ARMY SERVICE IN THE ALL-VOLUNTEER ERA*

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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 article 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 Qualification 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 postservice employment, the army significantly closes the Black-white earnings gap in our sample. *JEL Codes:* J15, J31, J45, H50.

I. 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 U.S. Army has recruited millions of young Americans to

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serve "with promises of individual opportunity" (Bailey 2009, ix). General Colin Powell said that "the military [has] given African-Americans more equal opportunity than any other institution in American society" (Powell and Persico 1996, 501). 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., Wilson and Kizer 1997; Breznitz 2005; Barr 2019). 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., Loughran and Heaton 2003; Autor et al. 2016; Bingley, Lundborg and Lyk-Jensen 2020).

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. In this article, we use data on the universe of Active Duty Army applicants from 1990 to 2011 and overcome the identification challenges inherent in disentangling enlistment decisions from other factors by exploiting two Armed Forces Qualification Test (AFQT) score cutoffs—at the 31st and 50th percentile of national math and verbal ability. The army rarely accepts applicants with AFQT scores below 31, often requires applicants to score 50 or higher to receive enlistment bonuses, and sometimes requires GED recipients to achieve a score of 50 or higher. 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 army applicants to their 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 after 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 benefits, we also find that the army considerably increases college attendance at both cutoffs. Although we find little effect of service on mortality, we do find large increases in disability compensation, which raises the monetary return to service but potentially reflects increased health risks.

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 with their counterfactual earnings trajectories in our sample, army service closes nearly all of the Black-white earnings gap. Moreover, the benefits of service are reflected in outcomes beyond earnings as we find that the army increases homeownership and marriage among Black Americans. 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 preapplication 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 postservice educational attainment explain only a small fraction of divergent returns to service by race. However, we 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.[1] Nevertheless, generous back-of-the-envelope calculations accounting for differences in army retention and pay (along with combat deployments, disability receipt, and postservice education) still 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 postservice earnings. Indeed, among Black applicants, army service increases the probability of employment in high-paying industries 19 years after enlisting. Service also increases Black applicants' employment in the public sector. These patterns are less evident for white applicants. 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.

I.A Related Literature

The Department of Defense (DoD) is the largest employer in the United States and affects the lives of many Americans—approximately 1 in 13 American adults and 1 in 7 men have served in the military (U.S. Census Bureau 2018). Yet existing causal studies of military service are primarily identified using conscription lotteries, which the United States ended in 1973 (e.g., Angrist 1990; Angrist, Chen, and Song 2011; Card and Cardoso 2012; Bingley, Lundborg and Lyk-Jensen 2020).[2] The effects of enlistment on those who choose to serve in today's all-volunteer force may differ 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 with nonveterans

1. Army base pay is strictly a function of military rank and years of service.

2. An exception is Angrist (1998). One approach in this article is an identification strategy that compares the earnings of low-scoring applicant cohorts in the 1970s who were mistakenly allowed to enlist to the earnings of applicant cohorts in the early 1980s after this mistake was corrected. This approach suggests postservice earnings losses of around \$1,000 (in 2018 US\$) for both white and nonwhite servicemembers.

(e.g., Teachman and Tedrow 2007; Kleykamp 2013; Makridis and Hirsch 2021) or by comparing applicants who enlist with 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 broad 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 educational 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 (Bound and Turner 2002; Bedard and Deschênes 2006; Dobkin and Shabani 2009; Angrist, Chen, and Frandsen 2010; Angrist and Chen 2011; Autor, Duggan, and Lyle 2011; Johnston, Shields, and Siminski 2016). Our direct estimates also provide important context for studies that examine the effects 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). Last, we are also able to study the effect of service on potential measures of well-being like homeownership, neighborhood quality, and marriage, which help provide a broader view of whether the army acts as a vehicle of upward mobility.

Finally, to our knowledge, this 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. This article suggests that the active duty U.S. military, which disproportionately employs Black Americans (currently more than 200,000; DoD 2020), could have an important role in reducing racial income gaps, contributing to a growing literature on differences in income mobility by race (e.g., Bhattacharya and Mazumder 2011; Mazumder 2014; Akee, Jones and Porter 2017; Chetty et al. 2020). Our finding that army service increases annual earnings of Black applicants by 20% to 50% ($5,500 to $15,000) is comparable to the 31% earnings increase caused by moving a young child to a neighborhood with a 12 percentage point lower poverty rate as estimated from the Moving to Opportunity Project (Chetty, Hendren, and Katz 2016), but contrasts with related experimental evidence that suggests moving to lower-poverty areas has little impact on outcomes for older children and adults (Kling, Liebman, and Katz 2007; Ludwig et al. 2013; Chetty, Hendren, and Katz 2016). 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., Greenberg, Michalopoulos, and Robins 2003; Card, Kluve, and Weber 2018), though they are comparable to estimates from recent sectoral-specific job-training programs (Katz et al. 2022).

II. Background: Application, Service, and Postservice Experiences

II.A. *The Application Process*

The application process begins with a visit to a local army recruiting office. After verifying 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, and the second day consists predominately of physical tests, medical examinations, and a meeting with an enlistment counselor. Four of the 10 tests in the ASVAB contribute to an applicant's raw 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 DoD further requires that no more than 4% of recruits have AFQT scores below 31 and that at least 60% 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, often limits enlistment bonuses to applicants with AFQT scores of 50 or higher, and sometimes requires GED recipients to achieve an AFQT score of 50 or higher (DoD 2004; U.S. Army 2012). These regulations create discontinuities in the probability of army service based on applicants' first AFQT scores of 31 and 50 (see Figure I, Panel A).

Although applicants are unlikely to be able to manipulate their first AFQT score around the cutoff, they can retake the ASVAB. Applicants must wait at least one month after their first attempt, one month after their second attempt, and six months after a third or subsequent attempt to retake the ASVAB. Online Appendix Table A.1 shows that 38% of applicants just below the 31 AFQT cutoff (scores 26–30) and 16% of applicants just below the 50 cutoff (scores 45–49) retake the AFQT at least once, with similar retake rates by race at both cutoffs. Only 5.9% of applicants just below the 31 cutoff and 1.4% of applicants just below the 50 cutoff retake the AFQT at least twice.

The final step of the two-day appointment at a MEPS is a meeting with an enlistment counselor. This counselor discusses which military occupational specialties (MOS) or jobs the applicant is eligible for, contract duration (typically three to six 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 4 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 with just \$1,620 for servicemembers who have an AFQT score of 49.

II.B. Characteristics of Army Service

Approximately 40% of applicants who enlist choose combat occupations (e.g., infantry or combat engineer), while others work as logistical specialists, personnel clerks, mechanics, and a variety of other noncombat occupations. The modal enlistee serves for a single enlistment term of 3–4 years, but roughly

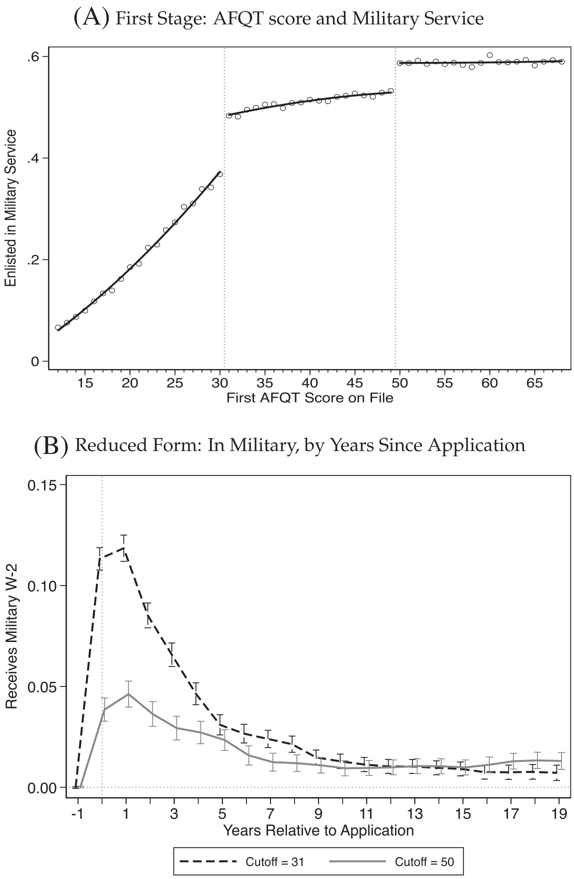

FIGURE I

AFQT scores and Military Service

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.

