



Improving the outcomes of youth with medical limitations: Evidence from the National Job Corps Study

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Abstract

Improving work outcomes for youth with disabilities and reducing their reliance on disability benefits are important policy priorities, but existing interventions have shown limited promise. We provide new evidence to inform this discussion by re-analyzing data from the 1990s National Job Corps Study, a randomized field experiment conducted nationwide in the United States. Job Corps, which provides comprehensive training to economically disadvantaged youth, is the nation's largest youth program outside of the school system. We examine youth who had medical limitations when they enrolled in the experiment, a group that has not previously been studied. During the 4 years after random assignment, participation in Job Corps increased the earnings of youth with medical limitations—substantially more so than for youth without medical limitations—and additionally reduced their receipt of disability cash benefits. Interventions designed specifically for such youth have not typically demonstrated reductions in benefit receipt. Hence, our re-analysis of the field experiment suggests that Job Corps could be a promising model for helping some youth with disabilities gain a foothold in the labor market and achieve greater self-sufficiency.

1 | INTRODUCTION

Expanding access to meaningful employment for youth and young adults with disabilities is a policy priority in the United States, given how they fare in the labor market compared with their peers. Youth with disabilities have substantially lower employment rates than other youth, especially if they come from lower-income families, do not complete high school, or face other barriers (Newman et al., 2011). As they transition to adulthood, these youth also face substantial challenges related to their health or impairments, access to medical services, finding adequate education and employment supports, and navigating a complex, fragmented support system (Osgood et al., 2010; US Government Accountability Office, 2012). Limited employment prospects or other functional limitations stemming from a medical condition often result in long-term reliance on the federal Supplemental Security Income (SSI) program, which provides means-tested benefits to children and adults with significant disabilities (Davies et al., 2009).

A potentially promising way to serve youth with disabilities is through Job Corps, a program initially established to serve young adults with economic disadvantages. Job Corps is one of the largest youth programs outside the traditional K–12 schooling system. The National Job Corps Study (NJCS), a large-scale experimental evaluation conducted in the mid-1990s, found that Job Corps helped improve medium-term (but not long-term) work and earnings outcomes for

youth and helped reduce both criminal activity and receipt of certain forms of public assistance (Chen et al., 2018; Schochet et al., 2001, 2008). Further, longer participation in academic and vocational instruction in Job Corps (which averaged around 7 months for youth in the NJCS) was linked to larger increases in postparticipation earnings (Flores et al., 2012). Other past research has found that the relatively more successful interventions for youth with disabilities have been those that provided more intensive employment services, as well as job placement and retention support (US Department of Labor [DOL], 2015; Wittenburg et al., 2013). The comprehensive and intensive nature of the Job Corps program may therefore prove to be well suited to youth with disabilities.

In this paper, we leverage the NJCS's experimental design and data to assess Job Corps' impacts on human capital, labor market, and other outcomes of 472 "youth with medical limitations" (YML). This is a group of youth who self-reported a "serious physical or emotional problem which limits the amount of work [they] can do or other regular daily activities" in the NJCS baseline survey. Our results indicate that Job Corps had substantial and favorable impacts on the education and earnings outcomes of YML who participated in Job Corps. Moreover, the program likely led to a meaningful reduction in the amount of SSI benefits they received ($p = 0.057$). To date, no major initiative targeted at youth with disabilities has demonstrated an ability to reduce reliance on disability benefits (Wittenburg et al., 2013).

Job Corps substantially increased human capital investments received by YML during the 4-year period after they were randomly assigned for the NJCS. Over that period, YML participants received approximately 1810 h of education and training, but they would have received only 780 if they had not had access to Job Corps. The per-participant impact of just over 1000 h is roughly equal to the number of hours of instructional time in a typical school year. Job Corps also improved high school completion rates by more than 15 percentage points among YML participants, relative to a counterfactual base of 43%. This effect came almost entirely through the attainment of General Educational Development (GED) degrees. These estimates are similar in magnitude to the education/training impacts we observed for other youth.

Participation in Job Corps also increased the self-reported earnings of YML. Per-participant impacts on earnings for this group were more than \$3000 annually in the second through fourth years after random assignment—a period when most were no longer in the program. (All financial amounts in this paper have been adjusted for inflation to 2016 dollars). These impacts amounted to increases of 50%–60% relative to YML participants' counterfactual average earnings and were much larger than the corresponding impacts for youth who did not indicate a medical limitation at baseline. Among these other youth, the largest per-participant impact on earnings in any single year was \$1700, realized in the fourth year—an 11% increase over what they otherwise would have earned in that year.

Further, Job Corps appears to have substantially reduced the amount of SSI benefits YML participants reported receiving during the 4-year follow-up period. The estimated per-participant impact of $-\$2000$ is close to being statistically significant at the 5% level ($p = 0.057$) and represents a 50% reduction from the \$4000 in benefits they would have otherwise received, on average. This reduction also appears to have been concentrated among YML with medical conditions that would have otherwise been associated with higher rates of SSI receipt (based on control group patterns). As mentioned, this type of impact would be particularly noteworthy given that other employment interventions for youth with disabilities have not achieved reductions in receipt of long-term disability benefits. However, as we discuss later, there could be important differences in impairments and capabilities between YML who enrolled in the NJCS and the broader population of youth with disabilities eligible for SSI—in part stemming from Job Corps eligibility rules that were in effect during the 1990s.

Taken together, our findings suggest that the intensive model of Job Corps could be a promising option for serving transition-age youth with disabilities, but additional research is needed to understand its effectiveness for such youth today. For example, our results are based on self-reported information from survey data, which suggests a need for caution in interpreting findings, even though our results are generally robust to adjustments used in the original NJCS evaluation to account for the potential influence of survey response issues. Research using administrative data could provide an improved understanding of the impacts, given differences between survey and administrative measures of earnings (Barnow & Greenberg, 2015). Further, because Job Corps is so comprehensive, it is also an expensive program. Positive impacts on earnings do not necessarily imply cost-effectiveness, so it would be valuable to conduct a benefit–cost analysis. Finally, the Job Corps program and economic context have changed in important ways since the NJCS was conducted. Most notably, program eligibility rules during the 1990s meant that it served youth with relatively mild limitations and screened out those with significant conditions; in contrast, Job Corps eligibility is now more inclusive and the program must provide disability accommodations. A better understanding of how—and how well—Job Corps serves

youth with disabilities today, and the characteristics of those served, is needed to understand the potential for expanding the use of Job Corps for youth with disabilities in the future.

The rest of this paper proceeds as follows. In Section 2, we provide additional background on Job Corps, including an overview of its operations structure, a summary of the NJCS evaluation, and a discussion of its potential effectiveness for youth with disabilities. In Section 3, we describe the YML sample from the NJCS evaluation and outline our main analysis methods. In Section 4, we present our main impact estimates across a range of training and labor market outcomes. In Section 5, we put these impact estimates in context by comparing them to impact estimates obtained for NJCS youth without self-reported medical limitations at baseline, as well as examining how YML impacts differed across subgroups. In Section 6, we present results from sensitivity analyses to gauge the extent to which our conclusions might depend on our reliance on self-reported data, and to assess whether estimated differences in key impacts persist between YML and other youth when accounting for differences in characteristics between the two groups. Section 7 includes additional discussion of our results and potential avenues for future research.

2 | BACKGROUND ON JOB CORPS

2.1 | Overview of the program

Outside of the traditional schooling system, Job Corps is the nation's largest education and training program for economically at-risk youth between the ages of 16 and 24. Since its inception in 1964, nearly 3 million youths have participated in Job Corps (Kirsch et al., 2014). The program currently serves approximately 50,000 youths through at least 120 local Job Corps centers nationwide (US DOL, 2016a). To be eligible for Job Corps, applicants must establish both a need for services and the potential to benefit from them. Among other factors, eligibility criteria cover determinations regarding income, specific education or employment barriers that qualify applicants for the program, whether Job Corps can meet the applicant's need for additional education/training, and whether an applicant can reasonably be expected to successfully participate in the program (US DOL, 2016b). Once eligibility is determined, applicants are assigned to a specific Job Corps center; most participants live on-site at the center (Kirsch et al., 2014).

