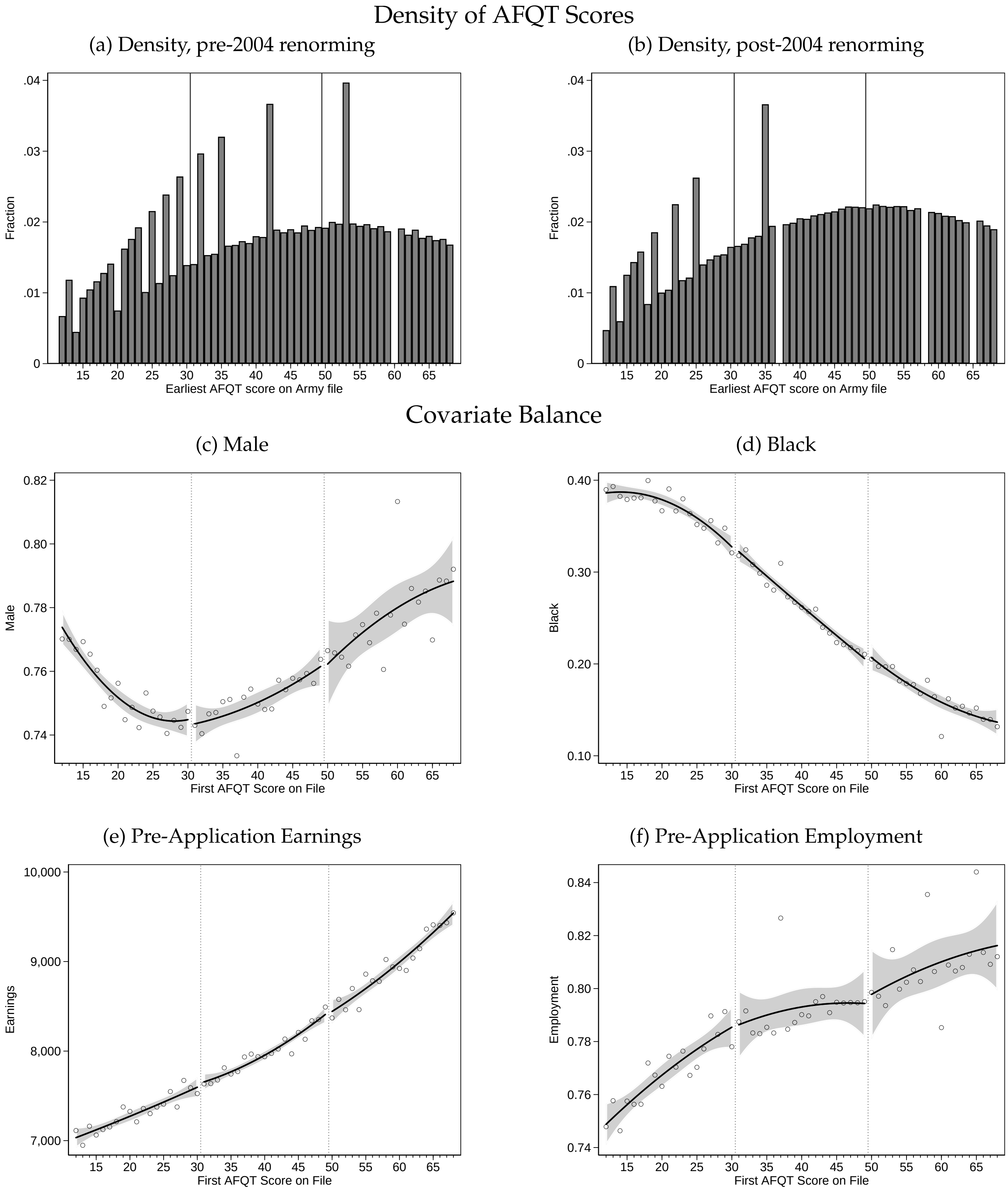


(b) Reduced Form: In Military, by Years Since Application



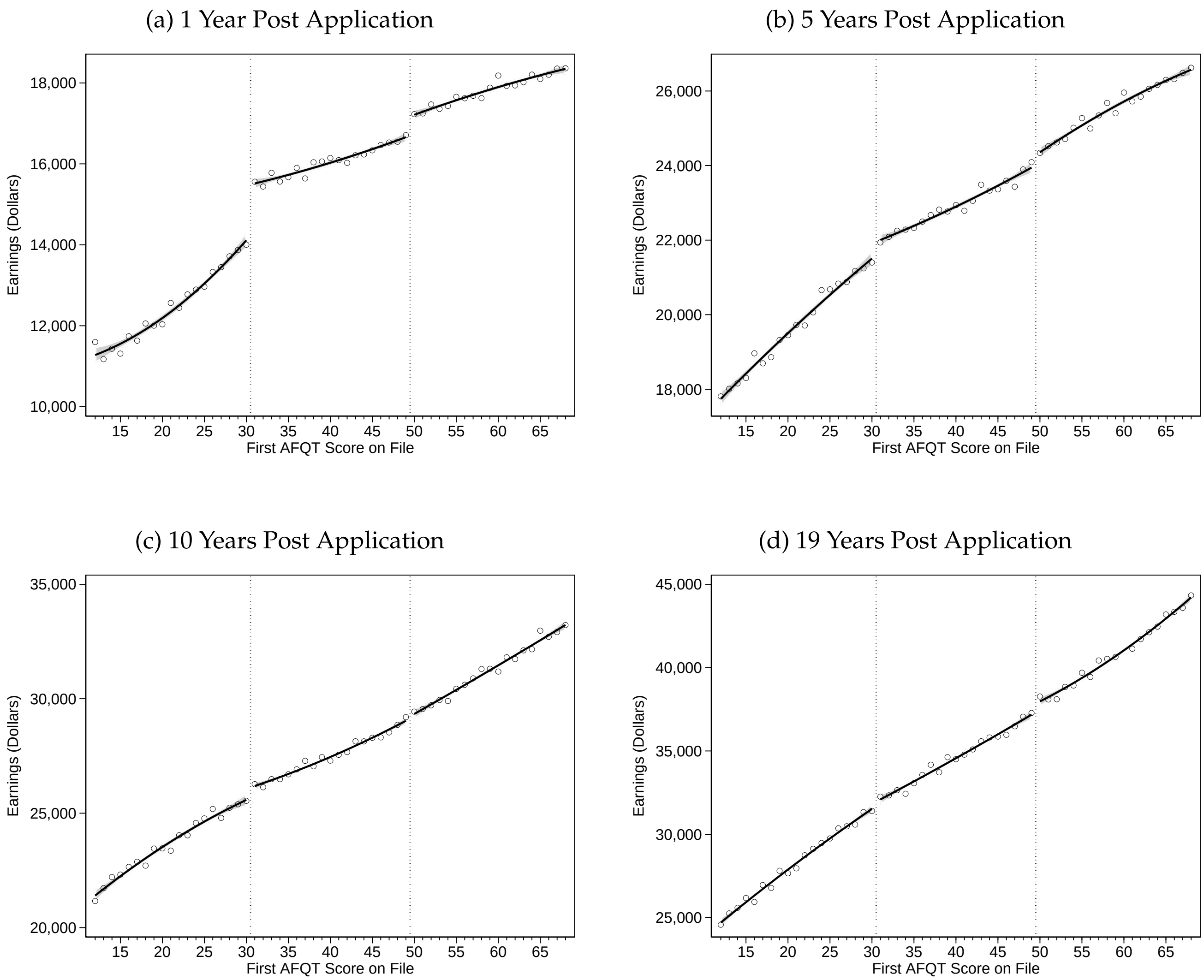
Notes: Panel (a) shows our first stage: it plots the probability of military service as recorded in the Army applicant data against applicants' earliest AFQT score on file. The two RD cutoffs at AFQT scores of 31 and 50 are indicated by dashed vertical lines. We see a clear discontinuity in the probability 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 the fraction of applicants that are male, panel (d) plots the fraction of Black applicants, panel (e) shows balance on 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 Table 2 shows the corresponding balance check regressions.

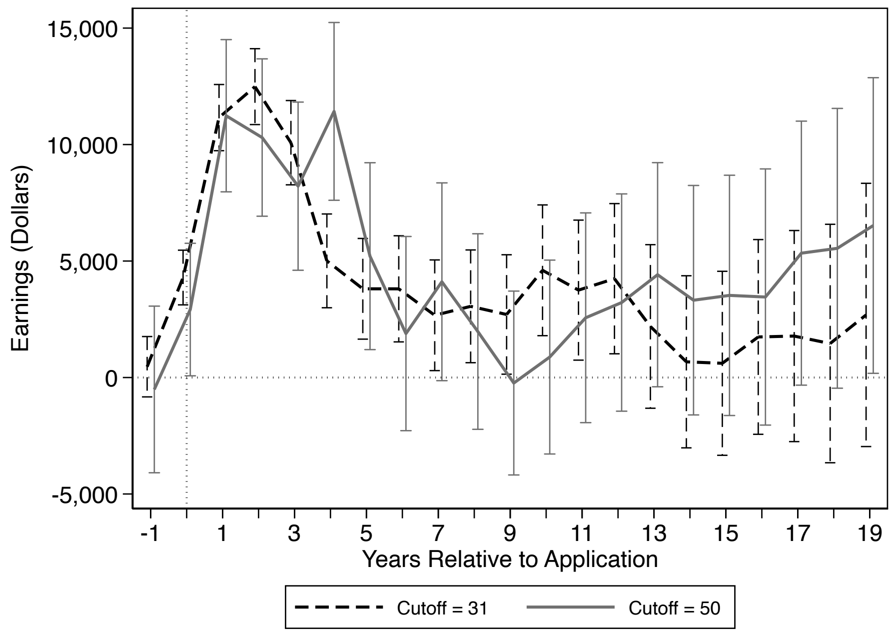
Figure 3: Reduced Form Plots: Earnings, 1, 5, 10, and 19 Years Post Application



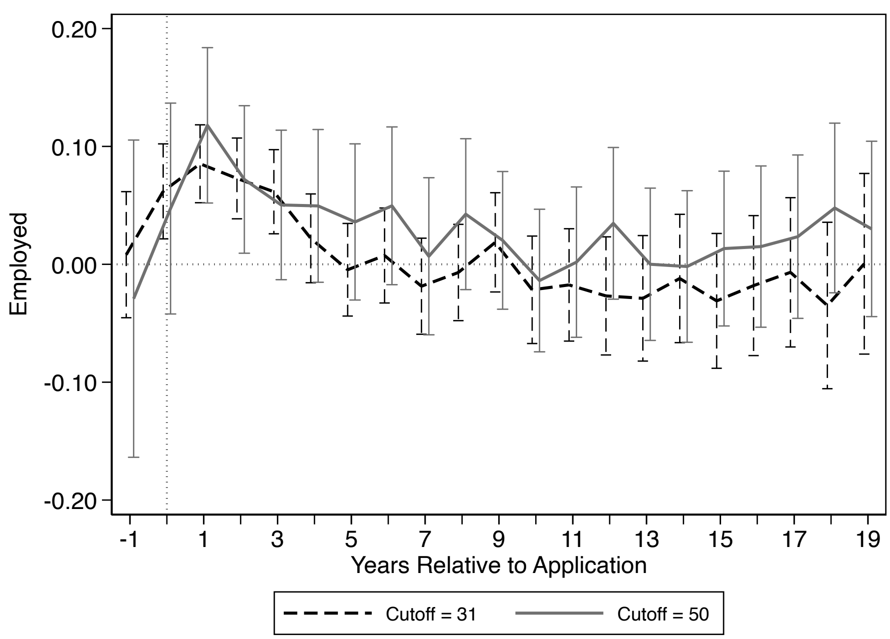
Notes: This figure plots our baseline earnings outcome 1, 5, 10, and 19 years after application as a function of the earliest AFQT score on file. Earnings are demeaned with respect to quarter-by-year of application fixed effects. Figure A.4 contains the reduced form plots for *all* years -1 to 19. Figure 4 panel (a) plots corresponding 2SLS RD estimates of the effect of enlistment on earnings for all years -1 to 19 since application.

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

(a) Earnings



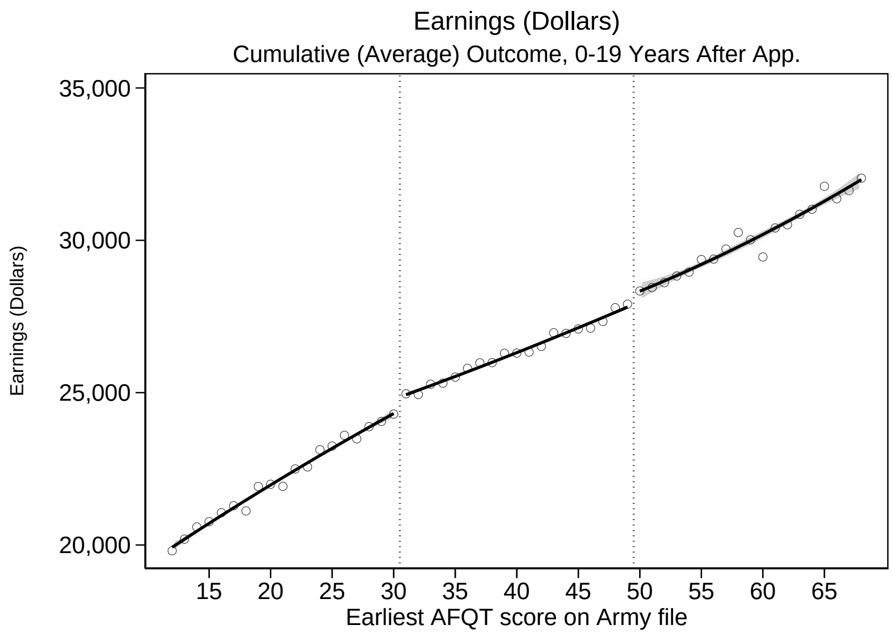
(b) Employment



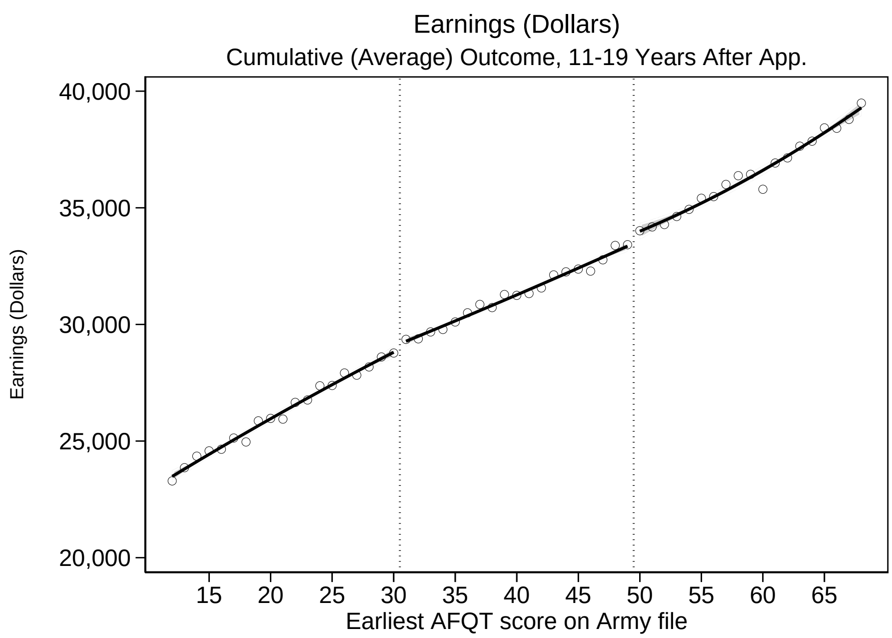
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings and employment (Equation 3). Each point along the dashed black line (or solid gray line) corresponds to a separate 2SLS RD estimate of the effect of enlistment on earnings or employment in the given number of years after applying to enlist, as indicated by the x-axis. Panel (a) plots coefficient estimates and 95% confidence intervals for earnings defined by inflation-adjusted W-2 and non-taxable military earnings (2018 dollars). Panel (b) plots coefficient estimates and 95% confidence intervals for employment as defined by Any W-2 Medicare wages. Section 3.3 and Appendix A.2 contain additional details on the construction of earnings and employment outcomes.

Figure 5: Earnings, Reduced Form

(a) 0-19 Years Post Application



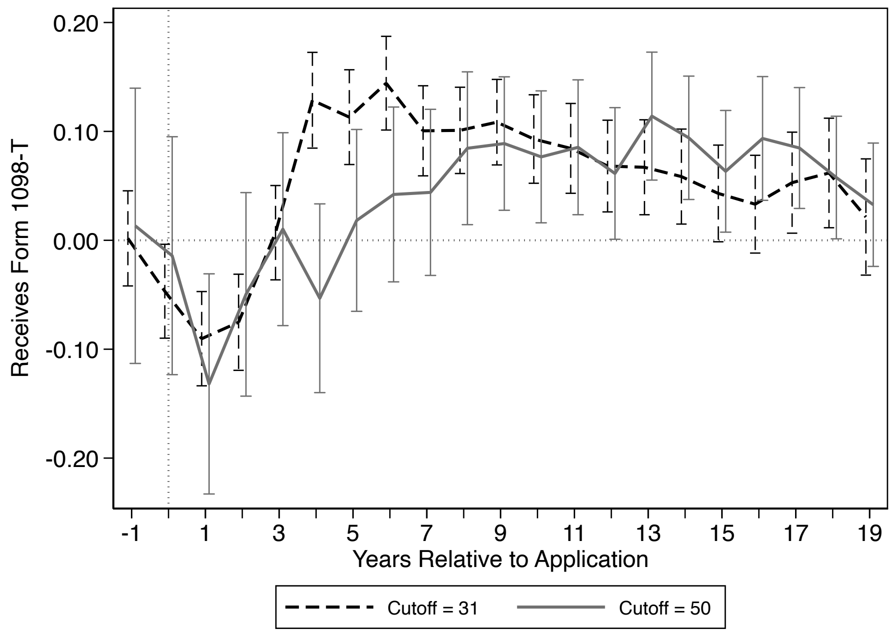
(b) 11-19 Years Post Application



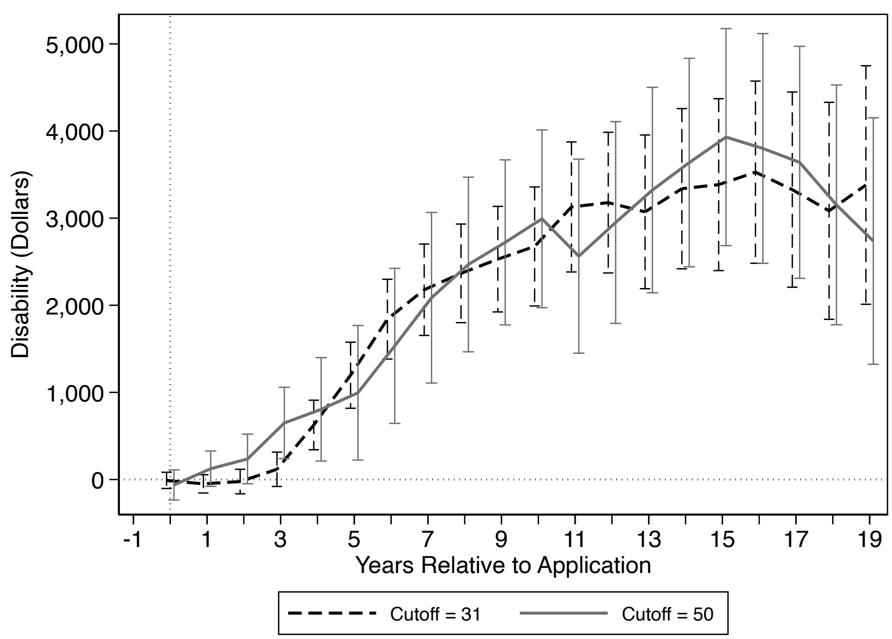
Notes: Panels (a) shows weighted average earnings between 0 and 19 years post-application by AFQT score and panel (b) shows weighted average earnings between 11 and 19 years post-application. Average earnings are weighted by the number of years the individual is in our sample, with zero wages imputed for individuals without reported earnings in a year covered by our data. Earnings are demeaned with respect to quarter-by-year of application fixed effects. 95% confidence intervals are indicated.

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

(a) College Attendance (any 1098-T)



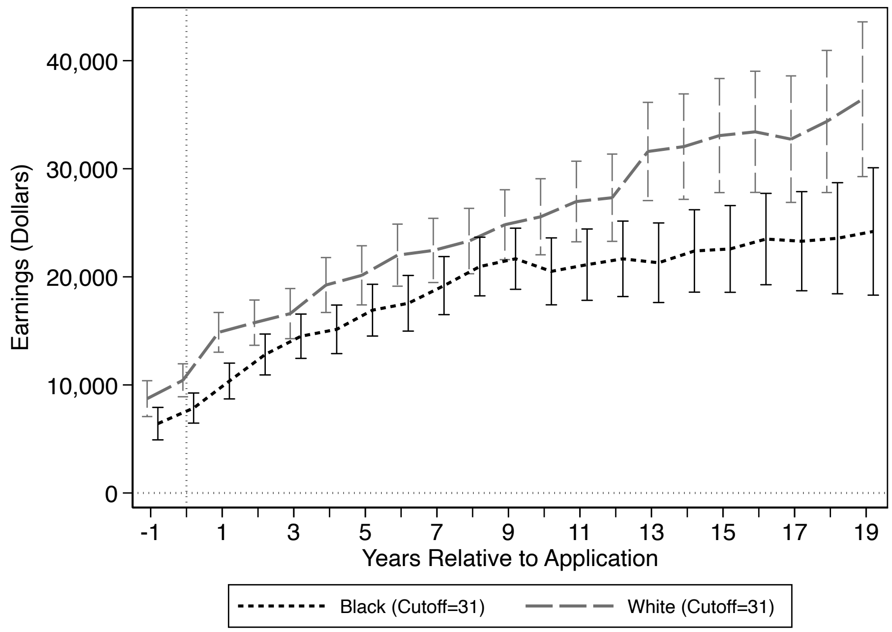
(b) Disability Comp. (VADC, SSI, and SSDI)



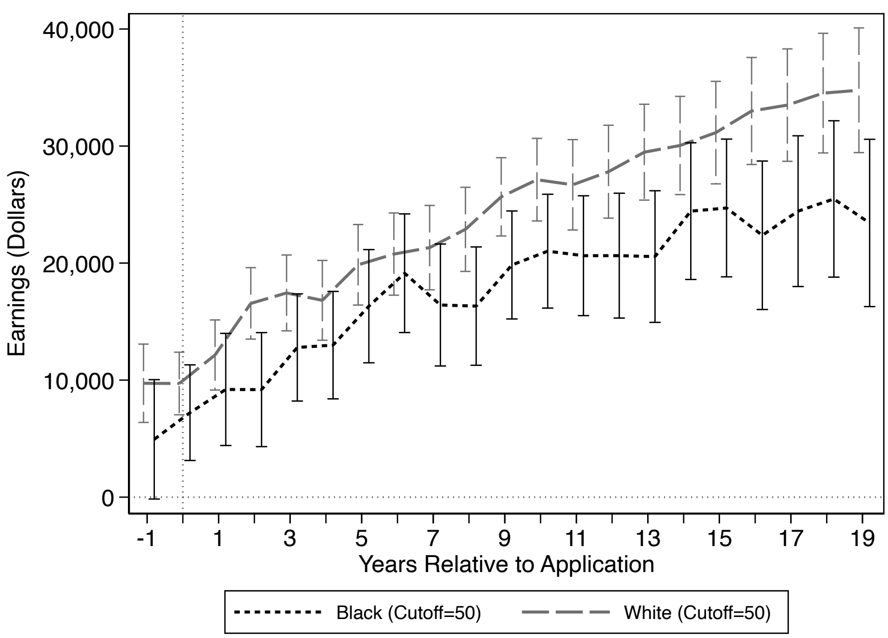
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on education enrollment and disability compensation. Panel (a) plots coefficient estimates and 95% confidence intervals for post-secondary attendance in the given year, defined as having a 1098-T on record. Panel (b) plots 2SLS RD estimates where the outcome is total disability compensation (i.e., the sum of annual VADC, SSI, and SSDI payments).