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. DoD casualty records indicate that 0.2% of enlistees are killed in action and about 2% are wounded in action.

II.C. 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 before 2008. Compared with the Montgomery GI Bill, the Post-9/11 GI Bill expanded eligibility, increased maximum tuition reimbursements, and introduced generous book and housing stipends.

Veterans can also apply for direct monetary compensation for injuries sustained or aggravated during their time in service through the VADC program. 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.[3] Many of the most common disabilities among recent veterans are consistent with physical overuse injuries. According to the 2019 VA Annual

3. One exception is VADC Individual Unemployability (IU) status. Veterans approved for IU status receive the highest possible amount of monthly VADC payments but are not allowed to participate in gainful employment.

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 to $3,500 depending on their degree of service-connected disability. Beyond direct monetary payments for service-connected disabilities, most veterans are eligible for lifetime free or subsidized VA healthcare, where the amount of cost sharing varies with income and service-connected disability rating.

III. Data and Sample

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 VADC records (1999–2018), and National Student Clearinghouse college education records (1999–2018).

III.A. Sample Construction

Our analysis sample consists of army applicants with a first AFQT taken 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 4.5% of applicants). Second, we exclude the approximately 10.5% of applicants who took their ASVAB in high school as part of the ASVAB Career Exploration Program.[4] Third, we restrict our analysis to 98.9% of the remaining applicants who we are able to match to Social Security records (see Online Appendix B.1).[5]

4. 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.

5. We also drop roughly 1.5% of applicants who have invalid AFQT scores (zero or missing), who are older than 35 and require a waiver to enlist, or who are younger than 17 and typically not eligible to apply outside of the Career Exploration Program.

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

Online Appendix Table A.2 presents summary statistics for all U.S. Army applicants between 1990 and 2011. Overall, applicants are young (20.7 years) and 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% versus 15%) and less likely to be Hispanic (11% versus 15%). Applicants are also more likely to come from disadvantaged counties in terms of household incomes, employment, and Chetty and Hendren (2018) measures of intergenerational mobility.[6] Moreover, we show in Online Appendix Figure A.1 that applicants come from families with about 15% lower median family income than a comparable national random sample.

III.B. Outcomes

For each person in our analysis sample, we link tax records from 1999–2018 (e.g., employer-filed W-2 forms) for up to 1 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 after application because those who serve are eligible for a generous army retirement pension at 20 years of service, which complicates the interpretation of wage and employment outcomes for 20 or more years after application.

Our primary outcome is individual earnings. We observe wages and earnings from two sources: (i) employer-provided

6. 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 intergenerational mobility estimates.

W-2 Wage and Tax statements and (ii) army administrative pay records on nontaxable allowances. Our baseline measure of pretax 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, 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).[7] 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 in each year. Online Appendix B.2 provides a detailed description of how we construct our earnings outcome and contains more information on military pay and allowances.

In addition to examining the effects of army service on earnings, we explore the effects of service 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. In addition, since higher-education institutions that receive federal financial aid are required to file a 1098-T on behalf of each enrolled student for whom they receive tuition or fees, we identify an individual as having attended college if her college submits a Form 1098-T on her behalf. We supplement the education outcomes with associate and bachelor's degree completion data from the National Student Clearinghouse (NSC). To measure disability compensation, we combine records from the VADC, SSDI, and SSI. 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. We also examine proxies for self-employment earnings, homeownership, and marriage from 1040 Schedule C/1099-MISC, 1098, and 1040 forms, respectively (see Online Appendix B.3 for details).

7. We do not include other benefits, such as health coverage, retirement contributions, or G.I. Bill tuition and related housing allowances, some of which are common across both military and civilian sectors.

IV. Estimating Framework

IV.A. Empirical Approach

Our empirical strategy takes advantage of cutoffs in the AFQT at scores of 31 and 50, as outlined in Section II. Figure I, Panel A 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 Online Appendix Table A.3) underlie the first stage of our fuzzy regression discontinuity (RD) identification strategy.[8] Online Appendix Figure A.2 and Table A.3 also report race-specific first-stage estimates, demonstrating strong first stages at both cutoffs for Black and white applicants.

In our fuzzy RD design, indicators for crossing the lower-ability (AFQT $\geqslant$ 31) and higher-ability (AFQT $\geqslant$ 50) cut scores act as instruments for our endogenous variable: an indicator variable for applicants who ever enlist in the U.S. military. While we define our endogenous variable as enlistment in any military service, the vast majority of enlistees in our army applicant sample joined the Active Duty Army. Crossing either cutoff has only modest effects on enlistment in non-Active Duty Army service, as can be seen in Online Appendix Table A.4, so our estimates can be reasonably interpreted as the effects of Active Duty Army service.[9] Specifically, our reduced-form estimating equation is:

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

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

8. In addition, Figure I, Panel B shows the estimated dynamic reduced-form effects of scoring at or above 31 and 50 AFQT cutoffs on military service. This panel reflects how 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.

9. Our data only include Active Duty Army applicants, but some eventually join other services, which have higher minimum eligibility thresholds. Roughly 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. Online Appendix Table A.4 shows how crossing the AFQT threshold affects enlistment in various branches of service.

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

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

$Enlist_i$ 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. $AFQT_i \geqslant CUT$ 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. In addition, $\boldsymbol{X_i}$ is a vector of preapplication characteristics, which always includes quarter-by-year of application fixed effects and additional controls when mentioned, and ϵ_i is an idiosyncratic error term. When estimating the effects of service among Black and white applicants, we estimate equations (1), (2), and (3) separately by race.

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. In addition, 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). Heteroskedasticity-robust standard errors are reported in all cases (White 1980).

The parameter of interest is β_2, which identifies the local average treatment effects (LATEs) of 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. At the lower cutoff, compliers are applicants who only receive an offer of enlistment when they achieve an AFQT score of 31 (offers to applicants with lower scores are rare) and who do not successively retake the ASVAB until a score of 31 or higher is realized. At the higher cutoff, most compliers are applicants who only receive a bonus offer when they achieve an AFQT score of 50, who are induced to serve because of the bonus offer, and who do not successively retake the ASVAB until a score of 50 is realized. In addition, some compliers at the higher cutoff are GED holders without a high-school diploma who only received an offer of enlistment when they achieved an AFQT score of 50. To the extent that the effects of service differ at each cutoff, one source could be differences in marginal applicants.

IV.B. 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). Although 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.[10]

To visually inspect the running variable for manipulation around either threshold, Online Appendix Figure A.3 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 DoD renormalized scores in July 2004, leading to a shift in the distribution of AFQT scores (Segall and Defense Manpower Data Center 2004). Notably, there is significant bunching at certain AFQT scores in both

10. 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% 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 six months between their third and fourth attempts, which reduces the likelihood of this behavior. Any issues introduced by this data limitation would likely be reflected by imbalance in baseline characteristics.

panels. Bunching occurs at points where multiple raw AFQT scores correspond to a single AFQT percentile score (Mayberry and Hiatt 1992; Segall and Defense Manpower Data Center 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 that applicants are unlikely to be manipulating their scores around the cutoff.[11] In comparison, Online Appendix Figure A.4 plots the distribution of each applicant's most recent AFQT score. These histograms reveal a strong effort on the part 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.