Job Corps provides a package of work-focused supports, including general education, vocational training, soft skills development, and ultimately job placement (Johnson et al., 1999; Kirsch et al., 2014; US DOL, 2016a, 2016b). Job Corps' education and training services are career-focused, aligned with industry standards, and oftentimes hands-on in nature. The program also emphasizes learning by doing through employer-based training opportunities and community service projects. In addition, participants receive social skills training and participate in other group activities designed to improve their employability. Throughout, the program provides a living allowance to active participants. Because Job Corps is a voluntary program, some eligible applicants are "no shows" and some participants exit prematurely (i.e., leave the program before completing their intended education or training). Job Corps staff monitor progress extensively, but services are ultimately self-paced. In the mid-1990s and -2000s, the average duration of services was around 8 months (Schochet et al., 2008; US DOL, 2009); quarterly performance reports indicate that average duration has risen to over 9 months in recent years.¹ Finally, exiting participants are provided with pre-employment counseling, job search assistance, and additional services to support job retention.

2.2 | The 1990s NJCS evaluation

To evaluate the effectiveness of Job Corps for youth enrolled in the mid-1990s, DOL conducted the NJCS, a large-scale experiment with a stratified randomization and sampling design. We provide a brief summary of the NJCS here; Schochet et al. (2008) give a broader overview and references to study reports with additional details.

The evaluation intake sample consisted of nearly all youth who applied to the Job Corps program in the contiguous 48 states between November 1994 and December 1995 and were subsequently found eligible. Eligibility criteria at that time were similar to those used today, with one important exception: The program previously excluded applicants with "health conditions ... that represent[ed] a hazard to themselves or others at a center, preclude[d] participation in Job Corps with an expectation of successful completion, or require[d] intensive and costly treatment" (Johnson et al., 1999). An implication of this exclusion is that the youth in our analysis may therefore have work-limiting but non-severe medical limitations.

The NJCS research sample consisted of (1) all youth applicants who were randomly assigned to the control group ($N = 5977$) and (2) a treatment group ($N = 9409$) that was randomly subsampled from the remaining pool of youth applicants. The experiment was stratified, with random assignment and sampling conducted using probabilities that were fixed for 16 combinations of factors that were relevant for meeting Job Corps program recruitment targets while conducting the evaluation.² Control group members were to be embargoed from receiving Job Corps services for a 3-year period; local and regional program staff helped enforce this embargo. This embargo was highly effective, with only around 1% of the control group taking part in Job Corps, although most (72%) enrolled in other education and training programs (Schochet et al., 2001). The most common alternatives to Job Corps were attending GED programs (42%); returning to high school (32%); and attending vocational, technical, or trade schools (29%). Some of these youth might have received assistance from other federal-state youth training programs, which were a common referral destination for youth found ineligible for Job Corps before the start of NJCS (Johnson et al., 1999). The control group was free to enroll in Job Corps once the 3-year embargo period ended, although only around 3% did so during the fourth year after random assignment (Schochet et al., 2001).

Data collected for the research sample included a baseline survey (fielded soon after random assignment); follow-up surveys at 12, 30, and 48 months after random assignment; program participation and cost data from the Job Corps management information system; and administrative data on earnings from the Social Security Administration (SSA, 2003) and a select set of state unemployment insurance agencies.³ The administrative earnings data were not retained in the evaluation's public-use files, so the analysis reported here is for self-reported outcomes from the follow-up surveys only; we return to the analytic implications of this below.

NJCS analyses indicated that Job Corps participation led to substantial short-run increases in receipt of education/training services and decreases in arrest rates, as well as medium-run improvements in labor market outcomes and decreases in receipt of public benefits (Chen et al., 2018; Schochet et al., 2001). In addition, youth who participated in Job Corps for longer periods also realized larger gains in terms of postparticipation earnings (Flores et al., 2012). These findings were all based on self-reported information from the 48-month follow-up survey.⁴

A longer-term assessment (Schochet et al., 2006) using administrative data found that the employment and earnings impacts of Job Corps were not sustained beyond the period covered by the 48-month survey. The evaluation also revealed that earnings and impact estimates were larger when using NJCS survey data than impacts when using administrative data, which is consistent with a more general pattern of differences found in the evaluation literature (Barnow & Greenberg, 2015). In the case of the NJCS, Schochet et al. (2006) determined that this disparity partly reflected some substantive differences, such as (1) informal employment not being reflected in the administrative data and (2) survey respondents' having a different distribution of outcomes than the population of Job Corps participants as a whole. However, their analysis also suggested that hours worked were likely overreported in the survey, and the authors could not rule out a "slight" upward bias in survey-based impacts due to treatment-control differences in nonresponse (although they also found no direct evidence of this).

Nonetheless, Job Corps is one of the few federal programs for which a rigorous evaluation has shown sizeable impacts on the labor market outcomes of low-income youth, even if only in the medium term. Also, as discussed later, our key results are largely robust to sensitivity checks for potential nonresponse differentials like those used in the original evaluation.

2.3 | Potential effectiveness for youth with disabilities

Several aspects of Job Corps might make it particularly beneficial for transition-age youth with disabilities, compared with other options. The intensity of services provided by Job Corps tends to exceed what is offered through other youth workforce programs, and Job Corps includes a strong job-placement component that is not found elsewhere. In addition, Job Corps provides wraparound supports that include medical examinations, treatment, and counseling for mental health and emotional problems. Other services include free meals, recreational activities, driver education, on-site child care support, and substance abuse treatment programs. Further, as noted above, most participants live in a residential facility, which might better allow for physical accommodations and alleviate potential transportation challenges. For participants with disabilities, Job Corps is also an opportunity to acquire human capital in an integrated setting and may provide an opportunity to live independently of their family for an extended period. Finally, Job Corps services can be tailored to each individual if needed. This feature could especially benefit youth facing complex challenges in the job market related to a medical condition.

There are also at least three mechanisms whereby Job Corps might reduce reliance on SSI benefits, which many youth with disabilities start receiving as children and continue to receive as adults (Hemmeter & Gilby, 2009). First, because the program provides room and board as well as an allowance, SSI payments to at least some youth, especially older ones, may be reduced.⁵ Second, increases in earnings could directly reduce SSI payments through a rule whereby benefits are reduced by \$1 for every \$2 earned above a modest “earnings disregard” threshold. Third, improvements in employment prospects may divert some participants from applying for SSI benefits. This effect might be particularly important for those who did not qualify when they were minors due to parental resources, which are no longer pertinent to the SSI means test at age 18, and/or whose benefits are initially terminated at an age-18 eligibility redetermination.

3 | DATA AND METHODS FOR ANALYZING YML IN THE NJCS EVALUATION

3.1 | Analysis sample and weights

We analyzed a sample of YML identified in the public-use data files from the NJCS, a group that has not been examined separately in the study's evaluation reports or subsequent work. We focus specifically on youth who reported having a limiting medical condition at baseline, or approximately 5% of all NJCS enrollees who completed baseline surveys. We further restricted our sample to those who completed the 48-month survey (used to measure outcomes for this analysis) and those with no missing data for weekly indicators of Job Corps participation during the embargo period (used to measure compliance with treatment assignment). Finally, because the NJCS treatment–control allocation was based on a stratified random assignment and sampling design (as discussed in Section 2.2), we kept only experimental strata containing at least one YML in both the treatment and control groups. The resulting sample includes 472 YML (271 treatment and 201 control).⁶ For comparison, we also describe the characteristics and report program impacts for 9366 other youth (5632 treatment and 3734 control) who met the same sample inclusion criteria but did not report a medical limitation at baseline.

We relied, in part, on the NJCS evaluation's analysis weights to account for both nonresponse and the stratified experimental design. Among YML meeting our other sample selection criteria, 82.2% had follow-up data available (83.1% in the treatment group and 81.0% in the control group). Among other youth, 81.8% had follow-up data available (82.0% in the treatment group and 81.4% in the control group). The NJCS nonresponse weights were developed using baseline characteristics through propensity models fit separately for the treatment and control groups (Schochet, 2001). The final NJCS analysis weights also accounted for randomization and sampling rates by experimental stratum. For our analysis, we made an additional adjustment to account for the relatively small size of the YML analysis group and the fact that some experimental strata were dropped due to our sample restrictions. To improve precision for in-sample estimates and avoid small-sample imbalances, we adjusted the weights so that the YML in the treatment and control groups follow the same (weighted) distribution across strata and have the same weighted sample size; we made the same adjustment for youth without medical limitations. Hence, our results are based only on comparisons of treatment and control group members within the same experimental stratum. Our main impact estimates also include stratum fixed effects and controls for baseline covariates, as discussed below.

