DO YOUTH EMPLOYMENT PROGRAMS WORK? EVIDENCE FROM THE NEW DEAL*

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Abstract: *We study the lifetime effects of the largest American youth employment and training program – the Civilian Conservation Corps (CCC), 1933-1942. We match newly digitized enrollee records to Census, WWII enlistment, Social Security, and death records. We find that longer duration in the CCC led to improvements in height, health status, longevity, and lifetime earnings but did not improve short-term labor market (employment and wage) outcomes. We address potential selection into CCC duration and correct our estimates of long-term CCC impacts using two newly developed control function approaches that leverage unbiased estimates of the short-term effects of Job Corps (the modern version of the CCC) obtained from an RCT. Our findings suggest that short- and medium-term evaluations of employment programs underestimate their overall impacts because they do not observe total lifetime outcomes and ignore health and longevity benefits. JEL Codes: I28, I38, H53, J26*

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I. Introduction

Unemployment rates are highest among the young, particularly those from poor backgrounds. Recessions exacerbate this difference: at the height of the Great Recession, unemployment rates for those over age 25 peaked at 8.4% in 2010 but were as high as 19.6% for those aged 16 to 24 (US Bureau of Labor Statistics 2018). To address high rates of youth unemployment, government-run employment training programs target young adults. However, the short-run labor market effects of these programs have been shown to be modest at best. Moreover, although there are many randomized control trials evaluating the labor market impacts of youth training programs, there is limited evidence of their effectiveness over the long-run or their effects on non-labor market outcomes (Card, Kluve, and Weber 2018, Barnow and Smith 2015, Crepon and van den Berg 2016).

We evaluate the *short-* and *long-run* effects of means-tested youth employment and training programs by studying the impact of the Civilian Conservation Corps (CCC), the first and largest employment program in U.S. history.[1] We use new econometric techniques that combine lifetime observational data and results from a modern RCT to obtain causal estimates of the lifetime effects of the program. The CCC, created in 1933 to address high youth unemployment during the Great Depression, employed young men aged approximately 17 to 25 in unskilled, manual labor. Under the Army's supervision, enrollees were sent to work in camps in rural areas where they were also fed, housed, and given access to medical care. In addition to work experience, the CCC provided academic and vocational courses as well as cash transfers to participants' families. Between 1933 and 1942, the CCC trained three million enrollees across 2,600 camps. Several programs in existence today, such as Job Corps, Youth Conservation Corps, and JobsFirstNYC, are modeled after the CCC (Levine, 2010).

We collect a new, large individual-level data set of CCC participants and their lifetime outcomes. We digitize administrative records for roughly 25,000 men from the CCC program in Colorado and New Mexico covering the population served by the program between 1938 and 1943. Our data include information on their demographic characteristics, compensation, enlistment duration and reasons for leaving the program. We match these enrollee records to

[1] Salmond (1967) reports that in 1932, 25 percent of youths were unemployed, and another 29 percent were only employed part-time. Rawick (1957) estimates that about 20% of youths were unemployed and another 30% were working part-time.

1940 Census records, WWII enlistment records, Social Security Administration records, and individual death records. These data allow us to investigate the effects of the CCC on important short- and long-run outcomes, including employment, earnings, and longevity, as well as potential mediators, such as education, health, and geographic mobility.

To estimate the effect of the program, we exploit variation in service duration of the enrollees. Treatment duration varied from a few days to more than two years, with the average enrollee participating for nine months. We estimate two sets of models. Our baseline model attempts to address possible endogeneity by examining the determinants of duration and assessing the extent of omitted variable bias. We show that the determinants of duration are complex and that those who trained for long periods were not necessarily from higher or lower SES backgrounds. Moreover, many enrollees with both short and long service durations ended their training for arbitrary reasons. Controlling for extensive individual and camp-level covariates has little impact on our estimates. We assess the sensitivity of our results to adding various covariates, informally and formally, as suggested by Oster (2017). We also use rich data from Colorado's CCC records to perform placebo tests. We find that neither pre-CCC employment nor health (height and weight) predict duration, though we do find a modest relationship between duration and pre-program education.

In our second model, we apply recently developed econometric methods, which combine RCT results on short-run outcomes and observational data on lifetime outcomes, to address any remaining selection and estimate the lifetime causal effects of the program. We use the experimental data from the Job Corps (JC) randomized controlled trial (RCT) – in the spirit of seminal work by Lalonde (1986) who used experimental data on job training – to shed light on the internal validity of research using observational data. Although the JC data pertains to youth training that took place in the 1990s, the program was modeled after the CCC and retained many similar features. Moreover, JC participants are quite similar to CCC participants with regard to most socio-economic characteristics, duration of training, and reasons for quitting. Evaluation of the JC RCT documents modest short-run labor market effects that do not persist in the medium term (Schochet et al. 2008 and 2018).

To leverage the results from short term impact evaluations from RCTs to correct for potential section bias in long term observational estimates, we apply and extend new econometric techniques developed by Athey, Chetty and Imbens (2020). Specifically, we make use of the fact that the RCT for JC allows us to estimate: 1) the causal impact of job training duration on short-run outcomes; and 2) the amount of bias in OLS (observational) estimates of the impact of the duration of job training on short-run outcomes.

We use this information to address remaining omitted variable bias in the CCC long-run estimates following two control function approaches. In the first approach, we assume that short-run treatment effects are similar across the JC and CCC. In the second approach, we assume that short-run omitted variable bias is similar in the JC and CCC. Our estimates change very little when we include the controls for selection based on the JC RCT, suggesting little bias in our observational CCC estimates. Furthermore, in both cases we explicitly bound the remaining bias generated from violations of each assumption.

We find no short- or medium-run *labor market* benefits associated with job training as part of the CCC, consistent with most of the existing work on contemporary job training programs for youth. The literature on contemporary job training programs, however, cannot trace lifetime effects. Moreover, a growing body of evidence suggesting that the short and long run impacts of programs targeting health and human capital are often very different, with some programs showing fade-out of initial gains and others showing that benefits increase over time (Almond et al. 2018).

When we examine long-term outcomes, we find significant long-term benefits associated with longer training. Those who spend one year in the CCC have higher lifetime earnings by 5.2%, live 1.3 years longer, claim benefits (disability or pensions) at older ages and have 10% lower rates of SSDI claims. These gains are consistent with and likely mediated by the improved health of the participants (measured by height and weight in young adulthood) as well as their increased geographic mobility towards healthier and richer areas.

We conclude that our long-run estimates of job training based on the CCC likely represent causal estimates. Therefore, job training evaluations that focus only on the labor market impact of the program and/or consider only short- and medium-term effects, may underestimate the overall benefits. Our findings also suggest that there are positive returns to investing in young adults, contrary to the commonly stated findings that returns on human capital investment are low after age 18. Our conclusion differs from that of the evaluators of the JC experiment who write, "Because overall earnings gains do not persist, the benefits to society of Job Corps are smaller than the substantial program costs" (Schochet et al, 2018). They also differ somewhat from those of Hendren and Sprung-Keyser (2020), who report low values of the MVPF for JC. The difference is due to the fact that we are able to incorporate large increases in longevity and changes in SSDI claims, as well as increases in lifetime earnings that were not previously known.

This paper makes three contributions to the existing literature. We provide the first estimates of the lifetime effects of a youth training program across many dimensions. There is an extensive literature investigating the effects of youth training programs. Card, Kluve, and Weber (2018) collect estimates from more than 200 program evaluations and conclude that the short-term effects of these programs on labor market outcomes is modest, though there is some important heterogeneity. They also observe that treatment effects appear to increase overtime but the evaluations they consider only track outcomes up to three years after the conclusion of the program. In their comprehensive literature reviews, Barnow and Smith (2015) and Crepon and van den Berg (2016) come to similar conclusions and highlight the need for longer term evaluations. In addition, except for a few studies that look at criminal behavior (eg, Heller, 2014), very few evaluations of job training programs examine non-labor market outcomes – they typically focus on employment and wages.

Second, we demonstrate how to combine observational data on lifetime outcomes with RCTs to make progress on estimating unbiased causal lifetime effects. Starting with Lalonde (1986), a large methodological literature has investigated whether observational approaches (e.g. OLS, propensity scores, matching) can be used to obtain unbiased estimates of the effects of youth training programs on labor market outcomes by comparing the results from these approaches to those obtained from randomized trials (See Heckmann et al., 1999, for a review). We depart from this literature by using the experimental results from JC to learn about the bias in the short term estimated impacts of the program and use that to adjust our estimates of the long-term effects of the program derived from observational data.

Last, this paper contributes to the broader evaluation of the New Deal programs developed during the Great Depression. The Great Recession of 2008 and the recent pandemic-induced recession have renewed interest in understanding whether and for whom government programs deployed during large economic crises can be effective. Fishback (2017) provides a comprehensive survey of the literature on the short-run effects of New Deal programs, and reports that New Deal programs increased internal migration, lowered crime, and reduced mortality in the short run (see also Fishback, Haines and Kantor, 2007, and Vellore 2014.) To our knowledge, there are no empirical studies of the causal effects of the CCC program or of any other New Deal program on individual lifetime outcomes. Overall, the results are consistent with the hypothesis that the program provided important in-kind goods and services to disadvantaged populations in a time of need, improving their long-term health, productivity, and longevity.

II. Background: The CCC Program

Program Overview. The CCC was created to provide "relief of unemployment through the performance of useful public work and for other purposes." The CCC had two objectives: 1) to provide relief to unemployed youth; and 2) to preserve and enhance natural resources. The prevailing view at the time was that the provision of work ("relief through work,") would be more beneficial to the unemployed than the receipt of cash transfers ("direct relief"). Moreover, work would reduce the probability that youth would commit crimes and cause social disturbances (Brock 2005).

The untapped work capacity of idle youth would be used to create national parks, preserve forests, irrigate land, and address the damage of the Dust Bowl. Most camps had 200 enrollees at a time and were located close to work sites. The CCC program was popular, and many communities welcomed the camps and the resources they brought (Parham, 1981). Moreover, the enrollees did not directly compete in terms of labor with private sector activities. A nation-wide poll in 1936 showed that more than 80 percent supported the continuation of the program (Paige, 1985). As the program evolved, it added education components in 1934, which became mandatory in 1937, and in 1941 military training was added to the program. [2] The program ended in 1942 due to the onset of World War II.

Eligibility. The CCC program was only open to men who were unmarried, unemployed, primarily between the ages of 17 and 25, and U.S. citizens.[3] Preference was given to those in greater need and CCC enrollees were often selected from families already enrolled in relief

[2] Although perhaps unintended, because the military was in charge of running the camps, another perceived benefit of the program was "enrollees made splendid soldier material" (McEntee 1942).
[3] There were some changes to these initial criteria, importantly age eligibility of juniors was modified twice. See Supplementary Appendix Figure 1.

programs.[4] Government reports at the time confirm that enrollees were also poorly educated, with little work experience, as well as undernourished (McEntee 1942).[5] Enrollees had to present themselves in good physical condition upon their enlistment examination and have no prior history of criminal activity.[6] Finally, they had to be willing to send a substantial portion of their wages to an assigned family member and be willing to move to their designated camp location for the duration of the enrollment period. After the enrollee signed the contract, there was a two-week conditioning period, after which enrollees were sent to their assigned camps.

Compensation and program cost. Enrollees were required to work 40 hours per week and were paid $30 per month, of which generally $25 was sent home to a designated family member.[7] The government paid for the transportation to and from the camp, provided housing, uniforms, food, dental and medical care, and workers' compensation insurance, costing an additional $36 per month. The estimated total annual cost per enrollee was $1,004.[8]

Duration of enrollment. Individuals initially enrolled for a six-month period, and were allowed to re-enroll, for a maximum of two years (4 terms). Although the *average* enrollee in our sample worked for 9.8 months, there is large variation. CCC contracts could be terminated unilaterally by the government, based on governmental needs, at any point. Individuals also deserted, resigned or were expelled prior to completing their contract. Enrollees could leave early if they had secured employment, were enrolled in a formal schooling program or for "urgent and proper call" reasons, for instance the death of a parent or some other personal emergency. Enrollee turnover was costly, and efforts were made to keep it low.

The CCC in Colorado and New Mexico. CO and NM were relatively poor states in this period. National Income Accounts for 1930 suggest that per capita annual personal income was $571 in CO, and $329 in NM, while the nationwide average was $618.[9] Due to the large number of parks and forests in these states as well as the severe impact of the Dust Bowl, Colorado and New Mexico had disproportionate participation in the CCC program. In CO, a total of 57,944

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[4] In 1935, it became a requirement that enrollees be drawn from relief rolls, though in practice this was not always the case. In 1937, this requirement was eliminated.

[5] E.g. in 1939 and 1940, about 52% had 8 years of schooling or less (Annual Report 1940).

[6] Enrollees were vaccinated against typhoid, paratyphoid and smallpox at enlistment.

[7] Later in the program, a portion was retained as savings and given to enrollees upon dismissal.

[8] See BLS (1941). Levine (2010) reports this program was considerably more expensive than Works Progress Administration as it was estimated to cost approximately $800 per enrollee. Critics of the program pointed out that direct relief would have cost an estimated $250 per year instead (McEntee 1942). The value of the training and of the work achieved in terms of conservation is not considered in this estimate.