Figure 7: Counterfactual Complier Earnings

(a) Earnings, 31 Cutoff

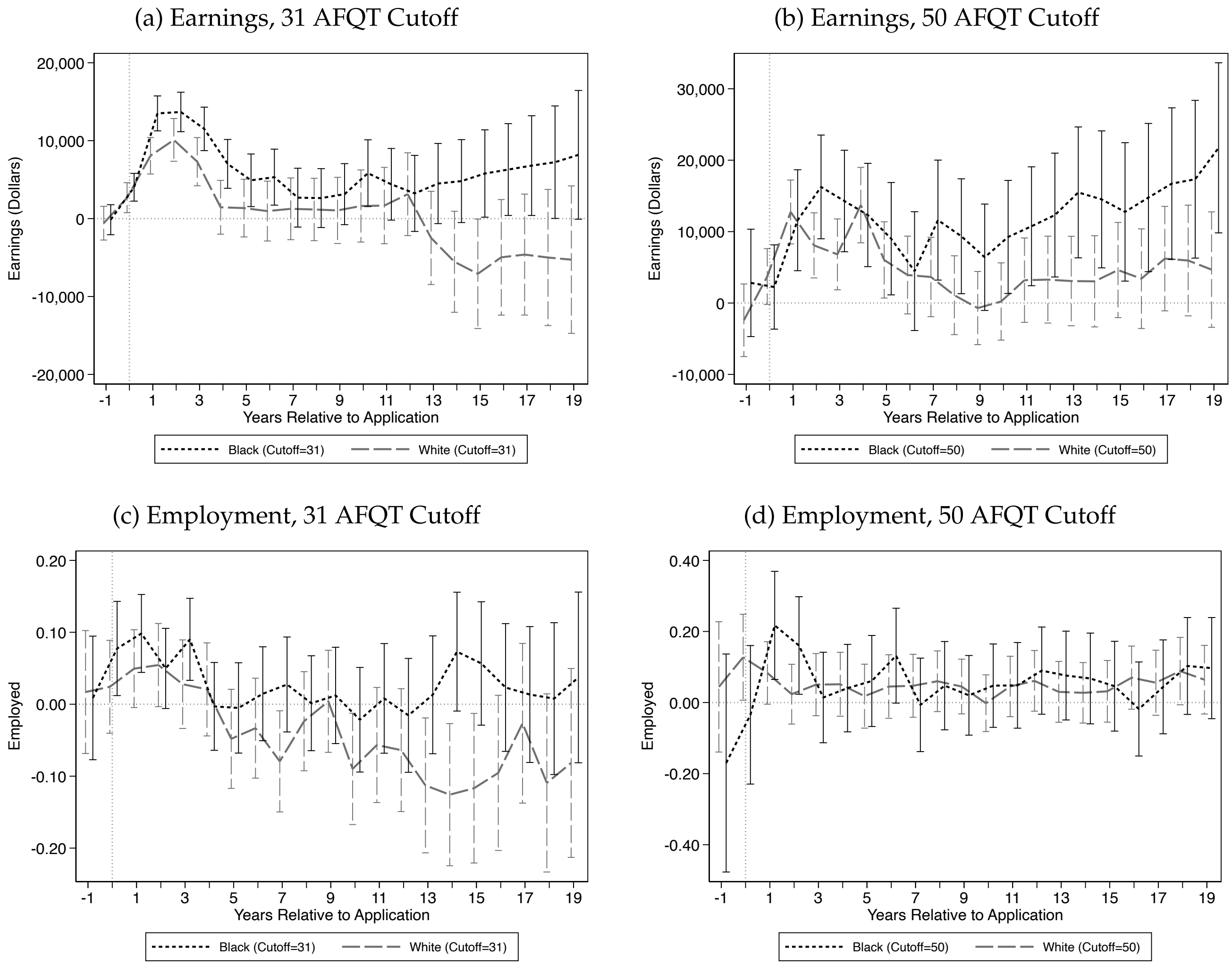


(b) Earnings, 50 Cutoff



Notes: This figure plots estimates of counterfactual average earnings for Black and White compliers at both cutoffs in the state of the world where they do not enlist by years since application. We estimate average potential outcomes y_i for compliers who do not enlist by running 2SLS regressions of $-y_i(1 - Enlist_i)$ on $Enlist_i$. Panels (a) and (b) show estimates of counterfactual earnings of applicants at the 31 and 50 cutoffs, respectively. Figure A.11 plots counterfactual employment, marriage, and homeownership trajectories.

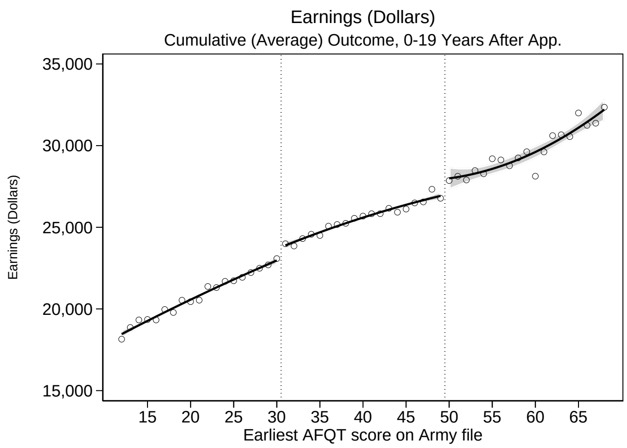
Figure 8: Effects of Enlistment for Black and White Applicants on Earnings and Employment



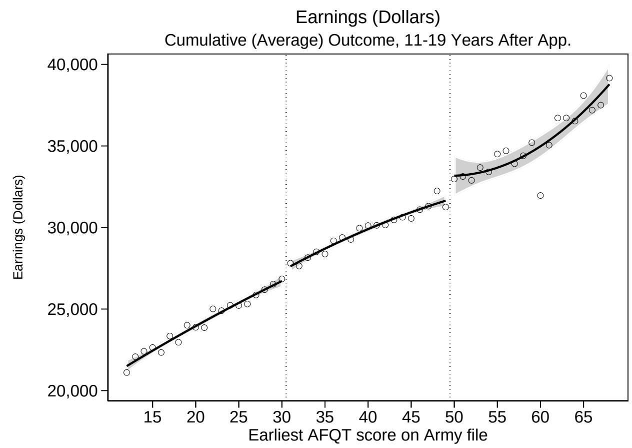
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings within subsamples split by race. Throughout, we compare estimates for Black applicants (the dotted black line) to those for White applicants (the dashed grey line). Panel (a) compares 2SLS earnings estimates at the 31 cutoff, panel (b) compares earnings estimates at the 50 cutoff, panel (c) compares employment (any positive W-2) estimates at the 31 cutoff, and panel (d) compares employment estimates at the 50 cutoff.

Figure 9: Earnings by Race, Reduced Form

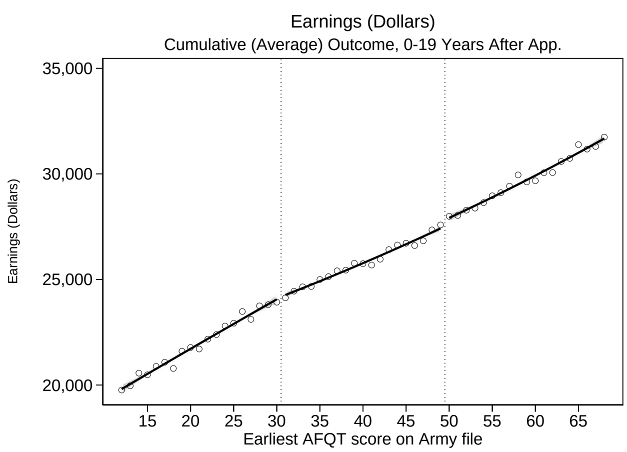
(a) Black Applicants (0-19)



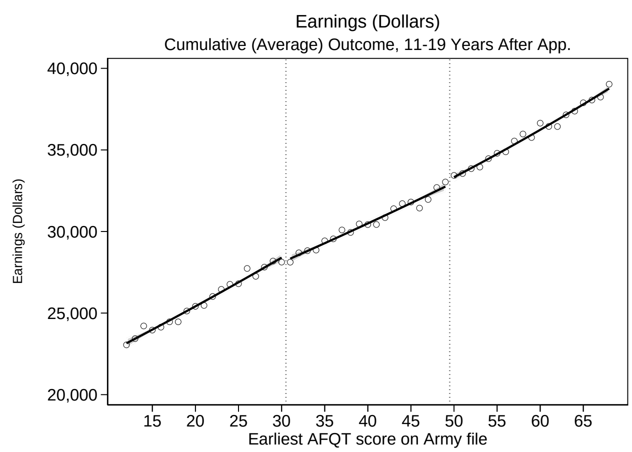
(b) Black Applicants (11-19)



(c) White Applicants (0-19)



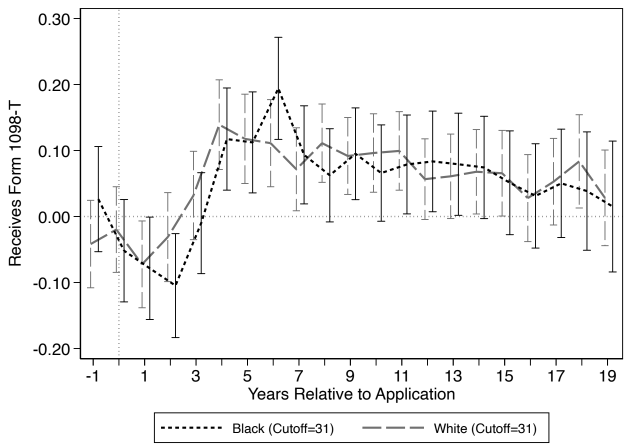
(d) White Applicants (11-19)



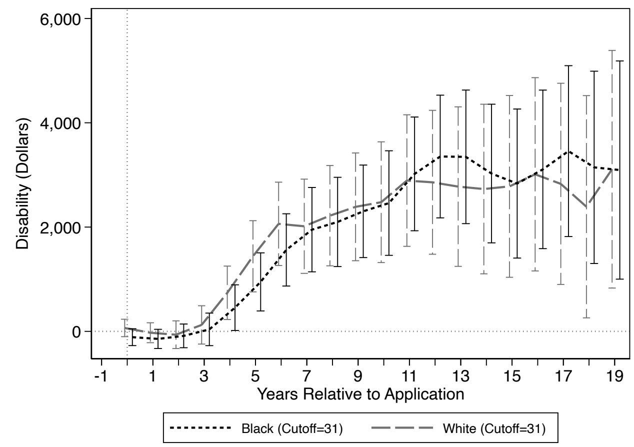
Notes: Panels (a) and (c) show weighted average earnings between 0 and 19 years post-application by AFQT score and panels (b) and (d) show weighted average earnings between 11 and 19 years post-application. Panel (a) presents average earnings for Black applicants 0-19 years after application, panel (b) presents average earnings for Black applicants 11-19 years after application, panel (c) presents average earnings for White applicants 0-19 years after application, and panel (d) presents average earnings for White applicants 11-19 years after application. Average earnings are weighted by the number of years the individual is in our sample, with zero wages imputed for individuals without reported earnings in a year covered by our data. Earnings are demeaned with respect to quarter-by-year of application fixed effects. 95% confidence intervals are indicated.

Figure 10: The Effects of Enlistment for Black and White Applicants on Other Outcomes (31 AFQT Cutoff)

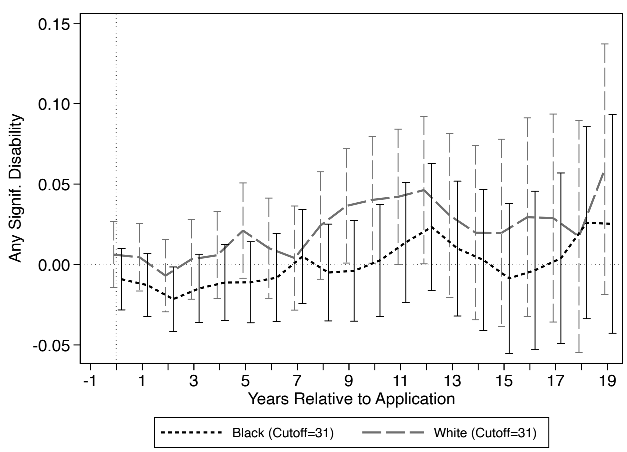
(a) Post-Secondary Attendance (Any 1098-T)



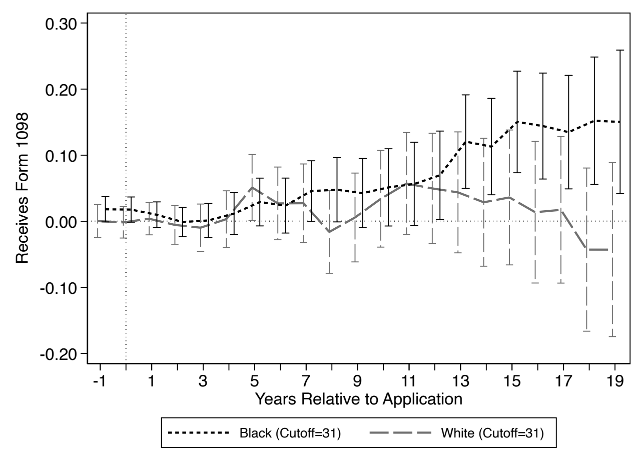
(b) Total Disability Comp. (VADC+SSI+SSDI)



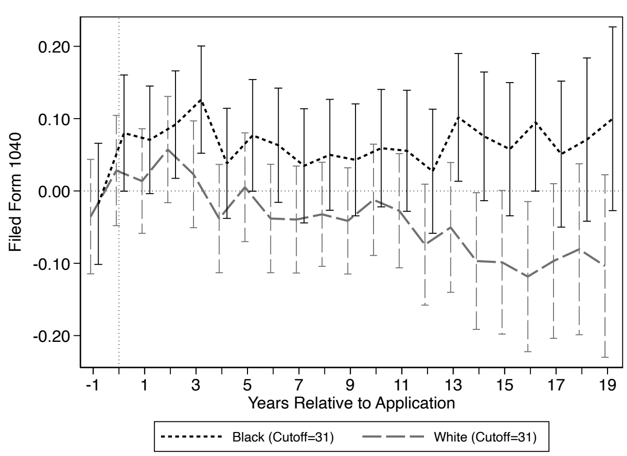
(c) Any Significant Disability



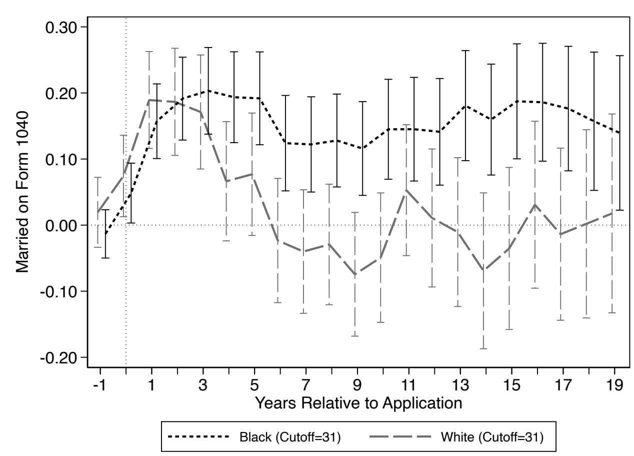
(d) Homeownership (1098 Filed)



(e) Any 1040



(f) Marriage (1040 Filing Status)



Notes: This figure plots 2SLS RD estimates of the effect of enlistment on the outcomes indicated in panel headings for the subsamples split by race. Throughout, we compare estimates for Black applicants (the dotted black line) to those for White applicants (the dashed grey line) at the 31 AFQT cutoff. Appendix Figure A.13 contains the plots at the 50 cutoff. Panel (a) compares post-secondary attendance estimates, panel (b) compares total disability compensation estimates, panel (c) compares any significant disability receipt estimates, where "significant disability" is defined as receiving a VADC combined disability rating of 100–which identifies an individual as fully disabled–or receiving any of SSI, SSDI, or VADC IU (each of which are work limiting), panel (d) compares mortgage estimates, panel (e) compares 1040 filing estimates, and panel (f) compares marriage estimates.

Tables

Table 1: Summary Statistics

	All Applicants (1)	Analysis Sample (2)	Enlisted (in A.S.) (3)	Did Not Enlist (in A.S.) (4)
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 Score	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
In High School	0.251	0.262	0.247	0.275
No HS Diploma	0.142	0.166	0.149	0.180
HS Diploma	0.536	0.531	0.559	0.506
Some College+	0.070	0.042	0.045	0.039
Observations	2,594,896	1,775,059	824,822	950,237

Notes: This table summarizes covariate means from Army applicant data at the time of first application. Column (1) describes characteristics for all applicants (AFQT scores between 1 and 99) from 1990-2011, whereas 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 earned a high school diploma but have not attended college; Some College+ includes anyone who has attended at least one semester of college.

Table 2: Covariate Balance (Reduced Form Estimates)

	(1) 31 Cutoff	(2) 50 Cutoff
	Time of Application	
Age	0.004	-0.016
	(0.018)	(0.017)
Male	-0.003	0.003
	(0.002)	(0.002)
White	-0.003	-0.002
	(0.003)	(0.003)
Black	0.004	0.000
	(0.003)	(0.002)
Hispanic	0.001	0.001
	(0.002)	(0.002)
In High School	0.003	0.002
	(0.002)	(0.002)
No HS Diploma	-0.000	-0.001
	(0.002)	(0.002)
HS Diploma	-0.005*	-0.001
	(0.003)	(0.003)
Some College+	0.002**	0.001
	(0.001)	(0.001)
Number of Observations	1,137,595	1,311,111
	Year Prior to Application	
Earnings	55.735	-21.338
	(79.423)	(76.107)
Employment	0.001	-0.001
	(0.003)	(0.003)
Filed Taxes (1040)	-0.001	0.001
	(0.003)	(0.003)
Post-Secondary Attendance	0.000	0.001
	(0.003)	(0.003)
Married	0.002	-0.002
	(0.002)	(0.002)
Number of Observations	555,286	658,666
P-value for Joint Significance	0.323	0.902

Notes: This table reports reduced-form RD estimates of Equation (1) where the left-hand-side variable is the covariate and pre-application outcome listed at left above. Column (1) reports covariate balance estimates for the 31 AFQT cutoff and column (2) reports covariate balance estimates for the 50 cutoff. The education categories are mutually exclusive, as described in the notes for Table 1.

Table 3: Average Effects on Earnings and Employment, 2SLS RD Estimates

	31 AFQT Cutoff		50 AFQT Cutoff	
	0-19 Yrs Since	11-19 Yrs Since	0-19 Yrs Since	11-19 Yrs Since
	(1)	(2)	(3)	(4)
		Panel (a): Earnings		
Enlist	4,255***	2,223	4,379***	4,096*
	(1,034)	(1,719)	(1,625)	(2,267)
Dep. Var Mean	24,805	29,366	28,052	33,677
		Panel (b): Log Earnings		
Enlist	0.313***	0.157*	0.322***	0.168*
	(0.056)	(0.087)	(0.077)	(0.099)
Dep. Var Mean	9.656	9.865	9.811	10.025
		Panel (c): Employment		
Enlist	0.004	-0.020	0.027	0.017
	(0.013)	(0.022)	(0.019)	(0.025)
Dep. Var Mean	0.839	0.797	0.851	0.808
Observations	1,137,595	969,081	1,311,111	1,109,460

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *average* earnings and employment outcomes. Columns (1)-(2) estimate average effects at the 31 AFQT cutoff, while columns (3)-(4) do so at the 50 cutoff. Each column looks at average outcomes over a different time horizon: 0-19 years since application or 11-19 years since application. In each column, we weight each observation by the number of years the individual is in our sample, with zero wages imputed for individuals without reported earnings in a year covered by our data. We estimate the effect of enlistment on average earnings in panel (a), average log earnings in panel (b), and average employment in panel (c). Those who are never employed are dropped from log earnings estimates in panel (b), with sample sizes of 1,129,395 in column (1), 891,720 in column (2), 1,303,381 in column (3), and 1,024,333 in column (4). 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)**	Died w/in 19 years **(6)**
			Panel (a): 31 AFQT Cutoff			
Enlist	-0.00095	-0.00648**	-0.00141	-0.00092	0.00629	-0.00057
	(0.00199)	(0.00326)	(0.00413)	(0.00682)	(0.00977)	(0.01444)
Number of Observations	1,137,580	1,137,580	1,137,580	1,016,628	800,795	582,299
Dep. Var. Mean	0.00131	0.00336	0.00566	0.01245	0.01858	0.02412
			Panel (b): 50 AFQT Cutoff			
Enlist	0.00074	0.00700	0.00676	-0.00872	-0.01982*	-0.01668
	(0.00336)	(0.00535)	(0.00676)	(0.00954)	(0.01156)	(0.01406)
Number of Observations	1,311,097	1,311,097	1,311,097	1,163,935	918,701	652,435
Dep. Var. Mean	0.00132	0.00352	0.00595	0.01285	0.01890	0.02357

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)-(6) show the effect of enlistment on deaths within 1, 3, 5, 10, 15, and 19 years, respectively. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 5: Aggregate Effects on Education and Disability, 2SLS RD Estimates

Panel (a): Education Outcomes

	31 AFQT Cutoff					50 AFQT Cutoff				
	Any College	Average Attend.	Any Degree	Assoc. Degree	Bach. Degree+	Any College	Average Attend.	Any Degree	Assoc. Degree	Bach. Degree+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enlist	0.147***	0.071***	0.072***	0.038**	0.034***	0.202***	0.028	0.013	-0.005	-0.012
	(0.031)	(0.012)	(0.019)	(0.016)	(0.014)	(0.071)	(0.027)	(0.054)	(0.044)	(0.042)
Observations	612,247	612,247	621,203	621,203	621,203	721,660	721,660	728,244	728,244	728,244
Dep. Var Mean	0.607	0.151	0.100	0.066	0.055	0.665	0.177	0.151	0.094	0.088

Panel (b): Disability Outcomes

	31 AFQT Cutoff					50 AFQT Cutoff				
	Any Disability	Average Disability	Any Signif. Disability	Avg. Signif. Disability	Avg. Comp.	Any Disability	Average Disability	Any Signif. Disability	Avg. Signif. Disability	Avg. Comp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enlist	0.253***	0.135***	0.040***	0.013	2,131***	0.262***	0.135***	0.042**	0.007	2413***
	(0.019)	(0.012)	(0.014)	(0.008)	(223)	(0.032)	(0.018)	(0.021)	(0.011)	(352)
Observations	1,159,354	1,159,354	1,159,354	1,159,354	1,159,354	1,328,772	1,328,772	1,328,772	1,328,772	1,328,772
Dep. Var Mean	0.163	0.080	0.070	0.033	1,126	0.193	0.096	0.064	0.028	1,327

