**THE LIFETIME IMPACTS OF THE NEW DEAL'S YOUTH EMPLOYMENT PROGRAM***

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***Abstract:*** *We study the lifetime effects of the first and largest American youth employment and training program in the U.S. – 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 service in the CCC led to improvements in height, health status, longevity, geographic mobility, and lifetime earnings but did not improve short-term labor market outcomes including employment and wages. We address potential selection into CCC duration using several approaches, most importantly two newly developed control function approaches that leverage unbiased estimates of the short-term effects of a randomized control trial of Job Corps (the modern version of the CCC). Our findings suggest that short- and medium-term evaluations of employment programs underestimate impacts because they fail to capture lifetime effects and often ignore or underestimate health and longevity benefits which increase in magnitude at later ages. While the benefits of the program were substantial, they only partly offset losses in income and longevity associated with the Great Depression. JEL Codes: I28, I38, H53, J26*

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## I. Introduction

The Great Recession of 2008 and the pandemic-induced recession of 2020 have renewed interest in understanding the effects of government programs implemented or expanded during economic crises. The Great Depression (1929-1941) was the largest economic downturn in modern history. In response, the federal government enacted a series of wide-scale poverty alleviation programs — the New Deal — which constituted the first and largest fiscal expansion during peacetime. These efforts were meant to provide temporary relief to individuals most affected by the Great Depression and to spur economic activity. Yet many of the programs (e.g. Social Security, farm subsidies) remain in place today. Economists have long debated the effectiveness of these policies in the short-run and particularly whether they helped or hindered economic recovery (Romer, 1992). Substantially less is known about what New Deal programs did for individual beneficiaries, particularly over their lifetimes.

We conduct the first lifetime evaluation of the largest federal youth employment program in U.S. history created to address high youth unemployment during the Great Depression: the Civilian Conservation Corps (CCC). During the Great Depression, youth unemployment rates were estimated to be greater than 25% and as high as 60% (Salmond, 1967; Rawick, 1957). The CCC employed poor men aged 17 to 25 in unskilled, manual labor. Under the Army's supervision, CCC 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, helped recipients find employment, and provided cash transfers to participants' families. Between 1933 and 1942, the CCC trained three million enrollees representing one third of all men aged 17-24, across 2,600 camps. Several programs in existence today, such as Job Corps (JC), Youth Conservation Corps, and JobsFirstNYC, are modeled after the CCC (Levine, 2010).

We create a new individual-level data set of CCC participants and their lifetime outcomes. We digitize administrative records for roughly 25,000 men from the CCC programs in Colorado and New Mexico covering the population served by the program in those states 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 the complete-count 1940 U.S. Federal Census, World War II (WWII) enlistment records, Social Security Administration records, and individual death records. These data allow us to investigate the effects of the CCC on two related but independent determinants of wellbeing: lifetime earnings and longevity. We also collect data on several other short- and long-run measures that likely influence lifetime income and longevity, such as education, health, and geographic mobility.

To estimate the causal effects of the CCC, we take multiple approaches that exploit variation in the service duration of participants, similar to a dose-response analysis. First, we investigate how training duration affects outcomes in an ordinary least squares (OLS) setting. We use multiple methods to assess the bias in OLS estimates. These include estimating the stability of estimates when we vary the controls included, as in Chan et al (2022), bounding the effects based on Oster (2017), and an instrumental variable technique that exploits the government's decision to ramp down or close camps as an exogenous source of variation in training duration. Finally, we use and extend new econometric techniques for causal inference that combine observational data from the CCC with data from a randomized control trial (RCT) of the Job Corps, a modern-era job training program that was modeled after the CCC.

Training duration in the CCC varied from a few days to more than two years, with the average enrollee participating for nine months. We find that those who trained longer were not necessarily from higher or lower socioeconomic status (SES) backgrounds. Moreover, many enrollees with both short and long service durations ended their training for arbitrary reasons. Thus, the direction of any omitted variable bias in OLS estimates is unclear. Controlling for extensive individual- and camp-level covariates has little impact on our estimates, which remain stable regardless of the set of controls included; moreover, Oster (2017) bounds remain tight. Placebo tests, in which we estimate whether duration predicts predetermined characteristics of the enrollees, also support our conclusion that omitted variables are not driving the relationship between training duration and long-term outcomes. Our IV estimates are similar to the OLS. Though not definitive because of the lack of precision in the second stage, we interpret this as corroborating evidence that our OLS estimates likely suffer little bias.