[9] Bureau of Economic Analysis NIPA 1929-today. SA1-3

men served, of whom 35,000 came from CO. In NM, a total of 54,500 served, of whom 32,300 came from NM (Cohen, 1980). Enrollees in Colorado and New Mexico were disproportionately Hispanic.[10]

III. Estimation Strategy and Identification Issues

We estimate the effect of the program on lifetime outcomes by comparing outcomes for those who served longer and shorter periods. This strategy is similar to what Flores et al. (2012) do to estimate the returns to the number of courses taken in JC and to Lechner et al. (2011), who evaluate impacts of short and long training programs in Germany. The intuition behind this approach is the following: if training increases skills through some standard production function, then more training should result in greater skills. We use the following specification,

$$Y_{ibj} = c + b * \left(duration\ of\ CCC\ service_{ibj}\right) + X_{ibj}B + e_{ibj} \qquad (1)$$

where Y_{ibj} is an outcome, such as employment or log of age at death for individual i born in year b training in CCC camp j, and X_{ibj} includes individual-level and camp-level covariates. The independent variable of interest is $duration\ of\ CCC\ service_{ibj}$, the duration of training in years. We estimate equation (1) clustering the standard errors at the application county and enrollment year-quarter level, though the results are not sensitive to this choice.[11]

The coefficient b identifies the causal effect of duration on a given outcome only if duration is uncorrelated with other determinants of the outcome, conditional on the observables. There are several threats to identification. First, duration is measured with error because dates are often incomplete or missing, possibly causing downward bias in the estimates. Second, there is a possible omitted variable bias on the individual-level. It may be that individuals with higher abilities trained longer because they benefitted more from the program, were less financially constrained and were able to better adapt to military life in camps (positive selection). Alternatively, poorer individuals may have had stronger incentives to train in the CCC because they were more in need of the CCC monthly payment (negative selection). Third, camp

[10] New Mexico also had a large share of Native Americans. Native Americans had their own CCC programs which operated separately within Indian reservations and were administered by the Bureau of Indian Affairs. See Parman (1971) for details. We have no data from this program.
[11] We also experimented with alternative approaches and estimate results clustering at the application county, enrollment year level. Overall, we found these alternatives do not materially impact our conclusions. The evidence suggests that there is little correlation across individuals.

characteristics that may affect both duration and outcomes are omitted. For example, individuals might have stayed longer in camps with good weather, and good weather could improve long-term health (positive selection). Alternatively, demand for work might have been greater in places hardest hit by the dust bowl, leading enrollees to stay longer in unhealthy locations (negative selection). In either case, the coefficient on duration would be biased.

To address these concerns, we take multiple approaches. First, we investigate the determinants of duration to determine the extent of possible selection issues. We also make use of the reasons why individuals dropped out to understand who leaves early and why. Then, we explore how the inclusion of individual- and camp-level covariates affect the estimates of the effect of duration, allowing us to estimate bias due to selection on observables. We estimate bounds using the method proposed by Oster (2017). For a subset of the data, we also conduct placebo tests to see if duration predicts pre-CCC enrollment outcomes (education, labor market experience, height and weight).

Finally, we use the data from the JC experiment to investigate and address potential selection in our estimates of the impact of the CCC on long run outcomes. We rely on and extend methods developed by Athey, Chetty, and Imbens (2020) that use experimental data on the impact of a given treatment on short-run outcomes to generate causal estimates of the impact of the treatment on long run outcomes found in observational data (but not the experimental data). Broadly, these methods use control functions generated using short-run estimates from the experimental JC data to adjust the estimates of the long-run impact of the program generated from the observational CCC data. We describe these methods in greater detail in section VIII.

IV. Data and descriptive statistics

A. Data collection

Colorado (CO) Enrollees. We digitized the entirety of CCC records contained at the State Archives of Colorado. These records include original applications of all individuals who applied. The collection, which includes 18,644 individuals, accounts for the population of individuals who trained between 1937 and 1942.[12] The applications contain: name, address, date of birth, place-of-birth, height, weight, race, and social security number (SSN), marital status, whether the

[12] We established based on published reports from the CCC that the records account for the complete population of records starting in 1937 (see Supplementary Appendix Figure 4).

father or mother is living, number of brothers, number of sisters, number of family members in household, rural status, farm ownership, occupation of main wage earner in household, educational details, employment status and history, name of designated allottee and whether the individual was rejected. With the exception of information on height, weight and race, which were collected upon medical examination, the rest was self-reported. We observe the discharge information detailing the company and camp the individual attended, reason for dismissal, the date of dismissal, and whether the dismissal was honorable.

New Mexico (NM) Enrollees. New Mexico CCC records include information on 9,699 individuals, covering the population of individuals that trained in state from 1938 to 1942.[13] For each individual, the records contain the following: name, date of birth, address, family information (head of family, address of family, and relationship to enrollee), allottee information, enrollment date, assigned camp, date and reason for dismissal and whether the dismissal was honorable. NM records contain substantially less information on participants than CO records because only discharge forms are available.

Camp-level Data. We collected information on the exact location of camps, allowing us to link camps to historical weather patterns (temperature and precipitation), and the (Euclidian) distance of the camp to the closest towns and to each enrollee's hometown. Using the camp name, we can construct indicators for the agency (and thus the type of work) that created the camp. We also construct average characteristics of enrollees (such as the fraction under age 18) in each camp and point in time. Finally, we match camps to census county-level characteristics, such as unemployment rates.

Death Records. The administrative data from CO and NM was matched to death records (including the Social Security Death Master File and state-level death records) to identify the date of death and social security number of each enrollee. This match was done manually by trained genealogists at the BYU Record Linking Lab who found CCC enrollees in the collection of records kept by Ancestry.com and FamilySearch.org. A summary of this process is available in Supplementary Appendix 1D. We find death dates for about 82% of recipients (Table I), (88% of CO recipients and 75% of NM recipients) representing much higher match rates than typically

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[13] We established based on published reports from the CCC that the records account for the complete population of records starting in 1938 (see Supplementary Appendix Figure 4).

found in the literature.[14] We use these data to compute the age at death.[15] We match the data using automated methods as a robustness check.

Social Security Records. Using social security numbers, we match our data to the Master Beneficiary Record File (MBR), maintained by the Social Security Administration, which contains information on individual lifetime earnings, disability, and retirement.[16] (More details are available in Supplementary Appendix 1E.) We are able to match 52% of our records to the MBR records. Only those that apply for benefits (social security pensions or disability) appear in the MBR. We have information on 80% of individuals who survived to age 65, indicating a high match rate for the targeted population. For all individuals we observe the age at retirement and whether or not they claimed Social Security Disability Insurance (SSDI) benefits. For individuals retiring in 1979 and later, we can observe the Average Indexed Monthly Earnings (AIME) which is computed as the average of the highest 35 years of earnings after adjusting for inflation.

1940 and WWII records. We match our records to the Federal Census of 1940 and to WWII Enlistment Records. These matches are made using the Abramitzky, Mill, and Perez (2018) algorithm. Details of the procedure are available in Supplementary Appendix 6D and 6E. The 1940 census includes location, demographics (race and ethnicity, marital status, place of birth, household information), and labor market information (employment occupation and wages). We successfully match 44% of individuals to the census, and 29% to WWII enlistment

[14] Our match rates are higher than those typically found in the literature (which range from 20 to 50%) for two reasons (Bailey et al. 2017, Abramitzky et al 2019). First, administrative records contain information not just on individuals but also on their family members. This greatly improves our ability to find individuals by using information from family trees and various vital registration records. Second, the death records come from various sources. Most commonly these come from the Death Master File (DMF) which includes the universe of death certificates in the US starting in the mid 1970s. But the collection also includes records from other sources, including state vital registration sources, deaths during WWII, and gravestones. A few individuals are observed as dying during CCC training.

[15] Mortality information is missing for some individuals for several reasons. First, some individuals died prior to 1975, which is the first year of complete death records in the Social Security Death Master File (For more information about coverage of the DMF, refer to Hill and Rosenwaike (2001). In this case, we might find a death record for them if one exists in state vital records. Second, some individuals might still be alive, so the age at death is censored. Based on SSA life tables we compute that about 1.1% of individuals born in 1920 (our median birth year) would be expected to be alive by 2017. Lastly, we might not have found individuals who died in the 1975-2017 interval due to measurement error and matching errors.

[16] We only observe SSN if they person reported it in the application in CO, or if it is available in the death certificate. However, SSNs are not available for anyone who died after 2008 (these are masked for privacy reasons) or for those who died young and never applied for a SS card.

records. This lower match rate to WWII records is expected: not all individuals enlisted or served in WWII, even when they were eligible and not all records of those who served survived.[17]

B. Sample Selection

For our analysis, we restrict attention to individuals for whom we can observe duration of training, camp, and the outcome of interest. This results in a sample of 23,722 men out of 26,290 (Appendix Table I row "Final analytic sample" out of row "All").

For the mortality analysis, we include only the 17,639 men with age of death information. This estimation sample generally is representative of the initial data (Table I). For the lifetime outcomes from the SSA, our sample includes 12,455 individuals, 52.5% of the original analytic sample. Again, this sample is fairly representative of the initial full sample in many dimensions (duration, YOB, age, height, weight, education, father alive, mother alive, household size, farm) with some notable exceptions (Table I). By construction, the age at death in this sample is higher because only those who survive to at least 62 are eligible to apply for pensions, unless claiming for disability. We also see fewer Hispanics, more people who lied about their age, and more people who sent money to their mothers. But these differences are small. Moreover, later we investigate the extent of sample selection and the effects of missing data and use imputations in alternative specifications.[18]

C. Summary Statistics: CCC Training and Lifetime Outcomes

Pre-CCC Characteristics. More detailed data for CO suggest that the enrollees were relatively disadvantaged (Table I). On average, enrollees were 18.7 years old, had completed 8.7 years of schooling and came from a household with 5 individuals. One in four came from a farm, 20% had a deceased father and 15% had a deceased mother. Despite height and weight examinations to exclude the unhealthy, 7% were underweight. Imputing the ethnic origin of the participants, we estimate that about 43% were Hispanic (see Supplementary Appendix 7). CCC enrollees came from poorer counties than the average males of the same age in CO and NM in the 1930 and 1940 census, consistent with their being recruited from relief rolls. Consistent with

[17] Several cards were lost to fire or were unreadable. See https://aad.archives.gov/aad/series-description.jsp?s=3360&cat=all&bc=sl
[18] We provide the full set of summary statistics in Appendix Tables 2a and 2b

the fact that CO and NM were very poor states, CO and NM enrollees were even more disadvantaged than the average CCC enrollee in the nation—they are substantially younger, shorter, weigh less, have more dependents, and more of them have fewer than 4 years of schooling (Supplementary Appendix Figure 6).[19] Data on the camps suggest that they were typically rural in nature, located relatively far from the enrollees' hometowns (150 miles on average).

Post CCC outcomes. Consistent with CCC enrollees being more disadvantaged at entry, they also have worse long-term outcomes than average for their cohorts: they died younger and earned less. The average enrollee eventually lived to age 70 (one year less than male cohorts born in 1920 who survived to age 17).[20] They also earned $405 in annual ages in 1940, compared $593 for men aged 18-32 in 1940 Census.

V. Determinants of Training Duration

Average training duration was 9.8 months, but there is large variation in the distribution.[21] Duration in months spikes exactly at 6, 12, 18 and 24 months, corresponding to 1, 2, 3 and 4 terms (Figure 1, Panel A.) However, most individuals (62%) dropped out in the middle of their assignment. Among those who left before completing their term, 21% deserted, around 14.5% were dismissed "for the convenience of the government" (e.g., the camp closed), 12% left for a job, and another 12% left because of an "urgent and proper call," for example family sickness or death (Figure 2 Panel A).

To investigate the determinants of duration we estimate simple OLS regressions of the duration of training as a function of individual, family, and camp characteristics. We include year-of-birth fixed effects (YOB) because different cohorts were eligible to train for different amounts of time (Supplementary Appendix Figure 5). We include county-of-enlistment by quarter-of-enlistment (CQE) fixed effects for two reasons. This addresses the fact that the number and types of camps that were opened varied over time and space, affecting where

[19] We compare the means in our estimation sample to the published national means. These were published in Appendix H of *Annual Report of the Director of the Civilian Conservation Corps: Fiscal Year Ended June 30, 1937* "Appendix H: Census of Civilian Conservation Corps Enrollees."
[20] This information comes from SSA cohort life tables.
[21] Aggregate data on the national CCC program from a 1937 CCC Census shows that the distribution of duration in our states (using CO) is skewed slightly towards shorter durations than the national distribution (Supplementary Appendix Figure 6).

individuals ended up serving and potentially the duration of training. It also addresses differential selection based on location and time over the program years: the type of individuals who apply for training (and other government benefits) varies substantially with local economic conditions (Méndez and Sepúlveda. 2012).

No clear relationship between personal characteristics and duration emerges in the data (Figure 3 and Appendix Table III). Individuals who were older trained for longer durations. Those who lied about their age, trained for shorter. Those who were farther away from home also trained for shorter. Surprisingly, individuals with a higher weight, who were presumably healthier individuals, trained for *shorter* durations. Height, which is a marker of improved nutrition and health during the growing years, does not predict training duration. Those with more education trained for longer but so did those who came from larger households or whose parents were deceased.[22]

This evidence is not consistent with a single narrative of selection. There appear to be three groups of enrollees. First, those who served for longer because they were positively selected, such as those with more education or older. A second group seems to be negatively selected, coming from farms, and larger, Hispanic households. Third, some appear to have more or less random reasons to drop out due to luck, such as a job appearing, a camp closing or having an emergency at home.

The evidence also suggests that conditional on individual characteristics and place and time of enrollment, camp conditions mattered (Appendix Table III). For instance, in places with less rain and milder weather, individuals trained for longer, as did those assigned to camps farther from cities. Peer characteristics also mattered. Durations were longer in camps with larger Hispanic shares of the population or with more men under 18, but shorter in camps with many men who misrepresented their age or sent smaller amounts to their families.

In sum, the primary evidence shows that desirable traits in an enrollee or in a camp did not necessarily lead to longer durations, and there is no single narrative of selection.

[22] These results are qualitatively similar if we estimate regressions separately for CO and NM (see Appendix Table III) but some coefficients are only significant in one state. There are no cases in which the coefficients are statistically significant and of opposite signs.

VI. The Long-Term Effect of CCC Training on Mortality, Lifetime Earnings and Disability

We now investigate the effect of enrollment duration on lifetime outcomes: mortality, earnings, age at retirement and disability claiming.

A. Impact of CCC Duration on Mortality

For this analysis, we restrict attention to individuals who we linked to a death certificate and who died after age 45 (to avoid WWII related deaths). The results are not sensitive to these restrictions. The longer an enrollee trained, the longer he lived (Figure 4). The relationship is positive and linear.