3.2 | Baseline characteristics and medical conditions of YML

Consistent with how the Job Corps program is targeted, the YML in our analysis tended to be from more disadvantaged backgrounds than those participating in other programs for youth with disabilities. For example, 37% of those who were younger than 18 at baseline received welfare assistance. In contrast, only about 10%–14% of high-school-aged special education students received welfare assistance in the 1990s (Wagner et al., 2003). Additionally, compared with special education students, greater portions of the high-school-aged YML in our sample were Black and had a recent history of work.

Our data on YML are also consistent with Job Corps' policy of admitting applicants with relatively treatable medical conditions during the 1990s. The categories of impairments available in the data (Table 1) span conditions that could impose various degrees of limitations on the extent to which these YML could work or on the types of work they could do. To better understand the severity of the medical problems facing YML at enrollment, we assessed the extent to

TABLE 1 Distribution of medical conditions and rates of SSI receipt among YML in the analysis sample

Type of medical condition	Prevalence (fraction of analysis sample)	SSI receipt rate in the third year after random assignment (percentage, control group)
Asthma, allergies, respiratory conditions	0.29	6.4
Mental disorders	0.17	32.6
Upper and lower extremities, arthritis	0.15	19.6
Back	0.14	11.7
Heart or high blood pressure	0.07	19.7
Ulcers, diabetes, stomach, kidney, spleen	0.05	7.7
Epilepsy, cerebral palsy	0.03	31.3
Hearing, visual	0.03	40.1
Headaches, migraines	0.02	0.0
Other	0.05	5.5
Total	1.00	16.2

Notes: The types of medical conditions listed in the table are labeled to reflect the categories given in the documentation for the Job Corps baseline survey. The original evaluation established these categories by back-coding YML survey respondents' verbatim answers to the question, "What kind of serious health problem do you have?" All figures in the table were calculated using the main analysis sample of YML and nonresponse/stratification weights described in Section 3. The prevalence of medical condition type is based on the 468 YML whose health problems could be classified for the original evaluation. SSI receipt rates are for the subset of those YML who were assigned to the control group ($N = 200$).

Abbreviations: SSI, Supplemental Security Income; YML, youth with medical limitation.

which they were associated with subsequent SSI benefit receipt in the control group.⁷ We found that 16.2% of all YML in the control group received SSI in the third year after random assignment (Table 1), as did 21.5% of those who were age 18 or older at random assignment. This rate is markedly lower than that among transition-age youth participants in employment programs designed for people with disabilities during the mid-1990s (Hayward & Schmidt-Davis, 2000). Even so, this measure of SSI receipt for YML was triple the rate (5.3%) observed for other youth in the control group (Supplemental Table A7).

Nonetheless, among YML in the control group, SSI receipt in the third year after random assignment ranged from 0% to 40% across medical conditions. In subsequent analyses, we used a binary variable to summarize these conditions according to whether the control group's rate of SSI receipt in the third year was above average. The set of "higher SSI propensity" medical conditions includes mental disorders, issues affecting the extremities, arthritis, heart and blood-pressure issues, epilepsy, cerebral palsy, hearing problems, and vision problems.

Table 2 summarizes overall baseline characteristics for YML as well as treatment-control differences. Consistent with our impact analysis, the estimates are based on models that use the nonresponse/stratification weights described above.⁸ We assessed a range of covariates covering demographics, family structure, health, receipt of public assistance, criminal activity, education and training, work experience, and motivations for applying to the Job Corps program. Only one of the 54 differences between the treatment and control groups is statistically significant, which is consistent with the number that would arise from chance. The few noticeable differences do not follow a clear pattern that would suggest a particular direction of bias for impact estimates. Supplemental Information Table A3 summarizes the baseline characteristics for youth in the study who did not have medical limitations at baseline. As seen there, treatment-control differences for other baseline characteristics were even smaller among these youth than what we found for YML, likely because of the larger sample size. Only two such differences for other youth were significant at the 10% level.⁹

3.3 | Estimating and interpreting impacts

For all outcomes, we estimated two treatment effects that have different interpretations: (1) the intent-to-treat (ITT) effect and (2) the complier average causal effect (CACE). The ITT is the effect of being assigned to the treatment group. Per the framework of Angrist et al. (1996), the CACE is a local average treatment effect that represents the impact of

TABLE 2 Baseline characteristics and treatment and control differences for YML in the analysis sample (weighted)

Variable	Pooled mean	Treatment-control difference	Standard error of difference
<i>Age category</i>			
Ages 15–16	23.8	−1.2	(4.1)
Ages 17–18	39.4	−0.5	(4.8)
Age 19 or older	36.8	1.7	(4.7)
<i>Race/ethnicity</i>			
White, non-Hispanic	37.8	0.4	(4.7)
Black, non-Hispanic	44.7	0.5	(4.8)
Hispanic	12.3	0.5	(3.2)
Other race/ethnicity	5.1	−1.4	(2.0)
<i>Native English speaker</i>	92.6	−2.9	(2.5)
<i>Never married, not living together</i>	89.6	0.9	(3.0)
<i>Parenthood</i>			
No children	79.6	0.1	(4.0)
Has child younger than two	13.3	0.1	(3.3)
Has child at least 2 years old	7.1	−0.2	(2.7)
<i>Household structure</i>			
Living with both parents	14.1	3.3	(3.3)
Living with one parent	46.2	−2.7	(4.9)
Living with nonparent adult	18.8	−4.4	(3.8)
Living with no other adults	20.9	3.8	(4.1)
Youth is household head	12.4	−3.3	(3.2)
Number in household (count)	4.3	0.1	(0.2)
<i>Family's receipt of welfare while growing up</i>			
Never	42.9	1.2	(5.1)
Occasionally	21.3	0.1	(4.2)
Half the time	10.7	−2.4	(3.1)
Most of the time	25.2	1.1	(4.3)
<i>Receipt of public assistance in the year before random-assignment (RA) date</i>			
Aid to families with dependent children	30.4	−1.7	(4.5)
Food stamps	47.1	0.8	(5.0)
Other welfare	31.9	−0.9	(4.8)
<i>Housing arrangements</i>			
Living in public/subsidized housing	26.5	6.5	(4.3)
Family rents home without subsidy	29.4	−0.1	(4.5)
Family owns home	44.1	−6.4	(4.8)
Contributes to rent or mortgage	29.8	−1.9	(4.7)
<i>Nature of medical condition</i>			
Had condition for <3 years	31.1	5.1	(4.7)
Had condition for at least three but <6 years	18.3	3.6	(3.9)

(Continues)

TABLE 2 (Continued)

Variable	Pooled mean	Treatment-control difference	Standard error of difference
Had condition for six or more years	50.6	-8.7*	(5.0)
Had condition associated with a higher propensity of SSI receipt	44.0	0.9	(4.9)
<i>Self-assessment of general health</i>			
Excellent	21.5	0.9	(4.0)
Good	40.9	-0.2	(4.9)
Fair or poor	37.6	-0.7	(4.8)
<i>Risky health behavior</i>			
Smoked cigarettes in the past year	60.2	5.3	(4.7)
Drank alcohol in the past year	63.1	2.0	(4.7)
Smoked marijuana in the past year	36.4	4.0	(4.7)
<i>Involvement with the criminal justice system</i>			
Ever arrested or charged	32.2	3.7	(4.7)
Arrested multiple times	14.9	3.9	(3.6)
Ever convicted or pleaded guilty	21.0	3.3	(4.2)
Ever served time in jail	6.8	0.3	(2.8)
<i>Education and training</i>			
Had not attended high school by RA date	14.8	-1.1	(3.4)
Attended but did not complete high school by RA date	70.3	2.0	(4.4)
Completed high school by RA date	14.9	-0.9	(3.5)
Attended education or training program in the year before RA date	68.3	-1.0	(4.5)
<i>Work experience</i>			
Ever had a full-time or part-time job	82.0	1.7	(3.7)
Had a job in the year before RA date	66.7	-0.5	(4.6)
Earnings over the past year (dollars)	4618	-220	(826)
<i>Reasons for joining Job Corps</i>			
Joined to get away from community problems	59.8	-2.0	(4.8)
Joined to get away from home	58.2	-2.4	(4.8)
Joined for general self-improvement	9.7	1.2	(2.9)
Joined to be able to find work	88.2	-3.4	(3.2)
Joined to improve financial situation	4.9	0.0	(2.1)
Joined for other specific reason	10.9	-1.1	(3.1)
<i>Expectations of Job Corps</i>			
Expected to improve self-control or discipline	58.1	-1.1	(4.8)
Expected to improve self-esteem	54.4	1.1	(4.9)
Expected to improve ability to get along with people	56.0	5.8	(4.8)
Expected new friendships	62.8	-0.7	(4.7)