Finally, in the spirit of seminal work by Lalonde (1986), we use the experimental data from the Job Corps (JC) RCT to shed light on the internal validity of our research which uses observational data. Although the JC data pertains to youth training that took place in the 1990s, the program was modeled after the CCC and shares many features. Moreover, we demonstrate that both JC and CCC participants were relatively disadvantaged compared to their contemporaries along many of the same margins.

Using the JC RCT, we implement two control function approaches to correct for bias under different assumptions about bias stability. In the first approach, we follow Athey et al. (2020) and assume that the *short-run treatment effect is the same* across the JC and CCC. Any difference is interpreted as bias in our observational CCC estimates, which we correct for when we generate our long run lifetime estimates. In the second approach, we extend Athey et al. (2020) and develop a new, more realistic approach for this setting in which we let the treatment effects differ but assume that the *selection bias* is similar in the JC and CCC. Our OLS estimates change very little when we add control functions from either approach, consistent with little bias in our observational CCC estimates.

We find no short- or medium-run *labor market* benefits associated with job training in the CCC. With respect to lifetime outcomes, however, we find significant benefits associated with longer training. Those who spend one year in the CCC have 5.2% higher lifetime earnings, live about a year longer, claim benefits (disability or pensions) 0.4 years later in life, and have 10% lower rates of Social Security Disability Insurance (SSDI) claims. These gains are consistent with and likely mediated by the improved education and health of the participants (measured by height and weight in young adulthood) as well as their increased geographic mobility toward healthier and richer areas. Interestingly, the gains in longevity are only apparent after age 55.

Overall, our 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. However, while these gains were substantial, we estimate that they may not have been large enough to fully compensate individuals for large losses associated with the Great Depression.

This paper makes several contributions to the existing literature. We provide the first estimates of the lifetime effects of a New Deal program on individual beneficiaries. Fishback (2017) provides a comprehensive survey of the microeconomic research on the short-run effects of New Deal programs based on local-area (not individual-level) analyses. He reports that New Deal programs increased internal migration, lowered crime, and reduced mortality in the short-run, though they tended not to improve employment (see also Fishback, Haines and Kantor 2007, and Arthi 2018). Our short-term results are consistent with this evidence. Other work has investigated effects on the long-term economic path of regions receiving programs such as the Tennessee Valley Authority (Kline and Moretti 2014). Two recent papers examine the relationship between living in an area that received more New Deal programming and long-term individual outcomes. Modrek et al. (2022) investigate the effects of growing up in districts with more New Deal emergency employment on the long-term outcomes of children from Wisconsin and find mixed evidence regarding their benefits. Similarly, Jou and Morgan (2023) evaluate the effect of New Deal transfers to counties on the longevity of their inhabitants and find benefits particularly for young males. Neither paper investigates the direct effects of a given program on its participants.

We also contribute to the extensive empirical literature investigating the effects of youth training programs. Our first contribution to this literature is to provide a more comprehensive evaluation of training program impacts by examining non-labor market outcomes over a participant's lifetime. In particular, we investigate effects on longevity, an important determinant of wellbeing (Jones and Klenow 2016), and on migration, another important determinant of both health and economic outcomes. Recent work, including reviews of the job training literature (Barnow and Smith 2015; Crépon and van den Berg 2016), have emphasized that training programs can have substantial benefits on non-labor market outcomes such as education (Cohen and Piquero 2009; Berk et al. 2020), crime (Heller 2014, Modestino 2019), or mortality (Gelber et al. 2016), at least in the short- to medium-run. To our knowledge, no previous studies of youth training programs have investigated lifetime impacts, and no paper has tracked longevity. Longevity gains substantially increase the benefits of the programs measured by the Marginal Value of Public Funds (MVPF).