Next, we estimate an accelerated failure time model of the age at death on duration in which we add controls for the characteristics of the enrollees and the camps to examine whether and how our estimates change in response. The first column of Table II Panel A with no controls shows a very precise coefficient on duration of 0.013. Controlling for cohort fixed-effects and county-of-enrollment-quarter-of-the-year (CQE) fixed-effects (column 2) does not change the coefficient estimate. Including family and individual characteristics in column 3 (ever rejected from the CCC, disabled, non-junior member, age, dollars per month allotted, gap in service, distance from the camp to home, whether Hispanic, and for those in CO only, highest grade completed, household size, life on a farm, height and weight at enlistment, whether mother or father deceased, tenure in the county prior to CCC enrollment and reason for discharge) lowers the coefficient to 0.011.[23] Adding camp characteristics in column 4 (mean precipitation in the camp, min and max temperature, type of camp, distance to closest city), peer characteristics in column 5 (average age, share Hispanic, average allottee amount and gaps in service), or camp fixed effects in column 6 changes the coefficient very little. The magnitudes imply that one more year of training increased the age at death by one year (roughly 1.3 percent of 73.6 years of life). Given that the average duration was 9.84 months, the program increased age at death by 0.8 years for the average enrollee. When we limit our sample to CO where the records contain more

[23] Full regression with coefficients for controls in Appendix Table IV.

important baseline information, such as education, height, etc., the results are again similar (column 7).[24]

The fact that the coefficient is essentially unchanged from columns 1-7 as we add more detailed controls suggests that selection bias may be small. However, to more formally assess the magnitude of the omitted variable bias, we re-estimate these coefficients under various assumptions about the unobservables following Oster (2017). If delta (the proportionality value) is assumed to be 1 (i.e., unobservables as important as observables) then our coefficient would be 0.0136. Alternatively, if delta is assumed to be -1, we would estimate 0.0127. Thus, one more year of training would increase the age at death between 0.96 and 1.02 years.[25]

Finally, to examine possible non-linearities, Figure 5 shows the results of the regression of probability of survival to age *x* on duration for every age between 45 and 90. The coefficients are small and statistically insignificant at younger ages, when the survival is very high. They become positive and statistically significant starting at age 56, continue to increase and peak between ages 68 and 78, and then decline thereafter. As a function of the baseline survival rate, which is declining throughout, the effects rise until age 67, and then decline.

Sample attrition. About 20% of the original sample is missing information on age at death. We assess whether missing age at death is systematically related to training duration (with or without conditioning on covariates). Table III Panel A shows that, without controls, the missing rates are not a function of training duration. But conditional on camp, family and individual characteristics, age at death is about 9% (1.7/18) *less* likely to be missing for those who trained for an additional year. This suggests that differential attrition could bias our OLS estimates. To address this issue, we estimate survival models where we make various assumptions about the missing data. The results in Appendix Table V show that our findings are robust to various imputation approaches.

Quality of the longevity data. Our main results use the information found by trained genealogists from multiple sources to determine the age at death. To assess the quality of the data and whether the hand matching procedure introduces unknown biases, we replicate the

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[24] For NM and for CO records with missing data we impute using the mean and include a series for dummies to indicate when the covariate is missing.

[25] We also examined a specification including an indicator variable "completed term" which equals 1 when participants complete increments of 6-month terms. We did not see large differences in the duration coefficient. However, when splitting the sample to those who completed 0 terms and those who completed at least 1 term, we see no effect of duration for those with 0 terms and a similar effect for those who completed at least 1 term.

results using machine matches only. To do this we use the EM algorithm to match our records to the Death Master File. The results in Appendix Table VI show that we still obtain a positive and statistically significant coefficient of duration on age at death, similar in magnitude to our main estimates.

B. Impact of CCC duration on lifetime income

We estimate the impact of program duration on lifetime income as proxied by the AIME (Average Indexed Monthly Earnings), which is the average of an individuals' best 35 years of real earnings as reported to the SSA. This amount is used to calculate pensions. The AIME is only available for those claiming after 1979 but a specification check suggests that the results would be similar if we were to extend to those claiming prior to 1979.[26]

The mean AIME as a function of duration for the sample claiming after 1979 shows a flat or slightly negative relationship between duration and AIME. This relationship reverses once we control for year of birth (Figure 6). This is because more recently born cohorts had shorter durations (due to the end of the program) but larger incomes, generating a spurious negative correlation. In OLS regressions without covariates there is indeed no relationship between duration and the AIME (Table II Panel B, column 1). Consistent with the figure the coefficient becomes large, positive and statistically significant when we add controls for birth cohort and for quarter and county of enlistment (column 2). The estimates imply an increase of $67 additional monthly earnings for those who participated for one year, representing an increase of 7% relative to average earnings. As we add more controls across the columns, the estimated coefficient declines to $50, or 5.2% of average monthly earnings (Table II Panel B, column 6).

These results do not appear to be driven by sample selection or attrition in the SSA data. There is no effect of duration on whether we match an enrollee to MBR. Nor does the effect of duration on longevity change when we limit to the sample matched on the MBR (Table III Panels B and C).

We can compare our estimated returns to a year in the CCC to the returns from a year of schooling. OLS estimates of the returns to schooling from other sources range from 5% (Goldin

[26] We do have another, slightly noisier, proxy of earnings (the PIA which is based on the AIME) that is available prior to 1979 as well. We find similar results for the PIA pre- and post-1979. See Appendix Table VII for results.

and Katz 2000) to 8% (Clay et al 2012). Thus, the returns to one year of CCC training (5.2%) are on the lower end of the returns of a year of schooling.

C. Impact of CCC Duration on Age at Benefit Claiming and SSDI Claiming Rate

We are also able to estimate the impact of CCC duration on age at which individuals first claim benefits from the Social Security Administration (either disability or pensions) and on whether individuals become disabled, measured by Social Security Disability Insurance (SSDI) claiming. We find that one year of CCC enrollment increases the age at claiming benefits by half a year, relative to mean age at claiming of 60 years (Table II Panel C), suggesting CCC men were in better health, retired later and lived longer. This is consistent with existing work showing that early retirement is associated with death at younger ages (Waldron, 2001, Fitzpatrick and Moore 2018).

We can examine this health channel directly by looking at how duration affects SSDI claiming, a measure of health. Twenty one percent of the sample claims SSDI benefits. When the full set of controls is included, we find that one year in the CCC reduces claiming by 2.2 percentage points, or 10 percent (Table II Panel D). Overall, we find that CCC participation improves health in the long run as measured by delayed benefit claiming, reduced SSDI claiming and greater longevity.

D. Treatment Effect Heterogeneity

Recent reviews of training programs (Card, Kluve, and Weber, 2018, Barnow and Smith 2015, Crépon and van den Berg 2016) suggest substantial heterogeneity in the effects of training programs. We explore heterogeneity in Appendix Table VIII. We find that the poorest and most disadvantaged benefitted more. The effects were also larger for those that came from counties with higher unemployment rates. These findings are consistent with Card et al. (2018)'s finding of larger effects of job training in recessions and among the more disadvantaged. Our results differ in two dimensions: we find larger gains for the young, and significant benefits for Hispanics.[27]

[27] We suspect these differences are due to several factors: 1-we compute Hispanic ancestry and do not rely on self-reports, b-our enrollees are from only 2 states with large number of Hispanics in the population; 3-the country of origin among our enrollees differs substantially from today.

VII. Short-Term Outcomes: Evidence from the 1940 Census & WWII Records

We estimate the short-run effect of CCC enrollment in an effort to both compare our estimates with existing work on the short run impact of more recent job training programs and to understand the mechanisms behind our long run impacts of the CCC. We first investigate the effects of CCC duration on employment and wages, the standard outcomes that are typically assessed in job training programs. Next, we investigate other mechanisms such as formal education, health improvements, and geographic mobility, all of which have been associated with improved longevity and labor market outcomes in previous work.

A. Labor market outcomes: Evidence from the 1940 census

For this analysis, we constrain our sample to 9,518 men who participated in CCC before January 1st, 1940, of whom we find 43% in the 1940 census. Duration is unrelated to whether we locate an enrollee's census record. [28]

CCC training duration appears to have little effect on the short-run labor market outcomes of CCC participants (Table IV).[29] Most men (91%) are in the labor force, and longer CCC training had at best a very small effect on this outcome: a 2.1% increase relative to the mean. We observe no effect on employment (conditional on labor force participation) during the week prior to the Census. There is a small, negative and imprecise effect of duration on earnings.[30] Overall, our results are consistent with the conclusions reached in recent reviews that the labor market effects of job training are more positive in the long run than in the short run.

B. Health and Military Service: Evidence from WWII Enlistment Records

We estimate the short run impact of the CCC on health as measured by height and weight using WWII enlistment records. Unlike the 1940 census records, duration does predict their presence in the WWII records: an additional year of training leads to a robust and significant 3.8

[28] Duration does not predict whether we find an enrollee in the 1940 census once we include birth cohort and county-quarter fixed effects (Table IV top panel).

[29] In Table IV, we only present specification with camp fixed effects corresponding to Column (6) in Tables 2 and 3. Appendix Table IX presents results on all specifications for Census outcomes and Appendix Table X for WWII outcomes.

[30] For example, the largest coefficient for weeks worked is -0.937 which corresponds to 3.4% change relative to the mean of 28 weeks worked. Similarly, we observe a negative but statistically insignificant effect on earnings, corresponding to about a 3% decrease in wages at the mean.

percentage point increase in the probability we find the individual in the WWII enlistment records, a 12% increase relative to the mean (Table IV Column 6). This result is not surprising: the army organized and administered life in the camps, and CCC men who trained for a long time were well acquainted with military life. Two percent of men in our sample ended their CCC engagement to enlist in the military directly. Given that we have not found differential matching rates in any of our other data, we do not believe differential matching explains this result. Rather, we conclude that the program increased the likelihood of serving in the military. This could reflect greater familiarity with the military after serving in the CCC or it could reflect the acquisition of additional non-cognitive skills that increased the likelihood of success in the military.

For the 5,500 observations we match to WWII records which contain height, weight and schooling, we find that one more year of training translated into roughly 1 more inch of height. This effect is large by historical standards: for example, it took British men 100 years for their average height to increase by 6 inches (Fogel 1994). This result holds conditional on height at CCC enlistment, indicating additional growth after CCC enrollment rather than initial differences in height.

There are multiple reasons why the program increased heights among CCC men despite the fact their average age was 19. First, these results are consistent with existing work showing that undernourished populations grow more slowly and achieve their final adult height at older ages (Steckel 1986). Individuals in the CCC were poor and they received food and medical care, including vaccinations, as part of their participation in the program, likely improving their nutritional status. Second, national reports of the CCC program show that the average height gain in the CCC was half an inch (McEntee 1942). Our estimates are likely larger because our population is more disadvantaged than the average CCC enrollee. Finally, 9% of our sample of CCC enrollees were likely younger than they reported.

Consistent with this, we also observe a 5-6 percent increase in BMI, a common indicator of short-term nutrition. The final CCC report documents an average weight gain of enrollees during the program of 11 pounds (McEntee 1942), and our results suggests that 40-60% of these gains persisted.[31] These results are also consistent with our finding that CCC service lowered

[31] For an average enrollee in our sample, adding 11 pounds would translate to a gain of 8%.

SSDI claiming and increased longevity. We conclude that the CCC improved overall health among participants.

C. Effects on Education, and Geographic Mobility

We also show results for formal years of schooling and geographic mobility, which are observed in both the Census of 1940 and WWII Enlistment Records. For these outcomes, we combine information from the two sources to maximize sample size.[32] We control for the time since discharge (or equivalently the year of observation) to account for the fact the outcomes are not observed at the same time for all enrollees.

We find a positive and statistically significant effect of duration on years of schooling of about 0.17 years, relative to a mean of 9.2 years of schooling, controlling for education at baseline (Table IV Column 9).[33] This represents one tenth of the standard deviation in schooling in the WWII records, and is larger than the effect of many education policies, such as child labor laws, on educational attainment during the early 20th century.[34] This effect likely represents a combination of additional schooling completed as part of the CCC and schooling obtained after the CCC. CCC reports indicate that 8% of men obtained additional schooling during the program.[35] Assuming 8% obtained one more year of school, this would result in a gain in years of schooling of 0.08. Given that 3.5% of enrollees in our data cited education as the reason for leaving the program, post-CCC education gains likely accounts for the rest.

Finally, we examine the relationship between duration and short- and long-term geographic mobility by comparing the county of individuals in their original CCC application with the county of residence indicated in the 1940 Census records, the WWII records and in the death certificates. Thirty five percent of participants moved in the short-term (by 1940 Census or

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[32] Because the WWII records contain the latest information, we take information from WWII if the enrollee can be found in WWII record and 1940 Census if cannot be found in WWII record and discharged before 1940. For education and marriage, we take the value at WWII, which is later than 1940, if observed in WWII and the value at 1940 Census if only observed in the Census. For moving, we code someone as moved if they moved counties in either 1940 or in WWII.

[33] When we restrict our analysis to those with non-missing baseline education, the estimate declines to 0.12 and remains significant at the 1% level (Appendix Table X Column 7).

[34] For example, see Lleras-Muney (2002) or Goldin and Katz (2008). One more year of compulsory schooling led to about 0.05 years of schooling.

[35] The final report states that over one hundred thousand enrollees (3%) were taught how to read and write in the CCC program, 4% of men received primary school degrees (8th grade), 0.6% got their high school diplomas and a handful (270 out of more than 3 million) obtained college degrees. Thus, about 7-8% obtained some schooling.

WWII enrollment). Training in the CCC substantially increased the likelihood of moving to another county: one more year of training increased the probability of moving by 5.7 percentage points, or 17% relative to the mean (Table V Column 2).[36] This is substantial particularly during this period, which was characterized by historically low migration nationwide.[37] Moreover, when CCC men moved, they moved to locations with higher paying weekly or annual wages (as of 1940) and lower mortality, measured by the average county level mortality from 1950 to 1968 (Table V Columns 3-4).[38] Over the long run, however, most individuals moved and the effect of duration on mobility fades (Table V Columns 5-7).

In sum, enrollees who served longer had better health, more schooling and were more likely to move to healthier, richer places, but training longer had no detectable effects in the short-run on labor market outcomes.

Before moving on, we conduct one last check of the validity of our OLS approach. For CO enrollees, we have baseline measures for several outcomes: height, weight, education and prior labor market experience. In our main results we control for these. However, this allows us to test if duration predicts these pre-intervention outcomes. Appendix Table XII shows that duration does not predict these pre-CCC outcomes, except for education. These results suggest that by in large our approach produces unbiased estimates of the effects of the program, but some bias may remain. In the next section we estimate the long run and lifetime effects of the CCC, incorporating evidence from the JC RCT to address any remaining selection.