TABLE 2 (Continued)

Variable	Pooled mean	Treatment-control difference	Standard error of difference
Expected to improve math skills	62.7	0.6	(4.8)
Expected to improve reading skills	50.7	-1.2	(4.9)
Expected to receive training for specific job	94.6	-0.6	(2.1)

Notes: Estimates are percentages (means) and percentage points (differences) unless otherwise indicated. They are based on the main analysis sample of YML ($N = 472$) and calculated using regression models with stratum fixed effects in combination with the nonresponse/stratification weights described in the text. Results for each covariate exclude cases with missing data, so sample sizes differ by row—see Supplemental Information Table A1. The propensity of SSI receipt by medical condition was measured using data on the outcomes of YML in the control group in the third year after random assignment. Baseline earnings are expressed in 2016 dollars. Standard errors are robust to heteroscedasticity. The p value from a joint F -test of balance is 0.99. * denotes a treatment-control contrast that is significantly different from zero at the 0.10 level, respectively, based on a two-tailed test.

Abbreviation: YML, youth with medical limitation.

receipt of Job Corps services among youth who participate when assigned to the treatment group but who would not have participated if assigned to the control group. If the embargo were perfect, no members of the control group would have “crossed over” to participate in the program. In this case, the CACE impact would be equivalent to the treatment-on-treated impact (or impact per participant), which represents the average effect of Job Corps service receipt for all participants.

We focus on the CACE estimates, rather than the ITT estimates, because they provide a clearer gauge of program effectiveness for the youth who participated. ITT effects are diluted as a result of the substantial no-show rate in the treatment group, but this is not the case for CACE estimates. Because the crossover rate for the NJCS control group was negligible, we interpret CACE estimates as “per-participant” impacts.¹⁰ (We also refer to the treated mean for compliers as the “participant mean” and present a “counterfactual mean,” which we define as the estimated treated mean for compliers minus the estimated impact). CACE estimates are also better comparable between subgroups whose no-show rates differed notably, and they approximate per-participant impacts because control crossover rates were trivial for every subgroup. For completeness, ITT estimates are in Supplemental Information tables.

We used instrumental variables (IV) to estimate CACE impacts and standard regression models to estimate ITT impacts.¹¹ Our first-stage regressions for IV are of the form

$$J_{is} = \theta T_i + \kappa_s + \gamma' \mathbf{X}_i + \nu_i, \quad (1)$$

where J_{is} indicates whether individual i in stratum s enrolled in Job Corps during the 3-year embargo period, T_i is a binary variable denoting assignment to the treatment group, κ_s is a stratum fixed effect, \mathbf{X}_i is a vector of covariates, and ν_i is an error term. The covariates in \mathbf{X}_i correspond to the baseline measures shown in Table 2, excluding one category for each discrete-valued covariate and adding the square of baseline earnings. Second-stage regressions for IV are of the form

$$Y_{is} = \theta \hat{J}_{is} + \alpha_s + \beta' \mathbf{X}_i + \varepsilon_i, \quad (2)$$

where Y_{is} is the outcome, \hat{J}_{is} is the predicted value of enrollment from Equation (1) and all other terms are analogous to those in the first-stage regression. We estimate both regressions using the weights described previously and use robust standard errors to account for heteroscedasticity.

To compare CACE impacts across subgroups of YML, we created subgroup interaction terms for the participation indicator in Equation (2) and the treatment indicator in each of the two corresponding first-stage equations based on Equation (1). In all equations, we included separate stratum fixed effects for each subgroup. We did not, however, create subgroup-by-covariate interaction terms because of the relatively small sample size of YML. We conducted statistical tests of each subgroup impact after partitioning out degrees-of-freedom losses from covariates according to the relative size of the subgroup.¹² To compare impacts between YML and other youth, we estimated Equations (1) and (2) separately by group. When estimating the ITT impacts shown in the appendix, we used Equation (1), but with Y_{is} substituted in place of J_{is} . In all cases, we used t -tests to gauge the statistical significance of each impact and χ^2 tests to determine the significance of differences between groups.

The main estimates we report in the text are covariate-adjusted to compensate for the modest treatment-control differences shown in Table 2 for YML in the analysis sample. Including baseline covariates as regressors also provides an additional layer of robustness against potential nonresponse bias, as discussed previously, and is expected to improve the precision of the impact estimates. Missing values in the original data were relatively rare, never exceeding

10% for any covariate and below 5% for most (Supplemental Information Tables A1 and A3). Hence, given the relatively small initial sample size of YML, we imputed missing values of the covariates using a single draw from a multivariate chained imputation algorithm (implemented in Stata). Results for each outcome exclude cases with missing data for the given outcome. Supplemental Information Tables A5–A13 include sample sizes, means, and, for completeness, impact estimates that allow for stratum fixed effects only (with no controls for other baseline covariates).

4 | MAIN RESULTS FOR YOUTH WITH MEDICAL LIMITATIONS

4.1 | Training and education outcomes

Job Corps was highly successful in delivering education and training to YML. Job Corps enrollees typically have substantial deficits in their literacy and numeracy skills, with fewer than 20% of YML having a high school diploma at entry (see Table 2). Job Corps aims to alleviate these deficits through remedial education and GED preparation, as well as by providing vocational training to facilitate entry into the labor market. During the 4 years after random assignment, YML participants received over twice as many hours of education or training as what they would have received if they had not had access to Job Corps (Table 3). The first-year impact alone was 879 h, with smaller additional impacts in the second and third years after random assignment. The pattern of impacts is consistent with the timing of program exits; approximately 76% of YML had exited by the end of the first year after random assignment, and 94% had done so by the end of the second year.

Job Corps also led to substantial increases at the extensive margin of education/training receipt. The program increased receipt of education or training by 45 percentage points in the first year and by 24 percentage points over the 4-year period after random assignment. The overall impact on hours of education/training per participant across the

TABLE 3 Per-participant impacts on education/training outcomes of YML

Outcome	Counterfactual mean	Per-participant impact	Standard error of impact
<i>Receipt of education or training</i>			
Any education/training in year 1	53.9	45.4***	(5.9)
Any education/training in year 2	46.2	2.9	(6.9)
Any education/training in year 3	24.0	8.2	(6.5)
Any education/training in year 4	27.0	1.1	(6.4)
Any education/training over 4-year period	76.1	23.9***	(5.2)
<i>Amount of education/training received (hours)</i>			
Hours of education/training in year 1	295	879***	(73)
Hours of education/training in year 2	228	138**	(62)
Hours of education/training in year 3	106	87*	(49)
Hours of education/training in year 4	121	−5	(40)
Total hours of education/training over 4-year period	783	1030***	(140)
<i>High school completion rate</i>			
Had a GED at the end of year 4	27.2	14.6**	(6.3)
Had a high school (HS) diploma at the end of year 4	15.3	−0.3	(2.6)
Had a GED or HS diploma at the end of year 4	42.5	14.4**	(6.4)

Notes: Estimates are percentages (counterfactual means) and percentage points (impacts and standard errors) unless otherwise indicated. Each row presents covariate-adjusted CACE impact estimates for the given outcome using the main analysis sample of YML ($N = 472$), the nonresponse/stratification weights, and the regression specification described in Section 3. Standard errors are robust to heteroscedasticity. *, **, and *** indicate that the impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test.

Abbreviations: CACE, complier average causal effect; GED, General Educational Development; YML, youth with medical limitation.

4-year period was 1030 h—approximately the number of hours in a standard school year. This impact represents an increase of 120% over the 783 h YML would otherwise have received.

Together, the estimates imply that most of this impact was based on the intensiveness of the Job Corps program as opposed to the increase in the share of youth who received any education or training. More than four-fifths of the overall impact arose from the larger number of hours of education/training provided by Job Corps, compared with what program participants might otherwise have obtained.¹³

Job Corps participation further resulted in large increases in GED attainment among YML. Program participants were 14.6 percentage points more likely to have attained a GED by the end of the fourth year after random assignment, constituting a 50% increase relative to the counterfactual completion rate (Table 3). The likelihood of receiving a high school diploma was not substantially altered by Job Corps, however. Given that past research has found that the GED per se has minimal pecuniary returns in the labor market (Heckman et al., 2014), impacts of Job Corps on earnings would come through human capital formation or job placement rather than the attainment of this credential.