In addition, 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. Online Appendix Figure A.5 plots averages for certain baseline characteristics by AFQT score for all applicants, and Online Appendix Figures A.6 and A.7 plot averages of baseline characteristics by AFQT score when restricting the sample to Black or white applicants. There does not appear to be any substantial imbalance across either cutoff.

We complement these figures with Table I, which reports estimates of equation (1) where the dependent variables are the baseline characteristics. Table I, columns (1) and (2) confirm the balance in covariates across AFQT cutoffs shown in Online Appendix Figure A.5. 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

11. 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 .87 at the 31 cutoff and .24 at the 50 cutoff).

TABLE I

COVARIATE BALANCE (REDUCED-FORM ESTIMATES)

	All		Black		White	
	31 cutoff (1)	50 cutoff (2)	31 cutoff (3)	50 cutoff (4)	31 cutoff (5)	50 cutoff (6)
Time of application						
Age	0.004	– 0.016	0.001	– 0.008	– 0.039	– 0.024
	(0.018)	(0.017)	(0.033)	(0.041)	(0.025)	(0.021)
Male	– 0.003	0.003	– 0.004	0.008	– 0.003	0.004
	(0.002)	(0.002)	(0.005)	(0.005)	(0.003)	(0.003)
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	0.008*	0.005	– 0.002
	(0.002)	(0.002)	(0.004)	(0.005)	(0.004)	(0.003)
No HS diploma	– 0.000	– 0.001	– 0.004	– 0.006*	0.004	– 0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
HS diploma	– 0.005*	– 0.001	– 0.002	– 0.006	– 0.007*	0.002
	(0.003)	(0.003)	(0.005)	(0.006)	(0.004)	(0.003)
Some college+	0.002**	0.001	0.004**	0.004	– 0.002	0.001
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)
Number of observations	1,137,595	1,311,111	346,383	284,808	548,871	790,004
Year prior to application						
Earnings	55.735	– 21.338	– 20.250	127.260	– 62.295	– 91.706
	(79.423)	(76.107)	(140.197)	(170.878)	(115.799)	(97.692)
Employment	0.001	– 0.001	0.001	– 0.008	0.002	0.002
	(0.003)	(0.003)	(0.006)	(0.007)	(0.005)	(0.004)
Filed taxes	– 0.001	0.001	– 0.003	0.009	– 0.004	– 0.001
	(0.003)	(0.003)	(0.006)	(0.007)	(0.004)	(0.003)
Postsecondary attendance	0.000	0.001	0.004	– 0.009	– 0.004	0.002
	(0.003)	(0.003)	(0.006)	(0.007)	(0.004)	(0.003)
Married	0.002	– 0.002	– 0.002	0.001	0.002	– 0.004
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Number of observations	555,286	658,666	139,225	119,460	272,278	391,256
p-value for joint significance	.323	.902	.596	.169	.314	.685

Notes. This table reports reduced-form RD estimates of equation (1) where the left-hand-side variable is the covariate and preapplication 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 Online Appendix Table A.2. IRS outcomes in the year prior to application are available for the 2000–2011 applicant cohorts.

cutoffs (p-values of .32 and .90, respectively) are consistent with balance. Subsequent columns of Table I confirm balance in covariates across both AFQT cutoffs separately for Black and white applicants. Altogether, the results in Table I, combined with the lack of observable manipulation in the AFQT histograms, suggest that there is no systematic sorting around either threshold.

V. Effects of Army Service

We begin this section by presenting the effects of enlistment on earnings and employment for army applicants between 1990 and 2011. We also report effects of service on mediators of earnings, including education, mortality, and disability. These overall estimates mask substantial heterogeneity by race. In Section V.B, we show that Black enlistees experience differentially large long-run increases in earnings. In contrast, army service produces insignificant, small, and sometimes negative long-run earnings effects for white applicants. These differences in the effects of service by race 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 VI.

V.A. Effects of Army Service on Labor Market Outcomes

1. *Earnings and Employment.* In Figure II we show the relationship between earnings and first AFQT score at 1, 5, 10, and 19 years after application.[12] The size of the jump at each threshold—the reduced form—divided by the corresponding first stage in Online Appendix Table A.3 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 Figure III, Panel A.[13] 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 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 1–14 years after application and are approximately \$3,000 larger 15–19 years after application

12. Online Appendix Figure A.8 plots the relationship between AFQT scores and earnings for every year −1, 0, ..., 19 relative to application.

13. Every estimate underlying Figure III is reported in Online Appendix Table A.5. Given that IRS data start in 1999 and end in 2018, these estimates are based on an unbalanced panel of application cohorts (see Online Appendix Figure A.9, Panel A). We discuss heterogeneity by application cohort later in this section as well as in Online Appendix C.3.

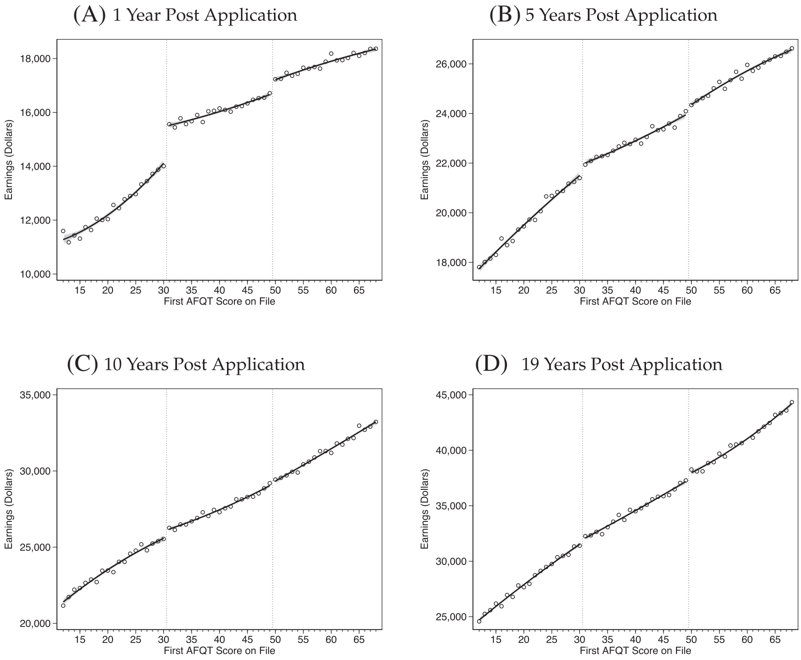

FIGURE II

Reduced-Form Plots: Earnings, 1, 5, 10, and 19 Years After Application

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. Online Appendix Figure A.8 contains the reduced-form plots for all years −1 to 19. Figure III, Panel A plots corresponding 2SLS RD estimates of the effect of enlistment on earnings for all years −1 to 19 since application.

(though not statistically distinguishable from those at the lower cutoff).