VIII. Incorporating Evidence from the Job Corp RCT to Address Selection Bias in the CCC

In estimating the long-run and lifetime effects of the CCC, we build on the seminal work of Lalonde (1986) who used experimental evidence on the impact of job training to assess results based on observational data. In particular, we apply and extend methods developed by Athey, Chetty and Imbens (2020) that enable researchers to exploit data from randomized controlled trials to address potential bias in results based on observational data in a control function

[36] As in Table IV, we only present results on specification with camp fixed effects corresponding to Column (6) in Tables 2 and 3. Appendix Table XI presents full table of all specifications.
[37] In the 1940 census 12% of people report living in a different county than in 1935. https://www.census.gov/dataviz/visualizations/010/
[38] We use this measure instead of the county mortality from 1940 onwards because of the disruptions that occurred during the WWII.

framework. In particular, we exploit the availability of data for a 1994 *randomized evaluation* of the modern equivalent of the CCC: the Federal Job Corp Program (JC).[39] We address potential endogeneity of our CCC estimates using two approaches that make use of the JC RCT.

Both approaches require the JC to be an externally valid experiment for the CCC, either for the treatment effect directly or for the degree of selection bias. The two programs have considerable similarities in both the observable characteristics of program participants and the short-run impacts, suggesting that the JC may be a comparable experiment. After presenting comparisons of the CCC and JC in the next section, we proceed to describe our two approaches to correct for bias in the CCC estimates in greater detail, and present our bias-corrected estimates of the impacts of the CCC on long term outcomes. Finally, we quantify how relaxing the assumptions changes our estimates.

A. Comparing CCC and JC Enrollees

Comparing participant characteristics. If we restrict attention to men in JC, second column of Table VI shows that overall, JC and CCC participants are similar.[40] Both are young (19 years old on average) and have relatively few years of schooling. JC participants have completed 10 years of schooling, compared with 8.5 for the CCC enrollees, and 19% have graduated from high school compared with 12% of the CCC enrollees. The CCC sample has considerably more Hispanics, due to the fact we concentrate on CO and NM, whereas the JC data is national.

Participants are also similar in terms of duration of enrollment and reasons for leaving. Mean duration is 9.8 months (s.d. 7.47) for CCC and 5.8 months (s.d. 6.6) for JC. The main reason for the lower duration of the JC participants is that 20% never trained (Figure 2). Conditional on training, the duration among the treated group in JC is 7.8 months. Reasons for leaving are also similar across the two programs.[41] Finally, and perhaps most importantly, when we try to predict

[39] The current website (https://www.doleta.gov/job_corps/) states that "The program helps eligible young people ages 16 through 24 complete their high school education, trains them for meaningful careers, and assists them with obtaining employment." "Students can earn a high school diploma or the equivalent, and college credits. Job Corps also offers tuition-free housing, meals, basic health care, a living allowance, and career transition assistance."

[40] JC participants differ from CCC participants in two key respects: JC includes women and married individuals, whereas the CCC excluded both.

[41] About 30% of JC enrollees complete the program, compared with 38% of the CCC. And of those who leave before completing, 30% in the JC and 22% in the CCC "deserted" while 12% and 4%, respectively, left because of employment opportunities.

duration in the JC, we also find evidence of both positive and negative selection into duration, as well as evidence that duration is random for some, just as we found in the CCC data (Figure 3).[42]

Comparing short term effects. We reproduce the short-run JC randomized evaluation results in Schochet et al. (2008) using only the sample of males (Table VII).[43] In the first column we present estimates that compare the outcomes of those assigned to treatment to those of the control. In the second column we present the implied effects of training duration by estimating the 2SLS effect of duration using the randomized treatment status as an instrument. These estimates represent the causal effect of duration under a certain set of assumptions.[44] The third and fourth columns show OLS estimates for duration for JC and for CCC, respectively. Overall, short term labor market outcomes in JC were more positive than in CCC, but in both the CCC and the JC data, education and mobility increased. The table also shows that the JC increased self-reported health, consistent with our findings that CCC improved health, measured by height and weight.[45]

Comparing longer-term effects. The estimated long-run effects on income and disability are also similar to estimates obtained from the RCT of JC. Given that enrollees participated for 0.83 years, the effect of CCC on lifetime earnings are about 4.6%. The latest evaluation of JC, which tracks individual tax records 20 years after the program (Schochet 2018), finds that participation in JC had a statistically insignificant increase in wages of 2%, with our effects (4.6) well within their 95% confidence interval [-4%; 8%]. It also reports a 40% reduction in SSDI benefits among older JC participants, though not in the overall sample.[46]

In sum, participants in JC are similar in many dimensions and they experienced qualitatively similar long-run improvements in income and health. They also experienced similar improvements in their education, health and mobility in the short run, but they differed in the

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[42] We find that education, Hispanic ethnicity, non-native speakers trained longer and individuals with a criminal history or those with shorter work histories trained for shorter periods of time (Figure 3 Panel B). As in the CCC, participants that found employment and those that deserted, were rejected or had urgent and proper calls also served shorter durations compared to those that completed their term.

[43] The results in the first column are almost identical to those in Schochet et al. (2008) except that we are restricting the sample to males, and we constructed a few new outcomes (years of education, mobility and marriage).

[44] The key assumption is that there is no direct effect of the assignment to treatment on the outcome beyond its effect on the duration of the training. Appendix Table XIII presents balance tests of baseline characteristics of the JC applicants.

[45] Results are similar if we use the entire scale or only look at whether the respondents are in excellent health alone.

[46] The JC evaluation only uses 15 years of labor market outcomes, whereas we use 35 years. The shorter length of the evaluation may lower the estimated returns.

short-term labor market outcomes. We now formally describe our method of using short-run, experimental estimates from JC to adjust long-run, observational estimates from the CCC.

B. Combining Experimental and Observational Samples: Set-up

We pursue two approaches that exploit experimental data from the JC to address potential selection in the estimates of long-run impact from the CCC. The first approach assumes comparability of short-run treatment effect of the JC and the CCC. The second assumes comparability of selection into longer duration in the JC and the CCC.

The setting for both approaches is as follows. We assume the short term (denoted ST) outcome is a linear function of the treatment and observed and unobserved covariates:

$$y_{iS}^{ST} = \tau_S^{ST} W_{iS} + X_{iS}\gamma_S^{ST} + \alpha_{iS}^{ST}$$

y_{iS}^{ST} is the short-term outcome for sample $S \in \{CCC, JC\}$; W_{iS} is duration of training in program (either CCC or JC); τ_S^{ST} is the short-term treatment effect; X_{iS} includes other controls; and α_{iS}^{ST} is the unobserved component (residual), which is possibly correlated with W_{is} in the CCC sample and is the source of endogeneity of W_{iCCC}. In JC only, we observe a binary treatment status dummy T_i, uncorrelated with α_{iS}^{ST} given the experimental nature of the data, but correlated with training duration W_{iJC}, which allows us to correct for the endogeneity using the random assignment as an instrument.

Similarly for the long-term (LT) outcomes, we have:

$$y_{iS}^{LT} = \tau_S^{LT} W_{iS} + X_{iS}\gamma_S^{LT} + \alpha_{iS}^{LT}$$

Going forward we make the following two assumptions:

Assumption 1: $\alpha_{iJC}^{ST} \perp W_{iJC} | X_i, T_i$ in JC (duration is random given treatment status and X)

Assumption 2: LT and ST residuals are linearly related as:

$$\alpha_{iCCC}^{LT} = \delta \alpha_{iCCC}^{ST} + \varepsilon_{iCCC}^{LT} \text{ and } \varepsilon_{iCCC}^{LT} \perp W_{iCCC} | X_i, \alpha_{iCCC}^{ST}$$

This is a key assumption: the sources of the endogeneity for the short-term and long-term outcomes share a common component. Therefore, if we are able to control for α_{iCCC}^{ST} in our regression of CCC duration on long-term outcomes, we generate an unbiased estimate of the long-run treatment effect.

C. First Approach: Assuming Identical Short-run Treatment Effects for JC and CCC

For our first approach, we follow Athey, Chetty, and Imbens (2020) in assuming that the experimental sample has external validity and therefore $\tau_{CCC}^{ST} = \tau_{JC}^{ST}$. This implies that the estimate of the ST treatment effect obtained from the RCT of JC is an unbiased estimate of the ST effect in the CCC. We extend their procedure to allow for endogeneity of duration in the JC sample that we address using instrumental variables.[47] Appendix A details the procedure.

After obtaining unbiased estimate of the JC's short-term effects ($\hat{\tau}_{JC}^{ST}$), which is also an unbiased estimate of CCC's short-term effects, we can calculate the short-term CCC residuals, $\hat{\alpha}_{iCCC}^{ST} = Y_{iCCC}^{ST} - \hat{\gamma} X_{iCCC} - \hat{\tau}_{JC}^{ST} W_{iCCC}$. We then include the residuals as control functions in the long-term regressions. If 1) there is equality of the short-term causal effects in JC and CCC, and 2) the residual unobserved component in the long-term regression after controlling for the short-term residual in CCC is uncorrelated with duration, then the coefficient on duration estimated including the control functions gives us the LT causal effect of interest. For robustness, we also try varying short-run residuals we include that we can calculate from multiple short-run outcomes.

The results from this exercise are in Table VIII for our long-run outcomes of longevity, AIME, retirement age, and SSDI claiming with control function generated using education as the short-run outcome. For all samples, we compare the OLS estimate without including any control function (Panel A) on a consistent sample with the estimate including control functions generated from short-run regressions in this approach (Panel B).

The LT estimates of the effect of duration on longevity are unaffected by the inclusion of control functions for all samples and specifications, suggesting the bias in the OLS estimates is small (Table VIII Panels A and B). This can occur because a) the LT unobservables are uncorrelated with duration, indicating little endogeneity; b) the LT unobservables are uncorrelated with LT outcomes, or c) the ST and LT unobservables are different and the controls do not capture the endogeneity. Note that this is not a result of the small treatment effects estimated in the JC trial – the bias correction is always based on the difference between JC RCT

[47] We extend the methods in that paper to account for the differences in settings. In the original Athey et al. (2020) paper the treatment is binary, whereas in our case it is continuous. Secondly the experiment does not directly yield estimates of the effect of the continuous duration variable, so we employ 2SLS methods instead to obtain causal estimates. As a result, we need to make additional assumptions. See Appendix A for details.

and the CCC, not on the treatment effect in JC alone.[48] The results are similar if we use other ST outcomes to compute the control functions or include different set of controls.[49]

While encouraging, these results rely on an assumption that ST treatment effects in JC would be identical in the CCC, after adjusting for some covariates. This assumption seems reasonable for education or geographic mobility but perhaps unreasonable for labor market outcomes. The returns to training, like the returns to other human capital investments, are also likely to depend on post-investment market conditions. There is evidence that the returns to schooling are stochastic and vary considerably over time (Goldin and Katz, 2008 and Rosenzweig and Udry, 2020). The post-WWII economy was better for low-skilled labor than the economy of the early 2000s, which had stagnant wages for low-income groups (Piketty, Saez and Zucman 2018) and low-skill laborers (Autor, Katz and Kearney 2008). This suggests that the assumption of constant treatment effects over time and place might be too restrictive.

D. Second Approach: Assuming Equivalent Selection into JC and CCC

For our second control function approach, we allow for the possibility that the short-term treatment effects are *not* the same in JC and CCC, but instead we assume that the selection bias arising from endogeneity of the duration of the training is the same in JC and CCC. For this approach, we estimate the ST treatment effect in the JC using both OLS and IV techniques, using random assignment to treatment as the instrument for duration. The difference in these two estimates, $\hat{\mu} = \hat{\tau}^{ST}_{JC,OLS} - \hat{\tau}^{ST}_{JC,2SLS}$, represents the omitted variable or selection bias in observational estimates of job training on short-run outcomes. For example, the 2SLS estimate of the effect of duration on education in JC is 0.393, whereas the OLS estimate of duration in the treated arm of JC is 0.360 (Table VII). Any difference between the two estimates is indicative of selection bias. Under the assumption that the selection bias is similar in CCC, the difference between the OLS and the 2SLS estimates in JC can be used to construct an estimate of the

[48] The bias correction is based additionally on the relationship of two different sets of outcomes (ST and LT) in CCC, not on the treatment effect in JC alone. As long as ST and LT selection are related within CCC, the process should correct any bias of our estimate. If the ST TE is 0 in the RCT then the control function is the ST outcome, which potentially still generates a change in the estimated coefficients.

[49] In Appendix Table XIV we show results with control functions calculated using different short-run outcomes, including mobility, short-run labor market outcomes, and including all control functions simultaneously. The result that including control functions do not change the OLS result *keeping consistent the sample of enrollees* does not change. The sample size varies significantly across the inclusion of residuals due to observability of short-run outcomes varying.

unbiased short-run treatment effects, which in turn is used to construct a control function for the LT regressions.

Using the JC estimates of selection bias ($\hat{\mu}$), we adjust our OLS estimate of CCC short-term outcomes, $\hat{\tau}_{CCC}^{ST} = \hat{\tau}_{CCC,OLS}^{ST} - \hat{\mu}$. We then use this adjusted estimate of the treatment effect to calculate the control functions as before. That is, we use this adjusted estimate of the short run treatment effect of the CCC to calculate a residual in regressions of short run outcomes. We use this residual to construct the control function. See Appendix A for the detailed procedure.

The results from this exercise are presented in Table VIII Panel C, using the control function calculated from our short-run education outcome.[50] Again, we find that regardless of the control functions that we include the estimates are stable and not different from the original OLS estimates, suggesting little bias in our LT estimates of the effects of training.

To assess whether the assumption of equivalent selection on unobservables is warranted, we compare selection into duration on observables in the CCC and the JC. Examining individual observable characteristics common across both settings, we conclude that there are important similarities across the two. Specifically, we find that: 1) Individuals who have never worked are more likely to serve for longer in the JC and the CCC, 2) Hispanic enrollees also serve for longer in both the JC and the CCC and 3) individuals with more schooling are more likely to serve for longer in both programs (See Figure 3).

E. Quantifying Violations of Assumptions in Both Approaches

Finally, we quantify how violations of the different assumptions in each approach changes the estimated coefficient of the long-run effect. Essentially, both our approaches rely on obtaining an unbiased estimate of the short-run treatment effect in the CCC. In both cases, define $\phi = (\hat{\tau}_{CCC}^{ST} - \tau_{CCC}^{ST})$, the difference between our "recovered" short-run estimate and the true treatment effect. Any $\phi \neq 0$ leads to a bias of $-\delta * \phi$ in the estimate of the long-run treatment effect with control functions generated from our approaches. In case of the first approach, ϕ is the difference between JC and CCC short-run treatment effect. In the second approach, ϕ stems from the difference in the estimate of the selection bias in the short-run regression for JC and CCC. In

[50] Appendix Table XV presents results using other control functions, as in Appendix Table XIV. Again, results change very little by including the control functions while keeping sample consistent, though the main effects are different and more imprecisely estimated in small samples.