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *aggregated* educational and disability outcomes. Columns (1)-(4) estimate the effects at the 31 AFQT cutoff, while columns (5)-(8) do so at the 50 cutoff. Effects on education outcomes are reported in Panel (a) and effects on disability outcomes are reported in Panel (b). We define "Any College" as any 1098-T filing in any of the years observed between 0-19 years post application. Similarly, "Associates Degree+", "Bach. Degree+", are defined as a corresponding degree recorded in the National Student Clearinghouse in any years observed between 0-19 years after application. "Average Attendance" is the average annual college attendance (based on 1098-T filing) among applicants in any years observed between 0-19 years after application, where observations are weighted by number of years observed. Due to the dynamic effects of enlistment on education, estimates in panel (a) are limited to the 1999-2011 application cohorts. "Any Disability" is defined as receiving any VADC, SSI, or SSDI benefits in any of the years observed between 0-19 years post application. "Average Disability" is the average annual receipt of any VADC, SSI, or SSDI benefits in any years observed between 0-19 years after application, where observations are weighted by number of years observed. "Avg. Signif. Disability" is computed in the same way, except partial VADC disability is excluded (VADC IU, VADC disability rating=100%, SSDI, and SSI are all counted). "Avg. Comp." is the average annual compensation of any VADC, SSI, and SSDI benefits in any years observed between 0-19 years after application, where observations are weighted by number of years observed. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table 6: Average Effects on Earnings and Employment, Differences by Race

	0-19 Years		11-19 Years	
	Black	White	Black	White
	(1)	(2)	(3)	(4)
	Panel (a): Average Earnings, 31 Cutoff			
Enlist	6,037***	823.1	5,482**	-2,833
	(1,607)	(1,740)	(2,532)	(2,922)
Observations	346,383	548,871	302,572	467,607
Dep. Var Mean	23,317	24,685	27,121	29,188
P-value for Equivalence	0.028		0.032	
	Panel (b): Average Earnings, 50 Cutoff			
Enlist	12,390***	4,263**	14,914***	4,071
	(3,216)	(2,131)	(4,336)	(2,929)
Observations	284,808	790,004	246,640	673,821
Dep. Var Mean	26,847	27,933	31,571	33,429
P-value for Equivalence	0.035		0.038	
	Panel (c): Average Employment, 31 Cutoff			
Enlist	0.026	-0.042*	0.024	-0.087**
	(0.022)	(0.023)	(0.033)	(0.038)
Observations	346,383	548,871	302,572	467,607
Dep. Var Mean	0.843	0.835	0.805	0.788
P-value for Equivalence	0.032		0.029	
	Panel (d): Average Employment, 50 Cutoff			
Enlist	0.059	0.048*	0.061	0.051
	(0.037)	(0.025)	(0.049)	(0.033)
Observations	284,808	790,004	246,640	673,821
Dep. Var Mean	0.854	0.848	0.817	0.802
P-value for Equivalence	0.803		0.870	

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *average* earnings and employment outcomes separately for Black and White applicants. Columns (1)-(2) look at average outcomes between 0-19 years since application, columns (3)-(4) look at 11-19 years since application. In each column, we weight each observation by the number of years we observe the corresponding individual in our data. We estimate the effect of enlistment on average earnings at the AFQT=31 cutoff in panel (a), average earnings at the AFQT=50 cutoff in panel (b), average employment at the AFQT=31 cutoff in panel (c), and average employment at the AFQT=50 cutoff in panel (d). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 7: Aggregate Effects on Education and Disability by Race, 2SLS RD Estimates

			31 AFQT Cutoff					50 AFQT Cutoff		
	Any College	Average Attend.	Any Degree	Assoc. Degree	Bach. Degree+	Any College	Average Attend.	Any Degree	Assoc. Degree	Bach. Degree+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel (a): Education Outcomes, Black Applicants										
Enlist	0.105**	0.069***	0.084**	0.052*	0.036	0.214	-0.014	0.086	0.093	0.013
	(0.047)	(0.022)	(0.035)	(0.029)	(0.028)	(0.139)	(0.065)	(0.133)	(0.107)	(0.112)
Dep. Var Mean	0.704	0.193	0.129	0.079	0.077	0.763	0.228	0.190	0.109	0.122
Panel (b): Education Outcomes, White Applicants										
Enlist	0.163***	0.070***	0.051*	0.025	0.035*	0.187*	0.014	-0.009	-0.047	-0.017
	(0.053)	(0.018)	(0.027)	(0.023)	(0.019)	(0.102)	(0.034)	(0.070)	(0.059)	(0.052)
Dep. Var Mean	0.542	0.123	0.081	0.057	0.038	0.623	0.154	0.134	0.088	0.073

			31 AFQT Cutoff					50 AFQT Cutoff		
Panel (c): Disability Outcomes, Black Applicants										
	Any Disability	Average Disability	Any signif. Disability	Avg. signif. Disability	Avg. Comp.	Any Disability	Average Disability	Any signif. Disability	Avg. signif. Disability	Avg. Comp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enlist	0.278***	0.146***	0.023	-0.000	2,038***	0.219***	0.127***	0.001	-0.010	1,415**
	(0.032)	(0.019)	(0.024)	(0.013)	(347)	(0.064)	(0.039)	(0.042)	(0.022)	(712)
Dep. Var Mean	0.159	0.077	0.073	0.033	1,038	0.198	0.098	0.070	0.030	1,314
Panel (d): Disability Outcomes, White Applicants										
Enlist	0.215***	0.116***	0.047*	0.022	1,954***	0.245***	0.132***	0.033	0.006	2,406***
	(0.032)	(0.020)	(0.025)	(0.015)	(384)	(0.042)	(0.024)	(0.028)	(0.015)	(467)
Dep. Var Mean	0.170	0.085	0.075	0.036	1,201	0.191	0.096	0.065	0.030	1,330

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *aggregated* educational and disability outcomes by race. Columns (1)-(5) estimate the effects at the 31 AFQT cutoff, while columns (6)-(10) do so at the 50 cutoff. Effects on education outcomes are reported in Panels (a) and (b) and effects on disability outcomes are reported in Panel (c) and (d). See the notes to Table 5 for additional outcome details. Sample sizes vary race, cohort, and data source. In panel (a), columns 1-2 have 158,398 observations, columns 2-5 have 160,453 observations, columns 6-7 have 135,228 observations and columns 8-10 have 136,543 observations. In panel (b), columns 1-2 have 298,564 observations, columns 2-5 have 301,801 observations, columns 6-7 have 428,392 observations and columns 8-10 have 431,762 observations. In panel (c), columns 1-5 have 353,789 observations and columns 6-10 have 289,344 observations. In panel (d), columns 1-5 have 557,244 observations and columns 6-10 have 799,603 observations.
Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 8: Economic Opportunity, Race, and Long-Run Effects of Service

	Effects of Service, 11-19 Years After Application			
	(1)	(2)	(3)	(4)
	Panel (a): Reweighting, 31 Cutoff, Black			
	Benchmark	Reweight	Reweight (+1040)	
Enlist	5,927**	5,506	5,530	
	(2,502)	(3,723)	(4,282)	
Observations	299,074	299,074	299,074	
	Panel (b): Reweighting, 50 Cutoff, Black			
	Benchmark	Reweight	Reweight (+1040)	
Enlist	14,379***	15,524***	15,227**	
	(4,404)	(5,954)	(6,452)	
Observations	243,928	243,928	243,928	
	Panel (c): Disadvantage Index, 31 Cutoff, Black-White Delta			
	Benchmark	Add Disadv.	Baseline (+1040)	Add Disadv. (+1040)
Black×Enlist	8,864**	9,008**	8,768**	8,922**
	(3,655)	(3,677)	(3,651)	(3,673)
Disadvantage×Enlist		1,592		1,909
		(1,633)		(1,634)
Observations	761,110	761,110	761,110	761,110
	Panel (d): Disadvantage Index, 50 Cutoff, Black-White Delta			
	Benchmark	Add Disadv.	Baseline (+1040)	Add Disadv. (+1040)
Black×Enlist	9,892*	9,532*	9,774*	9,227*
	(5,281)	(5,299)	(5,276)	(5,299)
Disadvantage×Enlist		3,821**		4,354**
		(1,930)		(1,924)
Observations	909,881	909,881	909,881	909,881

Notes: Panels (a) and (b) re-estimate the specification in Table 6 with inverse probability weights constructed from a logit regression in which the dependent variable is a dummy for being a White applicant. Column (2) includes the following independent variables in the logit regression: fiscal year of application fixed effects, gender, a quintile in age, initial education dummies, and quintiles of rates of employment, median income, poverty, and single-parent households measured in 1990 from county of residence reported on the application. In column (3), we additionally include several variables constructed from applicants childhood households 1040 filing information: eligibility to be claimed as a dependent (i.e. under 19 years of age in 1996, the first year where we can observe dependent linkages in the tax data), whether the applicant is on a household tax return, whether the child is claimed as a dependent on a household tax return, a quintile of family income reported on the tax return, whether the family income was below 15k, and whether the applicant was claimed as a dependent on a single-parent tax return. We observe these for approximately 50% of the estimation sample and the eligibility to be claimed as a dependent variable effectively dummies out those applicants for whom these are unobserved. In Panels (c) and (d) we estimate a 2SLS model that instruments for Enlist × Black with $\mathbb{1}(AFQT \geq CUT)$×Black in columns (1) and (3) and that instruments for both Enlist × Black and Enlist × Disadvantage with $\mathbb{1}(AFQT \geq CUT)$×Black and $\mathbb{1}(AFQT \geq CUT)\times$ Disadvantage in columns (2) and (4). The disadvantage index — the standard deviation of the additive inverse of predicted earnings 11-19 years — is constructed using a leave-one-out procedure for applicants just to the left of each threshold using the same variables used in the reweighting models. All panels and columns drop the 1 percent of applicants for whom county of application is missing. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Online Appendix

Data Appendix

A.1 Matching Army Records to Social Security Numbers

Federal tax return data can be linked to Army applicant data using Social Security Numbers (SSN). We attempt to match as many Army applicants to Social Security records as possible while limiting erroneous matches. Most Army applicants (96.1%) uniquely match on SSN and date of birth (DOB) to a Social Security record. For Army applicants who do not uniquely match on SSN and date of birth (DOB), we attempt to match them to a Social Security record by matching exactly on some combination of SSN, DOB, and first and last name where one of the three items (SSN, DOB, or name) is allowed to be "close" rather than exact. 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). Performing this supplementary match improves our overall match rate from 96.1% to 98.9%.

A.2 Details on Individual Earnings

We construct our individual earnings outcome as the sum of observed wages from employer-provided Form W-2 Wage and Tax statements plus non-taxable military allowances from Army administrative pay records that would normally be reported on civilians' Form W-2 as wages but are not included on Form W-2 issued by the military.

From Form W-2, we use box 5 Medicare wages instead of box 1 wages because it is a more inclusive measure of earnings. Namely, deductible retirement contributions by all employees and basic pay income of servicemembers received while deployed are only reported as Medicare wages.

Like Loughran et al. (2011), we include military pay not subject to taxes because they are included in the official definition of Regular Military Compensation (RMC) according to Section 101(25) of Title 37, United States Code, and are an important part of servicemember compensation. In addition to providing direct and standard compensation for military work, they are not reported on the Form W-2, and those who do not join the military are rarely eligible to receive similar tax-free payments that would not otherwise be included as wages on the Form W-2. Military pay that is not subject to taxes consists of Army housing allowances (Basic Allowance for Housing), direct payments for food (Basic Allowance for Subsistence), and pay associated with combat deployments or assignments in foreign countries (Hardship Duty Pay, Imminent Danger Pay, Hazardous Duty Pay, and Family Separation Allowances).

Basic Allowance for Housing (BAH) is an allowance paid to servicemembers who are not provided with government housing. BAH is the largest tax-exempt military pay allowance, typically accounting for two-thirds or more of a servicemembers' overall tax-exempt military pay. All servicemembers are either provided with housing and utilities free of charge (commensurate with their rank and dependent status) or, more frequently after the first couple years of service, BAH payments of approximately equivalent value. BAH is determined by location, rank, and dependency status and is meant to provide equitable housing (rent and utilities) to what servicemembers would have been provided by the government. Among servicemembers in our sample, monthly BAH payments average roughly $1,000 but reach as high as $3,000 for more experienced soldiers assigned to bases in high-cost locations. 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.

We note that there are relatively few situations where housing benefits provided to civilian employees are tax exempt. Employer-provided housing is typically taxable unless each of the following conditions is 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) (Source: https://www.irs.gov/publications/p15b, accessed September 2019). The one exception to these rules is that housing benefits provided to clergy members are typically tax exempt, though they constitute an extremely small fraction of U.S. employment (Source: https://www.irs.gov/taxtopics/tc417, accessed September 2019).

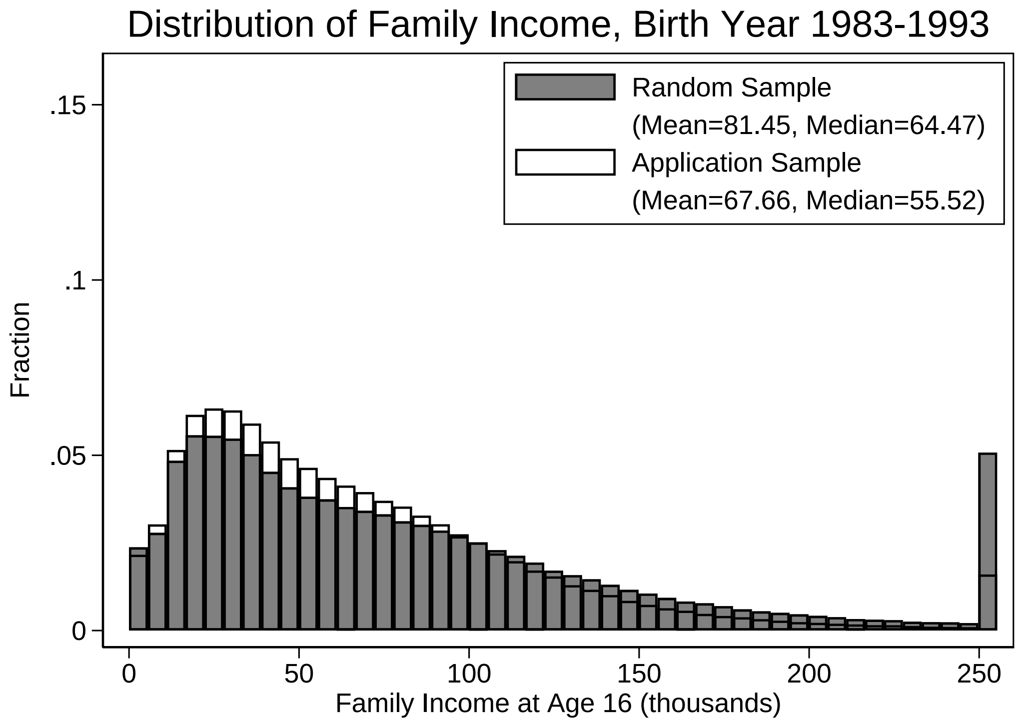
Basic Allowance for Subsistence (BAS), usually the second-largest non-taxable payment to servicemembers, is meant to offset the cost of food. In 2019, BAS payments to enlisted servicemembers were $369 per month. Hardship Duty Pay is paid to servicemembers who are assigned to locations with living conditions that are substantially worse than in the continental United States. Imminent Danger Pay (IDP) is paid to servicemembers 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.

Altogether, we find that the tax-exempt payments outlined above account for 17-25% of Army servicemembers' compensation. While civilians are unlikely to receive these types of 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 (rare) tax-exempt civilian payments or any self-employment earnings (we separately examine the latter). Furthermore, our individual earnings measure does not account for a variety of potentially tax-exempt benefits such as health coverage, GI-Bill tuition and related housing payments, or retirement contributions, some of which are common across both military and civilian jobs. Lastly, in considering pre-tax pay we do not account for the after-tax benefit of the exclusion of certain military pay from taxes, including the BAH, BAS, and other tax-free payments.

Appendix Figures

Figure A.1: Comparison of Applicant Family Incomes to Those of a Nationally Representative Sample

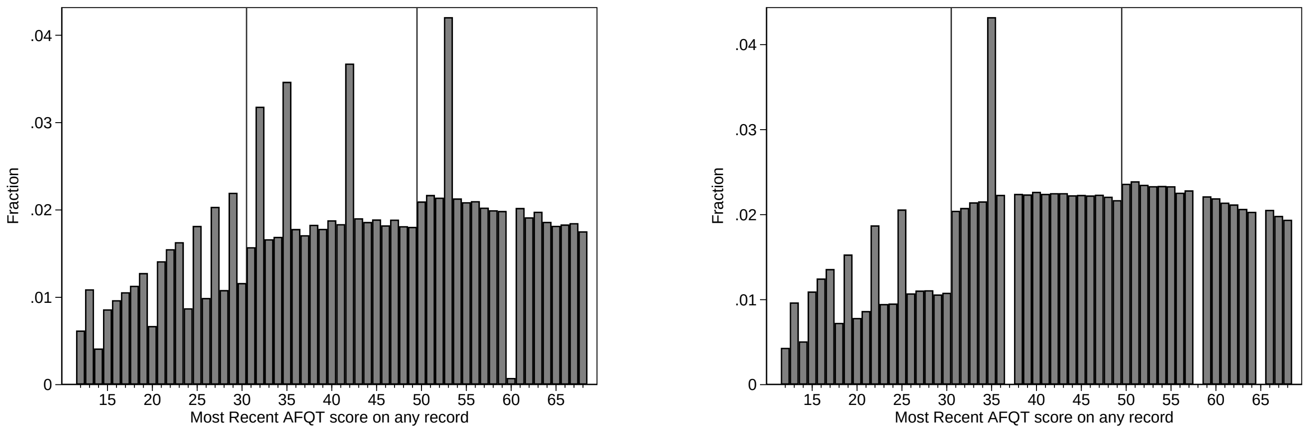


Notes: This figure plots family income at age 16 for all applicants (AFQT 1-99) in birth cohorts from 1983-1993 and compares this to family income at age 16 for a random sample of the same birth cohorts. We compare our sample of applicants to a random sample of individuals from the same birth cohorts selected using the random four-digit endings of SSNs. Using the tax data, we calculate the proportion in each sample that is claimed as a dependent at age 16 and, if claimed by a tax unit, examine the distribution of Form 1040 family income in that year. Since those who immigrated to the U.S. after childhood will mechanically not be claimed on a U.S. tax return at age 16, and those individuals will be more highly represented in a random sample of SSNs (as those applying to the Army in late childhood and early adulthood have to be in the U.S. by the time they apply to the Army, if not earlier, whereas there is no such restriction for the random sample), we create an apples-to-apples comparison of family background by limiting both samples for the purposes of this exercise to those who listed a parents social security number on their social security card application, which is a requirement for applicants under the age of 18 (so those who received an SSN as an adult immigrant would not be included). As a result, while the share of Army applicants who are claimed as dependents at age 16 increases with this restriction from 87% to 93%, the share of the random sample claimed as dependents at age 16 increases at a much higher rate, from 72% to 94%, resulting in very similar claim rates (which lessens concerns about selection over whose family income we can observe). In fact, that Army applicants are slightly less likely to be claimed relative to the random sample after this restriction is imposed is consistent with the fact that tax filing rates are increasing in income and that we find that army applications come from lower socio-economic-status backgrounds on average.

Figure A.2: Density of *Most Recent* AFQT Scores

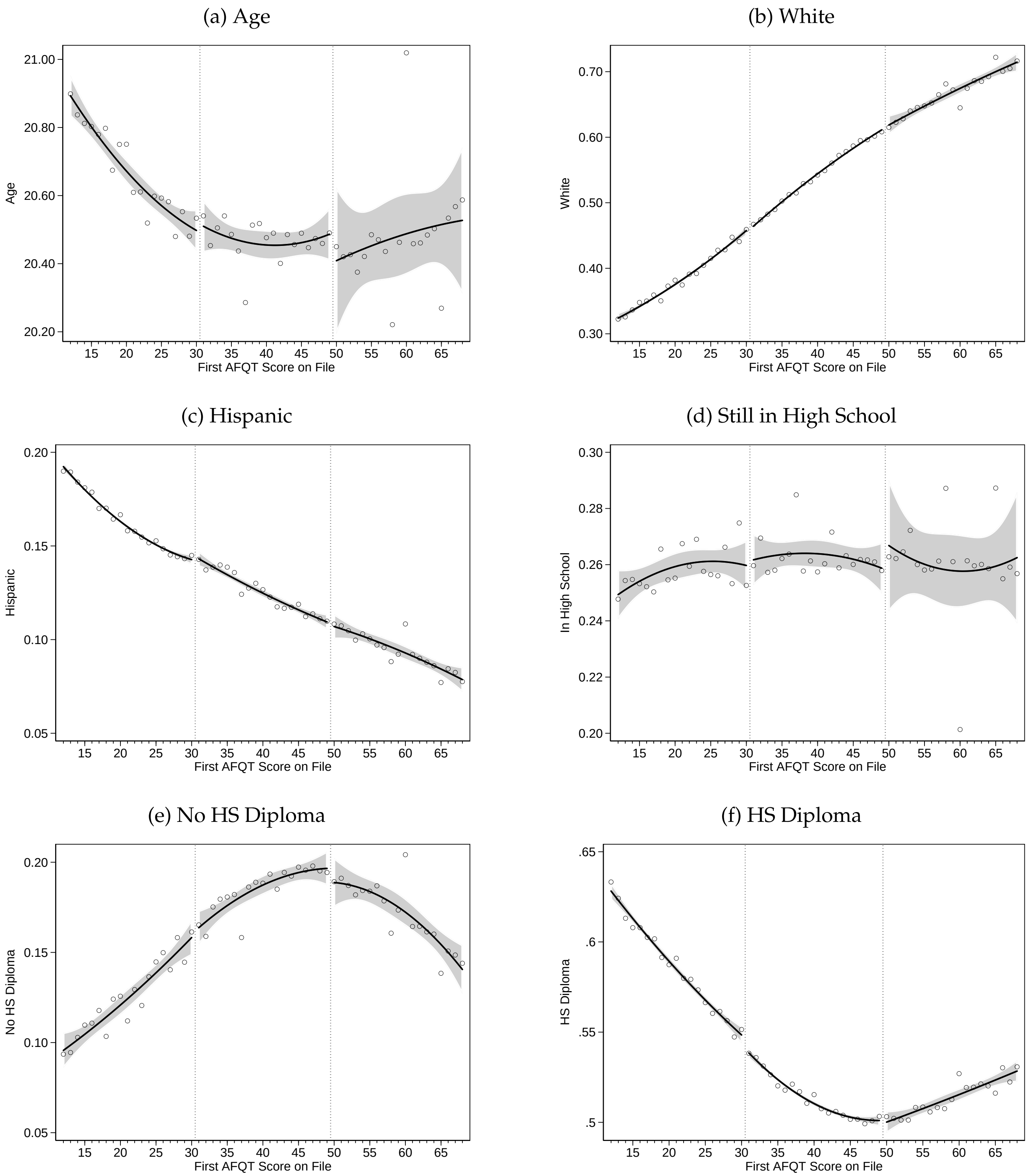
(a) Last AFQT Density, pre-2004 renorming

(b) Last AFQT Density, post-2004 renorming



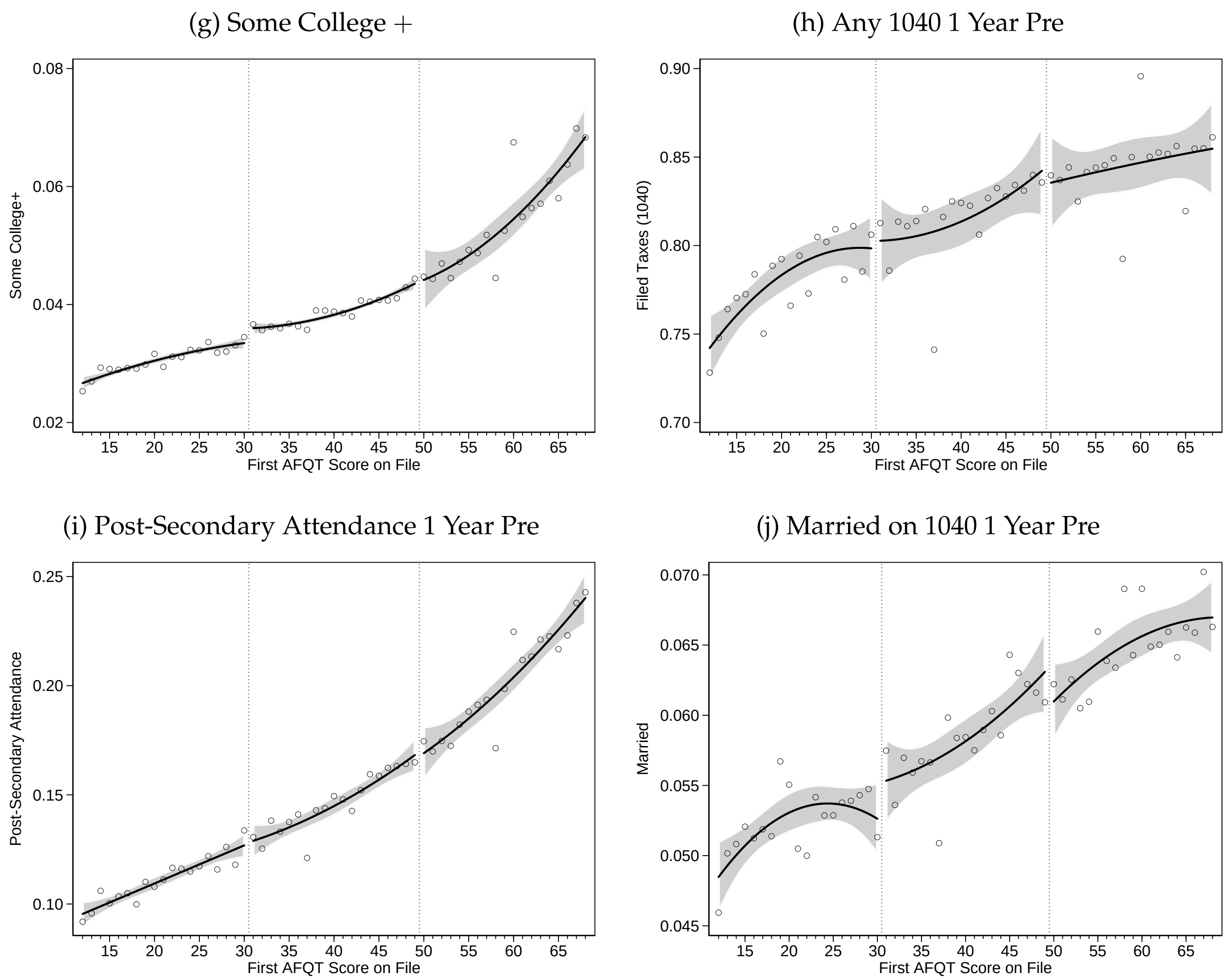
Notes: Panels (a) and (b) show the distribution of *most recent* AFQT scores on record before and after the April 2004 ASVAB re-norming, respectively. In contrast to Figure 2, the distribution of *most recent* AFQT scores exhibits bunching at both cutoffs.