4.2 | Labor market outcomes

Participation in Job Corps led to large improvements in earnings for YML in the second through fourth years after random assignment, a period when most were no longer participating in Job Corps (Figure 1). During the first year, YML who took up Job Corps earned \$546 less than what they otherwise would have received, but the difference was not statistically significant. The earnings of participants jumped significantly relative to the counterfactual in the second year, at which point more than three-quarters of participants had exited Job Corps. Per-participant impacts were \$3490 in the second year, \$4104 in the third year, and \$4304 in the fourth year—increases of 57%, 51%, and 38%, respectively, over what their earnings would have been without access to Job Corps (Table 4).

Overall, Job Corps produced an estimated earnings impact of \$9708 for YML participants over the 4-year period, a 29% increase over their counterfactual base. Most of this impact likely comes from growth in the amount of time employed. As seen in Table 4, estimates for the number of weeks and hours worked were large, positive, and statistically significant in the second through fourth years after random assignment. Moreover, proportionate increases in the time spent employed due to Job Corps participation track the proportionate gains in earnings noted above. For example, the per-participant impacts for total weeks worked corresponded to an increase of 32% over the counterfactual base across the 4-year follow-up period.

4.3 | Receipt of public assistance

The earnings impacts of Job Corps appear to have led to increased economic self-sufficiency among YML, further resulting in reductions in public assistance receipt, particularly SSI receipt. The point estimates in Table 5 suggest small reductions in the receipt of benefits through the Aid to Families with Dependent Children (AFDC), Temporary Assistance for Needy Families (TANF), and food stamps programs combined. For example, over 4 years, there was a

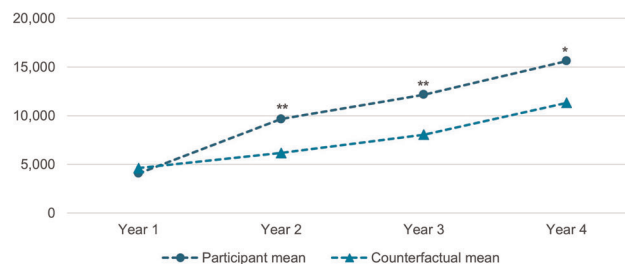


FIGURE 1 Earnings for YML participants, by year. *Notes:* All estimates were calculated using the main analysis sample of YML ($N = 472$) and weights described in Section 3. Participant means are estimated average treated outcomes for compliers; counterfactual means are compliers' treated means minus a covariate-adjusted CACE impact for the given outcome. Precision is based on standard errors that are robust to heteroscedasticity. *, **, and *** indicate that the underlying impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test. CACE, complier average causal effect; YML, youth with medical limitation

TABLE 4 Per-participant impacts on labor market outcomes of YML

Outcome	Counterfactual mean	Per-participant impact	Standard error of impact
<i>Earnings (dollars)</i>			
Earnings in year 1	4,615	−546	(1,086)
Earnings in year 2	6,165	3,490**	(1,487)
Earnings in year 3	8,057	4,104**	(1,589)
Earnings in year 4	11,318	4,304*	(2,263)
Total earnings over 4-year period	33,269	9,708**	(4,655)
<i>Employment rate (percentages for means and percentage points for impacts)</i>			
Any employment in year 1	53.0	8.2	(6.7)
Any employment in year 2	64.2	9.4	(6.4)
Any employment in year 3	69.0	9.7	(6.1)
Any employment in year 4	61.7	16.9***	(6.1)
Ever employed over 4-year period	92.6	4.1	(3.3)
<i>Weeks worked</i>			
Weeks worked in year 1	13.2	−1.8	(2.3)
Weeks worked in year 2	14.4	7.9***	(2.7)
Weeks worked in year 3	17.7	7.7***	(2.7)
Weeks worked in year 4	20.4	8.9***	(2.8)
Total weeks worked over 4-year period	67.5	21.1***	(8.0)
<i>Hours worked</i>			
Hours worked in year 1	540	−79	(111)
Hours worked in year 2	624	370***	(133)
Hours worked in year 3	754	392***	(129)
Hours worked in year 4	887	479***	(147)
Total hours worked over 4-year period	3,077	998***	(376)

Notes: See notes to Table 3. Financial amounts have been adjusted for inflation to 2016 dollars. *, **, and *** indicate that the impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test.

Abbreviation: YML, youth with medical limitation.

small and statistically insignificant impact of −\$533 (in 2016 dollars) on the amount of welfare/food stamp benefits received, an 11% reduction relative to the counterfactual average. In contrast, the estimated impact on total SSI benefits received was −\$2088—a 52% reduction relative to the counterfactual average—and statistically significant at the 10% level ($p = 0.057$).

The size of Job Corps' impact on total SSI benefits is consistent with the federal SSI benefit rates at the time of the study and the impacts on SSI receipt rates among YML. To assess the plausibility of the estimated reduction in total SSI benefits over the 4 years, we calculated the maximum possible reduction in total SSI benefits, given our estimates of the impact on the rates of SSI receipt each year. The monthly SSI benefit rate during the study period, adjusted for inflation, was \$726, so participants could have received at most \$8712 per year ($12 \times \726). If SSI recipients who stopped receiving benefits would otherwise have collected the full benefit amount, we would expect a 4-year reduction of \$2797 ($= \$8712 \times [0.079 + 0.089 + 0.088 + 0.065]$) in total benefits collected. This number represents an upper bound for what the impact could have been, because it assumes that participants would have otherwise received the maximum amount of benefits. As expected, the estimated reduction of \$2088 is below this maximum. As noted previously, reductions in SSI receipt could arise through several mechanisms, but it is not feasible to sort out the relative importance of each mechanism in the present analysis.

TABLE 5 Per-participant impacts on receipt of public assistance by YML

Outcome	Counterfactual mean	Per-participant impact	Standard error of impact
<i>Welfare or food stamp benefits</i>			
AFDC/TANF or food stamp receipt in year 1	37.5	-2.3	(5.2)
AFDC/TANF or food stamp receipt in year 2	34.7	-3.3	(6.4)
AFDC/TANF or food stamp receipt in year 3	31.6	-5.5	(6.2)
AFDC/TANF or food stamp receipt in year 4	25.0	-1.6	(5.4)
Amount of AFDC/TANF or food stamp benefits collected over 4-year period (dollars)	4925	-533	(1105)
<i>SSI benefits</i>			
SSI receipt in year 1	13.9	-7.9*	(4.5)
SSI receipt in year 2	16.5	-8.9**	(4.5)
SSI receipt in year 3	16.5	-8.8*	(4.6)
SSI receipt in year 4	14.4	-6.5	(4.3)
Amount of SSI benefits collected over 4-year period (dollars)	3825	-2008*	(1052)

Notes: See notes to Table 3. Entries are percentages (counterfactual means) and percentage points (impacts and standard errors) unless otherwise indicated. Financial amounts have been adjusted for inflation to 2016 dollars. *, **, and *** indicate that the impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test.

Abbreviations: AFDC, Aid to Families with Dependent Children; SSI, Supplemental Security Income; TANF, Temporary Assistance for Needy Families; YML, youth with medical limitation.

4.4 | Other outcomes

We did not find robust evidence that Job Corps alleviated the medical limitations faced by YML. Job Corps could conceivably improve health outcomes by directly providing healthcare; by providing information about available services or how to engage in self-care; by increasing their earnings and, therefore, spending power; and by helping them find work for which their medical conditions were not a limiting factor. However, Job Corps did not have any statistically significant impacts on the prevalence of medical limitations at any of the follow-ups (Table 6).

TABLE 6 Per-participant impacts on other outcomes of YML

Outcome	Counterfactual mean	Per-participant impact	Standard error of impact
<i>Prevalence of medical limitations</i>			
Medical limitation at the time of 12-month survey	34.3	-7.2	(6.4)
Medical limitation at the time of 30-month survey	30.5	-6.9	(6.6)
Medical limitation at the time of 48-month survey	18.2	3.3	(6.1)
<i>Arrest rates</i>			
Arrested/charged in year 1	21.5	-11.1**	(4.9)
Arrested/charged in year 2	12.7	-3.2	(4.3)
Arrested/charged in year 3	16.8	-3.8	(4.7)
Arrested/charged in year 4	10.7	-0.4	(4.8)

Notes: See notes to Table 3. Entries are percentages (counterfactual means) and percentage points (impacts and standard errors). *, **, and *** indicate that the impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test.