Appendix A, we fully quantify 1) in the first approach, how the bias relates to percentage difference in the treatment effects between JC and CCC, or 2) in the second approach, how it relates to percentage difference in the correlation between duration and the unobserved component in the short-run regression. This allows us to examine how much our estimate of the long-run effect will differ if we were to assume various degrees of violation in our key assumptions.

We present the "bias" terms as the last row of each panel as symmetric bounds around the estimate. These terms can be multiplied by a desired percentage differences in either 1) the JC and CCC short-run treatment effect for the first approach, or 2) the correlation between duration and unobserved component in the second approach. We see that because the bias terms are extremely small, only very large differences in either the short-run treatment effects between JC and CCC or selection bias between JC and CCC would make a meaningful difference in the estimates. We take this as additional evidence that our long-run estimates are robust to different kinds of adjustments for omitted variable bias.

Implications for current JC participants. The only way to infer long term benefits for modern job training programs such as JC is to rely on historical data. However, historical data, by definition, will have been collected in a different context, raising concerns about comparability. There are many reasons to believe the lifetime estimates from the CCC apply to JC participants. The first is the fact that both the participants and the programs share important similarities. The second is that both show positive and statistically significant effects on education, income, geographic mobility, and health. If we assume that mortality going forward is determined by the same factors which have historically determined mortality (if we assume the production function for longevity has remained stable over time), then we should observe increases in longevity for current JC participants. While the short run labor market effects differ for the JC and the CCC, that is likely because the labor markets in the 1940s and the 1990s were very different. In contrast, if one were to look over a participant's lifetime, differences in labor markets would decline (with both CCC and JC participants experiencing strong and weak labor markets over their lifetimes). We conclude that the common effects on education, income, health and mobility (some of the most important predictors of lifetime income and health) across the two programs suggest that the lifetime benefits estimated in the CCC are likely to generally hold for JC participants as well.[51]

IX. Discussion

Was the program worth it? To answer this question, we calculate the Marginal Value of Public Funds (MVPF) following the approach by Hendren and Sprung-Keyser (2019). The CCC program costs include: 1) upfront cost of the program and 2) increases in social security payouts from enrollees both living longer and having increased PIA. These costs are mitigated by 1) tax increases from earnings benefits of the program, 2) decrease in SSDI claiming, 3) decrease in either SSDI length or retirement length from increase in claiming age. The program benefits include: 1) willingness to pay (WTP) for life extensions, 2) increase in after-tax earnings, 3) $30 per month wage paid (most of which went to families), and 4) the value of other services received by enrollees during the program, such as room and board. These benefits are decreased by the loss of SSDI income from decrease in claiming rate. The MVPF is estimated to be 6.0 including the WTP for life increases, disability reductions, and increases in claiming ages, but only 2.5 if we only count the earnings effects, with reductions in mortality accounting for most of the difference.[52] Thus our conclusions differ from those in Hendren and Sprung-Keyser (2019) who find that the MVFP is below one for JC participants. [53] The key difference is due to our ability to look at *lifetime* effects on *multiple* outcomes, and particularly health and longevity. Moreover, our MVPF likely misses additional benefits. The program likely benefitted not just enrollees, but also their families, and the communities and the landscape where the CCC operated. Future work should attempt to measure these effects to conduct a more extensive cost-benefit evaluation.

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[51] The literature on the determinants of mortality shows that health, education, income and residential location are important determinants of mortality. Height and normal BMI are both associated with longevity (Fogel 1994). Education (Cutler et al. 2006) and greater lifetime earnings are also associated with lower mortality (Chetty et al. 2016). Finkelstein et al. (2019) and Deryugina and Molitor (2019) show that individuals who move to low-mortality locations experience subsequent lower mortality themselves.

[52] Assumptions made and details of calculation are presented in the Online Supplementary Appendix. Some of the increases in life expectancy could lead to greater government spending through Medicare, potentially lowering the marginal value of public funds (Hendren and Sprung-Keyser 2019)—we ignore these. Families received transfers, which could have benefitted them but also potentially distorted their behaviors. We do not have estimates of these effects. We also do not assess the general equilibrium effects of job training programs. Recent research suggests these effects could be substantial and possibly offset the benefits to individuals (Crepon et al. 2013).

[53] They compute an MVFP of 0.18 for JC. This computation does not incorporate the lower SSDI claims or the potential life extensions we compute here.

While we believe that improved health in early adulthood is a likely mediator, historical accounts suggest that the program may have positively affected other "soft skills," improved mental health, and enlarged social networks. For example, enrollees reported making many life-long friendships and experiencing improvements in their state of mind. Additionally, the Army ran the CCC camps and imposed rules of behavior that were likely unusual for most individuals and may have been beneficial. Criminality is an important outcome which may have been affected as well. Though we do not observe these outcomes directly, we do observe that the CCC increased the probability that young men served in the Army, consistent with a change in either discipline or attitudes towards national service. Future work should assess these claims.

Our results have important implications for evaluations of job training programs. The majority of evaluations focus on labor market outcomes in the short- to medium-term and find small and/or insignificant effects. We confirm these findings in our data. But we observe large changes in lifetime incomes and in outcomes that are not usually studied, namely health, military service, and geographic mobility. These findings suggest that it is essential to evaluate multiple mechanisms and indicators of well-being when assessing the impacts of various interventions in the short and the long term.

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Appendix A Control Function Approach

In this section we explore the control function approach in detail, beginning with the original approach in Athey, Chetty, and Imbens (2020) then discussing our extension.

A.1 Athey Chetty Imbens (2020)

In Athey Chetty Imbens (2020) (henceforth ACI) the set-up is an experimental sample with only the secondary (short-term) outcome and observational sample with both the secondary and primary (long-term) outcomes. The question they address is how the experimental sample can be used to obtain the treatment effect on the long-term outcome that is observed only in the observational sample.

ACI has four assumptions that allows us to recover τ_O^P, reproduced here:

Assumption 1. (External Validity of the Observational Study) *The observational sample is a random sample of the population of interest.*

This assumption exists to set the baseline of the analysis to the observational sample, and is essentially definitional.

Assumption 2. (Internal Validity of the Experimental Sample) *For $w = 0, 1$,*

$$W_i \perp\!\!\!\perp \left(Y_i^P(w), Y_i^S(w)\right) | X_i, G_i = E \tag{A1}$$

This assumption allows us to estimate treatment effects in the experimental sample without bias. **Assumption 3.** (Conditional External Validity) *The experimental study has conditional external validity if*

$$G_i \perp\!\!\!\perp \left(Y_i^P(0), Y_i^P(1), Y_i^S(0), Y_i^S(1)\right) | X_i \tag{A2}$$

Assumption 3 implies that the conditional average treatment effect in both samples is the same as $E[Y_i^S(1) - Y_i^S(0)|X_i, G_i = O] = E[Y_i^S(1) - Y_i^S(0)|X_i, G_i = E]$. Assumption 3 also implies that $\tau_O^S = \tau_E^S$ and $\sigma_O^S = \sigma_E^S$.

Finally, the last assumption relates the secondary (short-term) outcomes to primary (long-term) outcomes:

Assumption 5. (Latent Unconfoundedness) *For w = 0,1,*

$$W_i \perp\!\!\!\perp Y_i^P(w) | X_i, Y_i^S(w), G_i = O \tag{A3}$$

This allows ACI to identify τ_O^P by inferring the bias in the observational sample from the estimated treatment effects on the secondary outcome in the two samples, and transfer that to the primary (long term) outcome.

A.2 ACI Linear Setting and Our Approach

In ACI linear setting, the short-term outcomes have the following formulation,

$$\begin{aligned} Y_i(0) &= X_i^T \gamma + \alpha_i \\ Y_i(1) &= Y_i(0) + \tau_g \\ Y_i &= \tau_g W_i + X_i^T \gamma + \alpha_i \end{aligned}$$

Furthermore, they assume a stronger version of A5,

Assumption 5' Linear Latent Unconfoundedness

$$\begin{aligned} \alpha_i^P &= \delta \alpha_i^S + \varepsilon_i^P \\ W_i &\perp\!\!\!\perp \varepsilon_i^P | X_i, \alpha_i^S, G_i = O \end{aligned}$$

In our approach, we differ with ACI's linear setting in two ways. First, we use continuous treatment, which makes Assumption 5 into a stronger one. Second, instead of the ACI Assumption 3 that the experimental sample is externally valid for the observational sample, we consider two different approaches: first, we assume that the short-term treatment effect are the same between the two samples, and second, we assume that the short-term bias is the same and utilize the instrument in the JC sample. In the most favorable case both lead to the same results because the observational study has internal validity from the outset.

Our first approach takes the assumption that the short-term treatment effect between CCC and JC samples are the same, or in our notation, $\tau_E^S = \tau_O^S$. Using the IV approach in the JC sample, we can obtain an unbiased estimate of τ_E^S, which in turn gives us an unbiased estimate of τ_O^S. Finally, we can construct the control function as in ACI

$$\hat{\alpha}_i^S = Y_i^S - W_i \hat{\tau}_O^S - X_i^T \hat{\gamma}^S \tag{A4}$$

and include the control function in the long-term regression of the observational sample.

Our second approach assumes that the (linear) selection bias is the same between CCC and JC. In this approach, we exploit the fact that we have an instrument for duration in the JC sample. Therefore, we can think of the difference between the IV estimate and the OLS estimate gives us an estimate of the bias in the JC sample,

$$\hat{\sigma} = \hat{\tau}_{E,OLS}^S - \hat{\tau}_{E,2SLS}^S \tag{A5}$$

Then, adjusting the OLS estimate of the short-term treatment effect from the CCC sample $\hat{\tau}_O^S - \hat{\mu}$ gives us an unbiased estimate of the short-term treatment effect of the CCC sample. Finally, we construct the control function as before and include in the long-term regression of the observational sample.

We present a complete step-by-step description here. To make notations easier to interpret in the description of the approaches, we replace the experimental sample subscript E by JC for Jobs Corps and observational sample subscript O by CCC for CCC. Additionally, we replace secondary outcome sample superscript S by ST for short-term and the primary outcome sample superscript P by LT for long-term.

Approach 1: Assuming treatment effect is the same

1. Using the (experimental) JC data we estimate the short-term treatment effects for outcomes available in both the CCC and JC data. These include schooling, employment, earnings and geographic mobility. Using the JC sample, we instrument for the duration W using the random assignment T. This procedure gives us an unbiased estimate of the short-run treatment effect in the JC, as well as in the CCC (by assumption).

2. Estimate the residual in the CCC data using the estimated ST treatment effect from the JC RCT ($\hat{\tau}_{JC}^{ST}$)

$$\hat{\alpha}_{iCCC}^{ST} = Y_{iCCC}^{ST} - \hat{\gamma} X_{iCCC} - \hat{\tau}_{JC}^{ST} W_{iCCC} \tag{A6}$$

3. Include the ST residuals calculated in step 2 ($\hat{\alpha}_{iCCC}^{ST}$) as controls in the LT CCC regressions:

$$Y_{iCCC}^{LT} = X_{iCCC}\gamma_{CCC}^{LT} + \tau_{CCC}^{LT} W_{iCCC} + \delta\alpha_{iCCC}^{ST} + \varepsilon_{iCCC}^{ST} \tag{A7}$$

Approach 2: Assuming selection bias is the same

1. Estimate ST treatment effect from RCT using both OLS and 2SLS. We use random assignment to treatment as instrument for duration to construct the 2SLS estimates. We construct the OLS estimates using the treated arm of the experiment only.

2. Estimate the selection or omitted variable bias term ($\hat{\mu}$) by subtracting JC's 2SLS estimate from JC's OLS estimate of ST treatment

$$\hat{\mu} = \hat{\tau}_{JC,OLS}^{ST} - \hat{\tau}_{JC,2SLS}^{ST} \tag{A8}$$

3. Estimate ST treatment effect in the CCC sample and adjust it by the estimated selection or OVB term ($\hat{\mu}$)

$$\hat{\tau}_{CCC}^{ST} = \hat{\tau}_{CCC,OLS}^{ST} - \hat{\mu} \tag{A9}$$

4. Estimate the residual of the ST treatment effect using our adjusted estimate of the

short-term treatment effect ($\hat{\tau}^{ST}_{CCC}$)

$$\hat{\alpha}^{ST}_{iCCC} = Y^{ST}_{iCCC} - \hat{\gamma} X_{iCCC} - \hat{\tau}^{ST}_{CCC} W_{iCCC} \tag{A10}$$

5. Include estimated residual in LT treatment effect regression in order to generate an unbiased estimate of the long-term impact of the CCC on outcomes.

$$Y^{LT}_{iCCC} = X_{iCCC} \gamma^{LT}_{CCC} + \tau^{LT}_{CCC} W_{iCCC} + \delta \alpha^{ST}_{iCCC} + \varepsilon^{ST}_{iCCC} \tag{A11}$$

A.3 Quantifying the Effect of Violations of Assumptions

In each of our two approaches, we make an assumption that allows us to recover τ^S_O without bias in large samples. In the first approach, we assume that $\tau^S_{CCC} = \tau^S_{JC}$, and in the second approach, we assume that $\sigma^S_{CCC} = \sigma^S_{JC}$.

In practice it is plausible that neither assumption holds exactly. So, let us suppose that both these assumptions are violated, and we estimate short-term TE in the observational sample with bias. let the bias be denoted by so $\phi = \hat{\tau}^S_O - \tau^S_O$. In our first approach, ϕ is the difference between JC and CCC short-term treatment effects, In our second approach, ϕ is the difference in the short-term bias between JC and CCC. We can characterize the biases for the two approaches. In general if the short term effects are similar, even if not identical, the first approach is preferable, whereas if the biases are similar, but not identical, the second approach is preferable.

Then,

$$\begin{aligned}
\hat{\alpha}^S_i &= Y^S_i - W_i \hat{\tau}^S_O - X^T_i \hat{\gamma}^S \\
&= \alpha^S_i - W_i * \phi \\
\hat{\alpha}^P_i &= \alpha^P_i - (\delta * \phi) W_i
\end{aligned}$$

and so regressing primary outcomes on duration, X, and control function will be mis-specified

$$Y_i^P = (\tau_P - \delta * \phi)W_i + X_i^T\gamma + \delta\alpha_i^S + \varepsilon_i^P \tag{A12}$$

which yields a final bias of $-\delta * \phi$.