Figure A.3: Additional Covariate Balance Plots



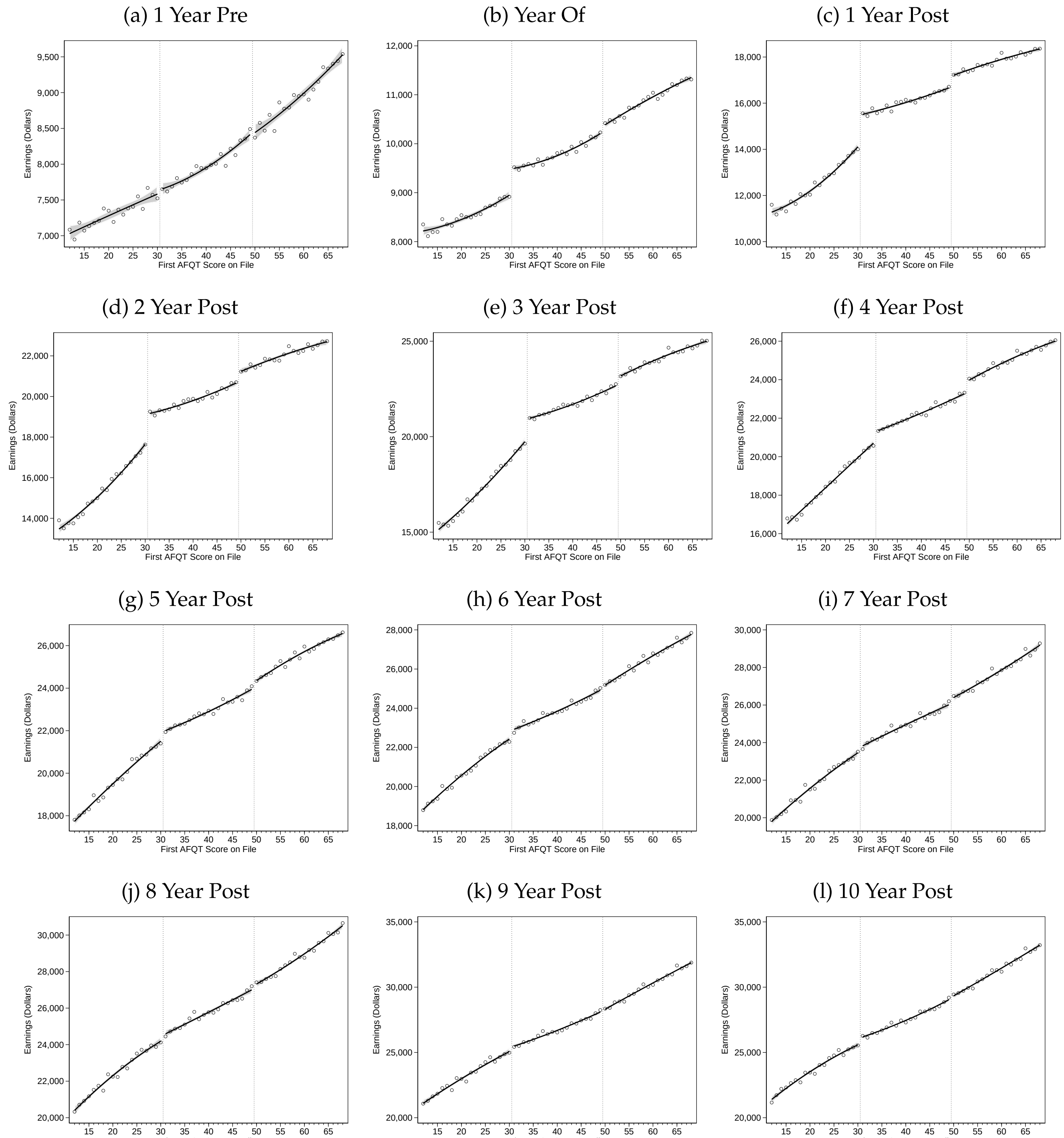
Notes: Figure A.3 (along with Figure 2 panels (c)-(f)) plots the reduced form relationship between first AFQT on file and the covariates/pre-application outcomes in Table 2.

Figure A.3: Additional Covariate Balance Plots (Continued)



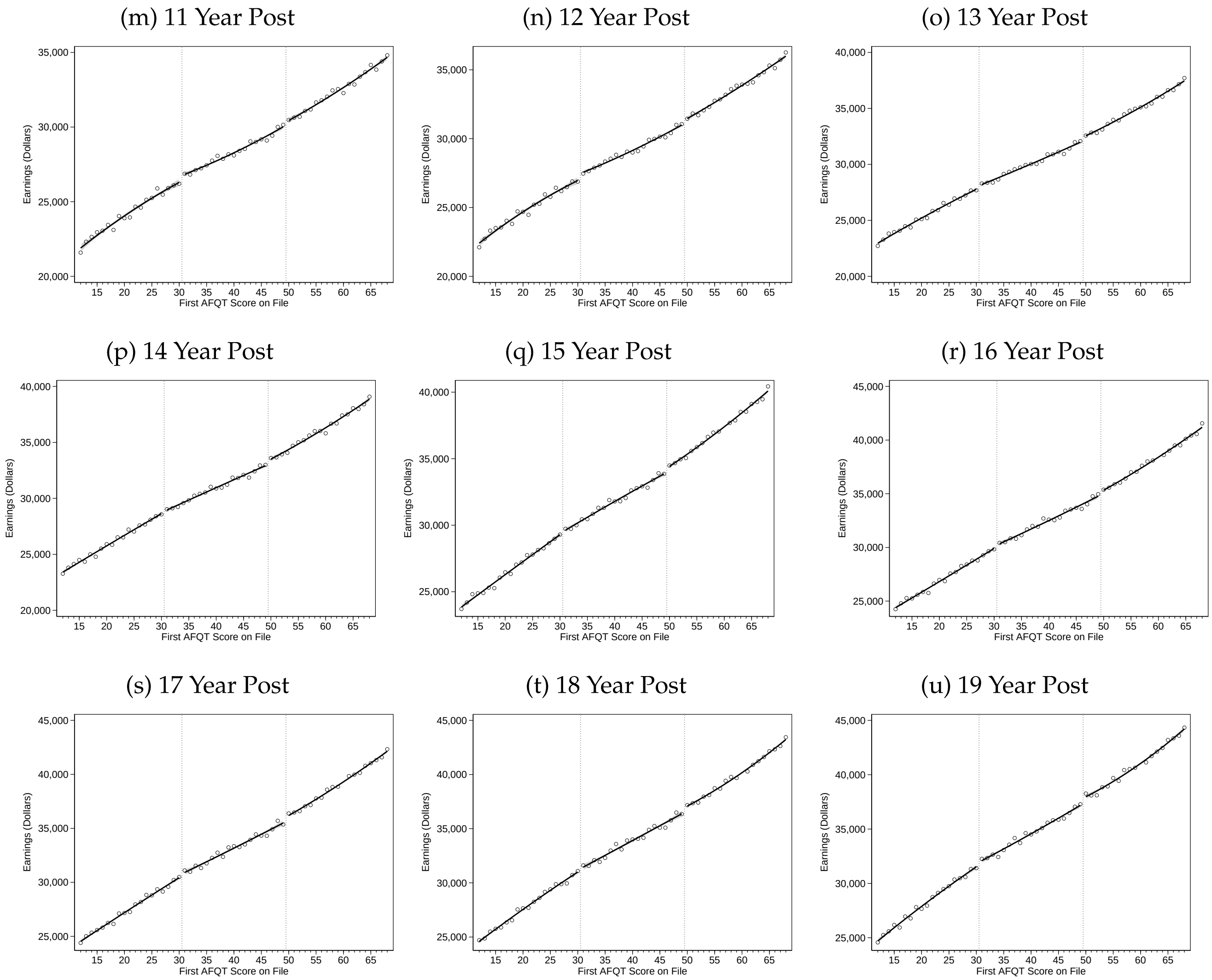
Notes: Figure A.3 (along with Figure 2 panels (c)-(f)) plots the reduced form relationship between first AFQT on file and all the covariates/pre-application outcomes in Table 2.

Figure A.4: Reduced Form Plots For Baseline Earnings Estimates



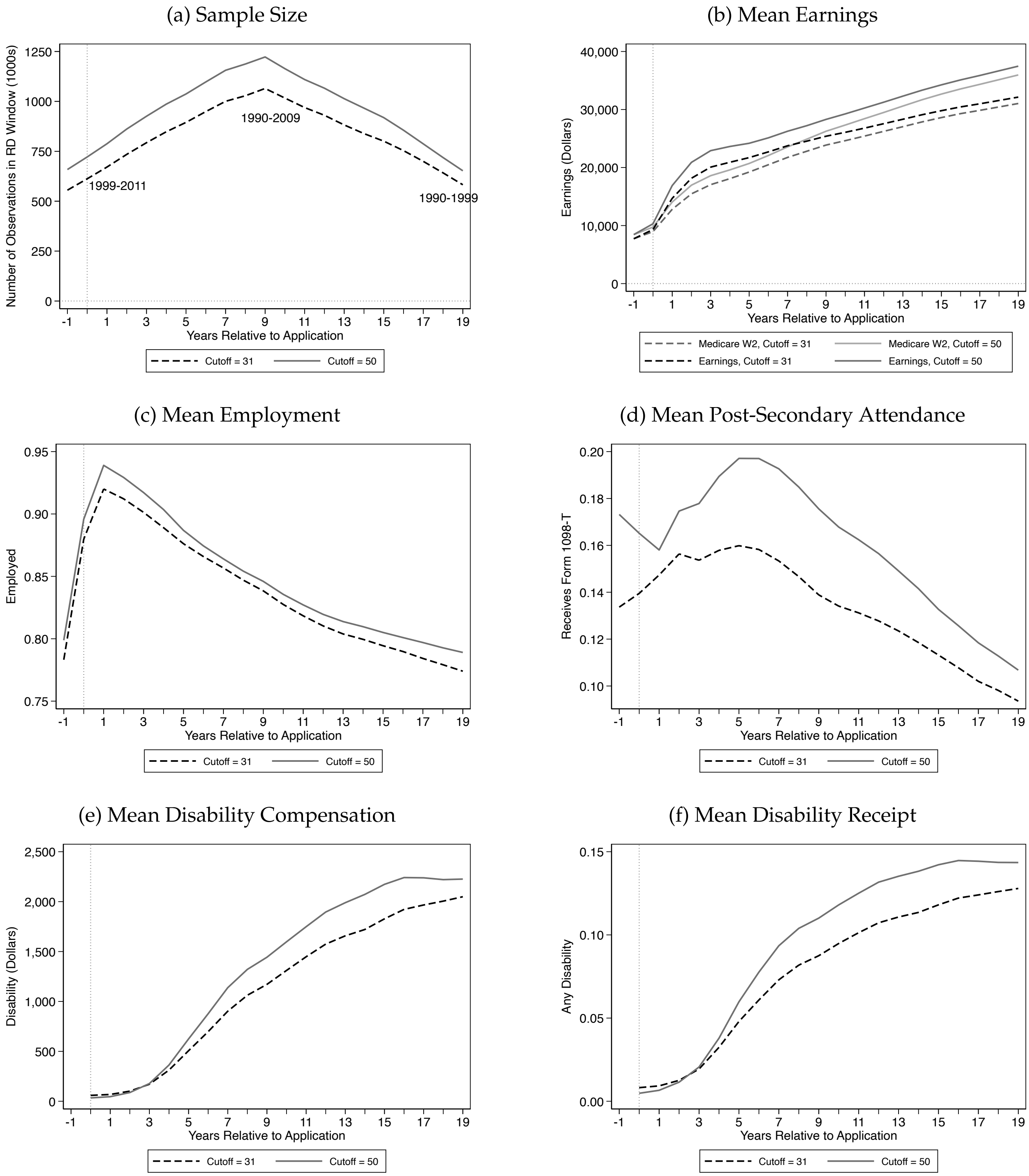
Notes: This figure plots our baseline earnings outcome -1 to 10 years after application as a function of the earliest AFQT score on file. Earnings are demeaned with respect to quarter-by-year of application fixed effects.

Figure A.4: Reduced Form Plots For Baseline Earnings Estimates (continued)



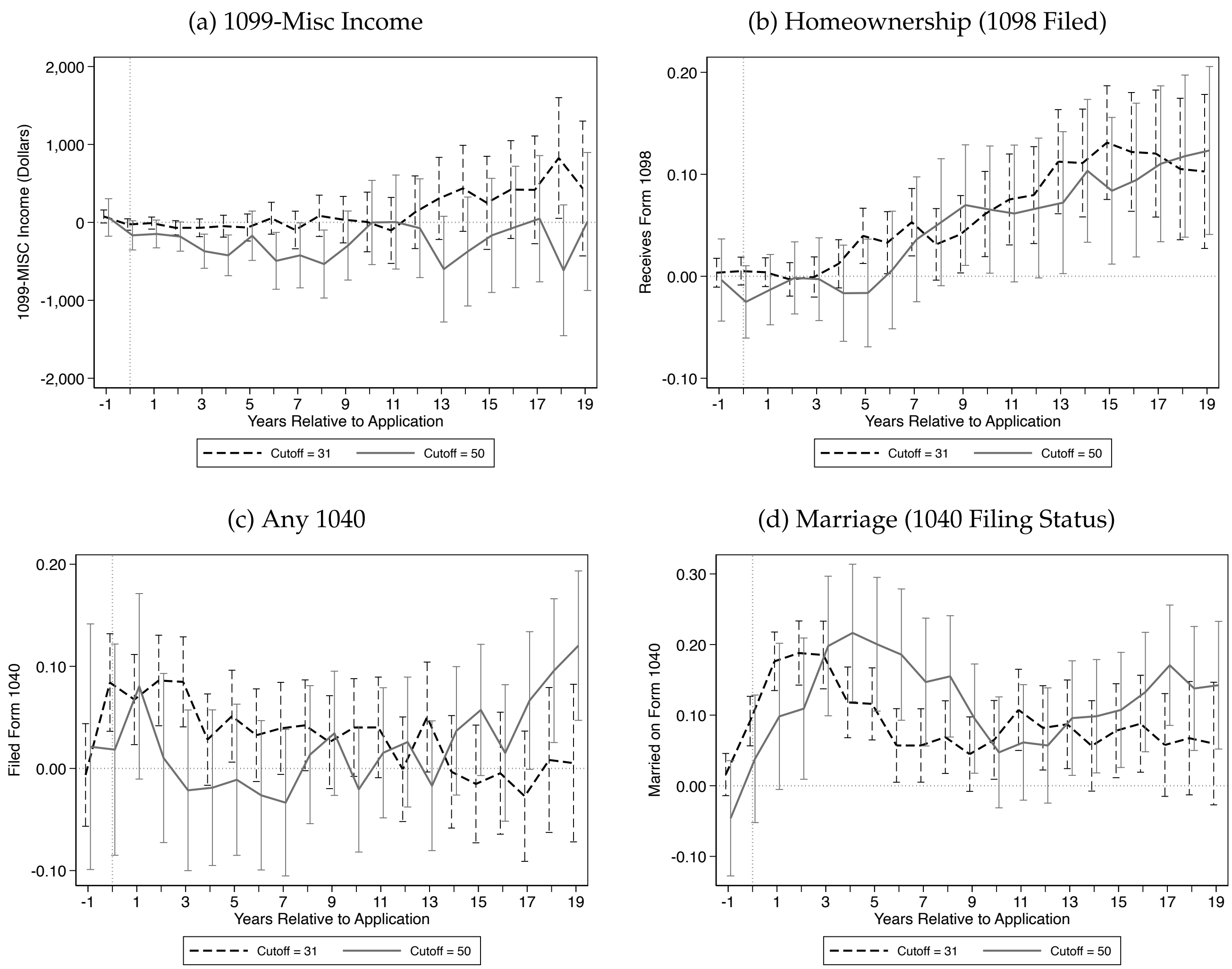
Notes: This figure plots our baseline earnings outcome 11 to 19 years after application as a function of the earliest AFQT score on file. Earnings are demeaned with respect to quarter-by-year of application fixed effects.

Figure A.5: Sample Size and Mean Outcomes By Years Since Application



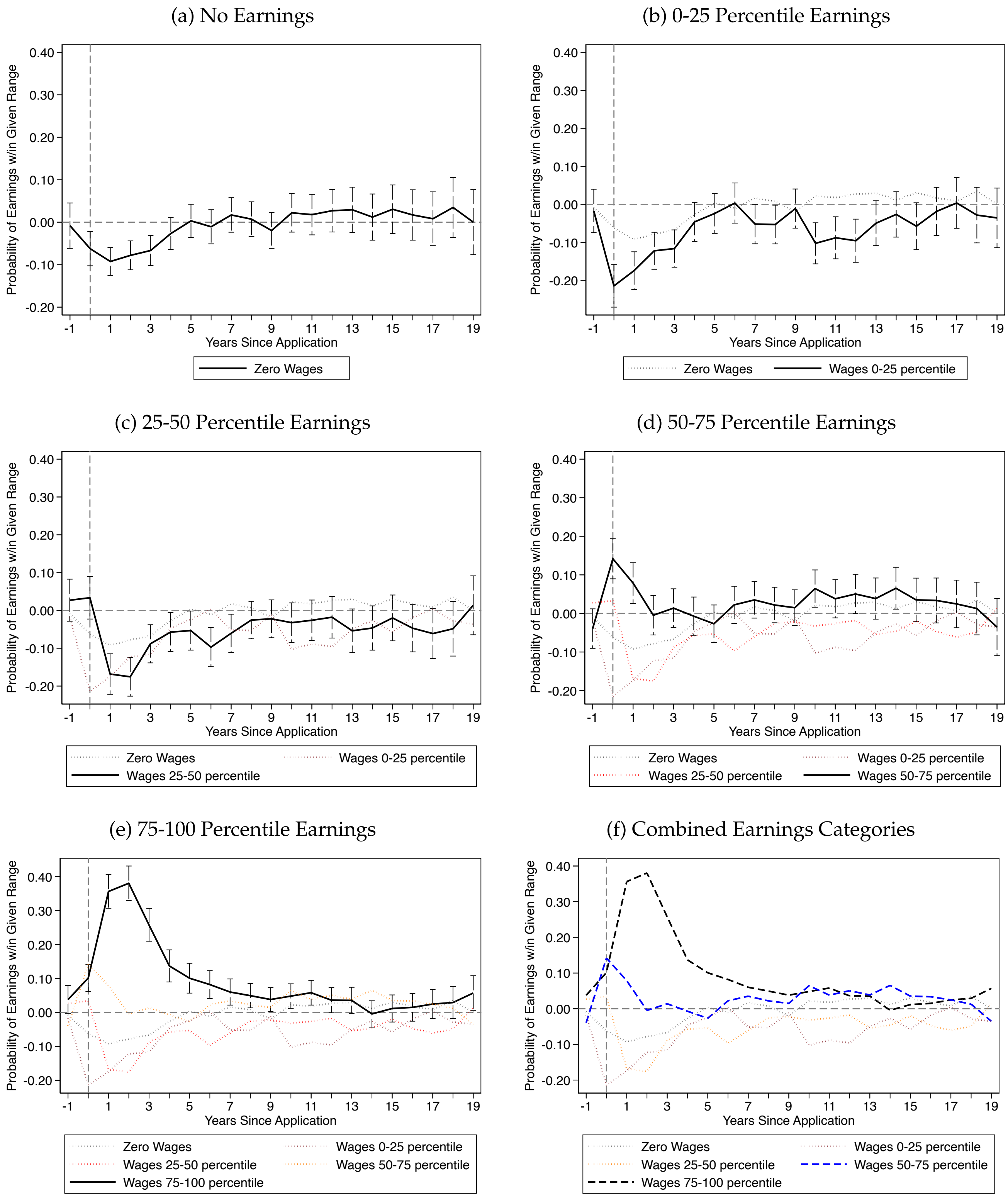
Notes: These figures plot our sample size, mean earnings (medicare W-2 + non-taxable bonuses and allowances in 2018 dollars), mean employment (any W-2), mean post-secondary attendance (any Form 1098-T), mean disability compensation (VADC+SSI+SSDI in 2018 dollars), and mean disability receipt (any VADC, 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.6: Effects of Enlistment on Other Outcomes (2SLS RD Estimates)