Figure III, Panel B 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.[14]

14. In addition, Online Appendix Figure A.10, Panel A shows that service has no effect on self-employment earnings in the first 10 years at the lower cutoff and

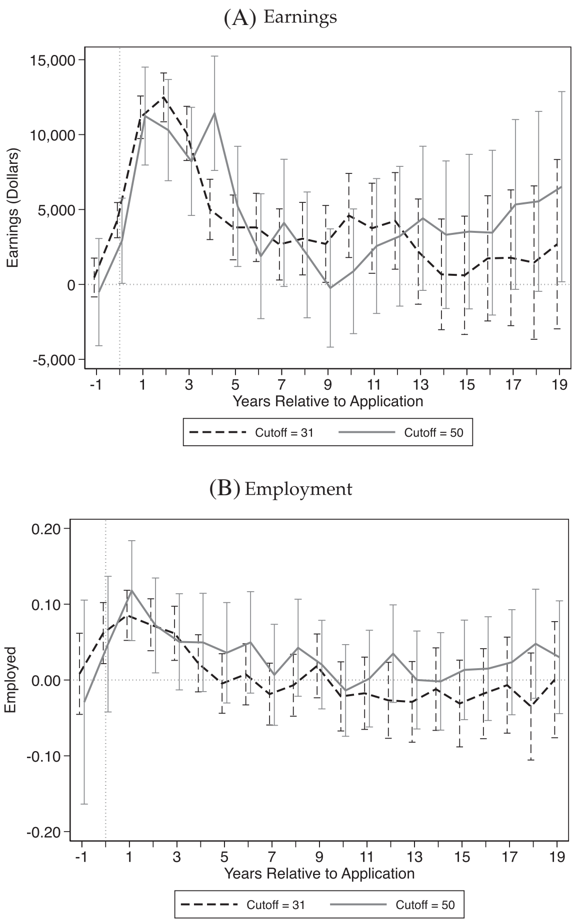

FIGURE III

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

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 nontaxable military earnings (2018 dollars). Panel B plots coefficient estimates and 95% confidence intervals for employment as defined by any W-2 Medicare wages. Section III.B and Online Appendix B.2 contain additional details on the construction of earnings and employment outcomes.

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 Online Appendix Figures A.11 and A.12 we assess how service affects one's place in a nationally representative earnings distribution and do not find evidence of this kind of dispersion.

To better understand how enlistment affects cumulative earnings and employment, Figure IV and Table II 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 their 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 II and III. Because we have a limited number of earnings records for each applicant—tax record availability begins in 1999 and ends in 2018—we weight each individual by the number of years she could potentially be observed in tax records in each of our aggregate estimates. Figure IV, Panel A 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 Table II, Panel A).[15]

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 Table II, column (2), army service increases long-run annual earnings at the 31 AFQT cutoff by $2,000 in Panel A (statistically insignificant) or, when using log earnings, by

a small negative effect at the higher cutoff. In the long run, service has little effect at either cutoff.

15. Online Appendix Table A.6 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.

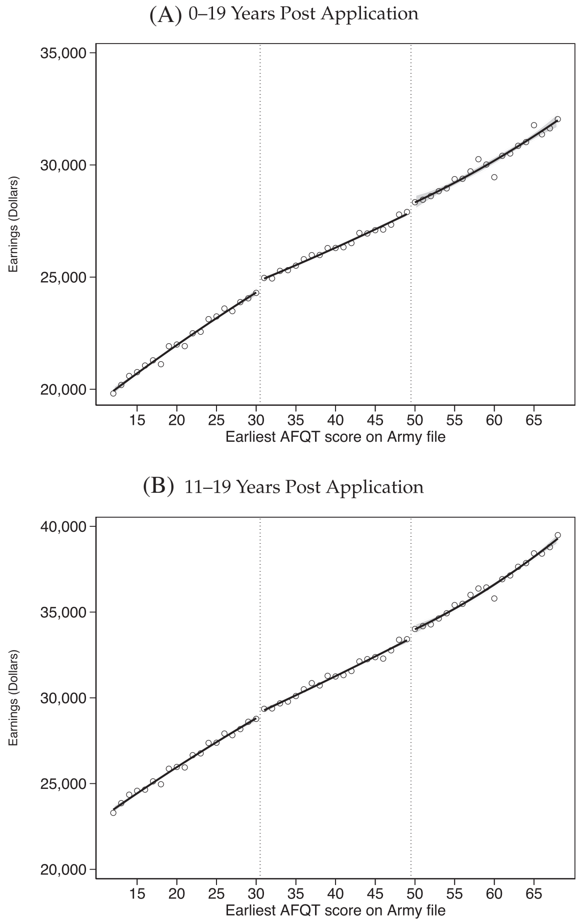

FIGURE IV

Average Earnings, Reduced Form

Panel 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.

TABLE II

AVERAGE EFFECTS ON EARNINGS AND EMPLOYMENT, 2SLS RD ESTIMATES

	31 AFQT Cutoff		50 AFQT Cutoff	
	0–19 Yrs since (1)	11–19 Yrs since (2)	0–19 Yrs since (3)	11–19 Yrs since (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) and (2) estimate average effects at the 31 AFQT cutoff, while columns (3) and (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 could potentially be observed in tax records since tax record availability begins in 1999 and ends in 2018. We impute zero wages for individuals without reported earnings in a year covered by our data (i.e., 1999 through 2018). 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%.

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). Although we observe positive effects of army service on earnings at both cutoffs, Table II, Panel C shows that enlistment does not affect overall or long-run average employment at either AFQT cutoff.

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.

i. *Postsecondary Attendance.* Figure V, Panel A plots the effect of enlistment on postsecondary attendance, defined as having

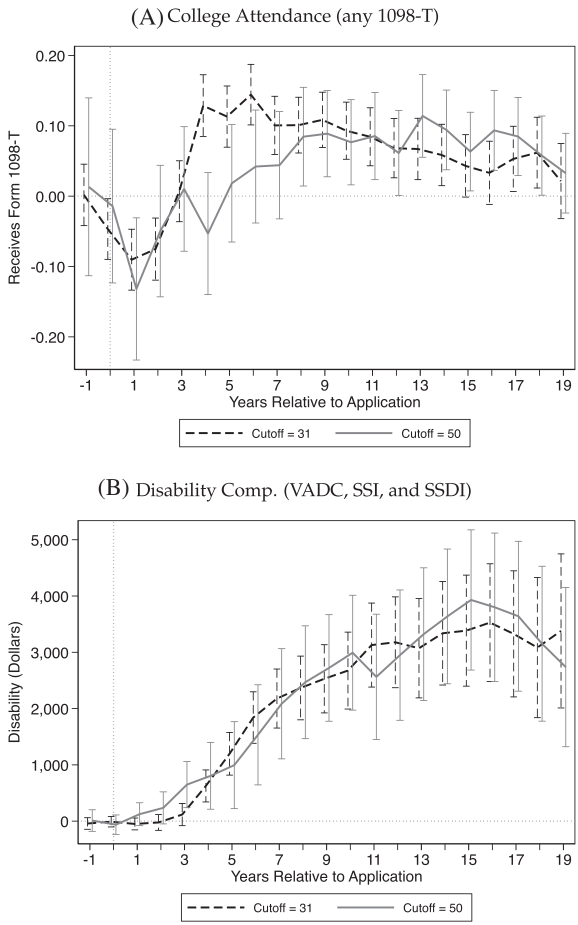

FIGURE V

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

This figure plots 2SLS RD estimates of the effect of enlistment on postsecondary enrollment and disability compensation. Panel A plots coefficient estimates and 95% confidence intervals for postsecondary 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).

a 1098-T in the given year. In the short term, enlistment is a substitute for education with enlistment decreasing postsecondary 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 education 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 Online Appendix Table A.7, Panel A, 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 Online Appendix Table A.8, 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. In addition, 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, Troske, and Coomes (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.[16] 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.

ii. *Mortality.* Given the potential combat and training risk faced by servicemembers, in Table III we estimate the effect of service on mortality within 1, 3, 5, 10, 15, and 19 years of application. Although 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 army service may reduce mortality in the first few years after application, but the estimates become more

16. Applying estimates from Jepsen, Troske, and Coomes (2014) to our sample, an associate's degree is worth approximately $6,700 a year. Therefore, the increase in associate's degrees of 3.8 percentage points accounts for $254. Ashworth and Ransom (2019) estimate a college graduation wage premium of approximately 45%. Therefore, the 3.4 percentage point increase in bachelor's degrees accounts for a $380 increase in average earnings in our sample.