Our second contribution to the literature on job training is to study *lifetime* labor market outcomes. Our lifetime measures do not reflect periodic market fluctuations and incorporate gains from experience. Stochastic variation in labor market conditions causes the estimated effects of the returns to human capital investments to fluctuate (Rosenzweig and Udry, 2020). As a result, point-in-time measures of labor market outcomes typically studied in job training evaluations can only provide a partial and possibly biased assessment of training program effects. This is particularly true if recipients are entering the labor market during periods of weak labor demand. Moreover, if the benefits of training are similar to the benefits of education, their return may only be observed over time. Indeed Card, Kluve, and Weber (2018) find that the short-term effects of over 200 job training programs on labor market outcomes are modest, but that the treatment effects do appear to increase over time.[1] Kluve et al. (2019) arrive at similar conclusions in a meta-analysis of 113 youth training programs around the world. However, most evaluations track participants for only a few years—the longest evaluation we are aware of only tracks participants for 10-20 years.[2]

Our results confirm that the impacts of the program are much larger over the lifetime than in the short-term. As a result, the MVPF for the CCC that includes lifetime outcomes far exceeds one that includes only short- and medium-term impacts. We attribute this in part to the effects of the program on non-labor market outcomes, including improved health and increased geographic mobility, which can generate long-term but not necessarily short-term effects. The different short- and long-term results are consistent with 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).

Our final contribution is methodological. We demonstrate how to combine observational data on lifetime outcomes with RCTs to make progress on estimating unbiased causal long-term program effects under different assumptions by extending the Athey et al. (2020) control function approach. These methods can be applied to other settings where long-run observational and short-run experimental data are available.

## 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 prevailing view at the time was that “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

[1] Recent work shows some youth programs can have sustained benefits over the short- and medium-term. For example, Katz et al. (2022) document gains in earnings of sectoral employment programs.
[2] Although Shochet (2021) analyzed medium-term outcomes in the 20-year follow-up of Job Corps, they did not measure lifetime impacts. Our study is the longest follow-up of a training program we know.

a time and were located close to work sites. The CCC program had 80 percent popular support and many communities welcomed the camps and the resources they brought (Parham, 1981; Paige, 1985). As the program evolved, it added education components in 1934, which became mandatory in 1937, and military training in 1941. The program ended in 1942 due to the onset of World War II (Figure A.I).

*Eligibility.* The CCC program was only open to male citizens who were unmarried, unemployed, and primarily between the ages of 17 and 25. Preference was given to those in greater need and CCC enrollees were often selected from families already enrolled in relief programs. Government reports at the time confirm that enrollees were also poorly educated, with little work experience, as well as undernourished (McEntee 1942).[3] Good physical condition, confirmed with an examination at enlistment, and no prior criminal activity were required. Camp assignment was based only on the needs of the camp, not enrollee preference.

*Compensation and program cost.* Enrollees were required to work 40 hours per week and were paid $30 per month, $25 of which was sent home to a designated family member. The government paid for 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 (BLS, 1941), equivalent to $18,000 in 2022, which is roughly half of per enrollee spending in Job Corps, the main federal youth training program today.

*Duration of enrollment.* Individuals initially enrolled for a six-month term and were allowed to re-enroll for a maximum of two years (4 terms). 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. They could be expelled for misconduct. Efforts were made to reduce enrollee turnover.

*The CCC in Colorado and New Mexico.* CO and NM were relatively poor states at this time. Per capita annual personal income was $571 in CO and $329 in NM, while the nationwide

[3] For example, in 1939 and 1940, about 52% of CCC enrollees had 8 years of schooling or less (Federal Security Agency's Annual Report 1940).

average was $618 (U.S. department of Commerce, 1999). 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. Enrollees in Colorado and New Mexico were disproportionately Hispanic.[4]

## 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 in a dose-response framework.[5] This strategy of limiting comparisons to only those individuals who enrolled and served generates less potential selection bias than one that compares enrollees to non-enrollees. This strategy is similar to Flores et al. (2012) who 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. We estimate the following specification:

$$Y_{icj} = \alpha + \beta * \left(duration\ of\ CCC\ service_{icj}\right) + X_{icj}\gamma + e_{icj} \qquad (1)$$

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

The coefficient $\beta$ 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. The main threat to identification is the potential selection into duration, either positive or negative. Individuals with higher abilities may have 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

[4] 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.

[5] We could not construct a suitable control group of individuals that did not serve in the CCC. In assessing the effects of training programs using observational data, Lalonde (1986) and Smith and Todd (2005) emphasized the need for "a rich set of variables related to program participation and labor market outcomes," to create suitable control groups, and also that "the nonexperimental comparison group be drawn from the same local labor markets as the participants." In the absence of these rich controls, observational estimates tend to deliver negative treatment effects.

to train longer in the CCC because they were more in need of the CCC monthly payment and had fewer outside options for employment (negative selection). Additionally, though individuals could not select their camps, unobserved camp characteristics could affect both duration and outcomes. 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).