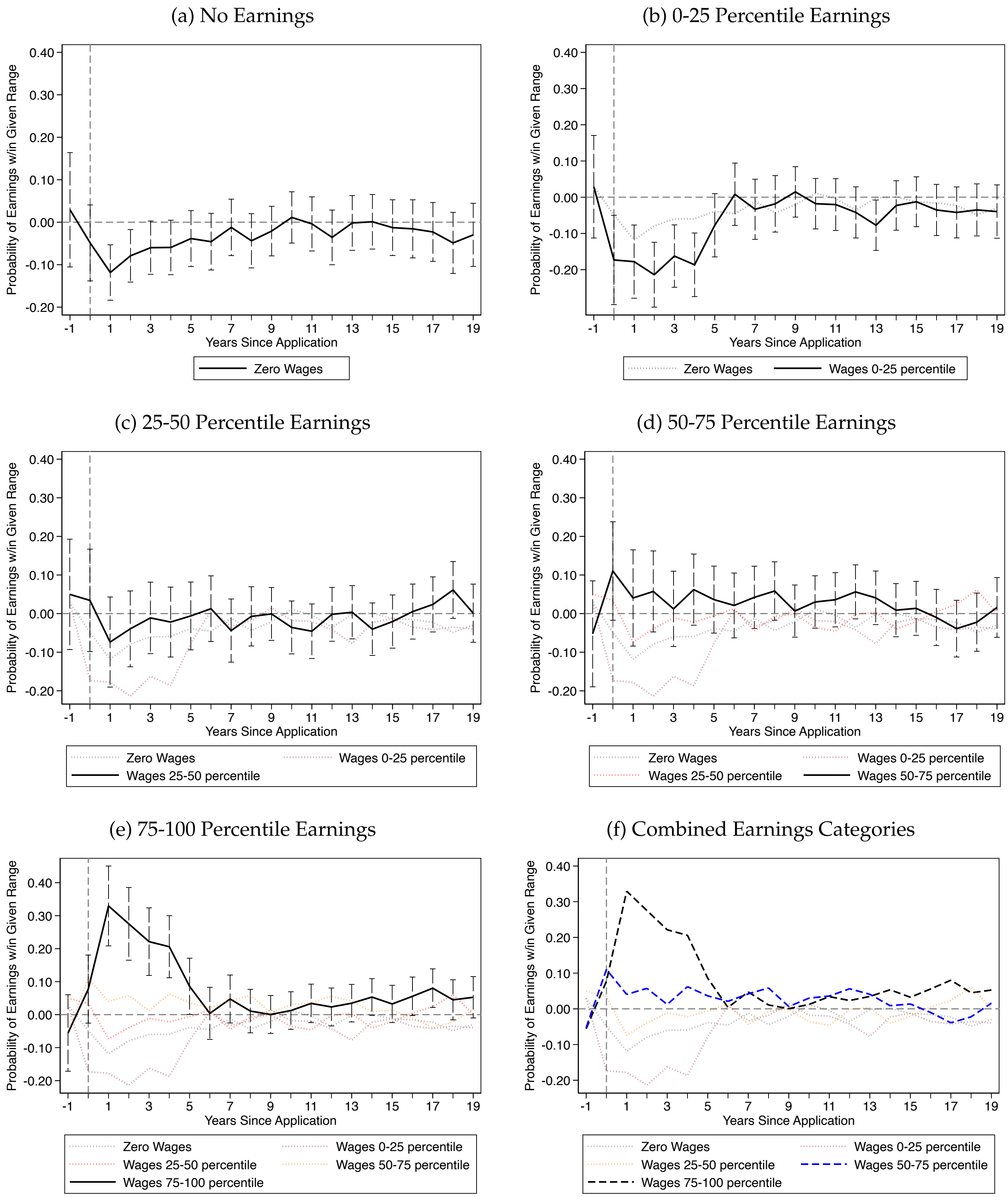
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on total 1999-Misc income, homeownership, filing a 1040, and marriage. Panel (a) shows estimates of the effect of enlistment on total 1099-MISC income from information-returns in 2018 dollars (individuals without a 1099-MISC have this set to 0). Panel (b) shows estimates of the effect of enlistment on having a mortgage defined as having a 1098 form (Mortgage Interest Statement) in the given year. Panel (c) shows estimates of the effect of enlistment on 1040 filing at each cutoff. Panel (d) 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.

Figure A.7: Distributional Effects (31 AFQT Cutoff)



Notes: This figure plots 2SLS RD estimates of the effect of enlistment on five categorical earnings outcomes among applicants in the window around the 31 cutoff. In panel (a) "No Earnings" is defined as not having any W-2 earnings. For panels (b)-(e), we (i) draw a 1% national sample of individuals with positive W-2 earnings, (ii) identify national earnings percentiles for each birth cohort, sex, and tax year combination, (iii) use these national earnings percentiles to identify what earnings percentile category (e.g. 0-25 Percentile) individuals in our sample belong to in each year (using earnings as defined in Section 3.3). Panel (f) combines each prior category into one plot.

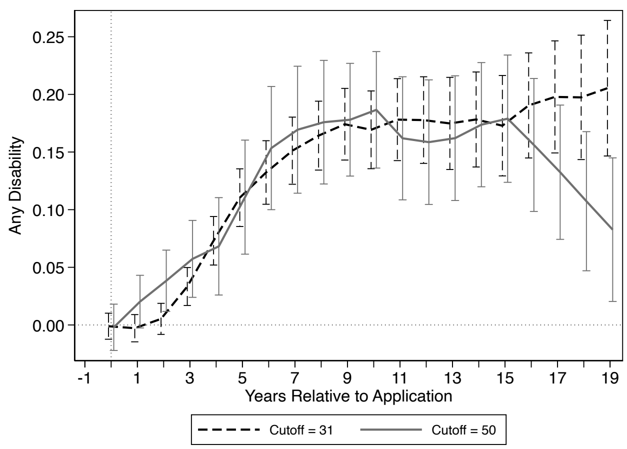
Figure A.8: Distributional Effects (50 AFQT Cutoff)



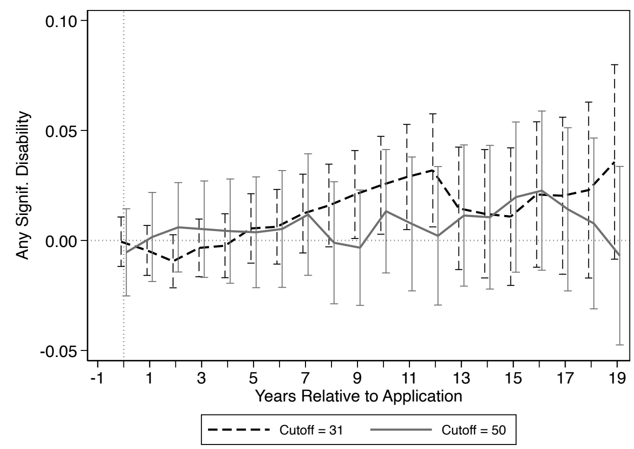
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on five categorical earnings outcomes among applicants in the window around the 50 cutoff. In panel (a) "No Earnings" is defined as not having any W-2 earnings. For panels (b)-(e), we (i) draw a 1% national sample of individuals with positive W-2 earnings, (ii) identify national earnings percentiles for each birth cohort, sex, and tax year combination, (iii) use these national earnings percentiles to identify what earnings percentile category (e.g. 0-25 Percentile) individuals in our sample belong to in each year (using earnings as defined in Section 3.3). Panel (f) combines each prior category into one plot.

Figure A.9: Effects of Enlistment on Additional Disability Outcomes

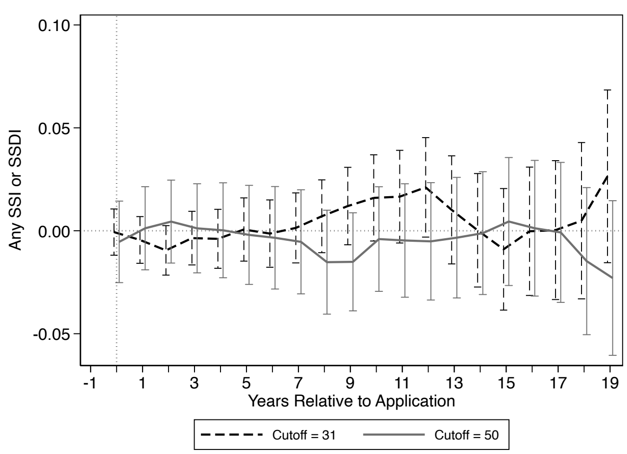
(a) Any Disability Compensation



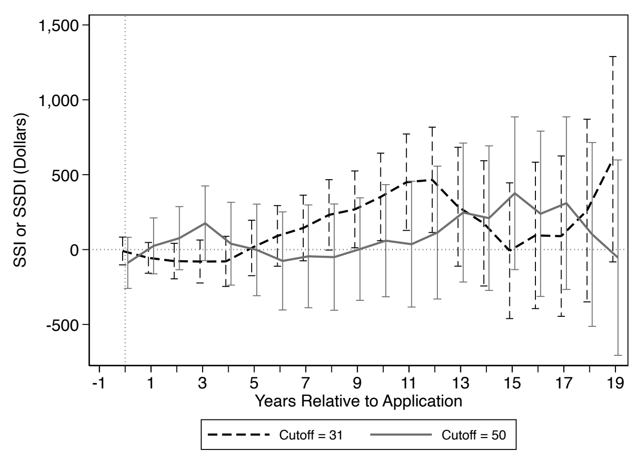
(b) Any Significant Disability



(c) Any SSI/SSDI



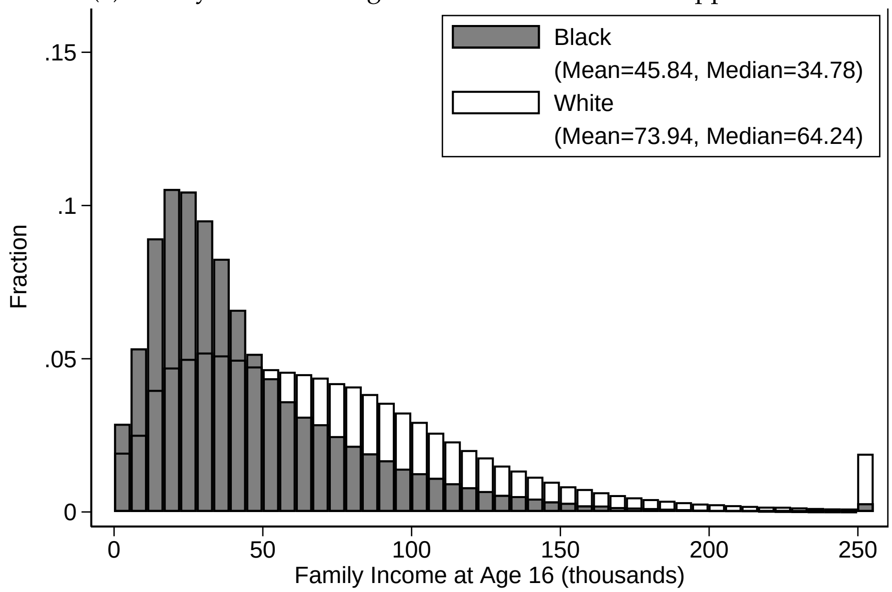
(d) Total SSI + SSDI Compensation



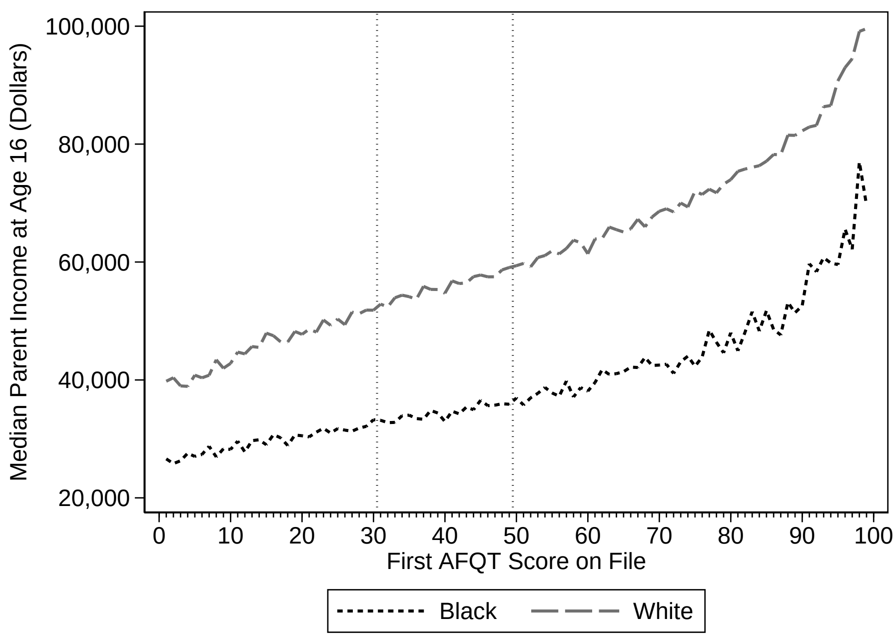
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on receiving any disability compensation (VADC, SSI, or SSDI) in panel (a), on any significant disability compensation (VADC IU, VADC combined disability rating of 100%, SSI, or SSDI) in panel (b), on any SSI or SSDI in panel (b), on any SSI or SSDI compensation in panel (c), and on total SSI + SSDI compensation in panel (d). 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: Family Income Comparison For Black and White Applicants

(a) Family Income At Age 16 for Black and White Applicants

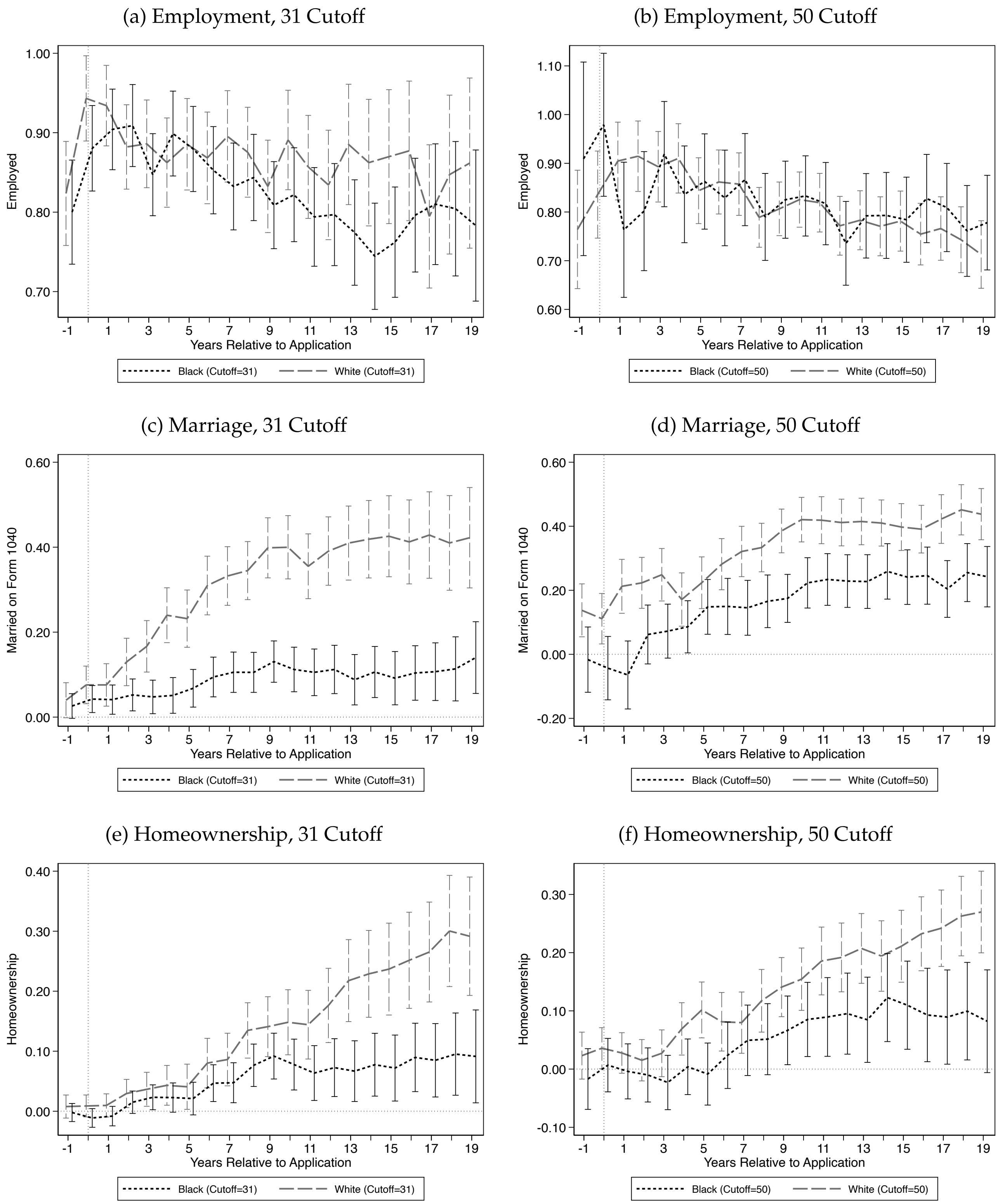


(b) Median Family Income vs. AFQT for Black and White Applicants



Notes: Panel (a) shows the distribution of family income at age 16 for all the Black and White applicants (AFQT 1-99) in birth cohorts from 1983-1993 that we are able to match to family income. See the notes to Figure A.1 for additional details. Panel (b) shows how median family income varies by first AFQT score for Black and White applicants separately.

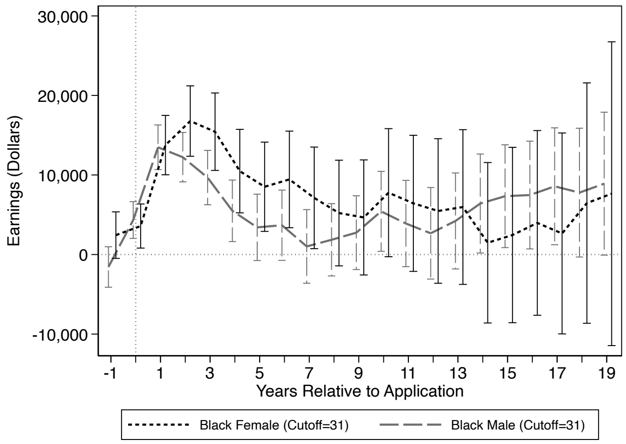
Figure A.11: Counterfactual Outcomes for Enlisted Compliers



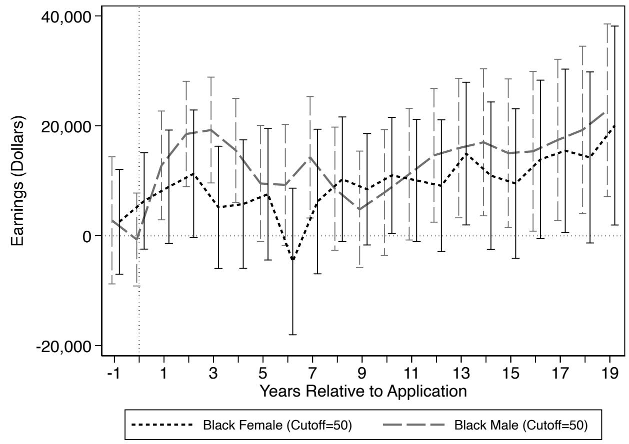
Notes: This Figure complements Figure 7 by plotting estimates of other counterfactual average outcomes of Black and White compliers at both cutoffs in the state of the world where they do not enlist by years since application. We estimate average potential outcomes y_i for compliers who do not enlist by running 2SLS regressions of $-y_i(1-Enlist_i)$ on $Enlist_i$. Panels (a) an (b) show counter-factual employment, panels (c) and (d) show 1040-based marriage (unconditional on filing a 1040), and panels (e) and (f) show average homeownership rates (1098 filing).

Figure A.12: Effects of Enlistment on Earnings by Sex and Race

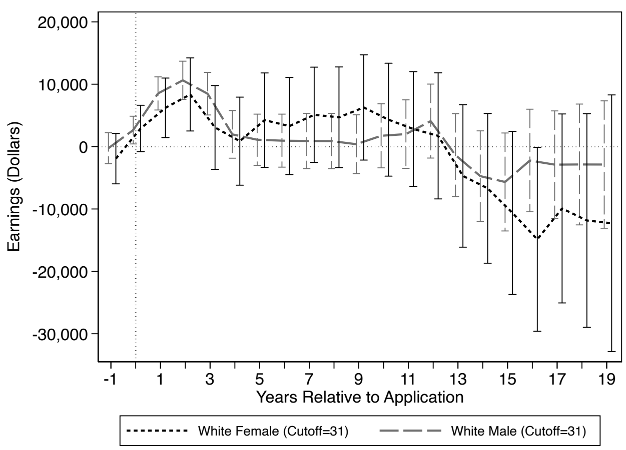
(a) Effects for Black Women and Men, 31 Cutoff



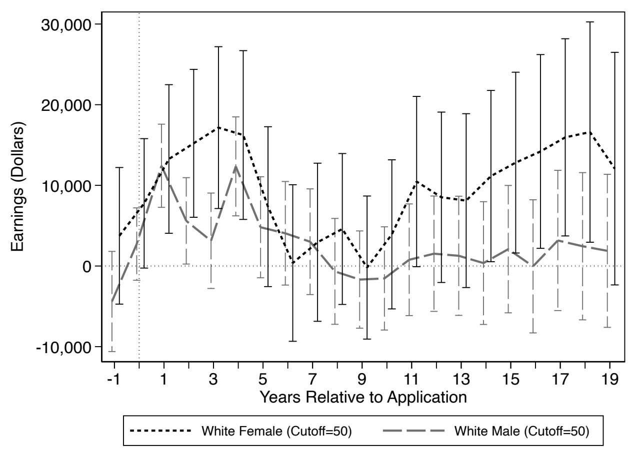
(b) Effects for Black Women and Men, 50 Cutoff