TABLE III
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 III is required. Fewer than 20 applicants have death dates prior to application and we drop these. Our outcome, an indicator for death 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%.

imprecisely estimated and statistically indistinguishable from zero in later years. At the 50 cutoff point estimates hint at reduced long-term risk, but we are wary of drawing any conclusions at either cutoff given the lack of precise effects. Overall, our findings in Table III suggest that mortality is not significantly affected by army service and therefore is unlikely to be a meaningful driver of our observed earnings and employment effects.

iii. *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 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, Duggan, and Guo 2019). Figure V, Panel B reports estimates of the effect of enlistment on our measure of annual disability compensation: the sum of VADC, SSDI, and SSI payments.[17] The effect of enlistment on disability compensation is nearly identical at both cutoffs, reaching $2,000 a year by seven years out and steadily increasing to over $3,000 in later years.

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 Online Appendix Figure A.13, Panel A). When we examine whether individuals ever receive disability at any point, Online Appendix Table A.7, Panel B 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, 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. Online Appendix Figure A.13, Panel B shows that while point estimates are usually positive at both cutoffs, these estimates are

17. Data use agreements prevent us from linking VA, SSA, and NSC data to IRS data. Estimates from these outcomes include the approximately 1% of applicants that we could not link to any IRS records. Because we only have SSDI and SSI data from 1999 through 2015, we extrapolate these outcomes to 2016–2018 using 2015 values, adjusting for inflation.

generally below 3 percentage points and only occasionally statistically significant.[18]

Large effects on disability compensation are consistent with the possibility that military service negatively affects health, as suggested by findings reported in Stiglitz and Bilmes (2008), Tanielian and Jaycox (2008), Cesur and Sabia (2016), and others. Non-work-limiting VADC, which is exclusively available to veterans and uses different screening criteria than civilian disability programs, explains the vast 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 $3,000 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 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.

V.B. 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 Chetty and Hendren 2018; Schwandt and Von Wachter 2019; Chetty et al. 2020). 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

18. Online Appendix Table A.7, Panel B indicates that army service increases the probability of ever receiving compensation for a significant disability by 4 percentage points at both cutoffs. Online Appendix Figure A.13, Panels C and D show that enlistment's effect on SSI or SSDI receipt is generally small, positive, and insignificant.

that army service especially benefits Black servicemembers and helps 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.

Before moving forward, we note that sample limitations do not allow us to recover reliable estimates of the effects of service for Hispanic applicants. While Online Appendix Figure A.14 suggests positive long-run earnings effects for Hispanic applicants at the 31 cutoff and no discernible effects at the 50 cutoff, our 2SLS estimates are imprecise and generally statistically insignificant.[19] Furthermore, unlike estimates for Black and white applicants, our estimates for Hispanic applicants are sensitive to bandwidth, kernel, and functional-form specifications. While the number of Hispanic applicants is a limitation in our sample, the share of Hispanic servicemembers has been growing in recent years (Barroso 2019). As a result, we are hopeful that future work may be able to answer important questions about the effects of service in this population.

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. Online Appendix Figure A.15 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: Online Appendix Table A.9 shows that Black compliers at both cutoffs grew up in households with lower parental income than white compliers. Similarly, Online Appendix Table A.9 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). More generally, the table

19. We estimate that service increases overall (0–19 years after application) and long-run earnings (11–19 years after application) by $6,165 (std. err. = $2,463) and $5,192 (std. err. = $4,306), respectively, at the 31 cutoff and decreases overall and long-run earnings by $730 (std. err. = $5,024) and $5,381 (std. err. = $7,494), respectively, at the 50 cutoff.

shows that the characteristics of Black compliers are typically 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.[20]

Finally, Online Appendix Figure A.16 reports estimates of counterfactual economic trajectories for Black and white compliers in our sample following Abadie (2002). We find that in the absence of military service, Black compliers at the cutoffs would have earned roughly $8,000–$12,000 less per year than white compliers at the same respective 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 or other variables specific to the population near each cutoff from which applications are drawn) Black-white average earnings gap among 30–39-year-old men (U.S. Census Bureau 2018). These differences in counterfactual trajectories for compliers extend to other important outcomes as well (see Online Appendix Figure A.18). For example, nonenlisting 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 affects economic outcomes for Black and white applicants.

2. *Earnings and Employment.* In Figure VI, 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 three years following application, the effects of service are positive for both groups but significantly larger for Black applicants. Between 4 and 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, Figure VI, Panel B shows less

20. In addition, Online Appendix Table A.10 shows that reweighting Black (or white) applicants to match the baseline characteristics of the average Black (or white) enlistee does little to the estimates, suggesting that treatment effects for compliers may not differ much from treatment effects for the typical enlistee of the given race.

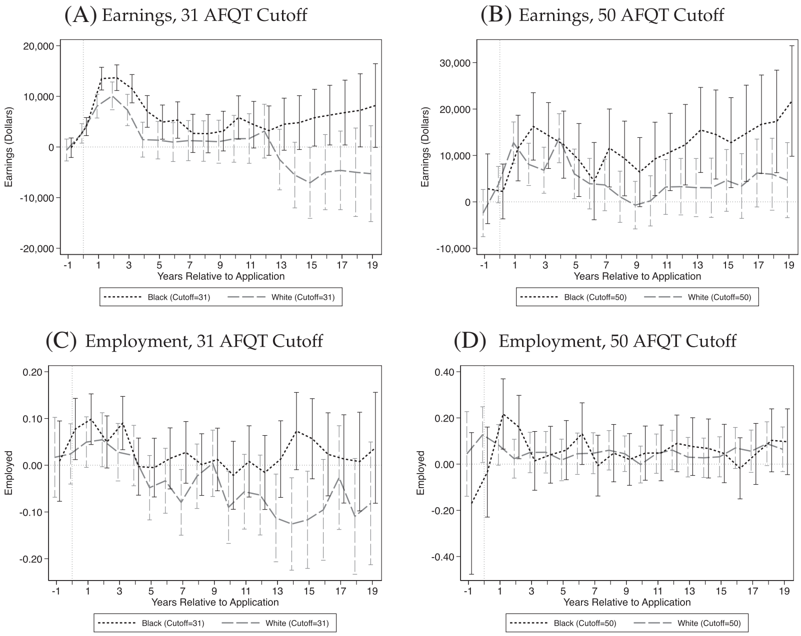

FIGURE VI

Effects of Enlistment for Black and White Applicants on Earnings and Employment

This figure plots 2SLS RD estimates of the effect of enlistment on earnings in subsamples split by race. Throughout, we compare estimates for Black applicants (the dotted black line) to those for white applicants (the dashed gray 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 W-2 Medicare wages) estimates at the 31 cutoff, and Panel D compares employment estimates at the 50 cutoff.

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. While our earnings measure includes only wage and salary income, these gaps are not offset by differences in self-employment earnings: in Online Appendix Figures A.19 and A.20, Panel A, we find that army service does not significantly affect self-employment earnings for Black or white applicants at either cutoff. Divergent effects of army service for Black and white applicants are also reflected in employment rates at

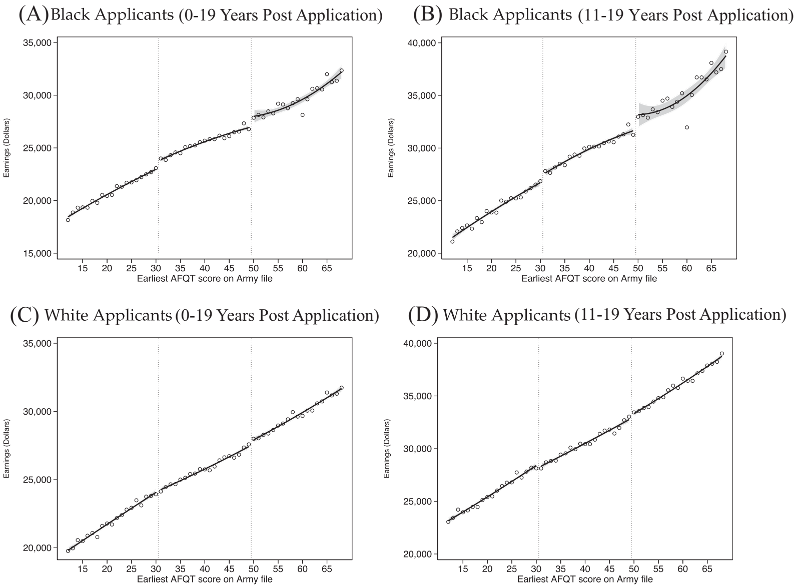

FIGURE VII

Average Earnings by Race, Reduced Form

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.

the lower cutoff. Figure VI shows 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 Figure VII, Panels A–D, 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 Table IV,

TABLE IV

AVERAGE EFFECTS ON EARNINGS AND EMPLOYMENT, DIFFERENCES BY RACE

	0–19 years		11–19 years	
	Black (1)	White (2)	Black (3)	White (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	.028		.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	.035		.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	.032		.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	.803		.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) and (2) look at average outcomes between 0 and 19 years since application, columns (3) and (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%.