To address these concerns, we take multiple approaches. First, we investigate the determinants of duration to assess possible selection. This includes an examination of the reasons enrollees leave before the end of term. Next, for a subset of the data for whom we have extensive pre-enrollment information, we conduct placebo tests to estimate whether duration predicts pre-CCC enrollment outcomes (education, labor market experience, height and weight).
In addition, we use three standard methods to assess the potential for omitted variable bias: 1) following Chan et al (2022), we estimate multiple OLS regression specifications, varying the subset of covariates for which we control in order to examine the sensitivity of our coefficient estimates to the choice and order of covariates included, 2) we estimate Oster (2017) bounds,[6] and 3) we instrument for training duration using exogenous variation in the reason for dismissal, which we discuss further in Section VI.

Finally, and most importantly, we implement a control function approach to address any potential remaining selection bias. To do so, we rely on and extend methods developed by Athey, Chetty, and Imbens (2020), and use control functions generated from short-run estimates from the experimental Job Corps data to adjust the estimates of the long-run impact of the program generated from the observational CCC data. We describe these control function methods in greater detail later as they are more novel.

## IV. Data and descriptive statistics

### A. Data collection

[6] An alternative bounding approach would be to use Lee (2009) bounds. However, we cannot construct Lee bounds in our case as we have a continuous treatment variable and multiple continuous covariates at once.

*Colorado (CO) Enrollees.* We digitized the original applications of all 18,644 individuals who applied to the CCC in Colorado between 1937 and 1942.[7] The applications contain: name, address, date of birth, place-of-birth, height, weight, race, social security number (SSN), marital status, whether the 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. Except for information on height, weight, and race, which were collected upon medical examination, all other information 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. 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 records are available.

*Camp-level Data.* For each camp we collect historical weather patterns (temperature and precipitation), the (Euclidian) distance of the camp to the closest towns and to each enrollee's hometown, the agency (and thus the type of work) that created the camp, and the average characteristics of enrollees at a given point in time, as well as Census county-level characteristics, such as unemployment rates.

*Death Records.* We link the CCC data with the Social Security Death Master File (DMF) and state-level death records to identify the date of death and social security number of each enrollee. We find death dates for 82% of recipients (Table I), representing much higher match rates than typically found in the literature.[8] This match was done manually by trained

[7] We established this based on published reports from the CCC that the records account for the complete population of records starting in 1937 (see Figure A.II).

[8] Our match rates are higher than those typically found in the literature (which range from 20 to 50%, see Bailey et al. 2020, Abramitzky et al. 2021) for two reasons. First, administrative records contain information not just on individuals but also on their family members. Second, the death records come from various sources, not just the DMF, including state vital registration sources, deaths during WWII, and gravestones. However, mortality information is still 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 DMF (Hill and Rosenwaike, 2001), though we can still find a

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 A4. We also 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 SSA, which contains information on individual lifetime earnings, disability (SSDI) claiming, and age at benefit claiming (Supplementary Appendix A5) for those who apply for benefits.[9] We match 53% of our records to the MBR records. But for those who survived to age 65, we match 80%, indicating a high match rate for the targeted population. For individuals retiring after 1978, we observe the Average Indexed Monthly Earnings (AIME) which is computed as the average earnings of the highest 35 years of earnings after adjusting for inflation.

*1940 and WWII records.* We match the enrollees to the 1940 Federal Census and to WWII Enlistment Records using the Abramitzky, Mill, and Perez (2020) Expectation Maximization (EM) algorithm (see Supplementary Appendix B for details). The 1940 census includes education, location and labor market information (employment, occupation and wages). We successfully match 45% of all individuals and 43% of those that serve before 1940 to the census, and 34% to WWII enlistment records. This lower match rate to WWII records is expected because not all individuals enlisted or served in WWII, even when they were eligible, and not all enlistee records survived.

## 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 (Table A.I).

Our data suffers from some attrition but is unusually complete for the key long-term outcomes we examine. For the mortality analysis, we include the 17,639 men (75% of the

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death record for them in state vital records or gravestone databases. Second, 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.

[9] We only observe SSN if the 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 Social Security card.

original sample) who have information on age of death and who died after age 45 (this restriction avoids WWII deaths but does not affect our results). This estimation sample is generally representative of the complete data (Table A.II). For the lifetime outcomes from the SSA, our sample includes 12,455 individuals, again representative of the full sample in most cases, except for the age at death, which by construction is higher in the SSA sample because only those who survive to at least 62 are eligible to apply for pensions, unless claiming for disability (Table A.IIa).