(c) Effects for White Women and Men, 31 Cutoff

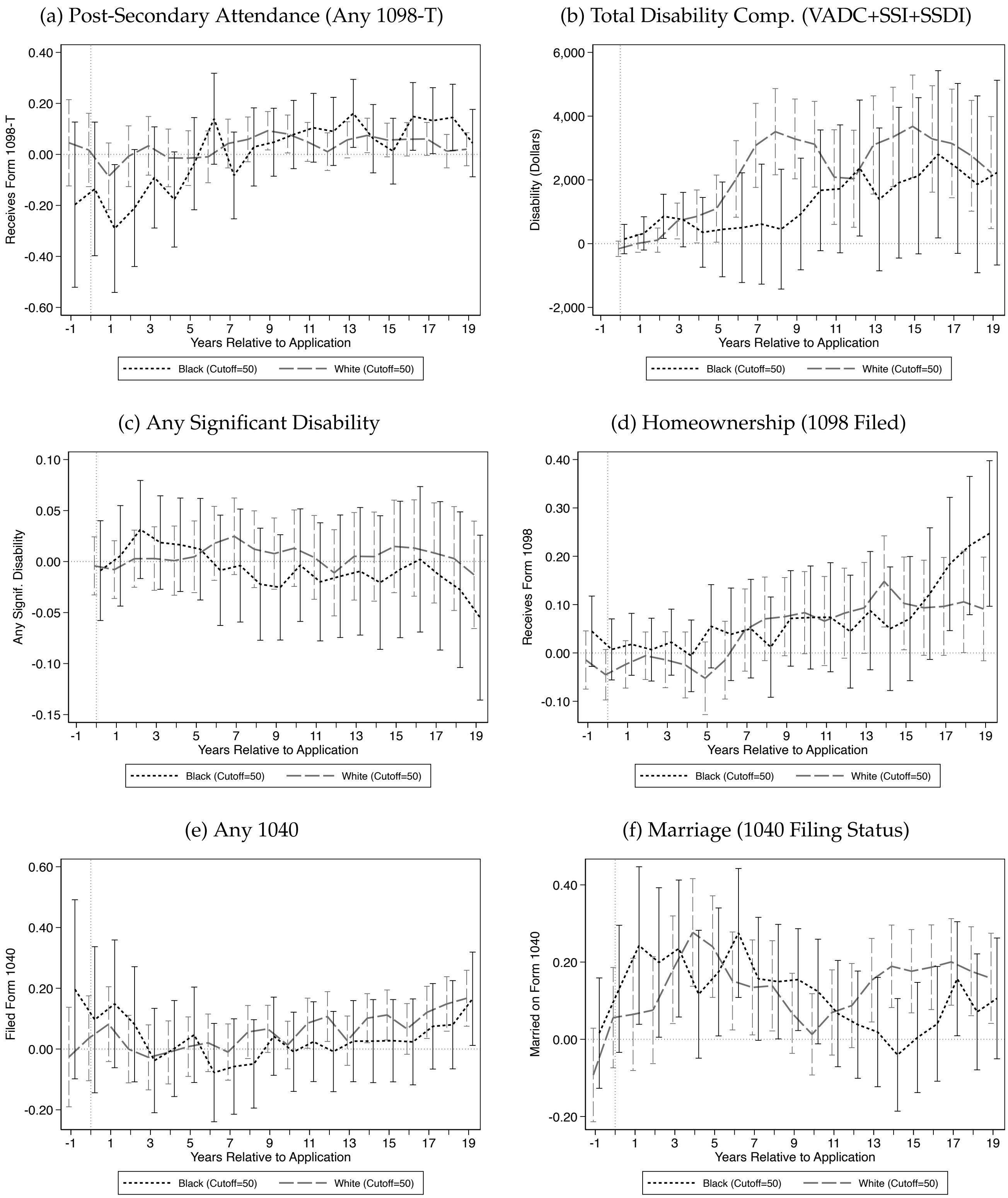


(d) Effects for White Women and Men, 50 Cutoff



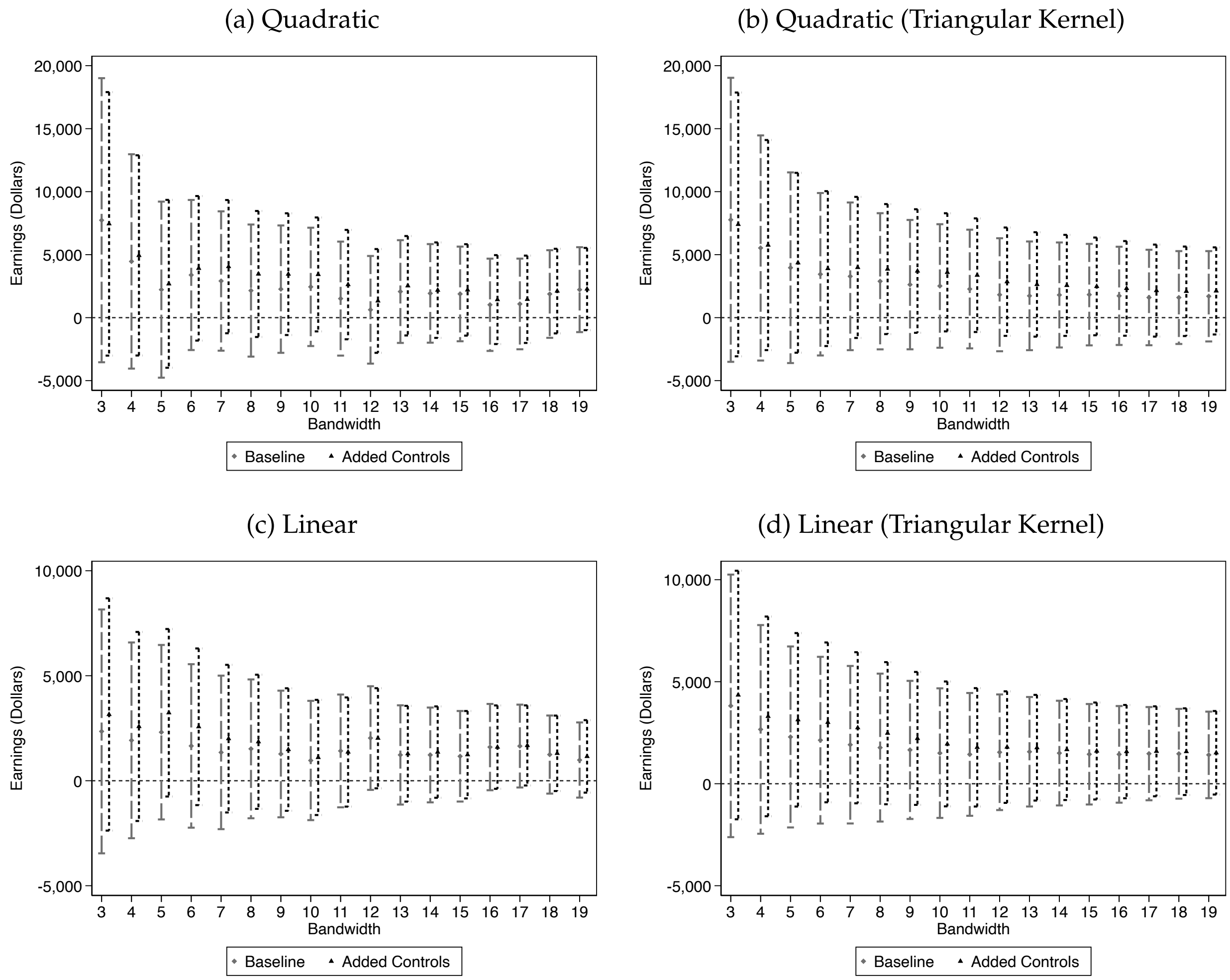
Notes: This figure reports 2SLS estimates of the effects of enlistment on earnings for sub-samples split by sex and race. Panel (a) compares the effects of Black women to Black men at the 31 cutoff, while panel (b) does so at the 50 cutoff. Panel (c) compares the effects of White women to White men at the 31 cutoff, while panel (d) does so at the 50 cutoff.

Figure A.13: Other Outcomes for Black and White Applicants (50 AFQT Cutoff)



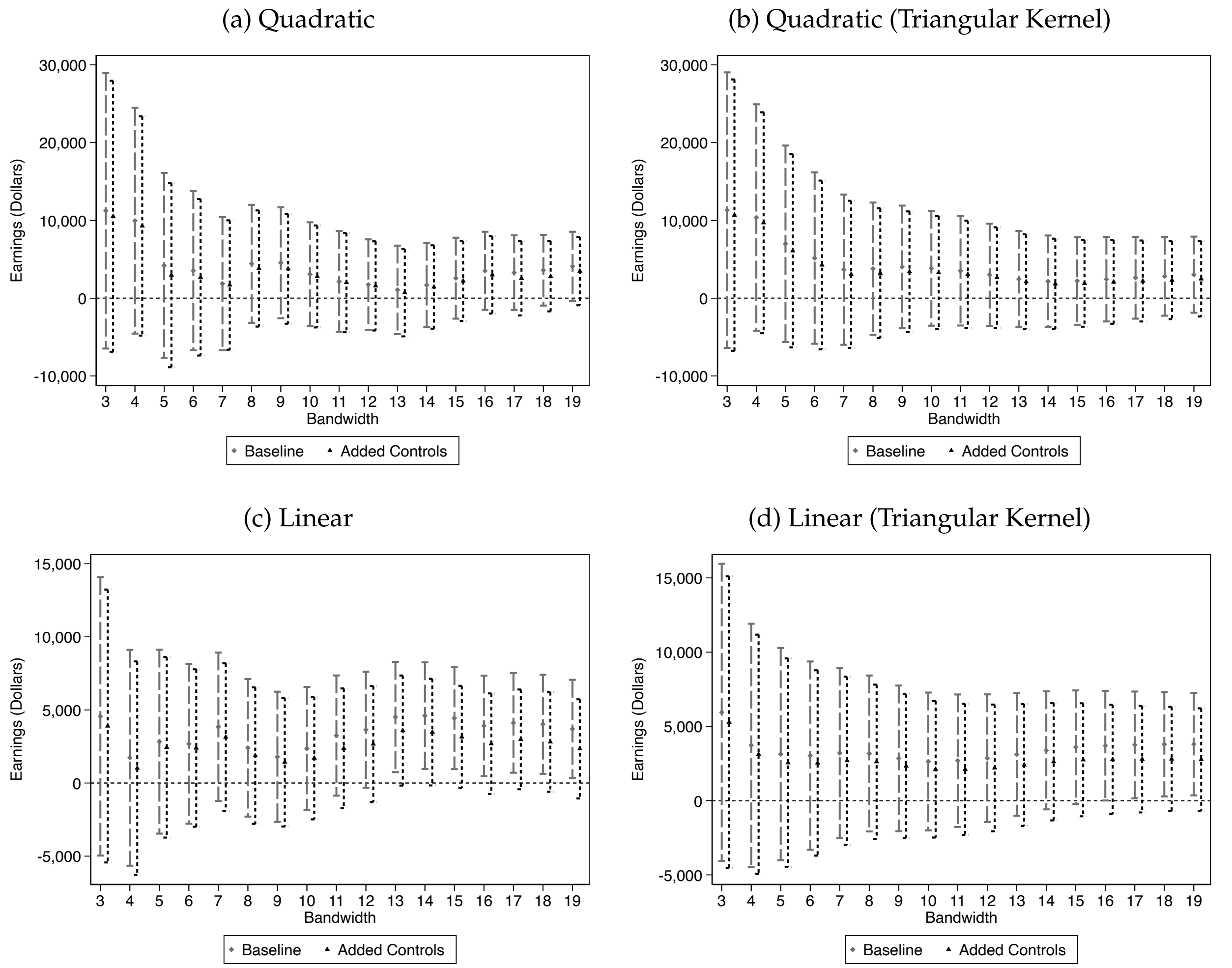
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by race. Throughout, we compare estimates for Black applicants (the dotted black line) to those for White applicants (the dashed grey line) at the 50 AFQT cutoff. Figure 10 contains the plots at the 31 cutoff. Panel (a) compares post-secondary attendance estimates, panel (b) compares total disability compensation estimates, and panel (c) compares any significant disability receipt estimates, where "significant disability" is defined as receiving a VADC combined disability rating of 100–which identifies an individual as fully disabled–or receiving any of SSI, SSDI, or VADC IU (each of which are work limiting), panel (d) compares mortgage estimates, panel (e) compares 1040 filing estimates, and panel (f) compares marriage estimates.

Figure A.14: Effects on Average Earnings 11-19 Years After Application, AFQT=31 Robustness Checks



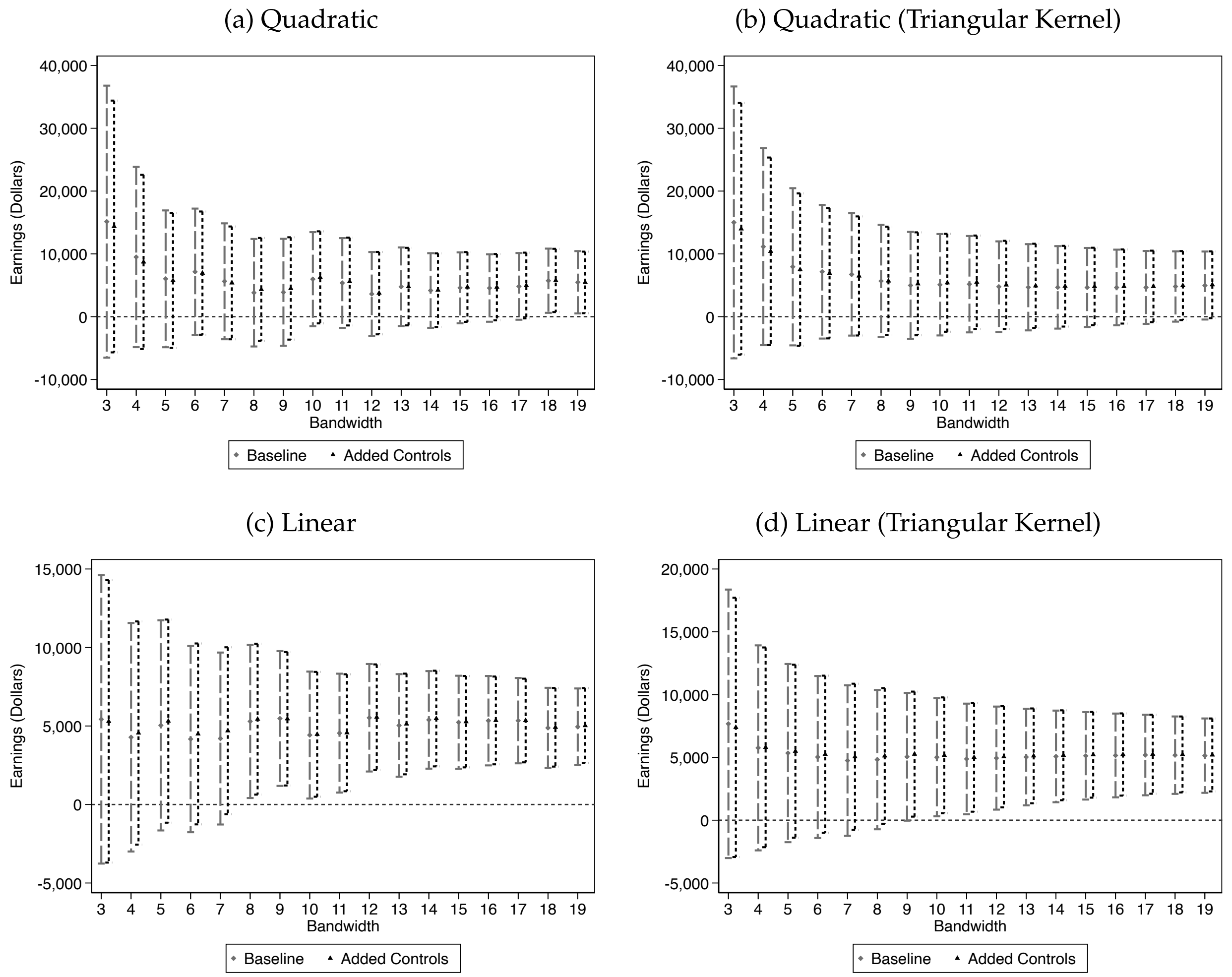
Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 years after application among applicants near the 31 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

Figure A.15: Effects on Average Earnings 11-19 Years After Application, AFQT=50 Robustness Checks



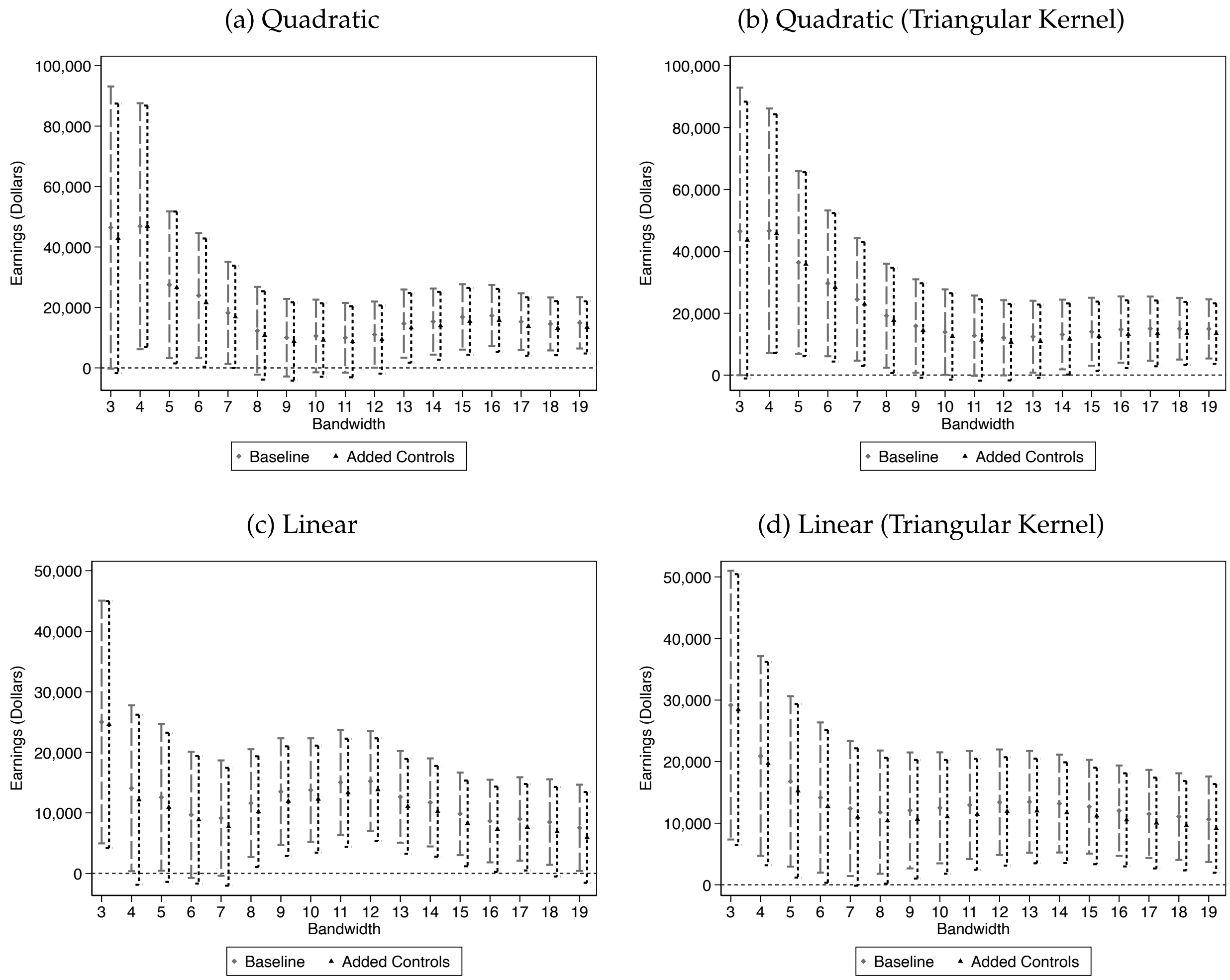
Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 years after application among applicants near the 50 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

Figure A.16: Effects on Average Earnings 11-19 Years After Application among Black Applicants, AFQT=31 Robustness Checks



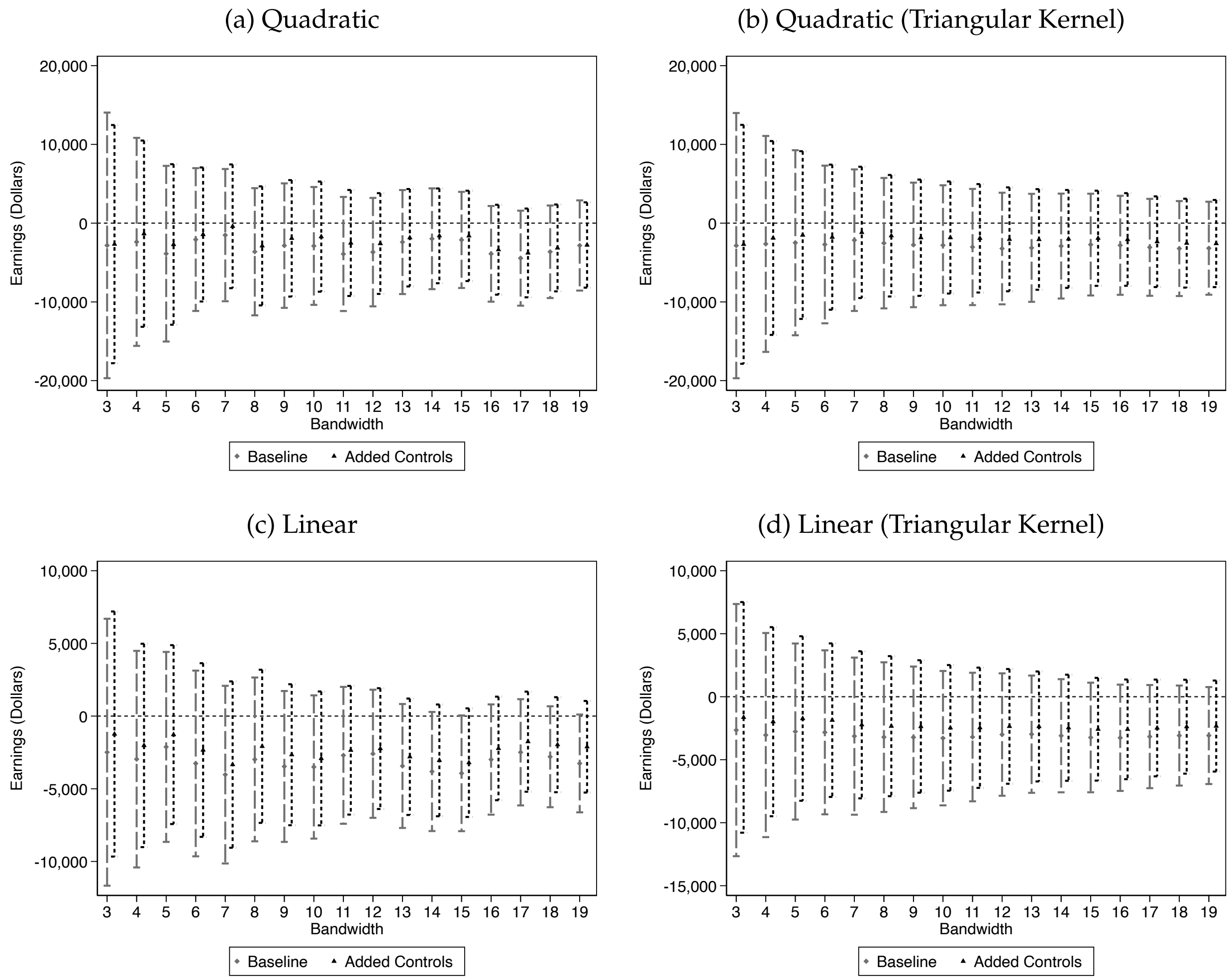
Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 years after application among Black applicants near the 31 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

Figure A.17: Effects on Average Earnings 11-19 Years After Application among Black Applicants, AFQT=50 Robustness Checks