Abbreviation: YML, youth with medical limitation.

We also found that Job Corps decreased the likelihood of being arrested or charged with a crime. The estimates in Table 6 suggest that program participation roughly halved the probability that participants would be arrested/charged with a crime—from 21.5% to 10.4%—during the first year after random assignment. Reductions in arrests were smaller and statistically insignificant in later years, suggesting that the first-year effect potentially arose because participants were diverted from criminal activity by intensive Job Corps program activities.

5 | DIFFERENCES IN IMPACTS ACROSS GROUPS

5.1 | Comparison of YML to other youth without medical limitations at baseline

The earnings impacts for YML reported in Section 4 are substantially greater than the impacts reported in the original NJCS evaluation for all youth participants (Schochet et al., 2001, 2003, 2008). Our estimates are not directly comparable to those reported in the original study reports, given differences in methodologies (see Section 3). Hence, we estimated impacts for youth who did not report medical limitations at baseline—who we refer to as “other youth”—using the same methodology and sample restrictions as we used for YML. Supplemental Information Tables A7–A9 present estimates of these impacts for other youth and compare them to impacts found for YML. Here, we compare impacts for a key subset of outcomes.

Youth without baseline medical limitations had a notably different earnings profile over time than did YML. As seen in Figure 2, these youth experienced an earnings loss in the first year after random assignment, presumably because of their participation in Job Corps. Positive impacts for these youth emerged only starting during the third year after random assignment and, even then, were smaller than those for YML. Further, the estimates indicate that non-YML would still have earned more even had they not participated in Job Corps than YML who did participate. This could be related to the medical conditions of the latter group or to other factors that differed between the groups—an issue we return to in Section 4.

YML derived dramatically larger earnings increases from Job Corps participation than did other youth, even though the education/training impacts were similar across groups. As seen in Table 7, YML experienced smaller declines in earnings during the first year after random assignment and substantially larger positive earnings impacts in subsequent years. Over the 4-year period as a whole, Job Corps increased the earnings of YML participants by \$9708, on average, whereas other youth participants basically broke even. Although YML who participated in Job Corps still earned less than the counterfactual level of other youth, Job Corps helped to reduce the earnings gap between YML and other youth by about 71% over the 4-year period. The impact of Job Corps on hours of training and education was only slightly higher among YML compared with other youth: 1030 h versus 984 h, a nonsignificant difference. In proportionate terms, these impacts represented a 131% increase over the counterfactual number for YML participants and a 114% increase for other youth participants. Impacts on the likelihood of attaining a GED or high school degree, however, were not measurably different for YML compared to other youth.

Our results also suggest that YML participation in Job Corps led to comparatively large reductions in their SSI receipt and, possibly, their arrest rates during the first year after random assignment. As might be expected, the estimated counterfactual rates of SSI receipt were higher for YML than for other youth—about twice as high. Reductions in annual SSI receipt as a result of Job Corps participation were 45%–57% among YML, relative to the counterfactual rates, but only 18%–27% among other youth (Table 7). Differences in estimated impacts between the two groups were marginally significant (with p values between 0.11 and 0.12) for the first 3 years after random assignment. This finding is consistent with larger earnings impacts arising for YML relative to other youth. Other differences in

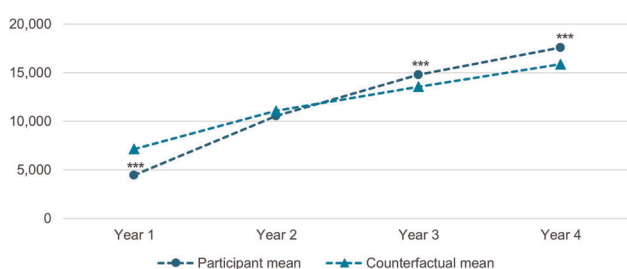


FIGURE 2 Earnings for youth participants without medical limitations at baseline, by year. *Notes:* See notes to Figure 1. All estimates are expressed in 2016 dollars. *, **, and *** indicate that the underlying impact estimate is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test

TABLE 7 Per-participant impacts for YML compared with other youth

Outcome	Estimates for YML participants			Estimates for other youth participants			Diff. in impacts	SE of diff.
	CF mean	Impact	SE of impact	CF mean	Impact	SE of impact		
<i>Amount of education/training received (hours)</i>								
Hours of education/training in year 1	295	879***	(73)	324	826***	(16)	53	(74)
Hours of education/training in year 2	228	138**	(62)	236	175***	(15)	-37	(64)
Hours of education/training in year 3	106	87*	(49)	173	16	(11)	72	(51)
Hours of education/training in year 4	121	-5	(40)	144	-10	(10)	5	(41)
Total hours of education/training over 4-year period	783	1030***	(40)	861	984***	(33)	46	(144)
<i>High school completion rate (percentages for means and percentage points for impacts)</i>								
Had a GED at the end of year 4	27.2	14.6**	(6.3)	25.5	15.5***	(1.2)	-0.9	(6.4)
Had a high school (HS) diploma at the end of year 4	15.3	-0.3	(2.6)	22.8	-2.0***	(0.6)	1.7	(2.7)
Had GED or HS diploma at the end of year 4	42.5	14.4**	(6.4)	48.4	13.7***	(1.3)	0.7	(6.5)
<i>Earnings (dollars)</i>								
Earnings in year 1	4,615	-546	(1,086)	7,130	-2,688***	(221)	2143*	(1108)
Earnings in year 2	6,165	3,490**	(1,487)	11,067	-516	(331)	4007***	(1523)
Earnings in year 3	8,057	4,104**	(1,589)	13,548	1,226***	(369)	2878*	(1631)
Earnings in year 4	11,318	4,304*	(2,263)	15,874	1,699***	(423)	2606	(2302)
Total earnings over 4-year period	33,269	9,708**	(4,655)	46,714	97	(960)	9611**	(4753)
<i>SSI benefits (percentages for means and percentage points for impacts unless otherwise indicated)</i>								
SSI receipt in year 1	13.9	-7.9*	(4.5)	6.2	-1.1	(0.7)	-6.8	(4.6)
SSI receipt in year 2	16.5	-8.9**	(4.5)	8.1	-1.7**	(0.8)	-7.2	(4.6)
SSI receipt in year 3	16.5	-8.8*	(4.6)	5.2	-1.4**	(0.6)	-7.4	(4.7)
SSI receipt in year 4	14.4	-6.5	(4.3)	3.9	-1.0*	(0.6)	-5.6	(4.3)
Amount of SSI benefits collected over 4-year period (dollars)	3825	-2008*	(1052)	1368	-359**	(149)	-1650	(1062)
<i>Arrest rates (percentages for means and percentage points for impacts)</i>								
Arrested/charged in year 1	21.5	-11.1**	(4.9)	13.7	-4.1***	(0.9)	-7.0	(5.0)
Arrested/charged in year 2	12.7	-3.2	(4.3)	11.0	-1.1	(0.9)	-2.1	(4.4)
Arrested/charged in year 3	16.8	-3.8	(4.7)	10.9	-0.3	(0.9)	-3.6	(4.8)
Arrested/charged in year 4	10.7	-0.4	(4.8)	11.0	-1.3	(0.9)	0.9	(4.9)

Notes: See notes to Table 3. Financial amounts are expressed in 2016 dollars. *, **, and *** indicate that the impact estimate or between-group difference in impacts is significantly different from zero at the 0.10, 0.05, and 0.01 levels, respectively, based on a two-tailed test.

Abbreviations: CF, counterfactual; GED, General Educational Development; SE, standard error; SSI, Supplemental Security Income; YML, youth with medical limitation.

short-term impacts could also have led to different medium-term earnings impacts. For example, the magnitude of the estimated per-participant impact on criminal activity in the first year after the random assignment was larger for YML than other youth (-10.6 vs. -4.1 percentage points), although the difference between groups was not statistically significant at conventional levels ($p = 0.162$).