In our first approach, where we assume that short-term treatment effects are identical, ϕ term is the difference between JC and CCC short-term treatment effects, so $bias = -\delta * (\tau_{CCC}^S - \tau_{JC}^S)$. Expressing this in terms of percentage difference in short-term treatment effects,

$$bias_1 = -\delta * \tau_{JC}^S * \%\Delta\tau^S$$

where $\%\Delta\tau^S = \frac{\tau_{CCC}^S - \tau_{JC}^S}{\tau_{JC}^S}$.

In our second approach, ϕ is the difference in the short-term bias between JC and CCC, so the bias is $bias = -\delta * (\Delta(\text{short-term bias}))$ or

$$bias_2 = -\delta * \left(\Delta\frac{1}{sd(W_i)^S} * \beta_2^S corr(W_i, U_i)_{JC}^S + \frac{1}{sd(W_i)_{CCC}^S} * \Delta[\beta_2^S corr(W_i, U_i)^S]\right)$$

where $\beta_2^S corr(W_i, U_i)_{JC}^S$ is a component of the omitted variable bias in short-run regression of JC and $\Delta[\beta_2^S corr(W_i, U_i)^S]$ is the difference in the components between CCC and JC.[1] Everything except $\Delta[\beta_2^S corr(W_i, U_i)^S]$ is observed.

So after the estimate of the long-term treatment effect is first adjusted by, $-\delta * \Delta\frac{1}{sd(W_i)^S} * \beta_2^S corr(W_i, U_i)_{JC}^S$, the remaining bias for the long term effect, expressed in terms of percentage difference of the short-term bias term is,

$$-\delta * \frac{\beta_2^S corr(W_i, U_i)_{JC}^S}{sd(W_i)_{CCC}^S} * \%\Delta[\beta_2^S corr(W_i, U_i)^S]$$

[1]In a regression setting $Y_i = \beta_0 + \beta_1 W_i + \beta_2 U_i + \eta_i$, the omitted variable bias when U_i is omitted can be expressed as $\beta_2 corr(W_i, U_i)\frac{sd(U_i)}{sd(W_i)}$

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Table I
Summary Statistics

	Analytic Sample			Mortality Sample			Analytic Sample (matched to MBR)		
	N	mean	sd	N	mean	sd	N	mean	sd
Characteristics in Enrollment Application									
Birth year	23,722	1,920	3.712	17,639	1,920	3.649	12,455	1920	3.546
Age at enrollment	23,488	18.75	2.122	17,449	18.73	2.170	12,330	18.74	2.242
Enrollment year	23,722	1,939	1.902	17,639	1,939	1.894	12,455	1939	1.889
Reported age younger than DMF*	23,722	0.0888	0.284	17,639	0.113	0.317	12,455	0.130	0.336
Reported age older than DMF*	23,722	0.167	0.373	17,639	0.219	0.413	12,455	0.253	0.435
Allottee is father	23,722	0.334	0.472	17,639	0.332	0.471	12,455	0.330	0.470
Allottee is mother	23,722	0.466	0.499	17,639	0.475	0.499	12,455	0.475	0.499
Hispanic (imputed using hispanic index)	23,722	0.484	0.500	17,639	0.451	0.498	12,455	0.432	0.495
Additional information in CO records									
Highest grade completed	14,507	8.592	2.109	11,235	8.674	2.081	8,225	8.700	2.055
Household size excluding applicant	7,870	4.745	2.600	6,283	4.763	2.591	4,730	4.725	2.575
Live on farm?	8,101	0.248	0.432	6,460	0.253	0.435	4,846	0.252	0.434
Height (Inches)	8,141	67.80	3.089	6,475	67.88	3.083	4,860	67.92	3.053
Weight (100 pounds)	8,234	1.385	0.171	6,561	1.390	0.172	4,922	1.391	0.171
Body Mass Index	8,115	21.21	2.178	6,461	21.23	2.174	4,849	21.23	2.190
Underweight	8,115	0.0694	0.254	6,461	0.0689	0.253	4,849	0.0685	0.253
Father Living	7,943	0.799	0.401	6,339	0.803	0.398	4,765	0.806	0.396
Mother Living	8,006	0.850	0.357	6,391	0.855	0.352	4,808	0.855	0.352
Service Characteristics									
First allottee amount (dollars per month)	22,970	21.63	3.772	17,088	21.67	3.721	12,097	21.70	3.683
Duration of service (yrs)	23,722	0.821	0.706	17,639	0.826	0.708	12,455	0.816	0.701
Ever Rejected?	23,722	0.0194	0.138	17,639	0.0201	0.140	12,455	0.0199	0.140
Camp Characteristics									
Distance from home to camp in miles (derived)	22,405	154.8	207.1	16,645	157.2	208.0	11,740	159.5	209.1
1st closest city distance form camp (miles)	23,480	26.68	22.50	17,454	26.57	22.26	12,322	26.40	22.06
Mean precipitation in camp 1933-1942	23,202	33.43	9.281	17,253	33.52	9.321	12,174	33.66	9.382
Mean min temp in camp 1933-1942	23,202	1.459	3.474	17,253	1.382	3.457	12,174	1.265	3.450
Mean max temp in camp 1933-1942	23,202	17.51	4.114	17,253	17.39	4.108	12,174	17.24	4.106
Death Certificate Data									
Age at death	19,377	69.82	16.84	17,639	73.62	12.03	12,348	74.76	9.25
=1 if missing age at death	23,722	0.183	0.387	17,639	0	0	12,455	0.009	0.092
Survive at 70	19,377	0.587	0.492	17,639	0.644	0.479	12,348	0.706	0.456
P(70), imputed to 0 if missing	23,722	0.479	0.500	17,639	0.644	0.479	12,455	0.700	0.458
Imputed Prob of Survival at 70 Using Age at Discharge	23,718	0.589	0.446	17,636	0.644	0.479	12,455	0.705	0.454
1940 Census Data									
Matched to 1940 Census	23,722	0.449	0.497	17,639	0.479	0.500	12,455	0.487	0.500
For those that served before 1940									
Year of birth	4,217	1,918	3.836	3,410	1,918	3.803	2,451	1918	3.559
Age at last birthday (in years)	4,217	21.77	3.836	3,410	21.75	3.803	2,451	21.74	3.559
Hispanic	4,217	0.279	0.449	3,410	0.258	0.438	2,451	0.245	0.430
White	4,217	0.991	0.0933	3,410	0.992	0.0903	2,451	0.991	0.092
In labor force	4,217	0.909	0.288	3,410	0.912	0.283	2,451	0.909	0.288
Working, conditional on labor force	3,833	0.711	0.453	3,110	0.718	0.450	2,228	0.711	0.453
Wage, conditional on working	2,983	405.3	361.0	2,424	401.8	337.4	1,764	410.8	360.7
Years of educ	4,159	8.770	2.477	3,363	8.842	2.445	2,415	8.873	2.420
Moved Residence Counties	4,215	0.299	0.458	3,408	0.291	0.454	2,450	0.296	0.457
WWII Records									
Matched to WWII records	23,722	0.306	0.461	17,639	0.338	0.473	12,455	0.347	0.476
Birth year	7,263	1,920	2.810	5,954	1,920	2.831	4,321	1920	2.815
Enrollment year	7,262	1,942	1.424	5,954	1,942	1.439	4,321	1942	1.45
Years of education	7,263	9.395	1.787	5,954	9.404	1.785	4,321	9.399	1.766
Height in inches**	5,971	67.52	6.089	4,876	67.70	6.098	3,510	67.73	6.164
Weight in lbs***	5,641	138.6	26.19	4,595	138.7	25.70	3,327	139.4	27.17
BMI	5,466	21.55	4.500	4,451	21.50	4.101	3,214	21.55	4.399
Ever Married	7,256	0.215	0.411	5,947	0.221	0.415	4,316	0.224	0.417
Moved Residence Counties	7,215	0.303	0.460	5,914	0.296	0.457	4,290	0.303	0.46
Birthplace Rest of US	7,215	0.230	0.421	5,913	0.237	0.425	4,295	0.244	0.429

Notes: Basic sample includes records with duration (begin and end date of enrollment), camp id and enrollment county. The analytical sample for the mortality analysis only includes those not missing death age and death age more than 45. When multiple records were found for a single individual we use the information in the first enrollment record. * Reported age being younger (older) than DMF OR than the oldest (youngest) reported if the individual has multiple enrollment spells. ** Dropped values below 40. *** Dropped values below 90 and over 350

Table II

Effect of Service Duration on Longevity and Lifetime Earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Depdent variable	No Controls	Add Birth, County-qtr Dummies	Add Indiv Controls	Add Camp Chars	Add Peer Chars	Add Camp FE	CO Only
Panel A: Longevity for the full sample (log death age)							
Duration of service (yrs)	0.013***	0.013***	0.011***	0.011***	0.013***	0.013***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Observations	17,086	17,086	17,086	17,086	17,086	17,086	10,944
R-squared	0.003	0.117	0.126	0.127	0.128	0.138	0.149
Mean Dep	73.62	73.62	73.62	73.62	73.62	73.62	73.30
Panel B: AIME (MBR sample claimed 1979 and later)							
Duration of service (yrs)	-0.083	67.178***	62.791***	62.450***	56.717***	50.134***	48.707***
	(9.563)	(12.565)	(12.840)	(12.889)	(14.378)	(15.555)	(18.481)
Observations	10,241	10,241	10,241	10,241	10,241	10,241	6,525
R-squared	0.000	0.188	0.204	0.205	0.206	0.222	0.236
Mean Dep	963.62	963.62	963.62	963.62	963.62	963.62	1010.70
Panel C: Retirement age							
Duration of service (yrs)	0.506***	0.507***	0.452***	0.462***	0.427***	0.401***	0.554***
	(0.069)	(0.092)	(0.094)	(0.095)	(0.105)	(0.114)	(0.127)
Observations	11,712	11,712	11,712	11,712	11,712	11,712	7,768
R-squared	0.005	0.157	0.167	0.168	0.169	0.184	0.192
Mean Dep	60.27	60.27	60.27	60.27	60.27	60.27	60.43
Panel D: SSDI (excluding unknowns)							
Duration of service (yrs)	-0.016**	-0.022***	-0.020**	-0.021**	-0.017*	-0.021**	-0.031***
	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.012)
Observations	10145	10145	10145	10145	10145	10145	6480
R-squared	0.001	0.154	0.161	0.163	0.164	0.181	0.205
Mean Dep	0.21	0.21	0.21	0.21	0.21	0.21	0.20

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample is restricted only to those that died after age >= 45. Column (1) includes only duration of service as regressor. Column (2) adds Birth and County-Year-Quarter of Enrollment fixed effects. Column (3) adds individual controls. Column (4) adds camp characteristics, such as distance from nearest city and average temperature. Column (5) adds peer characteristics, where peers are defined as other enrollees serving in the same camp at the same time. Column (6) adds camp fixed effects and removes camp characteristics. Column (7) runs the regression specification in Column (6) for only enrollees from our Colorado Records. For complete list of controls, refer to text or Appendix Table IV.

Table III

Effect of Service Duration on Missing Data and Sample Selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	No Controls	Add Birth, County-qtr Dummies	Add Indiv Controls	Add Camp Chars	Add Peer Chars	Add Camp FE	CO Only
Panel A: Does duration predict whether longevity is missing?							
Duration of service (yrs)	0.001	-0.017***	-0.020***	-0.020***	-0.017***	-0.015***	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	22,964	22,964	22,964	22,964	22,964	22,964	14,116
R-squared	0.000	0.111	0.196	0.197	0.198	0.206	0.200
Mean Dep	0.18	0.18	0.18	0.18	0.18	0.18	0.15
Panel B: Does duration predict being in the MBR sample?							
Duration of service (yrs)	-0.006	0.004***	0.010*	0.011*	0.009	0.005	0.002
	(0.005)	(0.001)	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Observations	22,980	22,980	22,980	22,980	22,980	22,980	14,116
R-squared	0.000	0.102	0.205	0.206	0.206	0.212	0.187
Mean Dep	0.53	0.53	0.53	0.53	0.53	0.53	0.57
Panel C: Is the effect of duration on longevity for the MBR sample the same as in the full sample?							
Duration of service (yrs)	0.013***	0.010***	0.009***	0.009***	0.012***	0.011***	0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Observations	11,953	11,953	11,953	11,953	11,953	11,953	7,913
R-squared	0.005	0.157	0.169	0.169	0.170	0.185	0.190
Mean Dep	74.81	74.81	74.81	74.81	74.81	74.81	74.78

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. See Notes on Table 2 for specifications in each column. Panel A explores the outcome of = 1 if death age is missing, = 0 otherwise. Panel B explores the outcome of = 1 if in the MBR sample, = 0 otherwise. Panel C explores the outcome log death age (same as Table 2 Panel A), but only for the sample of individuals found in the MBR sample.

Table IV

Effect of Service Duration on Labor Market Outcomes Observed in the 1940 Census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Data Source:	1940 census outcomes (individuals enrolling pre 1940)					WWII enlistment records			combined
Outcome	Found in Census Records	In Labor Force	Weeks Worked in 1939^	Total Annual Wage in 1939^	Ln Total Annual Wage | Working^	Found in WWII Records: served	Height (inches)	BMI	Education (yrs)
Duration of service (yrs)	0.012	0.019*	0.265	-14.497	-0.014	0.038***	1.143***	1.018***	0.169***
	(0.012)	(0.010)	(1.199)	(26.389)	(0.062)	(0.007)	(0.221)	(0.204)	(0.040)
Observations	9,518	4,052	2,360	2,148	1,749	22,963	5,770	5,287	9,586
Mean Dep	0.43	0.91	27.88	383.71	471.25	0.31	67.55	21.53	9.23

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. This table only displays specification in Column (6) of Table 2 on different outcomes observed in the 1940 Census. Sample are enrollees who serve before 1940 and can be matched to a 1940 Census Record. For results on all specifications, refer to Appendix Tables IX and X.