Panels A and B, our estimates indicate that enlistment increases the overall average earnings of Black applicants by $6,000 and $12,000 a year at the low and high AFQT cutoffs, 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 of around \$5,500 and \$15,000 per year at the low and high cutoffs, 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 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 at both cutoffs suggest that army service in the all-volunteer era substantially increases long-term earnings for at least some individuals, a contrast to Angrist (1998). The differences between our estimates and Angrist's could be due to differences in complier populations—compliers in Angrist have lower AFQT scores than compliers at either cutoff in our sample, have lower levels of education (DoD 2020), and likely differ on unobservable characteristics that could affect the value of service—or changes in the nature of service and combat, the signaling value of veteran status (Kleykamp 2006), veterans' health and education benefits (Barr 2015), or other changes in the value of army service over the past three decades. Second, the \$8,500–\$11,000 difference in estimated earnings effects is sufficiently large to effectively close the entire counterfactual earnings gap among untreated compliers over the same period (Online Appendix Figure A.17).

Table IV, Panels C and D show that differential effects of enlistment on employment can account for some of the 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 (Online Appendix Figure A.21). 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 the 31 and the 50 cutoffs. 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.

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 preapplication characteristics.

First, in Table V, we implement a propensity score reweighting approach that reweights Black applicants along a rich set of demographic, economic, and geographic characteristics to more closely resemble those of white applicants (see table notes for details). Table V, Panels A and B show that reweighting Black applicants barely moves our 2SLS estimates. Second, in Table V, Panels C and D, we estimate a model that allows for the effects of army service to differ by both race and a preapplication proxy for economic disadvantage. Using the same set of covariates used in the reweighting exercise, we construct an economic "disadvantage index," defined as the additive inverse of predicted average earnings 11–19 years out, in standard deviations. Columns (2) and (4) show that although 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 measures of disadvantage are not exhaustive, our results in Table V suggest that differences in the earnings effects of service by race are not easily explained by differences in economic opportunity.

Nevertheless, if motivations to enlist based on economic opportunity (in contrast to motivations to enlist based on noneconomic factors like patriotism; Krebs and Ralston 2022) differ by race along dimensions not adequately captured by local economic conditions or other observables in the exercises, this could have implications for the interpretation and generalizability of our results. Although we are unable to provide direct evidence

TABLE V

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**	6,029	5,770	
	(2,502)	(3,729)	(4,296)	
Observations	299,074	299,074	299,074	
Panel B: Reweighting, 50 cutoff, Black				
	Benchmark	Reweight	Reweight (+1040)	
Enlist	14,379***	16,060***	15,696**	
	(4,404)	(5,997)	(6,594)	
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 reestimate the specification in Table IV 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, quintiles of rates of employment, median income, poverty, and single-parent households measured in 1990 from county of residence reported on the application, and quintiles of state-level quarterly unemployment rates. In column (3), we also 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 $\mathbb{1}(AFQT \geqslant CUT) \times$ Black in columns (1) and (3) and that instruments for both Enlist × Black and Enlist × disadvantage with $\mathbb{1}(AFQT \geqslant CUT) \times$ Black and $\mathbb{1}(AFQT \geqslant CUT) \times$ disadvantage in columns (2) and (4). We allow the running variable to vary by race. The disadvantage index—the standard deviation of the additive inverse of predicted earnings 11–19 years—is constructed from applicants just to the left of each threshold (i.e., AFQT scores of 30 or 49) with the same variables used in the reweighting models using a leave-one-out procedure to avoid introducing endogenous stratification (Abadie, Chingos, and West 2018). All panels and columns drop the 1% of applicants for whom county of application is missing. Significance levels: *: 10% **: 5% ***: 1%.

on motivations for enlisting, we note that the earnings gap between Black and white untreated compliers (shown in Online Appendix Figure A.16) is similar to what we would expect in the broader U.S. population based on the AFQT, education, and gender composition of our sample, providing reassurance that Black compliers do not disproportionately select into service on the basis of counterfactual economic opportunities.[21] Furthermore, we find that Black and white Americans respond similarly to unemployment shocks on the application margin (see Online Appendix Table A.11). Though it remains possible that Black compliers are more likely to apply and enlist due to economic opportunities than white compliers on unobservable dimensions, we do not find evidence consistent with this in our sample.

4. *Racial Differences in Education, Disability, and Other Outcomes.* 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 differences in the effects of service on education or disability could contribute to the disparity in earnings estimates.

i. *Postsecondary Attendance.* One potential explanation for the differential earnings effect by race is a differential effect on college attendance, college quality, or graduation by race. In Figure VIII, Panel A and Online Appendix Figure A.20, Panel B, we explore differences by race in the effects of army service on college attendance, as measured by Form 1098-T records. These figures suggest that enlistment has similar dynamic effects on college attendance for Black and white applicants at both cutoffs. In Online Appendix Table A.12, Panels A and B, we examine aggregate education outcomes, including graduation and attendance, separately by race. We find that the overall effects on attendance are similar by race. In addition, we are unable to detect differences in graduation rates across race at either cutoff, though point estimates are higher for Black applicants.

21. Zhou and Pan (2021) document a Black-white earnings gap around $15,000 among 30–33-year-olds in the NLSY97. When we adjust the NLSY97 sample to match our sample by sex, education, and AFQT scores, we find Black-white income gaps of around $9,600 at the 31 AFQT cutoff and $7,800 at the 50 AFQT cutoff. These are similar to the counterfactual earnings gaps we document 10–13 years post application at the 31 cutoff ($5,000–$10,300) and 50 cutoff ($6,100–$8,900).

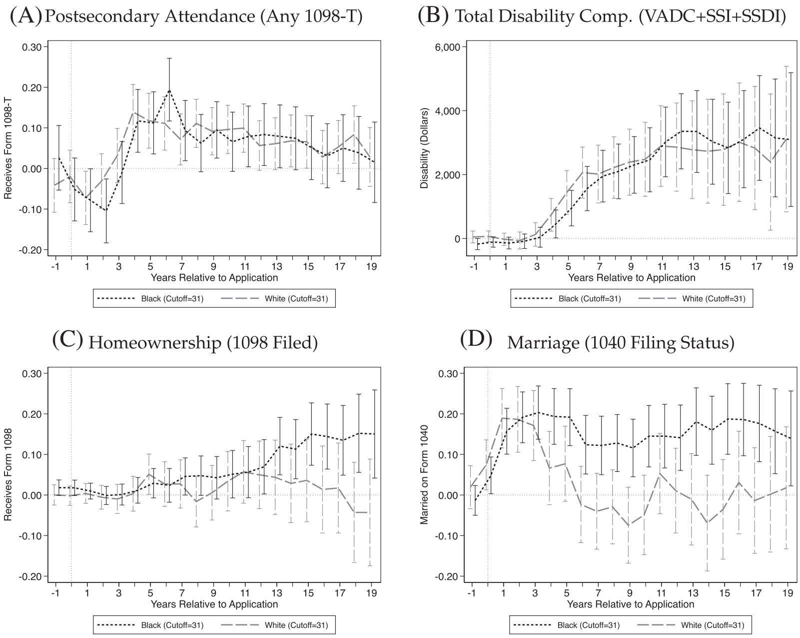

FIGURE VIII

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

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 gray line) at the 31 AFQT cutoff. Online Appendix Figure A.20 contains the plots at the 50 cutoff. Panel A compares postsecondary attendance estimates, Panel B compares total disability compensation estimates, Panel C compares mortgage estimates, and Panel D compares marriage estimates. Additional estimates of the effects of service on significant disability receipt and 1040 filing can be found in Online Appendix Figure A.19.