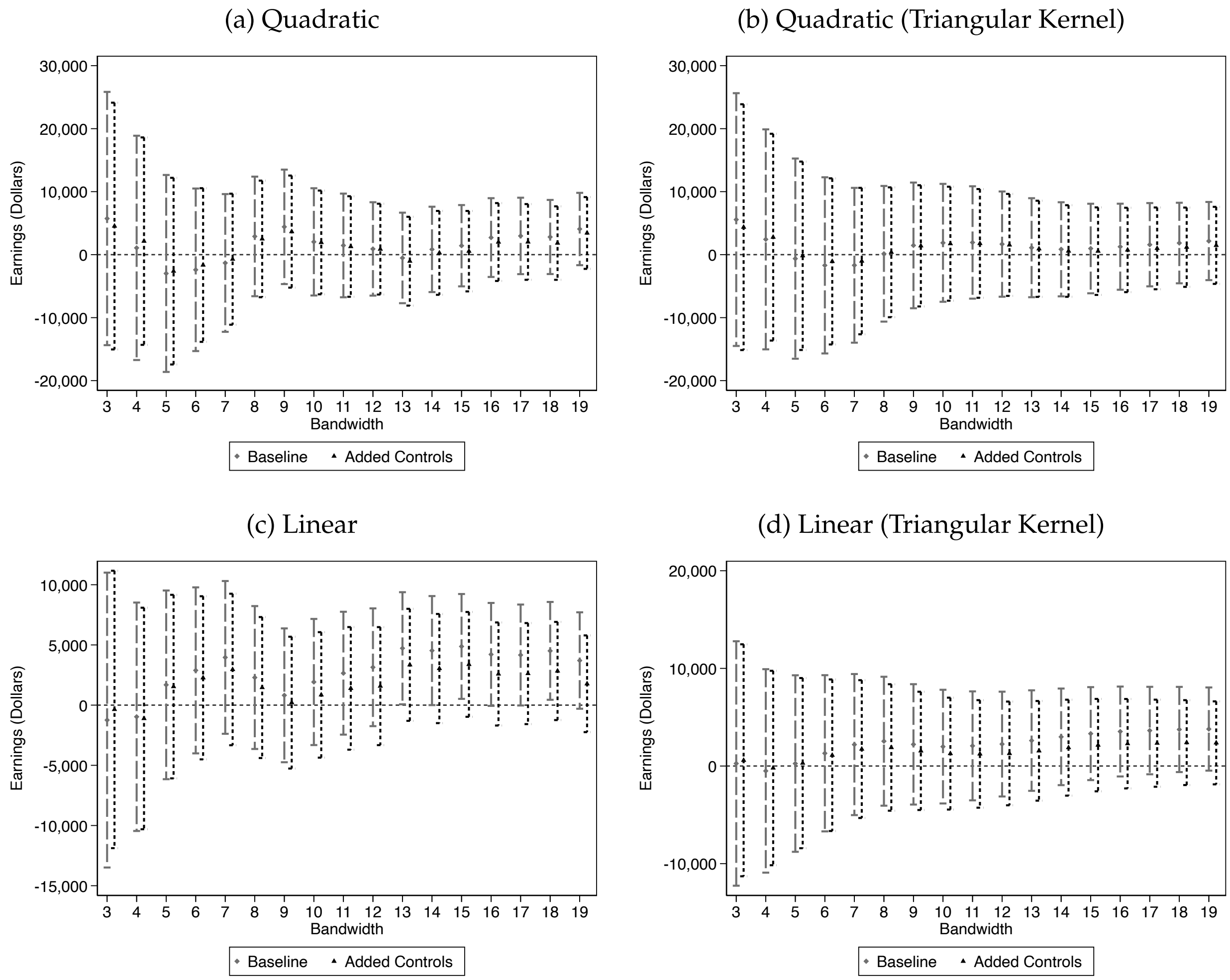
Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 years after application among Black applicants near the 50 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

Figure A.18: Effects on Average Earnings 11-19 Years After Application among White Applicants, AFQT=31 Robustness Checks



Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 after application among White applicants near the 31 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

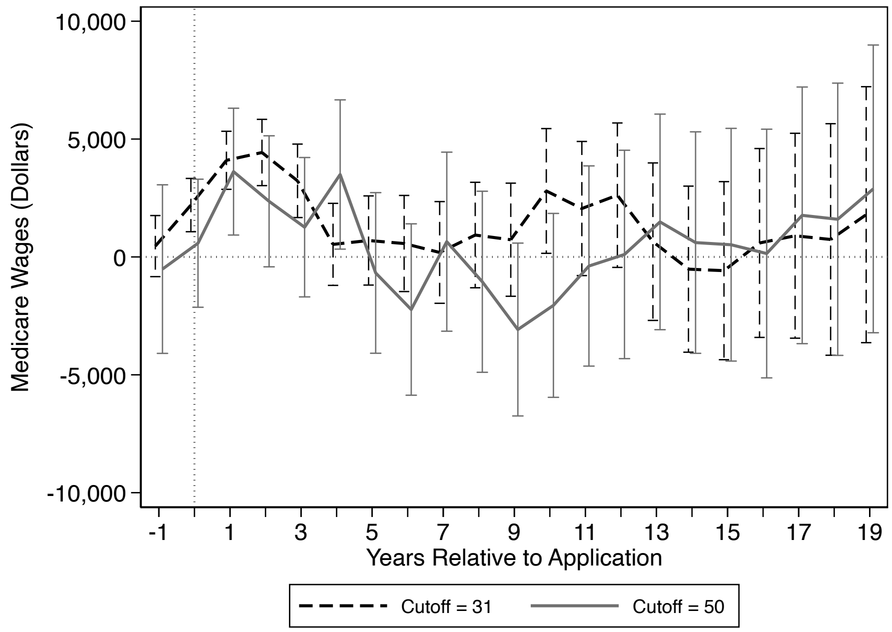
Figure A.19: Effects on Average Earnings 11-19 Years After Application among White Applicants, AFQT=50 Robustness Checks



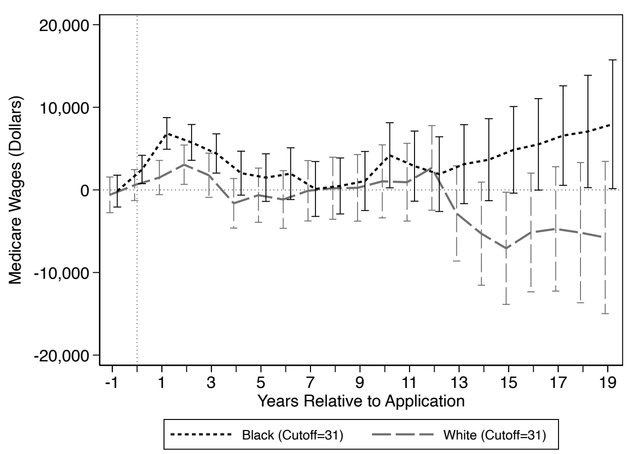
Notes: This figure shows 2SLS estimates of the effects of Army service on average earnings between 11-19 after application among White applicants near the 50 AFQT cutoff for various specifications, bandwidths, and controls. Specifications with controls include controls for: sex, race, age, education at time of application, and dummies for home of record state. Panel (a) shows quadratic (rectangular kernel) 2SLS RD estimates where BW=19 without controls is our primary specification. Panel (b) shows quadratic (triangular kernel) 2SLS RD estimates. Panel (c) shows linear 2SLS RD estimates (rectangular kernel). Panel (d) shows linear 2SLS RD estimates (triangular kernel).

Figure A.20: Effects of Enlistment on Earnings Without Including Housing and other Military Allowances (2SLS RD Estimates)

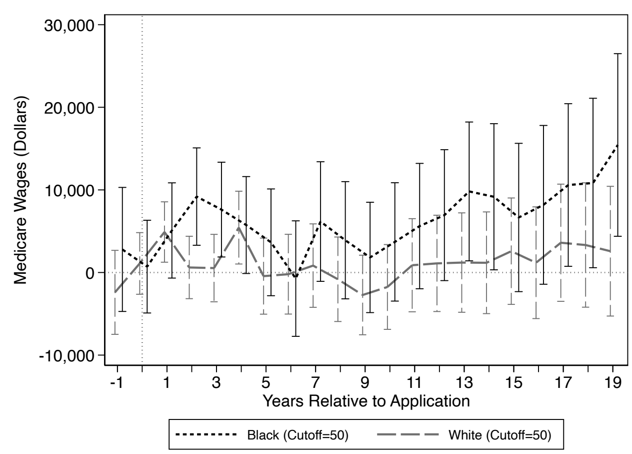
(a) All Applicants



(b) Black vs. White, AFQT=31

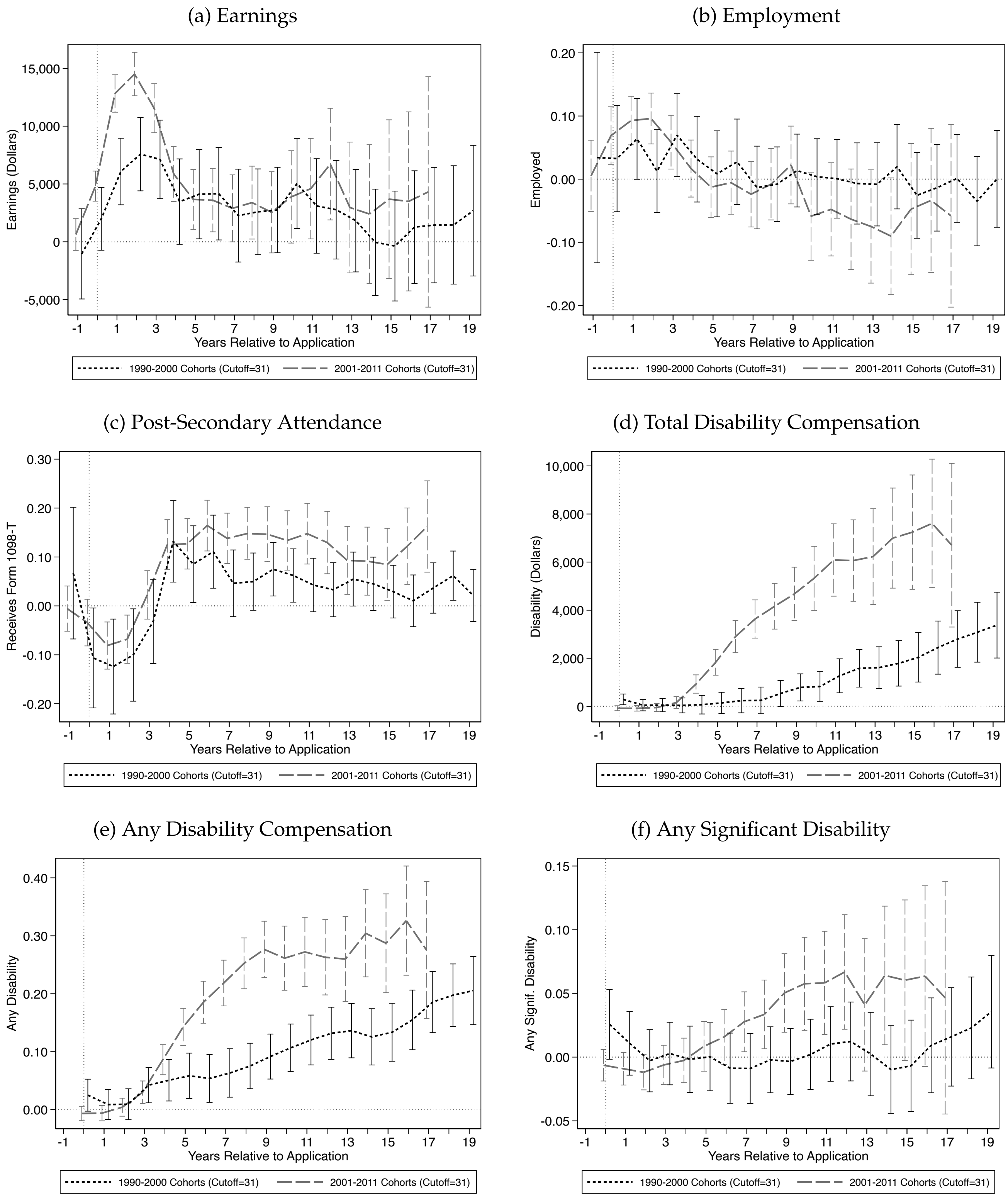


(c) Black vs. White, AFQT=50



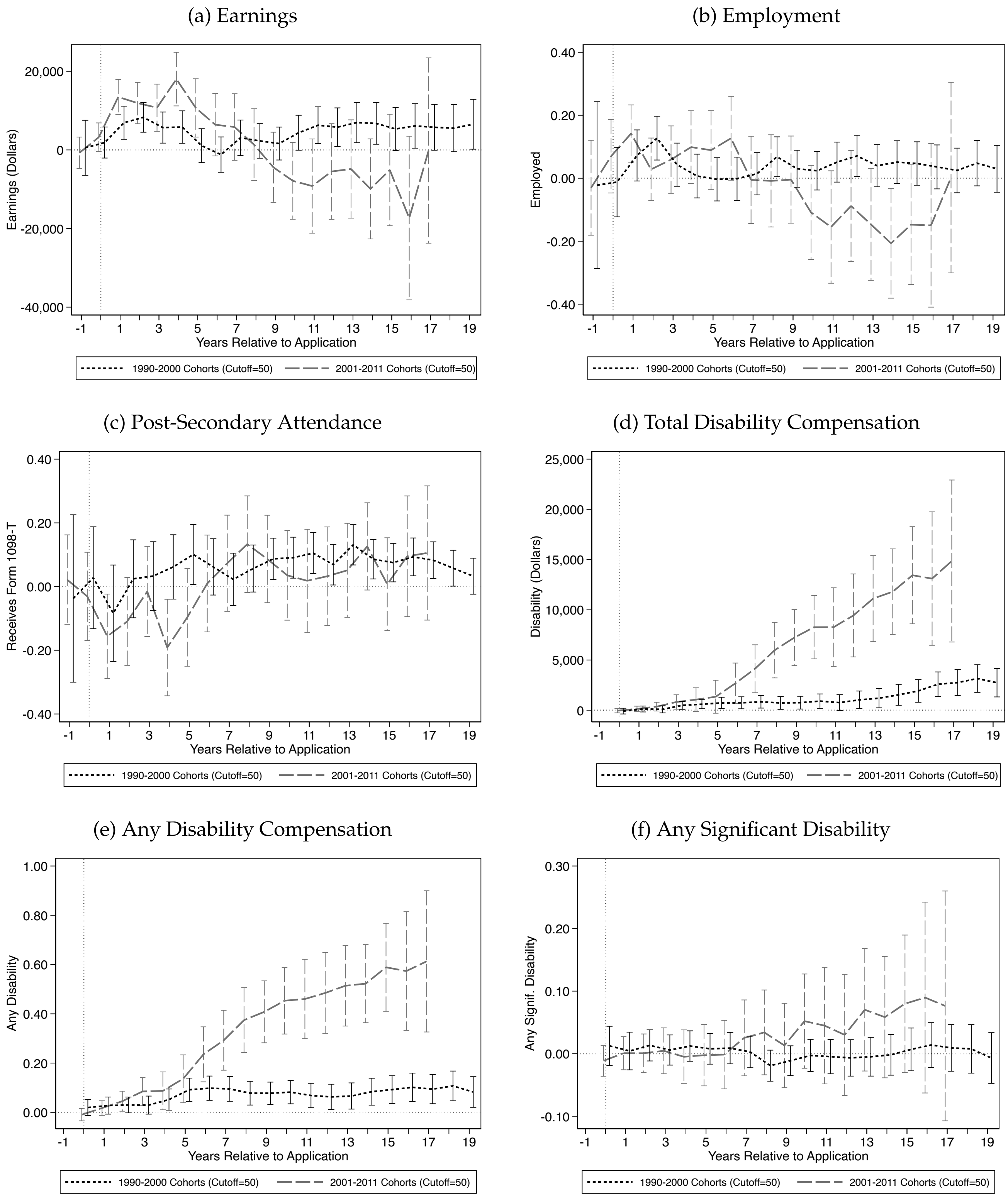
Notes: This figure plots 2SLS RD estimates of Equation (3) on raw Medicare W-2 earnings in years -2 to 19 after application. In panel (a) the dashed black line plots coefficient estimates and 95% confidence intervals for each year around the 31 AFQT cutoff, while the solid gray line does so around the 50 AFQT cutoff. In panels (b) and (c), the dotted black line plots coefficient estimates for Black applicants and the dashed gray line plots coefficients for White applicants.

Figure A.21: 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. Figure A.22 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 total disability compensation estimates (e) compares any disability receipt estimates, and panel (f) compares any significant disability receipt estimates defined by receipt of SSDI, SSI, VADC with a combined disability rating of 100%, or VADC Individual Unemployability.

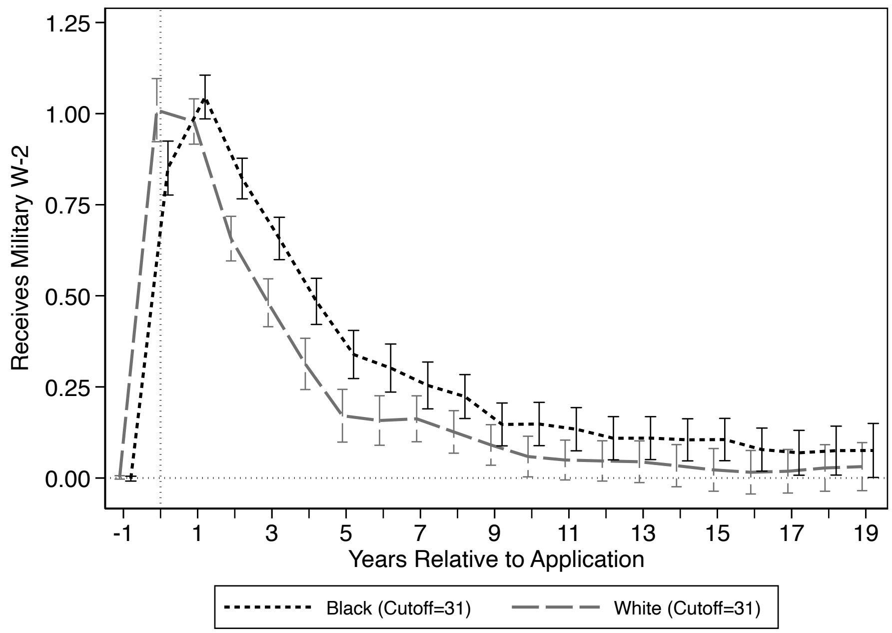
Figure A.22: Heterogeneity by Application Cohort (50 AFQT Cutoff)



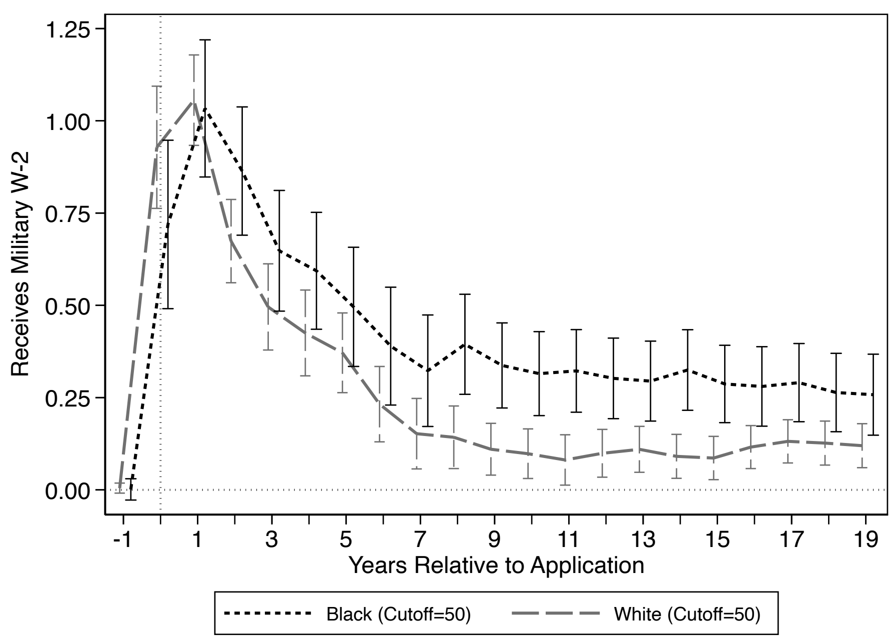
Notes: This figure plots 2SLS RD estimates of the effect of enlistment on earnings on subsamples split by application cohort. Throughout, we compare estimates for the 1990-2000 application cohorts (the dashed gray line) to those for the 2001-2011 cohorts (the dotted black line) at the 50 AFQT cutoff. Panel (a) compares 2SLS earnings estimates, panel (b) compares employment estimates, panel (c) compares post-secondary attendance estimates, panel (d) compares total disability compensation estimates, panel (e) compares any disability receipt estimates, and panel (f) compares any significant disability receipt estimates defined by receipt of SSDI, SSI, VADC with a combined disability rating of 100%, or VADC Individual Unemployability.

Figure A.23: Still in the Military, By Race

(a) Has a Military W-2 in Given Year, 31 Cutoff



(b) Has a Military W-2 in Given Year, 50 Cutoff



Notes: This figure reports 2SLS estimates on receiving a military W-2 in the given year since application. Throughout, we compare estimates for Black applicants (the dotted black line) to those for White applicants (the dashed grey line). Panel (a) plots coefficient estimates at the 31 cutoff, while panel (b) does so at the 50 cutoff.

Appendix Tables

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

	(1)	(2)
1(AFQT≥31)	0.100***	
	(0.003)	
1(AFQT≥50)		0.060***
		(0.003)
Number of Observations	1,137,595	1,311,111
Dep. Var. Mean	0.396	0.548
F-stat	1426	555

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: 2SLS RD Estimates For Main Outcomes

Years Since App	Earnings	Earnings	Emp.	Emp.	1098-T	1098-T	Any Dis.	Any Dis.
AFQT Cutoff:	31	50	31	50	31	50	31	50
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
-1	464	-511	0.008	-0.029	0.002	0.013		
	(661)	(1825)	(0.027)	(0.069)	(0.022)	(0.064)		
	[555286]	[658666]	[555286]	[658666]	[555286]	[658666]		
0	4292***	2918**	0.062***	0.047	-0.047**	-0.014	-0.001	-0.002
	(599)	(1451)	(0.021)	(0.046)	(0.022)	(0.056)	(0.006)	(0.010)
	[612247]	[721660]	[612247]	[721660]	[612247]	[721660]	[621203]	[728244]
1	11157***	11237***	0.085***	0.118***	-0.090***	-0.132**	-0.003	0.020*
	(725)	(1667)	(0.017)	(0.034)	(0.022)	(0.052)	(0.006)	(0.012)
	[671070]	[787748]	[671070]	[787748]	[671070]	[787748]	[681150]	[795268]
2	12487***	10301***	0.073***	0.072**	-0.075***	-0.050	0.005	0.038***
	(831)	(1723)	(0.017)	(0.032)	(0.023)	(0.048)	(0.007)	(0.014)
	[734580]	[861418]	[734580]	[861418]	[734580]	[861418]	[746048]	[870086]
3	10080***	8214***	0.062***	0.050	0.007	0.010	0.033***	0.057***
	(923)	(1842)	(0.018)	(0.032)	(0.022)	(0.045)	(0.008)	(0.017)
	[793037]	[925594]	[793037]	[925594]	[793037]	[925594]	[805690]	[935236]
4	5008***	11424***	0.022	0.050	0.129***	-0.053	0.073***	0.068***
	(1027)	(1947)	(0.019)	(0.033)	(0.022)	(0.044)	(0.011)	(0.022)
	[847770]	[986271]	[847770]	[986271]	[847770]	[986271]	[861441]	[996790]
5	3809***	5209**	-0.005	0.036	0.113***	0.018	0.110***	0.111***
	(1102)	(2047)	(0.020)	(0.034)	(0.022)	(0.043)	(0.013)	(0.025)
	[894892]	[1037158]	[894892]	[1037158]	[894892]	[1037158]	[909581]	[1048463]
6	3808***	1887	0.007	0.050	0.144***	0.042	0.132***	0.153***
	(1162)	(2126)	(0.021)	(0.034)	(0.022)	(0.041)	(0.014)	(0.027)
	[948893]	[1097382]	[948893]	[1097382]	[948893]	[1097382]	[964865]	[1109758]
7	2672**	4110*	-0.019	0.007	0.101***	0.044	0.151***	0.169***
	(1212)	(2166)	(0.021)	(0.034)	(0.021)	(0.039)	(0.015)	(0.028)
	[1000427]	[1155868]	[1000427]	[1155868]	[1000427]	[1155868]	[1017770]	[1169464]
8	3056**	1974	-0.007	0.043	0.101***	0.085**	0.164***	0.176***
	(1234)	(2141)	(0.021)	(0.033)	(0.020)	(0.036)	(0.015)	(0.027)
	[1028269]	[1187171]	[1028269]	[1187171]	[1028269]	[1187171]	[1047167]	[1202378]
9	2707**	-237	0.019	0.020	0.108***	0.089***	0.174***	0.178***
	(1308)	(2014)	(0.021)	(0.030)	(0.020)	(0.031)	(0.016)	(0.025)
	[1064711]	[1222565]	[1064711]	[1222565]	[1064711]	[1222565]	[1085845]	[1239794]
10	4602***	879	-0.022	-0.014	0.093***	0.077**	0.169***	0.187***
	(1432)	(2123)	(0.023)	(0.031)	(0.021)	(0.031)	(0.017)	(0.026)
	[1016643]	[1163949]	[1016643]	[1163949]	[1016643]	[1163949]	[1037347]	[1180842]
11	3752**	2565	-0.017	0.002	0.084***	0.085***	0.178***	0.162***
	(1533)	(2297)	(0.024)	(0.033)	(0.021)	(0.032)	(0.018)	(0.027)
	[969081]	[1109460]	[969081]	[1109460]	[969081]	[1109460]	[989135]	[1125879]
12	4242***	3219	-0.027	0.035	0.068***	0.061**	0.178***	0.159***
	(1645)	(2379)	(0.026)	(0.033)	(0.021)	(0.031)	(0.019)	(0.028)
	[930408]	[1066121]	[930408]	[1066121]	[930408]	[1066121]	[949830]	[1082083]
13	2192	4414*	-0.029	0.000	0.067***	0.114***	0.175***	0.162***
	(1791)	(2455)	(0.027)	(0.033)	(0.022)	(0.030)	(0.020)	(0.028)
	[882000]	[1013570]	[882000]	[1013570]	[882000]	[1013570]	[900658]	[1028985]
14	675	3319	-0.012	-0.002	0.059***	0.094***	0.178***	0.174***
	(1885)	(2513)	(0.028)	(0.033)	(0.022)	(0.029)	(0.021)	(0.027)
	[838698]	[966232]	[838698]	[966232]	[838698]	[966232]	[856578]	[981145]
15	611	3526	-0.031	0.013	0.043*	0.063**	0.173***	0.179***
	(2015)	(2631)	(0.029)	(0.033)	(0.023)	(0.029)	(0.022)	(0.028)
	[800809]	[918715]	[800809]	[918715]	[800809]	[918715]	[818021]	[933088]
16	1742	3456	-0.018	0.015	0.033	0.094***	0.190***	0.156***
	(2132)	(2805)	(0.030)	(0.035)	(0.023)	(0.029)	(0.023)	(0.029)
	[753318]	[855548]	[753318]	[855548]	[753318]	[855548]	[769635]	[869300]
17	1779	5339*	-0.007	0.023	0.053**	0.085***	0.198***	0.132***
	(2311)	(2891)	(0.032)	(0.035)	(0.024)	(0.028)	(0.025)	(0.030)
	[699583]	[787209]	[699583]	[787209]	[699583]	[787209]	[715114]	[800376]
18	1459	5546*	-0.035	0.048	0.062**	0.058**	0.197***	0.107***
	(2611)	(3064)	(0.036)	(0.037)	(0.026)	(0.029)	(0.028)	(0.031)
	[641983]	[718639]	[641983]	[718639]	[641983]	[718639]	[656572]	[731114]
19	2687	6525**	0.000	0.030	0.021	0.033	0.205***	0.083***
	(2883)	(3238)	(0.039)	(0.038)	(0.027)	(0.029)	(0.030)	(0.032)
	[582309]	[652445]	[582309]	[652445]	[582309]	[652445]	[596016]	[664251]