Table V

Effect of Service Duration on Geographic Mobility Over the Lifetime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Short term mobility (place in 1940 census or WWII enslitment differs from place of residence at enrollment in CCC)				Long term mobility (place of death differs from place of enrollment in CCC)		
	Moved to a Different State	Moved to a Different County	New County Has Higher Yearly Wage Than Sending County	New County Has Above Median Mortality Rate (1950-1968)	Died in a Different State	Died in a Different County	New County Has Above Median Mortality Rate (1950-1968)
Duration of service (yrs)	0.026***	0.057***	0.077**	-0.065***	-0.029*	0.005	0.006
	(0.007)	(0.011)	(0.034)	(0.024)	(0.015)	(0.011)	(0.015)
Observations	9,568	9,568	1,452	3,003	7,235	7,231	5,313
Mean Dep	0.09	0.33	0.59	0.38	0.5	0.8	0.25

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. This table only displays specification in Column (6) of Table 2 on different outcomes observed in the 1940 Census. Sample are enrollees who serve before 1940 and can be matched to a 1940 Census Record. For results on all specifications, refer to Appendix Table X.

Table VI

Characteristics of Eligible Job Corps Applicants and Comparison to CCC

Characteristic	Job Corps Data: All Applicants	Job Corps Data: Males only	CCC: Males Only
Baseline Characteristics			
Duration (in years, only positive durations)	0.67	0.652	0.819
Male	0.6	1	1
Age at application	18.8	18.728	18.75
White, non-Hispanic	0.3	0.304	NA
Black, non-Hispanic	0.5	0.451	NA
Hispanic	0.2	0.169	0.484
Other	0.1	0.076	NA
Years of education	10.2	10.042	8.581
High school diploma or more (including GED)	0.2	0.19	0.12
Ever arrested	0.3	0.332	NA
Had a job in the past year	0.6	0.662	NA
Ever had job	0.8	0.808	0.375
Average earnings in the past year (dollars)	2974.9	3255.739	NA
Mean for outcomes			
Duration for treated (years, duration > 0)	0.67	0.652	0.826
Duration for treated (years)	0.483	0.487	0.819
Years of school	11.145	11.07	9.403
Employment (in week of the survey)^	0.606	0.631	0.71
Weeks worked in previous year	30.62	32.17	27.88
Total ann. earnings in prev. yr	10538.31	11947.78	382.43
Total ann. earnings in prev. yr (weeks worked > 0)	12990.85	14471.77	466.69
Moved^^	0.198	0.207	0.34
Self-reported health status in 12 months^^^	1.786	1.733	NA
Self-reported health status in 48 months^^^	1.809	1.757	NA
Self-reported health excellent or good (12-month)*	0.838	0.855	NA
Self-reported health excellent or good (48-month)*	0.828	0.842	NA
Reason ended: End of term	0.31	0.302	0.378
Reason ended: Employment	0.042	0.038	0.116
Reason ended: Convenience of the government	0.001	0	0.145
Reason ended: Urgent and Proper Call	0.09	0.056	0.116
Reason ended: Deserted	0.331	0.373	0.223
Reason ended: Rejected upon examination	0	0	0.0101
Reason ended: No Record	0.228	0.232	0.0127
Observations: Baseline	14327	8646	NA
Observations: Outcomes	11313	6528	NA

Source: Jobs Corps Baseline data. ^employment is not conditional on labor force participation. ^^for Job Corps it is defined as living more than 20 miles away from baseline residence. For CCC it is defined as living in a different county than the county of residence at the time of enrollment. For Job Corps, employment is defined as having a job during the 208th week after the baseline survey (four years). ^^^Self-reported health status with 1 = excellent health, 2 = good, 3 = fair, and 4 = poor health. *Constructed variable that is equal to 1 if self-reported health status is 1 or 2 (excellent health or good health).

Table VII
Comparison to Job Corps

	Jobs Corps Data			CCC
	RCT		OLS	OLS
	Coefficient on Treatment Dummy (ITT)	2SLS Instrument Duration with Treatment	Coefficient on Duration (years)+	Coefficient on Duration (years)
Years of school	0.184***	0.393	0.360***	0.169***
	(0.039)	(0.084)	(0.041)	(0.040)
Observations	6,280	6,280	3,407	9,620
Employment (in week of the survey)^	0.026**	0.056	0.060***	-0.015
	(0.013)	(0.027)	(0.015)	(0.022)
Observations	6,022	6,022	3,285	3,684
Weeks worked in previous year	1.615***	3.443	2.629***	0.265
	(0.536)	(1.142)	(0.610)	(1.199)
Observations	6,235	6,235	3,382	2,360
Total Annual Earnings in previous year	969.765***	2,083.466	1,055.435***	-14.497
	(280.804)	(603.598)	(336.311)	(26.389)
Observations	6,081	6,081	3,317	2,148
ln(Earnings) | weeks worked>0	0.038	0.080	0.078**	-0.014
	(0.027)	(0.057)	(0.031)	(0.062)
Observations	5,009	5,009	2,753	1,749
Moved^^	0.018*	0.038	0.060***	0.057***
	(0.011)	(0.023)	(0.014)	(0.011)
Observations	6,301	6,301	3,419	9,568
Self-reported health excellent or good (12-month)^^^	0.035***	0.073	0.020*	
	(0.009)	(0.020)	(0.010)	
Observations	5,920	5,920	3,234	
Self-reported health excellent or good (48-month)^^^	0.016*	0.034	0.013	
	(0.010)	(0.020)	(0.011)	
Observations	6,279	6,279	3,407	
Duration of training in months			5.829	
Individual controls?	No	No	Yes	Yes

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample is Jobs Corps data males only. +Sample includes all treated, including those with zero duration. Controls include year and quarter of baseline, year and quarter of 48-mo followup survey, whether individual was enrolled in non-residential program and baseline characteristics such as whether individual had child, was ever arrested, had ever used drugs, had a job, had a job in the previous year, ever had a job, race, native language, on welfare as a child, education, baseline marital status and others. ^ Employment is not conditional on labor force participation. ^^ For Job Corps it is defined as living more than 20 miles away from baseline residence. For CCC it is defined as living in a different county than the county of residence at the time of enrollment. For Job Corps, employment is defined as having a job during the 208th week after the baseline survey (four years). Earnings conditional on employment only includes the earnings of individuals employed during the 208th week after the baseline survey. ^^^ Constructed variable that is equal to 1 if self-reported health status is 1 or 2 (excellent health or good health).

Table VII

Long term estimates using education control functions for identification

	(1)	(2)	(3)	(4)
	Log Death		*Retirement*	
Dependent Variable:	*Age*	*AIME*	*Age*	*SSDI*
Panel A: OLS Without Control Functions				
Duration of service (yrs)	0.013***	47.882**	0.509***	-0.02
	(0.004)	(21.770)	(0.189)	(0.014)
Panel B: Control Function Approach 1 (Athey et al 2020)				
Duration of service (yrs)	0.013***	52.363**	0.536***	-0.022
	(0.005)	(21.843)	(0.190)	(0.014)
Bounds to account for assumption violations*	±7.22E-5	±1.152	±6.93E-3	±5.78E-4
Panel C: Control Function Approach 2 (This Paper)				
Duration of service (yrs)	0.013***	46.809**	0.502***	-0.019
	(0.004)	(21.763)	(0.189)	(0.014)
Bounds to account for assumption violations*	±4.59E-06	±0.079	±5.42E-04	±4.19E-05
Observations	7,722	4,613	4,575	4,575

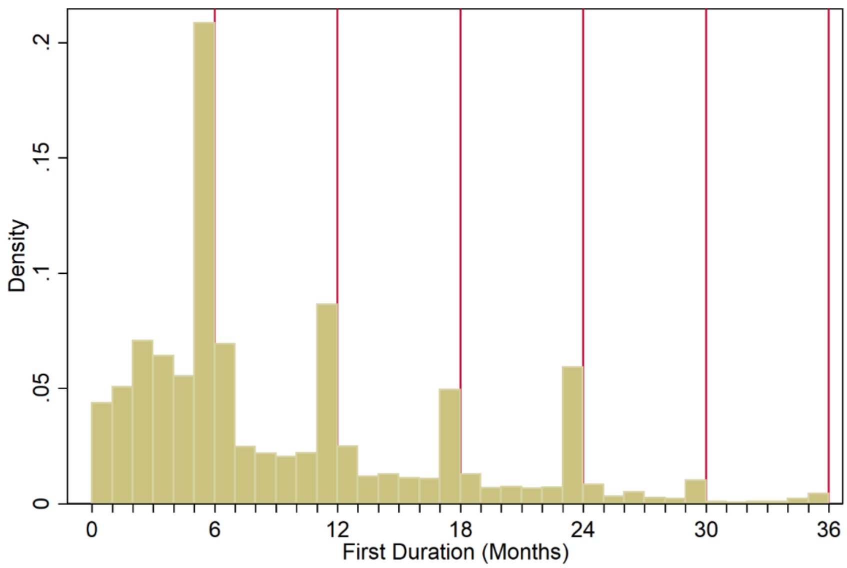
Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. This table reports the coeffcients on duration in a regression of log age at death, AIME, retirement age, and SSDI claiming. The sample is enrollees for which the control function using education can be computed using only common covariates between JC and CCC (enrollment age, age less than 18 indicator, highest grade level, hispanic status, whether helpd a previous job, whether graduated high school, household size, from rural hosehold, whether father is living, whether mother is living). See text for a description about how the control functions in Panels B and C are constructed.

* This term can be multiplied by desired percentage difference in treatment effect between JC and CCC (Panel B) or omitted variable bias between JC and CCC (Panel C) to calculate the final bounds.

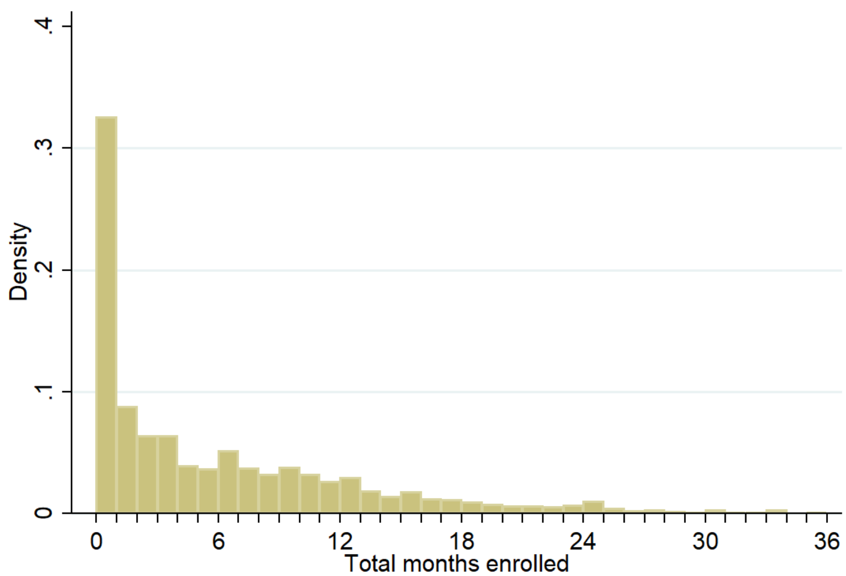
Figure I

Distribution of Service Duration in the CCC Records and Jobs Corps

Panel A: CCC



Panel B: Jobs Corps

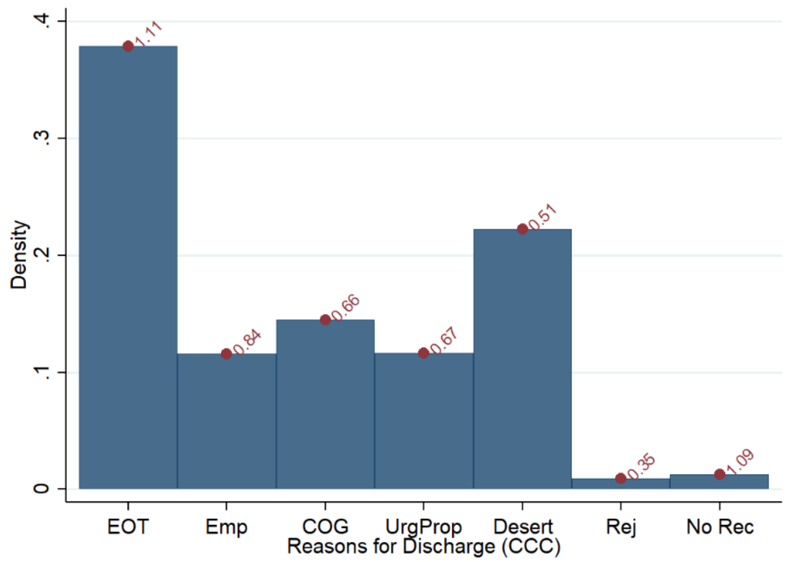


Notes: We exclude durations greater than 3 years (less than 1% of the observations) in this figure. Mean duration is 9.44 months (s.d. 7.47) for CCC and 5.8 months (s.d. 6.6) for Jobs Corps.