In our analysis, we investigate the extent of sample selection in our data and also the effects of missing data on our estimates, using imputations in alternative specifications to generate a range of estimates, all of which are relatively tight. This is consistent with modest bias from non-random attrition (see Table A.IIa and Table A.IIb for full set of summary statistics).

### C. Summary Statistics: CCC Training and Lifetime Outcomes

*Pre-CCC Characteristics.* On average, enrollees were 18.8 years old, had completed 8.6 years of schooling and came from a household with 5 individuals (Table I). 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 45% were Hispanic (see Supplementary Appendix A9).

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 (Table A.III). CO and NM enrollees were also more disadvantaged than the average CCC enrollee in the nation—they are substantially younger, shorter, weigh less, have fewer dependents, and more of them have fewer than 4 years of schooling (Figure A.III).[10] These findings are consistent with the fact that CO and NM were very poor states at that time. Not surprisingly, when we compare the Colorado and New Mexico CCC enrollees with similarly aged men in the 1930 and 1940 censuses, we find that the CCC enrollees were relatively disadvantaged in terms of education, employment and household income (Table II). We return to this in our control function estimates

[10] 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."

where we explore and compare the extent of relative disadvantage in the CCC and Job Corp samples.

*Post-CCC outcomes.* Consistent with CCC enrollees being disadvantaged at entry, they also had worse long-term outcomes than the cohort average. In particular, they died at younger ages and earned less in adulthood. The average enrollee eventually lived to age 70, one year less than male cohorts born in 1920 (and who survived to age 17), from SSA cohort life tables. They also earned $405 in annual wages in 1940, compared to $593 for men aged 18-32 in the 1940 Census.

## V. Determinants of Training Duration

Duration in months spikes around 6, 12, 18 and 24 months, corresponding to 1, 2, 3 and 4 terms (Figure I Panel A), with 62 percent of individuals dropping out in the middle of their assignment. Among those who left before completing their term, 21% deserted, 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" such as family sickness or death (Figure II Panel A). Average training duration was 9.8 months.

To investigate the determinants of duration we estimate 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 (Figure A.IV). We also include application county-by-enrollment year-quarter (CQE) fixed effects (e.g., one for those who enlisted in Denver, CO in the first quarter for 1939). This addresses the fact that the number and types of camps that were opened varied over time and space, and that the type of individuals who apply for training or 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 III and Table A.IV). There appear to be three groups of enrollees. One, those who served for longer because they were positively selected, such as those with more education or who were older. A second group that served longer are those coming from farms, larger, Hispanic households, and those who weighed less, indicating greater need and negative selection. A third group appears to have dropped out of the CCC for random reasons, cutting short their duration, such as unanticipated camp closures or emergencies at home. The evidence also suggests that conditional on individual characteristics, place and time of enrollment, camp conditions were correlated with duration of training, but again, not in any consistent way (Table A.IV). Taken together, the primary evidence shows that desirable enrollee or camp traits did not necessarily lead to longer durations: there is no single narrative of selection.

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

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

### A. Impact of CCC Duration on Longevity

Our graphical results indicate that the longer an enrollee trained, the longer he lived (Figure IV Panel A). We present bin scatter plots controlling for birth year using the methodology of Cattaneo et al. (2022). We fix the number of bins at 20 but allow polynomial approximation and smoothness to be chosen by the algorithm between 1 and 3.[11] The results also indicate that we cannot reject a log-linear relationship between training duration and longevity.

Given this relationship, we estimate an accelerated failure time model of the age at death on duration with added controls for the characteristics of the enrollees and the camps to examine whether and how our estimate changes. The first column of Table III Panel A (with no controls) shows a very precise coefficient on duration of 0.013. Controlling for cohort and CQE fixed effects (column 2) does not change the coefficient estimate. Including family and individual characteristics lowers the coefficient from 0.013 to 0.011 (column 3).[12] The coefficient remains stable as we add more extensive controls. 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 replacing camp characteristics by camp fixed effects in column 6 (or multiple other

[11] Alternatively, we can fix the polynomial approximation and smoothness at 1 and let the Cattaneo et al. (2022) algorithm choose the optimal bandwidth (Figure A.V). The results are robust between these two specifications.