5.2 | Variation in impacts across subgroups of YML

We also conducted an exploratory analysis of differences in impacts across subgroups of YML, although this analysis was hampered by limited sample sizes. As discussed in Appendix A, we found no significant differences in earnings impacts by age at baseline, gender, race/ethnicity, self-reported general health at baseline, or baseline medical conditions that were associated with differing propensities of subsequent SSI receipt in the control group (as discussed in Section 3). Nonetheless, estimated program impacts on dollars of SSI received differed significantly by age group and SSI-propensity groups. Job Corps participation led to 4-year reductions in SSI receipt of around \$6900 among older YML and \$5700 among those with higher-SSI-propensity medical conditions but did not lead to measurable changes in the total dollars of SSI received over the same period among younger YML or those with lower-SSI-propensity medical conditions.

6 | RESULTS FROM SENSITIVITY ANALYSES

6.1 | Assessing potential bias in earnings impact estimates given survey response issues

Two survey response issues identified in the NJCS could affect the internal validity of our estimates: (1) treatment–control differences in response rates, as discussed in Section 2; and (2) possible overreporting of hours worked, as discussed by Schochet et al. (2003). To assess the extent to which these issues might have affected our estimated impacts, we conducted three sensitivity checks along the lines of those in the original evaluation. As in Section 5, we focused on summary dollar amounts of earnings and SSI benefits covering the 4-year period after random assignment. The results are reported in Supplemental Information Tables A12 and A13.

First, trimming the sample to adjust for nonresponse differentials resulted in small changes to our estimated earnings impacts. For this check, we equalized the sample inclusion rates across all of the main study groups (defined by random assignment status and the presence of medical limitations at baseline) by removing sample members who took the longest to respond to the survey.¹⁴ Although we cannot assess whether respondents are different from nonrespondents, this check allows us to determine whether the impacts differ when including or excluding “marginal” respondents. If impacts were to differ notably, then we would be especially concerned about nonresponse bias. Reassuringly, this adjustment produced impacts for YML that generally differed by only 4%–6% from the unadjusted estimates (see Panel B of Supplemental Information Tables A12 and A13).

Second, applying nonresponse adjustment factors derived from administrative tax data resulted in earnings impact estimates that were moderately smaller than the unadjusted estimates. These adjustment factors were calculated by Schochet et al. (2003) on the basis of how earnings in the administrative data compared between each random-assignment group as a whole and the survey respondents in that group.¹⁵ Applying these factors to adjust the control group's survey earnings data should correct for potential internal validity concerns.¹⁶ Compared with the unadjusted estimate, this adjustment reduced the estimated impact for YML by approximately 10% but did not change the estimated size of the difference in impacts between YML and other youth (Supplemental Information Table A12, Panel C).

Third, adjusting for potential overreporting of hours resulted in smaller impact estimates but still suggested large benefits for YML (and limited effects for other youth). Following Schochet et al. (2008), we implemented this adjustment by scaling all earning measures down by 10%, yielding correspondingly smaller impact estimates (Supplemental Information Table A12, Panel D). Applying this scaling along with the nonresponse adjustment factors derived from administrative tax data (described above) yielded our smallest earnings impact estimate for YML: \$7870 over the 4-year follow-up period. However, the result remained (1) statistically significant at the 10% level, (2) within 20% of the unadjusted estimate, and (3) sizeable compared with the analogous impact estimate of $-\$879$ for other youth (Supplemental Information Table A12, Panel F).

6.2 | Accounting for differences in characteristics between YML and other youth

We found larger and sometimes statistically significant differences in the impacts of Job Corps for YML compared with other youth (Section 5.1), but these two groups also differed markedly in a range of baseline characteristics (see Supporting Information Tables A1 and A3). Hence, differences in impacts might be due to differences in these other characteristics rather than the presence or absence of self-reported medical limitations at baseline. As might be expected, YML were much more likely than other youth to report that they were in fair or poor general health at baseline (38% versus 12%). YML were also somewhat more socioeconomically disadvantaged than other youth. For example, YML were more likely to have received welfare most of the time in childhood, more likely to be living in public or subsidized housing, and less likely to have finished high school at baseline.

Accounting for these observed differences still yields impact estimates that differ markedly between YML and other youth. We re-estimated impacts for youth without medical limitations after reweighting to adjust for how they differed from YML in baseline characteristics.¹⁷ The 4-year earnings impact estimate for other youth is \$1415 after reweighting, which is larger than the initial estimate for this group (\$97) but is still significantly smaller (at the 10% level) than the \$9708 impact found for YML (Supplemental Information Table A12, Panel G). The 4-year impact estimate for SSI benefits received also increased in magnitude after reweighting but remained less than one-quarter the size of the estimated impact for YML; this between-group difference, like the initial estimate, was not statistically significant (Supplemental Information Table A13, Panel C).

7 | DISCUSSION

Examining data from the late-1990s NJCS, we found that Job Corps services for YML participants significantly increased their self-reported earnings, with impacts corresponding to 50%–60% of counterfactual average earnings. We also found evidence that Job Corps reduced reliance on SSI among YML, with SSI benefits received by YML cut by approximately half. These earnings and SSI impacts for YML were substantially larger in magnitude than the impacts we found for comparable youth who did not self-report a medical limitation at baseline. We also found especially strong impacts among YML over age 18 and among those YML who were at a relatively high risk of SSI receipt in the absence of Job Corps.

This pattern of results is encouraging when considering the potential cost-effectiveness of Job Corps for YML (compared with other youth), but data limitations prevent us from establishing the net social benefits for this group. The operating costs of Job Corps are high, and the original NJCS evaluation found that these costs were generally offset only for subgroups with large and sustained earnings impacts, with other social benefits (e.g., reduced crime) also playing a smaller role (Schochet et al., 2006). We were not able to reliably distinguish program costs between YML and other youth with the data at hand. However, we note that estimated earnings impacts over a 4-year period were on the order of \$7800 to \$10,000 for YML, while 4-year estimates ranged from \$900 to \$1400 for other youth. Administrative earnings data could help establish whether the large impacts for YML persisted substantially beyond the 4-year period covered by the survey, as well as help mitigate against potential survey response issues. Administrative data on disability benefits could also be used to produce improved estimates of the impacts of Job Corps on SSI receipt.

Our findings suggest that Job Corps increased the foothold in the labor market for YML who enrolled during the 1990s and thereby improved the work outcomes of at least some youth with disabilities. This result has potential value for policymakers, given that roughly three-quarters of a million youth with disabilities in the United States make the transition to adulthood annually, and future cohorts are likely to grow larger (Halfon et al., 2012; Slomski, 2012). The Workforce Innovation and Opportunity Act (WIOA), which went into effect in 2015, tasked vocational rehabilitation (VR) agencies with investing more to improve services for transition-age youth with disabilities. Although VR agencies have historically served youth, many are seeking new or improved ways to do so. Job Corps could be a promising model for them to explore.

However, for three reasons, the impacts reported in our study might not represent how effective Job Corps would be for the broader population of youth with disabilities today. First, YML in the NJCS had different health limitations compared with the broader population. In the NJCS, 17% of YML in our main sample (and 38% in the higher-SSI-propensity subgroup) reported having a mental disorder, but a much higher percent of SSI awards have gone to youth with mental disorders.¹⁸ Second, Job Corps has changed its rules about serving youth with disabilities. In the 1990s, Job

Corps eligibility criteria screened out youth with more significant health challenges. Today, such youth may now enroll, and the program must also offer accommodations designed to help youth with disabilities complete the program and obtain jobs, which could lead to different outcomes for them. Third, changes since the 1990s in other disability employment policies and programs have likely made it easier or more attractive for some young adults with significant disabilities to pursue careers. For example, WIOA sought to expand access to workforce services for people with disabilities in multiple ways. In addition, the 2010 Affordable Care Act made it easier for such young adults to access healthcare and long-term supports. Such supports could improve outcomes for youth with disabilities, regardless of whether they participate in Job Corps.

Nonetheless, the Job Corps of today might be of interest to a much larger share of youth with disabilities than it served in the 1990s—making it also important to develop the evidence base on the program's potential benefits for this group. As discussed in Section 2.3, Job Corps has a range of features that might make it particularly valuable for such youth, including several key features that past research on employment programs for youth with disabilities has identified as promising (Honeycutt et al., 2018). Moreover, around 30% of enrollees in recent years self-identified as having disabilities (US DOL, 2018). Our results, particularly for YML with high-SSI-propensity medical conditions, provide suggestive, proof-of-concept evidence that at least some of these enrollees could benefit from Job Corps as it operated in the 1990s. Although our results may not generalize to the youth served by Job Corps today, they point to the potential policy value of new evaluations of Job Corps services for such youth, and one such randomized trial has already launched.¹⁹

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DATA AVAILABILITY STATEMENT

All analyses are based on public-use data from the National Job Corps Study available from <https://www.aeaweb.org/articles?id=10.1257/aer.98.5.1864>.