Notes: This table contains the point estimates underlying outcomes in Figure 4, panel (a) of Figure 6, and panel (a) of Figure A.9. 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.3: Average Effects on Earnings, RD vs. OLS

	31 AFQT Cutoff		50 AFQT Cutoff	
	0-19 Yrs Since	11-19 Yrs Since	0-19 Yrs Since	11-19 Yrs Since
	(1)	(2)	(3)	(4)
	Panel (a): Primary (RD) Estimates			
Enlist	4,255***	2,223	4,379***	4,096*
	(1,034)	(1,719)	(1,625)	(2,267)
	Panel (b): OLS Estimates			
Enlist	8,295***	8,445***	7,750***	7,722***
	(37)	(59)	(35)	(55)
Dep. Var Mean	24,805	29,366	28,052	33,677
Observations	1,137,595	969,081	1,311,111	1,109,460

Notes: This table presents 2SLS RD and OLS estimates of the effect of enlistment on *average* earnings. Columns (1) and (2) estimate average effects at the 31 AFQT cutoff, while columns (3) and (4) do so at the 50 cutoff. In columns (1) and (3) the outcome is annual earnings averaged over 0-19 years since application. In columns (2) and (4) the outcome is annual earnings averaged over 11-19 years since application. All regressions weight each observation by the number of years we observe the corresponding individual in our data. We estimate the 2SLS RD effects of enlistment on total earnings in panel (a) and OLS estimates in panel (b). The OLS estimates include fixed effects for earliest AFQT on record along with application quarter-by-year fixed effects. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table A.4: 2SLS RD Estimates For College Attendance Type (Source: NSC)

Cutoff:	31	50
	(1)	(2)
Attend Post-Secondary	0.179***	0.102
	(0.032)	(0.074)
Attend 4-Yr College	0.162***	0.123*
	(0.028)	(0.071)
Attend 4-Yr Non-Profit (Pub. or Priv)	0.088***	0.159**
	(0.024)	(0.065)
Attend 4-Yr For-Profit	0.121***	0.019
	(0.021)	(0.052)
Attend 2-Yr College	0.122***	0.021
	(0.031)	(0.075)
Attend At Least Mod. Selective	0.042**	0.025
	(0.018)	(0.052)
Attend Min. Selective Or Less	0.151***	0.122
	(0.031)	(0.076)
Observations	621,203	728,244

Notes: Each row reports 2SLS RD estimates of the effect of enlistment on the stated outcome in years 0-19 after application for applicants near the cutoff identified in the column heading. Observations are weighted by number of years observed. Due to the dynamic effects of enlistment on education, estimates are limited to the 1999-2011 application cohorts. Column (1) presents estimates for applicants at the 31 AFQT cutoff and column (2) presents estimates for applicants at the 50 cutoff. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.5: Complier Characteristics, by Race

	31-Cutoff				50-Cutoff			
	Black		White		Black		White	
	sample mean	complier mean	sample mean	complier mean	sample mean	complier mean	sample mean	complier mean
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Male	0.664	0.706	0.801	0.825	0.655	0.579	0.801	0.798
Age	20.7	20.6	20.3	20.3	20.7	20.9	20.3	20.4
In High School	0.243	0.227	0.283	0.244	0.237	0.194	0.276	0.220
No HS Diploma	0.100	0.056	0.210	0.167	0.107	0.223	0.210	0.374
Some College +	0.041	0.033	0.026	0.027	0.058	0.023	0.036	0.039
Family Income	43,527	40,920	63,441	62,915	48,157	43,622	69,332	76,387
Region								
Northeast	0.129	0.118	0.147	0.146	0.142	0.154	0.145	0.133
Midwest	0.151	0.149	0.261	0.260	0.148	0.115	0.261	0.257
South	0.637	0.661	0.388	0.388	0.623	0.668	0.385	0.405
West	0.072	0.065	0.194	0.197	0.077	0.055	0.199	0.195
County Characteristics								
1990 Poverty rate	0.146	0.151	0.127	0.128	0.142	0.145	0.124	0.123
2000 Poverty rate	0.144	0.147	0.120	0.121	0.141	0.143	0.117	0.116
2000 Employment rate	0.580	0.576	0.596	0.595	0.583	0.578	0.600	0.602
1990 Median HH income	30,990	30,464	30,734	30,521	31,542	31,023	31,073	31,321
2000 Population density	2,671	2,581	869	856	2,954	2,952	828	740
1990 Single parent share	0.252	0.255	0.207	0.207	0.251	0.252	0.206	0.207

Notes: This table reports mean complier characteristics by race. Family Income is constructed as described in Figure A.1. County characteristic variables obtained from Opportunity Insights: *https://opportunityinsights.org/wp-content/uploads/2018/04/online_table4-2.dta*

Table A.6: Educational Attendance and Graduation By Race (Source: NSC)

Cutoff:	31	50	31	50
Race:	Black	Black	White	White
	(1)	**(2)**	**(3)**	**(4)**
Attend Post-Secondary	0.135***	0.183	0.185***	0.139
	(0.051)	(0.154)	(0.052)	(0.104)
Attend 4-Yr College	0.176***	0.144	0.134***	0.155
	(0.050)	(0.165)	(0.042)	(0.095)
Attend 4-Yr Non-Profit (Pub. or Priv)	0.046	0.300*	0.095***	0.208**
	(0.045)	(0.162)	(0.036)	(0.086)
Attend 4-Yr For-Profit	0.206***	0.011	0.062**	-0.056
	(0.041)	(0.138)	(0.031)	(0.068)
Attend 2-Yr College	0.088*	0.191	0.129**	-0.002
	(0.052)	(0.169)	(0.050)	(0.104)
Attend At Least Mod. Selective	0.040	0.044	0.048*	0.054
	(0.034)	(0.132)	(0.026)	(0.066)
Attend Min. Selective Or Less	0.078	0.224	0.145***	0.165
	(0.052)	(0.167)	(0.051)	(0.104)
Observations	160,453	136,543	301,801	431,762

Notes: Each row reports 2SLS RD estimates of the effect of enlistment on the stated outcome in years 0-19 after application for applicants near the cutoff identified in the column heading. Observations are weighted by number of years observed. Due to the dynamic effects of enlistment on education, estimates are limited to the 1999-2011 application cohorts. In column (1) we report estimates for Black applicants at the 31 cutoff, in column (2) we report estimates for Black applicants at the 50 cutoff, in column (3) we report estimates for White applicants at the 31 cutoff, and in column (4) we report estimates for White applicants at the 50 cutoff. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table A.7: 2SLS RD Cumulative Mortality Estimates By Years Since Application, by Race

	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)	Died w/in 19 years (6)
			Panel (a): 31 AFQT Cutoff Black			
Enlist	-0.00193	-0.00490	-0.00692	-0.01006	-0.00213	0.00153
	(0.00274)	(0.00455)	(0.00596)	(0.00993)	(0.01420)	(0.02196)
Number of Observations	346,382	346,382	346,382	313,083	271,111	207,158
Dep. Var. Mean	0.00098	0.00248	0.00451	0.01027	0.01679	0.02322
			Panel (b): 31 AFQT Cutoff White			
Enlist	0.00077	-0.00526	0.00815	0.00833	0.02599	-0.00517
	(0.00365)	(0.00586)	(0.00734)	(0.01215)	(0.01833)	(0.02536)
Number of Observations	548,863	548,863	548,863	492,808	381,014	276,587
Dep. Var. Mean	0.00150	0.00389	0.00643	0.01422	0.02105	0.02651
			Panel (c): 50 AFQT Cutoff Black			
Enlist	0.00539	0.01042	0.00973	-0.00054	-0.02185	-0.02728
	(0.00541)	(0.00865)	(0.01112)	(0.01685)	(0.02131)	(0.02673)
Number of Observations	284,806	284,806	284,806	255,246	221,591	165,347
Dep. Var. Mean	0.00089	0.00246	0.00443	0.01014	0.01646	0.02183
			Panel (d): 50 AFQT Cutoff White			
Enlist	0.00167	0.00656	0.00314	-0.01958	-0.02461	-0.01796
	(0.00470)	(0.00747)	(0.00943)	(0.01310)	(0.01581)	(0.01866)
Number of Observations	789,996	789,996	789,996	707,323	560,705	398,741
Dep. Var. Mean	0.00148	0.00391	0.00652	0.01400	0.02071	0.02541

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 for Black applicants, panel (b) shows the 2SLS RD estimates for White applicants at the 31 cutoff, panel (c) shows 2SLS RD estimates for Black applicants at the 50 cutoff, and panel (d) shows 2SLS RD estimates for White applicants at the 50 cutoff. Columns (1)-(6) show the effect of enlistment on deaths within 1, 3, 5, 10, 15, and 19 years respectively. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.8: Average Effects on Earnings 11-19 Years Post Application, Inference Checks

	31 AFQT Cutoff					50 AFQT Cutoff				
	2SLS RD		Reduced Form			2SLS RD		Reduced Form		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel (a): All					
Estimated Coef.	2,223	2,223**	200	200*	200	4,096*	4,096**	297*	297**	297*
	(1,719)	(1,126)	(156)	(101)	(164)	(2,267)	(1,955)	(166)	(144)	(155)
	[0.196]	[0.048]	[0.199]	[0.055]	[0.232]	[0.071]	[0.036]	[0.073]	[0.047]	[0.063]
Observations	969,081	969,081	969,081	969,081	38	1,109,460	1,109,460	1,109,460	1,109,460	38
Dep. Var Mean	29,366	29,366	29,366	29,366	29,366	33,677	33,677	33,677	33,677	33,677
Clustered by AFQT	–	x	–	x	–	–	x	–	x	–
Grouped Means	–	–	–	–	x	–	–	–	–	x
					Panel (b): Black Applicants					
Estimated Coef.	5,482**	5,482***	533**	533***	533*	14,914***	14,914***	1,159***	1,159***	1,160***
	(2,532)	(2,028)	(250)	(193)	(265)	(4,336)	(4,749)	(340)	(355)	(372)
	[0.030]	[0.007]	[0.033]	[0.009]	[0.053]	[0.001]	[0.002]	[0.001]	[0.002]	[0.004]
Observations	302,572	302,572	302,572	302,572	38	246,640	246,640	246,640	246,640	38
Dep. Var Mean	27,121	27,121	27,121	27,121	27,121	31,571	31,571	31,571	31,571	31,571
Clustered by AFQT	–	x	–	x	–	–	x	–	x	–
Grouped Means	–	–	–	–	x	–	–	–	–	x
					Panel (c): White Applicants					
Estimated Coef.	-2,833	-2,833	-223	-223	-223	4,071	4,071	288	288	288
	(2,922)	(2,478)	(228)	(187)	(234)	(2,929)	(2,851)	(209)	(211)	(213)
	[0.332]	[0.253]	[0.328]	[0.241]	[0.347]	[0.165]	[0.153]	[0.168]	[0.181]	[0.185]
Observations	467,607	467,607	467,607	467,607	38	673,821	673,821	673,821	673,821	38
Dep. Var Mean	29,188	29,188	29,188	29,188	29,188	33,429	33,429	33,429	33,429	33,429
Clustered by AFQT	–	x	–	x	–	–	x	–	x	–
Grouped Means	–	–	–	–	x	–	–	–	–	x

Notes: This table compares our main estimates on earnings 11-19 years post application with specifications that cluster standard errors by AFQT score or that collapse earnings by AFQT score (grouped means). Columns (1)-(5) estimate average effects at the 31 AFQT cutoff, while columns (6)-(10) do so at the 50 cutoff. We construct grouped means by first regressing individual earnings on an indicator variable for each AFQT score while including quarter-by-year of application fixed effects. Then we regress the estimated AFQT fixed effects on an indicator for an individual's AFQT score being at or above the 31 or 50 cutoff while controlling for a quadratic function of AFQT that is allowed to differ on each side of the cutoff and weighting by the number of applicants with a specific AFQT score. Standard errors are reported in parentheses. P-values are reported in brackets. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.9: Average Effects on Earnings and Employment 11-19 Years After Application: 90-99 Application Cohorts

	31 AFQT Cutoff			50 AFQT Cutoff		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Black	White	Overall	Black	White
	Panel (a): Cumulative Earnings					
	1,967	5,800*	-4,156	6,125**	16,218***	5,935*
	(2,301)	(3,381)	(3,839)	(2,553)	(4,814)	(3,233)
Observations	582,309	207,158	276,593	652,445	165,348	398,748
Dep. Var Mean	30,267	27,735	30,353	34,849	32,405	34,711
	Panel (b): Employment					
	-0.004	0.027	-0.058	0.047*	0.071	0.082**
	(0.028)	(0.044)	(0.048)	(0.027)	(0.053)	(0.035)
Observations	582,309	207,158	276,593	652,445	165,348	398,748
Dep. Var Mean	0.805	0.808	0.800	0.819	0.822	0.814

Notes: This table presents 2SLS RD estimates of the effect of enlistment on *average* earnings and employment 11-19 years after application for for the 1990-1999 application cohorts. With this restriction, we observe earnings and employment in all years 11-19 for every individual in the sample. Columns (1)-(3) report average effects at the 31 AFQT cutoff, while columns (4)-(6) do so at the 50 cutoff. Columns (1) and (4) do so for the full sample, columns (2) and (5) do so for Black applicants and columns (3) and (6) do so for White applicants. We estimate the effect of enlistment on average earnings in panel (a) and on average employment in panel (b). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.10: Explaining The Black-White Gap in Effects of Service

	Black-White Gap 31	50	Source(s)
Effect to be explained at 19 years after application	13,451	17,052	Figure 4
Highest Amount Explained			
Stay in service, $33,000 Army pay premium	1,463	4,559	Figures 4 and A.23, Asch et al. (2014)
Disability:significant disability designation	1,093	1,573	Figures 4, 10, and A.13, Maestas et al., (2013)
Education associates degree attainment	181	938	Table 7, Jepsen et al. (2014)
Bachelors degree attainment	43	506	Table 7, Ashworth & Ransom (2019)
Post Service Human Capital via Earnings from Army Jobs	-810	1,788	Table A.5, Hahn et al. (2020)
Gap that can be potentially explained	1,970	9,364	
Unexplained Amount	11,481	7,688	

Notes: The $33,000 Army Pay premium is estimated based on a QRMC estimate in 2009 that servicemembers are paid approximately $30,000 more than similarly qualified civilians 19 years after application. To get $33,000, we adjust for the fact that QRMC inflates Army wages by approximately 6% to reflect tax-advantaged earnings of servicemembers and then adjust for inflation using the CPI-U. We recover estimates of the effect of staying in service by taking the Black-White difference in the 2SLS estimates for being in the military 19 years after application and multiplying this difference by the $33,000 premium. We attain estimates for significant disability compensation by taking the Black-White difference in the 2SLS estimates for "Any significant disability" 19 years after application and multiplying this by the mean 19 year earnings around the 31 cutoff ($32,139) and 50 cutoff ($37,471), respectively. This estimate makes the strong assumption that veterans have no earnings with significant disability and would have had sample average earnings with significant disability. Studies such as Maestas, Mullen, and Strand (2013) and French and Song (2014), suggest that the effects of SSDI decrease employment by much less: approximately 26-28 percentage points on the margin. If significant disability receipt had similar effects on employment in our sample, then our estimates are significantly overstated.

Table A.11: Occupations, Deployment, Combat Injuries/Deaths

	31-Cutoff Black (1)	31-Cutoff White (2)	50-Cutoff Black (3)	50-Cutoff White (4)
Combat Occupation	0.267***	0.395***	0.105***	0.377***
	(0.024)	(0.026)	(0.040)	(0.030)
Non-Combat Occupation	0.733***	0.605***	0.895***	0.623***
	(0.024)	(0.026)	(0.040)	(0.030)
Ever Deployed (0-19)	0.438***	0.365***	0.489***	0.452***
	(0.030)	(0.030)	(0.074)	(0.047)
Ever WIA (0-19)	0.010	0.005	0.002	0.059*
	(0.009)	(0.011)	(0.036)	(0.031)
Ever KIA (0-19)	0.001	-0.004	-0.006	0.000
	(0.002)	(0.004)	(0.011)	(0.010)
		Occupational Fields		
Infantry	0.060***	0.218***	0.024	0.177***
	(0.015)	(0.022)	(0.023)	(0.025)
Corps of Engineers	0.076***	0.099***	0.035*	0.049***
	(0.012)	(0.014)	(0.018)	(0.014)
Field Artillery	0.088***	-0.001	0.025	0.081***
	(0.014)	(0.014)	(0.024)	(0.017)
Armor	0.041***	0.107***	-0.008	0.016
	(0.011)	(0.014)	(0.016)	(0.015)
Adjutant General	-0.027**	0.002	0.230***	0.070***
	(0.013)	(0.007)	(0.030)	(0.011)
Medical CMF	0.007	0.009	0.134***	0.068***
	(0.009)	(0.007)	(0.027)	(0.013)
Quarter Master Corps	0.389***	0.187***	0.524***	0.186***
	(0.025)	(0.018)	(0.046)	(0.019)
Maintenance	0.155***	0.258***	0.081***	0.190***
	(0.018)	(0.019)	(0.030)	(0.021)
Other CMF	0.210***	0.121***	-0.046	0.164***
	(0.021)	(0.021)	(0.047)	(0.027)
Observations	353789	557244	289344	799603

Notes: This table estimates the 2SLS effect of enlistment on being in a combat occupation, being deployed, or being wounded or killed in action. It also shows the 2SLS effect of enlistment on occupational fields (CMFs). The estimation sample is all 1990-2011 cohorts for the occupation-related outcomes; 1992-2011 cohorts for the ever deployed outcome; and 2001-2011 cohorts for the ever wounded or killed in action outcomes. To account for missing occupation codes, occupation estimates replace the endogenous variable with an indicator for whether or not we observe the enlistee's occupation. Observations for "ever deployed" are 302,453; 486,942; 250,161; 696,963 while observations for "ever WIA" and "ever KIA" are 120,527; 247,517; 103,898; 355,046. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.12: Sector 19 Years After Application

	Black	White	Black	White
	31 Cutoff		50 Cutoff	
	(1)	(2)	(3)	(4)
Military	0.034	0.015	0.227***	0.079***
	(0.036)	(0.030)	(0.052)	(0.027)
Public Sector	0.096**	0.036	0.113*	0.025
	(0.047)	(0.043)	(0.064)	(0.037)
For-Profit	-0.083	-0.091	-0.172*	0.009
	(0.073)	(0.076)	(0.090)	(0.059)
Non-Profit	-0.012	-0.033	-0.082*	-0.042*
	(0.036)	(0.030)	(0.045)	(0.025)
Not Working	-0.034	0.080	-0.097	-0.064
	(0.061)	(0.067)	(0.073)	(0.049)
Observations	207,158	276,593	165,348	398,748

Notes: Each row of this table estimates a separate 2SLS regression for the effect of enlistment on being employed in the stated sector at year 19. Sector is assigned using EINs for a person's highest paying employer in year 19 (i.e. the employer from which the applicant earned the most in that year). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.13: Average Industry Pay 19 Years After Application

	Black	White	Black	White	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (a): Average Industry Pay, 31 Cutoff					
Enlist	6,888**	-559	7,362***	2,469	6,770**	2,073
	(3,161)	(3,271)	(2,817)	(2,742)	(2,810)	(2,703)
Observations	207,158	276,593	162,482	212,134	162,482	212,134
P-value for Equivalence	0.104		0.184		0.193	
Dropping Non-Working			X	X	X	X
In Military FE					X	X
	Panel (b): Average Industry Pay, 50 Cutoff					
Enlist	13,440***	4,459*	12,224***	2,338	8,913**	775
	(4,124)	(2,631)	(3,677)	(2,472)	(4,041)	(2,519)
Observations	165,348	398,748	132,558	313,583	132,558	313,583
P-value for Equivalence	0.074		0.033		0.093	
Dropping Non-Working			X	X	X	X
In Military FE					X	X

Notes: This table estimates the 2SLS effect of enlistment on the average industry pay of applicants' highest-paying job 19 years after application. We first map each applicant's highest paying employer (i.e. the employer from which the applicant earned the most in that year) 19 years after application to their six-digit NAICS industry code using the Employer Identification Number (EIN) on their W-2 form. We then, using a 50 percent random sample of full-population W-2 earnings of workers between 32 and 44 years of age, compute and assign each six-digit industry its average annual pay. In columns (1) and (2), those not working are assigned a zero. In columns (3)-(6) we focus on those working. Columns (5) and (6) include a control for whether the applicant is in the military at year 19. Significance levels: $*$: 10% $**$: 5% $***$: 1%.