In addition to whether students attend college and graduate, there may be differences in the value generated by the institutions Black and white students attend or the majors they choose. In Online Appendix Table A.13, 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). In Online Appendix Table A.14, Panel A, we use average earnings of attendees to generate an alternate measure of education quality.[22] With this measure, we find that enlistment has very similar effects on education quality for Black and white applicants at the 31 cutoff, but may lead to a higher-quality education for Black relative to white applicants at the 50 cutoff. Finally, given research that suggests majoring in science, technology, engineering, and math (STEM) leads to significant earnings premia (e.g., Webber 2014; Altonji, Arcidiacono and Maurel 2016; Deming and Noray 2020), we estimate the effects on STEM degree attainment separately by race in Online Appendix Table A.14, Panel B. While imprecisely estimated, our estimates suggest no differences in the effects of enlistment on STEM degree attainment between Black and white applicants at the 31 cutoff, but hint at a possible small increase in STEM degree completion among Black relative to white servicemembers at the 50 cutoff. 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 at the 31 cutoff and may play a somewhat larger, though still modest, factor at the 50 cutoff, a point that we return to in more detail in Section VI.

ii. *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 Figure VIII, Panel B, 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, Online Appendix Figure A.20, Panel C 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 this difference largely dissipates in the long run.[23] In terms of significant disability, Online Appendix Figure A.19, Panel B and Online Appendix Figure A.20, Panel D show 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.

22. This outcome is the average of winsorized (99th percentile) 2019 W-2 earnings (in 2018 dollars) of individuals between the ages of 30 and 39 who attended the institution between 1999 and 2011. For those in our sample without a 1098-T, we impute a “college wage” equal to the 2019 average earnings of 30–39-year-olds in the population without a 1098-T recorded between 1999 and 2011.

23. Online Appendix Table A.15 shows effects of service on disability compensation 11–19 years after application at both cutoffs.

Online Appendix Table A.12, 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. Relatedly, we do not find evidence that army service affects mortality for Black or white applicants (Online Appendix Table A.16). Although unlikely to be a primary contributor, in Section VI we discuss the extent to which differences in disability receipt could explain the differential effect of service on earnings by race.

iii. *Homeownership, Neighborhood Quality, and Marriage.* Before concluding this section, we briefly examine whether the effect of army service differs by race for several additional outcomes including homeownership, neighborhood quality, and marriage (Figure VIII, Online Appendix Figures A.19 and A.20).[24] 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 people in our sample face greater barriers to homeownership than do white people (e.g., Charles and Hurst 2002), we may find larger effects of service on homeownership among Black applicants. Indeed, in Figure VIII, Panel C, 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 applicants. Consequently, enlistment closes most of the homeownership gap among untreated Black and white compliers at this cutoff (Online Appendix Figure A.18). Similarly, in Online Appendix Figure A.20, Panel E, 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).

Research from the Moving to Opportunity Project suggests that adults who move to higher-income neighborhoods experience better physical health, mental health, and overall subjective well-being (Ludwig et al. 2011, 2013). In Online Appendix Figure A.19, Panel C and Online Appendix Figure A.20, Panel F, we explore whether enlistment improves neighborhood quality, as measured by ZIP code–level average income. Online Appendix Figure A.19, Panel C suggests that enlistment may

24. The effects of service on homeownership and marriage for our whole sample can be found in Online Appendix Figure A.10.

generate some neighborhood upgrading for both Black and white applicants at the 31 cutoff, but estimates are imprecise. Online Appendix Figure A.20, Panel F shows that although enlistment doesn't generate neighborhood upgrading for white applicants at the 50 cutoff, it appears to significantly improve long-run neighborhood quality for Black applicants.

In Online Appendix Figure VIII, Panel D and Online Appendix Figure A.20, Panel G, 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 and 19 years after application (or half the counterfactual 11–19 gap in Online Appendix Figure A.18).[25] 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 incentivizes marriage with financial benefits such as increased housing allowances, such large effects on marriage among Black applicants long after most servicemembers have left the army is striking.

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 not likely explained by differences in effects on education or disability compensation. While we explore the causes of the Black-white gap in effects of army service in greater detail in Section VI, we first turn to more thoroughly examine our earnings results.

V.C. Robustness and Generalizability of Earnings Estimates.

In Online Appendix C.1–C.3, we probe the robustness of our earnings estimates, including to alternative specifications, and explore cohort heterogeneity. Our aggregate earnings estimates for Black, white, and all applicants are robust to the inclusion of

25. 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 when we condition our estimates on those who file.

demographic controls, alternative functional forms, alternative bandwidths, and alternate treatments of standard errors (see Online Appendix Figures A.22–A.27 and Table A.17). We also show that while the exclusion of tax-free military benefits predictably reduces the effects of army service for all applicants, we still find large and significant positive long-run effects for Black applicants (see Online Appendix Figure A.28). In addition, we show that our long-run earnings results change little when we restrict our analysis to a balanced panel of 1990–1999 applicants (see Online Appendix Table A.18).

With regard to cohort heterogeneity, our long-run estimates are predominantly driven by enlistees from the 1990s. While enlistees in the 1990s and 2000s share much in common, those from the 2000s saw significant changes to disability compensation (Ben-Shalom, Tennant, and Stapleton 2016) and veteran education benefits (Barr 2015), and, for some cohorts, were exposed to increased fighting in Iraq and Afghanistan. Furthermore, the composition of marginal enlistees at the height of operations in Iraq and Afghanistan differed somewhat from prior and subsequent periods, with marginal enlistees having lower education levels (DoD 2020) and being more likely to receive enlistment waivers (Murphy 2019). A natural question is whether our results are indicative of returns just to servicemembers from the 1990s or voluntary service more generally. Although data limitations preclude us from making clear distinctions between earlier (1990–2000) and later (2001–2011) application cohorts in the long run, the short-run effects of service on earnings appear to be somewhat larger for the later application cohorts (likely due to army benefit expansions). In the medium run, we find nearly identical effects across cohorts at the 31 cutoff, and statistically indistinguishable albeit lower point estimates for the later cohort at the 50 cutoff (see Online Appendix Figures A.29 and A.30). Finally, with the possible exception of the mid-2000s, we see few differences in mean counterfactual earnings or in the complier population over time in our sample (see Online Appendix Figures A.31 and A.32). In addition, application cohorts in the 2010s and today look more similar to cohorts from the 1990s than to cohorts from the height of the conflicts in Iraq and Afghanistan (DoD 2020). Altogether, our estimates likely reflect the long-term effect of typical army service during the all-volunteer era if not service during the unusually high casualty period of 2005–2007. That said, especially in light of recent education and VADC expansions, future research on more recent servicemembers is warranted.

VI. Understanding Black-White Differences in the Effects of Service

In Section V, we documented that Black and white applicants face different economic and household trajectories: in the absence of army service, Black applicants earn about $12,000 less than white applicants 19 years after application at both cutoffs (Online Appendix Figure A.16). Army service closes these earnings gaps. While we find some evidence that the army disproportionately benefits Black and white applicants from disadvantaged backgrounds (Table V), 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 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.