ENDNOTES

¹Performance reports are available from <https://www.jobcorps.gov/job-corps-reports>.

²These factors were gender, being designated for a nonresidential program slot by a Job Corps counselor, living in an area from which a high proportion of nonresidential females came, and time period.

³As reported by Schochet (2001), the baseline survey response rate was 93.1%, with a 1.5-percentage point treatment–control difference. The 48-month survey yielded the self-reported outcomes data, and it was fielded to a random subsample of those completing the baseline survey. The (conditional) response rates for the 48-month survey were 81.5% and 77.8% for the treatment and control groups, respectively.

⁴Several papers have also used NJCS survey data to estimate impacts on wage rates, using a range of methods to account for compositional changes arising from additional employment due to Job Corps, potential heterogeneity of impacts across the wage distribution, survey nonresponse, and treatment noncompliance (Blanco et al., 2013; Chen & Flores, 2015; Frumento et al., 2012; Lee, 2009). This research provides important economic insights into how Job Corps affected human capital accumulation, but we abstract from it in this paper given our focus on the policy implications of the potential for Job Corps to increase overall earnings.

⁵Among those living at a Job Corps center who were either emancipated or at least 18 years old, receipt of room and board would reduce maximum SSI monthly benefit amounts by one-third. However, the \$1-for-\$2 rule benefit reduction noted in the main text likely did not apply to the Job Corps allowance for most youth, because their earnings fell below the SSI “earnings disregard” threshold for students. In any case, the earnings outcome measures we examine exclude the allowance paid to program enrollees.

⁶We removed a total of 233 YML based on the additional sample restrictions. We excluded 220 youths who did not complete the 48-month survey and 5 additional youths who did not have complete data available about treatment compliance. We then removed 8 youths from the resulting sample because they were in strata that did not have at least one YML in both the treatment and control groups.

⁷The Job Corps evaluation did not collect baseline measures of SSI receipt, but the rate at which the control group collected SSI after random assignment provides a counterfactual point of comparison. We focus specifically on the third year after random assignment—the last year the evaluation's embargo was enforced.

⁸Supplemental Information Table A2 presents analogous estimates for YML based on unweighted data. The results follow a very similar pattern as for the weighted estimates described in the main text.

⁹Supplemental Information Table A4 presents analogous estimates for youth without medical limitations based on unweighted data. The results suggest a similar conclusion as for the weighted estimates described in the main text. There were more statistically significant differences with the unweighted data, Supplemental our decision to uses weights for our main analyses. However, the p value from a joint F -test of balance is 0.47, demonstrating that the experimental groups still balanced on the covariates overall even without weighting.

¹⁰During the 3-year embargo period, just under 70% of treatment YML in our analysis sample enrolled in a Job Corps center, whereas only 0.3% of YML assigned to the control group did so. In our analysis sample of other youth who did not have medical limitations, 74.3% of those in the treatment group enrolled in a Job Corps center during that 3-year period, while just 1.1% of those in the control group did so.

¹¹In the appendix, we also report estimated treated and untreated means for compliers based on Imbens and Rubin (1997), calculating the underlying observable sample statistics using the analysis weights described in the main text.

¹²For statistical tests, we partitioned out degrees-of-freedom losses from these pooled covariates according to the relative size of the subgroup. That is, if N is the total sample size, N_g and p_g are the number and proportion in subgroup g , there are S strata, and the regression includes K covariates with noninteracted coefficients, we used $N_g - p_g K - S - 1$ in place of $N - K - 2S - 2$ to calculate standard errors and degrees of freedom for group g .

¹³To see this, consider holding hours per participant fixed at the counterfactual level of 783. The extensive-margin impact would translate into an increase of only 187 h [= 783 \times 0.239].

¹⁴Individuals randomly assigned in earlier weeks tended to have a wider distribution of response times (presumably because they had more time to complete the survey before the study ended). As a result, we trimmed based on response-time percentiles calculated for groups of at least 250 respondents in adjacent random-assignment weeks.

¹⁵For random-assignment group g and follow-up year y , the adjustment factor was $f_{g,y} = \bar{A}_{g,y} / \bar{R}_{g,y}$, where $\bar{A}_{g,y}$ and $\bar{R}_{g,y}$ denote the average earnings in that year for the full group and survey respondents, respectively. These adjustment factors were all less than one, indicating a general tendency for survey respondents to have higher average earnings than nonrespondents.

¹⁶That is, we multiplied the control group's earnings in year y by $f_{C,y} / f_{T,y}$. Separate adjustment factors were not available for subgroups, so we applied the overall adjustment factors to both YML and other youth.

¹⁷Specifically, we used a logit model to calculate propensity-odds weights that adjusted the distribution of baseline characteristics for other youth to be more similar to the YML distribution; the model included all of the variables listed in Supplemental Information Table A3. We then estimated impacts for youth without medical limitations using these weights in combination with the base nonresponse/stratification weights.

¹⁸For example, according to SSA (2003, tables 48 and 49), around 63% of SSI awards in 1995 to children under age 18 were for mental disorders, as were 36% of awards to adults aged 18–64, and more recent statistics indicate similar patterns.

¹⁹In late 2019, Mathematica, Arnold Ventures, and a state VR office, initiated an evaluation using a randomized referral design for youth who would otherwise receive VR services. Although study enrollment was suspended shortly after it started due to COVID-19, if the study is ultimately carried out as planned, it will provide new insights into the effectiveness of adding Job Corps to the set of employment service options available to youth with disabilities through the VR system.

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SUPPORTING INFORMATION

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APPENDIX A: VARIATION IN IMPACTS ACROSS SUBGROUPS OF YML

We examined whether impacts differed between subgroups of YML to ascertain which types of these youth benefited the most from Job Corps. This analysis is inherently exploratory and limited by sample size, so we discuss the magnitudes of subgroup-specific estimates without regard to whether differences between groups are statistically significant. We formed three subgroups based on standard demographic measures that were also considered in the original NJCS evaluation: age at baseline, gender, and race/ethnicity. In addition, we formed two health-related subgroups that were not considered in the original study. First, we divided the sample on the basis of self-reported general health at baseline. Second, as discussed in Section 3, we formed subgroups based on types of baseline medical conditions that, in the control group, were associated with a higher or lower propensity to subsequently receive SSI. We conducted this analysis for two summary outcomes: total earnings during the 4 years after random assignment and total SSI benefits received over the same period (Supplemental Information Tables A10 and A11).

The relative sizes of earnings impact estimates across subgroups defined by age, gender, and race/ethnicity are qualitatively consistent with findings from the original NJCS evaluation (Schochet et al., 2008), and the SSI impacts follow a similar pattern. Estimated earnings impacts per participant were substantially larger for older participants than those for younger ones (\$21,561 vs. \$3548). More strikingly, the entire reduction in SSI receipt among YML appears to stem from the impact on those older than 18. One reason may be how SSI rules change as recipients age, as discussed in Section 2. Our estimates also indicate that the earnings and SSI impacts were somewhat larger for white participants than nonwhite participants, but the differences by gender were small.

Estimated program impacts on both earnings and SSI receipt were substantially larger among YML whose baseline medical conditions predicted a relatively high propensity to later receive SSI benefits. Participation appears to have resulted in markedly higher earnings impact estimates for this group relative to other youth (\$16,424 vs. \$3751). Combined with the estimated counterfactual means in Supplemental Information Table A11, the impact estimates suggest that Job Corps helped them, as a group, catch up to other YML. Job Corps participation also reduced the extent to which youth with medical conditions associated with a higher propensity of SSI receipt actually collected SSI benefits. The estimated impact on total SSI dollars received for this group was $-\$5700$ (highly significant); for other YML this impact was \$542 (not significant). Estimated earnings impacts of Job Corps participation were also somewhat larger among YML who were in worse general health at baseline, compared with those in better general health. However, the estimated reductions in actual SSI receipt were modestly larger among YML who were in better health at baseline than among those in worse health.