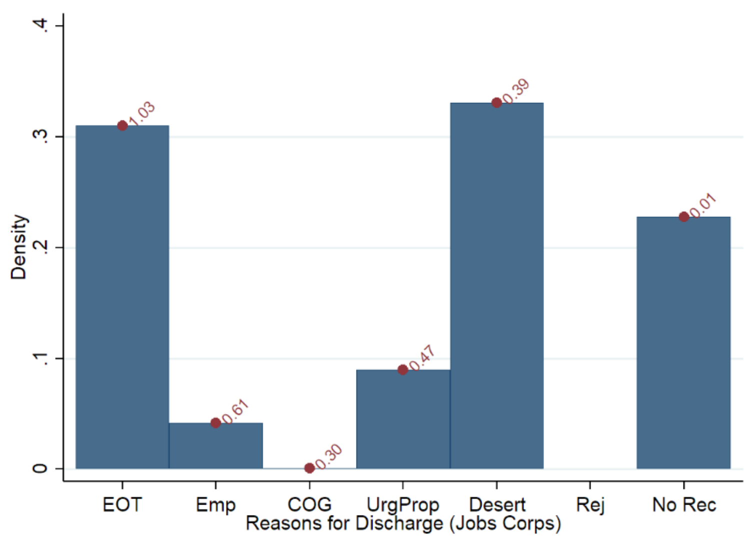
Figure II

Distribution of Reason for Discharge

Panel A: CCC



Panel B: Jobs Corps

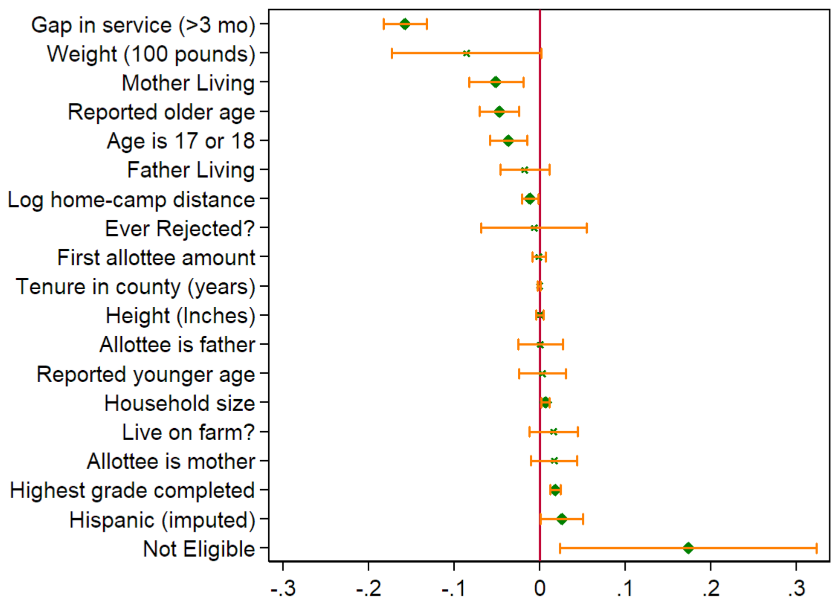


Note: Values on top of the bar graph are mean duration (in years) for each category: EOT (End of Term), Emp (employment outside the program), COG (Convenience of the Government), UrgProp (Urgent and Proper Call), Desert, Rej (Rejected), No Rec (No record). Reasons for Jobs Corps was harmonized to match with CCC's reasons for discharge.

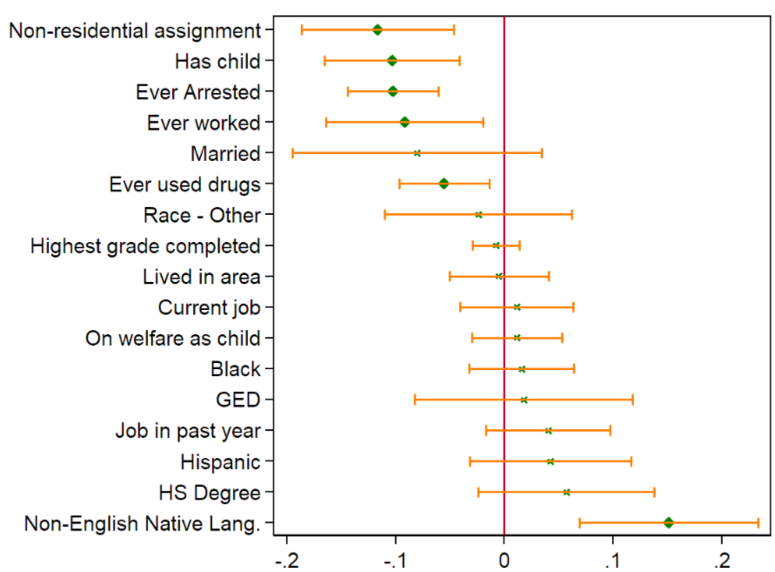
Figure III

Determinants of Duration

Panel A: CCC



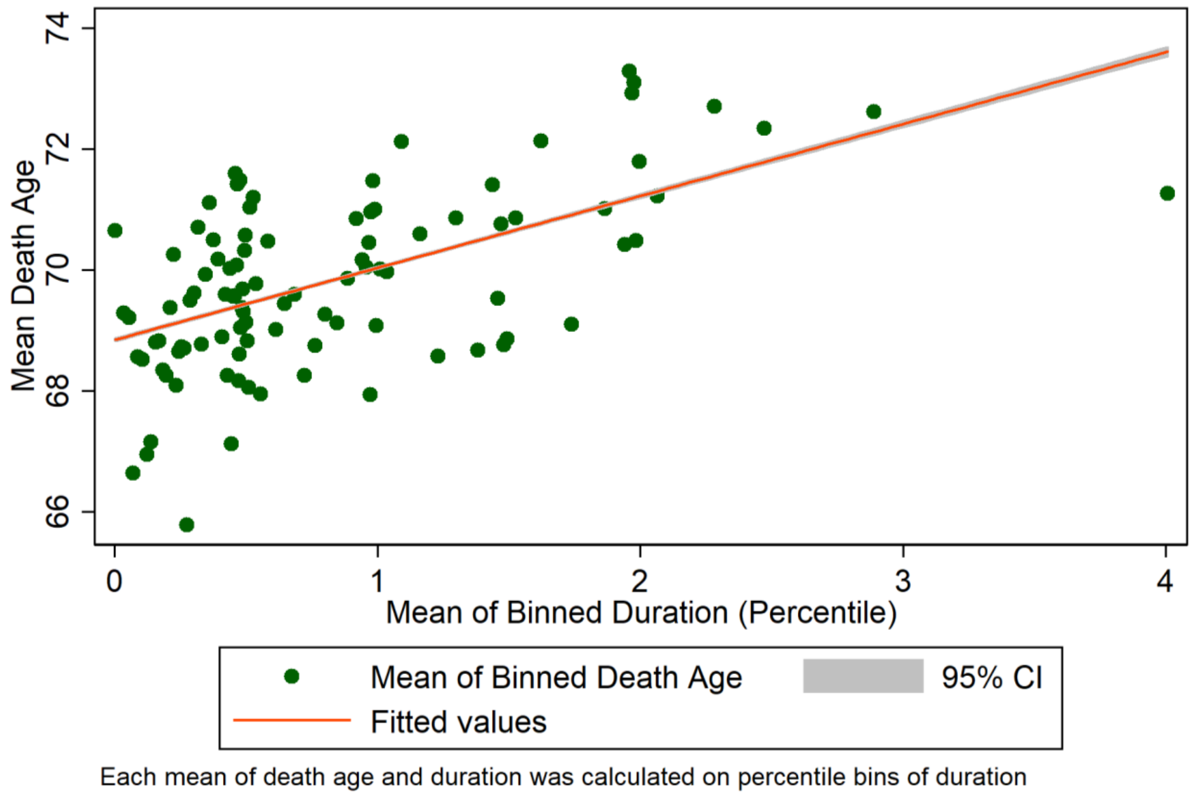
Panel B: Jobs Corps



Note: Estimates and 95% confidence intervals plotted for coefficient estimates on selected variables from regressing duration on various individual, camp, and peer characteristics. Coefficients in diamond are statistically significant at the 95% level. Mean duration for the estimation sample is 0.84 years for CCC and 0.49 years for Jobs Corps. Full results of the regression estimates are shown in Appendix Table III.

Figure IV

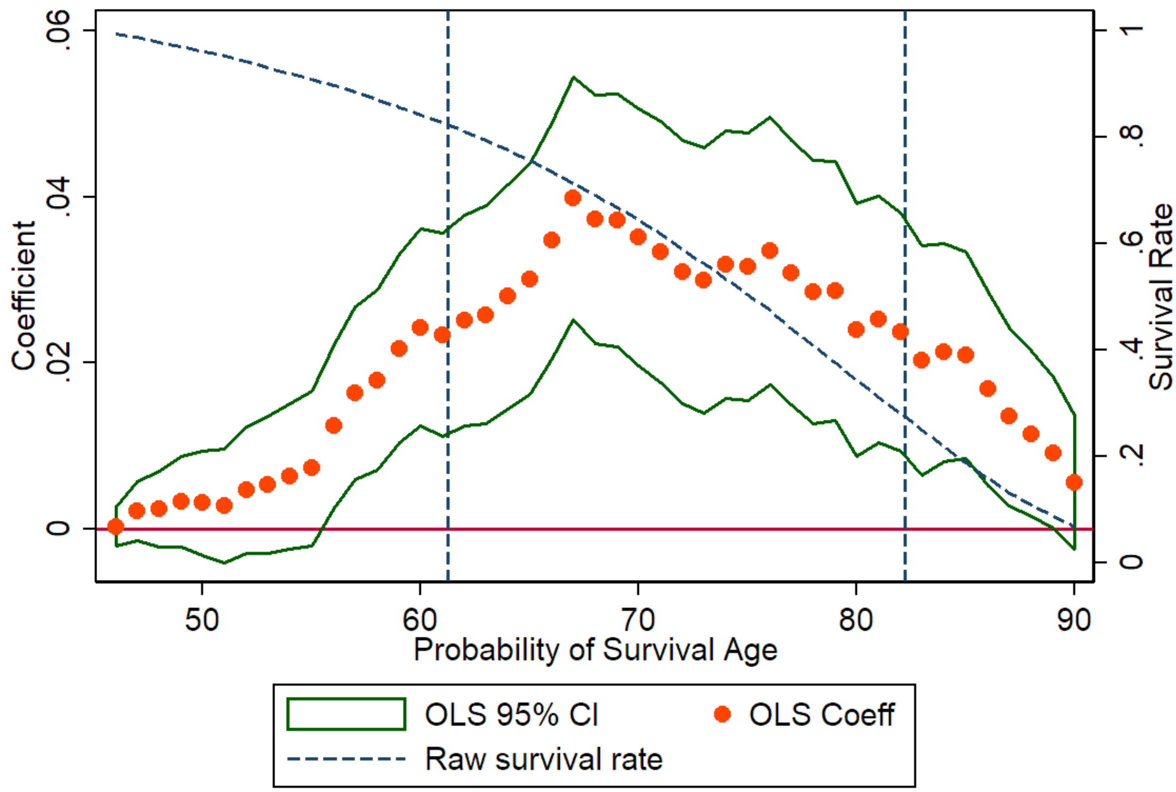
Longevity Increases with CCC Service Duration



Notes: figure plots the linear fit of mean death age within each percentile bin of duration. Data: Administrative records matched to death certificates. See text for more details.

Figure V

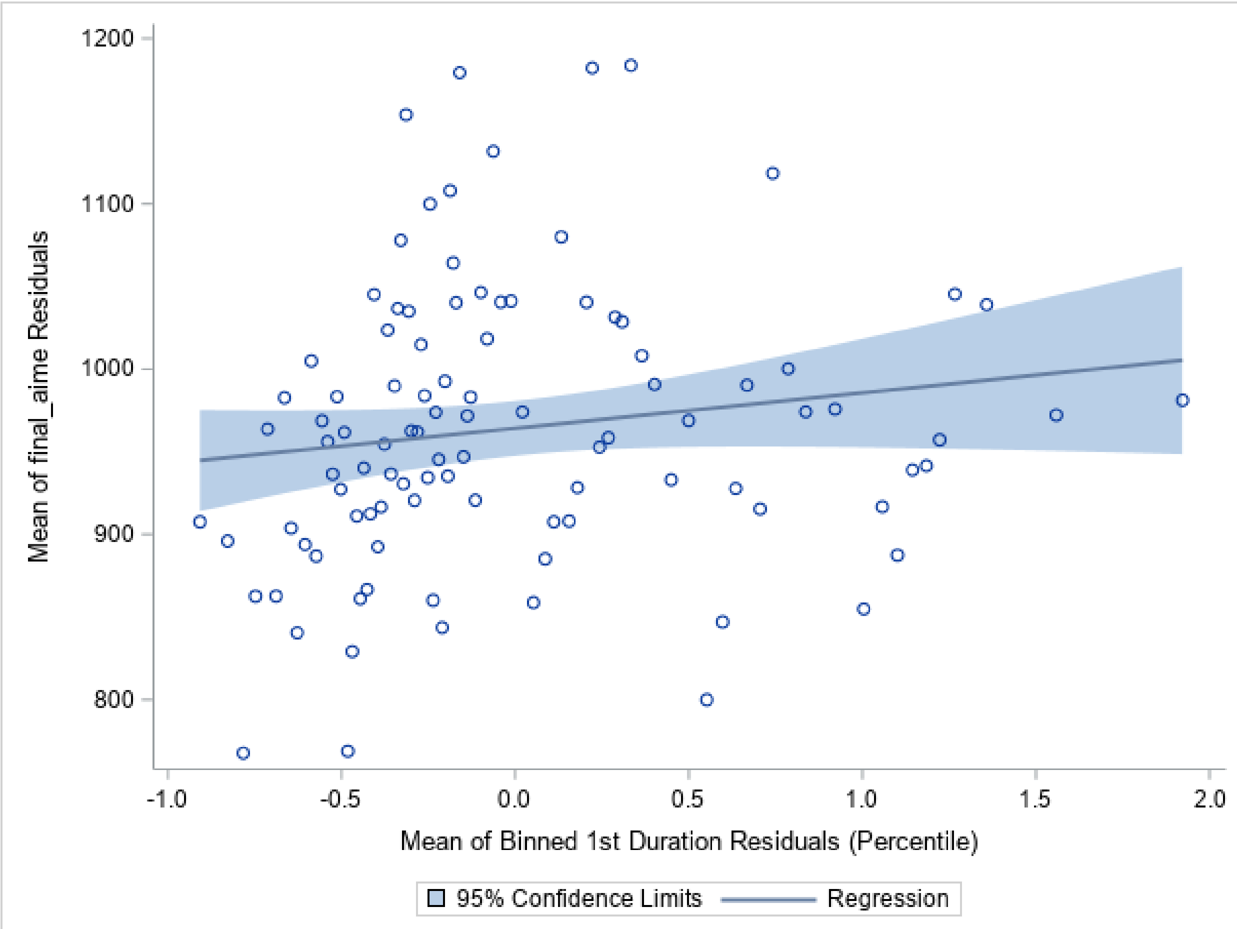
Effect of Service Duration on the Probability of Survival to Different Ages



Notes: On the left y-axis, this figure reports the coefficients (and standard errors) from running linear regressions of the probability that the person survived to a given age a on duration, where age ranges from age 45 to age 90. The regressions use the administrative data we collected and control for all observables at baseline (see Table II for details). On the right y-axis we plot the survival rate.

Figure VI

CCC Duration and AIME



Notes: Authors computation based on administrative program data matched to the Master Beneficiary Records, for those claiming 1979 or later. This restricts the sample to enrollees serving less than 3 years. It plots residuals from regressing AIME on birth year (y-axis), and regressing duration of service on birth year (x-axis).