VI.A. 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 counterfactual earnings (Online Appendix Figure A.16), we might expect Black servicemembers to stay in the army longer than white servicemembers. In Online Appendix Figure A.33, we estimate the effect of enlistment on retention in the army. Although 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.

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. To explain the gaps without any (differential) increases in postservice 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.[26] Fundamentally, the true size of any army pay premium among those 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, Hosek, and Mattock (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, leaving over \$10,000 to explain at both cutoffs. As such, enlistment must differentially increase the postservice nonarmy pay of Black veterans. The rest of this section asks which aspects of service or the transition out of service increase earnings potential.

VI.B. Combat, Deployment, and Disability

Differences in the risk and trauma soldiers face could potentially affect 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 (Carter, Smith, and Wojtaszek 2017). Online Appendix Table A.19 shows that this is also true among compliers in our specifications. Although soldiers in combat and noncombat occupations deploy to combat zones at comparable rates (Greenberg et al. 2021), those in combat

26. 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 dividing the Black-white differences in earnings effect sizes by the differences in retention at 19 years after application from Online Appendix Figure A.33. If we attempt to explain the positive earnings effects for Black applicants, rather than the Black-white gap, we also recover implausible premia for army service.

occupations may face greater exposure to the harmful consequences of war (see Chandra et al. 2011; Cesur, Sabia, and Tekin 2013; Cesur and Sabia 2016). In Online Appendix Table A.19, 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. Although we find no racial differences in being killed in action (Online Appendix Table A.19) or in total disability compensation (Figure VIII and Online Appendix Figure A.20), white applicants do appear more likely to be wounded in action at the higher cutoff (1.6 percentage points; Online Appendix Table A.19) and are more likely to receive compensation for a significant disability 19 years after application at both cutoffs (3.4–4.2 percentage points; Online Appendix Figures A.19 and A.20), 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 Online Appendix Table A.20). Of course, even if army service increases disability compensation by similar amounts for Black and white enlistees, there could still be differences in how Black and white servicemembers adjust their labor supply in the presence of VADC, though any such differences are likely too small to alter our conclusion.

VI.C. Occupations and Human Capital

Given that Black servicemembers tend to choose different army occupations than white servicemembers (Johnson, Trent, and Barron 2017), another possibility is that the specific occupations Black servicemembers hold generate skills that are more relevant to nonarmy occupations than the types of occupations white servicemembers hold. Online Appendix Table A.19 confirms that Black compliers tend to choose different occupational fields than white compliers. For example, infantry is the most overrepresented occupational group among white compliers while quarter master (e.g., unit supply, logistical, and culinary specialists) is the most overrepresented occupational group among Black compliers. While these patterns of selection could reflect differences in motivations for service by race (Lundquist 2008; Krebs and Ralston 2022), 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 Online Appendix Table A.19 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 (Online Appendix Table A.20). Note 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 with white applicants.

VI.D. 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 Online Appendix Table A.12, the long-term effect 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 Online Appendix Table A.13 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 the imprecisely estimated differences in degree completion in Online Appendix Table A.12 seriously and use the estimates of returns to associate's degrees from Jepsen, Troske, and Coomes (2014) and bachelor's degrees from Ashworth and Ransom (2019), overall differences in degree completion could explain roughly \$200 at the lower cutoff and \$1,400 at the higher cutoff. Our estimates in Online Appendix Table A.14 provide similar results, with no distinguishable differences on education quality by race at the 31 cutoff, but some suggestive evidence that at the 50 cutoff Black enlistees experience disproportionate increases in education quality relative to white enlistees. Furthermore, these modest differences in earnings due to education may be overstated because of 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 Online Appendix Table A.13).[27]

VI.E. 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 Online Appendix Table A.20). This leaves at least a \$7,000–\$12,000 gap to explain at each cutoff, suggesting that service improves the postservice civilian labor market earnings of Black veterans more than white veterans. Here we show that beyond providing a 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 with 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 public-sector jobs through government networks and by enabling many veterans to declare veteran's preference in the application process for federal, state, and local government positions (Lewis and Pathak 2014). Consistent with preferential treatment in the public sector, Black applicants who serve are more likely to be employed in the public sector in the long run (Online Appendix Figure A.19, Panel E; Online Appendix Figure A.20, Panel I; and Online Appendix Table A.21). Though not typically statistically distinguishable, this does not appear to be the case for white applicants.

27. Barr et al. (2021) examine the expansion of education benefits in the Post-9/11 G.I. 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.

More generally, we also find that service increases the likelihood that Black applicants are eventually employed in higher-paying industries. We 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 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% random sample of 32- to 44-year-old U.S. workers during the years we examine. The first two columns of Online Appendix Table A.22 reveal that army service increases average industry pay among Black applicants by \$7,000–\$13,000 as compared with \$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) control for military service. Although any such exercise that controls for endogenous variables is necessarily suggestive, it appears that industry pay differences are driven by increases in average civilian industry pay for Black applicants.[28] While the army may make it more likely that Black veterans find higher-paying jobs in 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 a stable, well-paying job and by opening doors to future higher-paid employment, army service offers many Black Americans a path toward 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 overall (Chetty et al. 2020) and in our sample (Online Appendix Figure A.16), and documented racial discrimination in the labor market (Lang and Lehmann 2012), several alternative explanations

28. Given that Black compliers who enlist tend to remain in the army longer than white compliers who enlist, controlling for military service would bias the difference in industry pay upward (downward) if those marginal Black applicants are negatively (positively) selected. However, we find no evidence that marginal Black applicants who remain in the military are differentially selected as indicated from predicted earnings derived from regression models using the preapplication characteristics of applicants just below each cutoff.

emerge—all worthy of further exploration. These include access to networks, 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.[29]

VII. Conclusion

In this article, 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. Although we find that army service increases cumulative earnings, postsecondary 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 at the 31 and 50 AFQT cutoffs, respectively. Our estimates suggest that the army can be a critical institution for improving income 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.

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. Although we cannot exactly identify which aspects of military service expand employment opportunities for Black veterans, future research, including audit studies, could

29. 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). In related work, Blair and Chung (2018) find that occupational licensing reduces racial wage gaps, particularly in the case of men, when the licenses preclude felons and thus credibly signal a worker's criminal history.

explore whether access to networks (e.g., Burks et al. 2015; Brown, Setren, and Topa 2016), 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. Such research could inform the efficacy of programs intended to address racial income gaps more broadly—such as expanding jobs programs, employment network services, and credentialing programs.

The effects of service in this study are estimated in a political context that has been supportive of policies focused on veterans' well-being (among them veterans' disability, the various G.I. bills, healthcare). Without this robust set of military-targeted policies, the effects of enlistment may well differ. Moreover, expansions of programs and policies aimed at helping the broader population—for example, job programs, universal healthcare, and college education—could have important implications for the U.S. military's ability to attract and retain talent. How a long-run political equilibrium balances the need for a volunteer force that is incentivized to enlist with providing greater opportunities for less advantaged adults more generally is an open and intriguing question.

Finally, our findings point toward future research that could lead to a greater understanding of the effects of service. Our findings on the effects of service on disability remind us of the potential negative health consequences of military service. Research that helps quantify the impact of these health risks on veterans' well-being, and the potential ameliorative effect of disability payments (Silver and Zhang 2022), is warranted. In addition, while we study pretax earnings, future work could study the effect of preferential military tax benefits on soldiers, as well as the overall tax implications of military service taking into account the short-run tax losses and long-run gains due to the effects of service on earnings. Last, identifying whether large earnings gains for Black veterans improve outcomes for their children should be the subject of future research (Goodman and Isen 2020).

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Code replicating the tables and figures in this article can be found in Greenberg et al. (2022) in the Harvard Dataverse, https://doi.org/10.7910/DVN/JLKZ17.

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