[12] These characteristics include: 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. For missing data we impute using the mean and include a series of indicators to denote imputation. Full regression with coefficients for controls in Table A.V. We also report alternative standard errors in Table A.VI from clustering on county or year-quarter of enlistment instead of CQE, which make no material difference.

FE as in Table A.VII) 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 9.85 months, the program increased age at death by 0.8 years for the average enrollee. The results are similar when we limit our sample to CO where the records contain more important baseline information for which we can control, such as education, height, etc. (column 7).

*Assessing the potential for omitted variable bias*. The coefficient stability across columns 1-7 as we add more detailed controls suggests that selection bias may be small. Nevertheless we conduct three additional exercises we described in Section III to assess the potential for omitted variable bias.

Varying the set of controls. Following Chan et al. (2022), we group the set of individual, family, and camp characteristics, and the CQE FE into eight groups and estimate OLS regressions with all possible combinations of the covariate groups. We then plot the minimum, average, and maximum coefficient estimates of training duration by the number of control groups included (Figure V). No minimum coefficient estimate includes zero and the estimates appear relatively stable across the inclusion of additional control groups.

Calculating Oster bounds. 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 (Table III).[13] Thus, one more year of training would increase the age at death between 0.96 and 1.02 years. Both exercises provide further evidence that our results are unlikely driven by omitted variables.

IV estimates. For the IV approach, we take advantage of the fact that some individuals were dismissed "for the convenience of the government" which typically occurred when a camp was winding down or closing upon project completion.[14] We consider this timing to be exogenous within a given camp. Since most enrollees who were discharged for the Convenience of the Government (COG) did so at the time they completed their term, we select those that also

[13] Table III reports Oster bounds for the specification used in column 6, with individual and peer characteristics and Camp FE.

[14] Consistent with this, we observe in the data that this reason for dismissal is much more common towards the end of the program in 1942 (27%) than in 1938 (1.8%). We also find that the fraction dismissed for COG is 21% among individuals who are sent to a camp that closes within a year, compared to only 12% of those assigned to camps that close later.

completed their terms and left voluntarily (the "end of term" or EOT group) as the appropriate comparison group. Our assumption is that had the COG group not been dismissed by the government, they would have either finished a longer enlistment term or re-enlisted, making EOT enrollees a suitable counterfactual for COG enrollees. This is supported by COG and EOT enrollees having a similar distribution of training duration, with spikes at the completion of terms, but EOT enrollees serving more terms, as expected (Figure A.VI). We construct the instrument as an indicator equal to 1 if the enrollee was dismissed by COG and 0 if EOT. Because the sample we use for this approach is smaller and the standard errors substantially larger, we view these results as complementary to the main analysis.

We present estimates from the first and second stages (along with the OLS counterparts for this smaller sample) based on the specification with peer characteristics in Table IV.[15] The instrument is predictive of duration: being dismissed for the convenience of the government reduces average duration. The F-statistic is large, more than 40.[16] The IV estimates for longevity shows a second-stage estimate of 0.013, similar in magnitude to the OLS estimates for the IV sample (0.014), which is in turn almost identical to our main 0.013 estimate in Table III. Although the IV estimates in this smaller sample are not significant at conventional levels, we cannot reject that they are the same as the OLS estimates.

*Timing of longevity gains*. To examine when over one's lifetime the longevity gains accrue, we present the results of the regression of probability of survival to age $x$ on duration, for every $x$ between 45 and 90 (Figure VI). The coefficients are small and statistically insignificant at younger ages when survival is very high. They become positive and statistically significant starting at age 56, peak between ages 68 and 78, and decline thereafter. As a function of the baseline survival rate, which is declining throughout, the effects rise until age 67, and then decline. Thus, the benefits of the program in terms of mortality are not apparent until after individuals reach mature ages. This is consistent with the findings in Lleras-Muney and Moreau (2022): health gains in childhood and young adulthood only manifest themselves in old ages when health starts to decline, and the least healthy individuals die.

[15] We present the specification that includes camp peer characteristics (as in column 5 of Table III) but not camp*enrollment FE due to little variation in COG within camp and enrollment year-quarter. The IV estimates for all specifications are presented in Table A.VIII and the results are mostly consistent across specifications.
[16] Recent work by Lee et al. (2023) shows that if the F-statistics is lower than 104.5 then 95% CI need to be adjusted. This adjustment is very small in our case and does not materially change our conclusions.

*Sample attrition.* About 18% 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 A.IX 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 different assumptions about the missing data. The effect of duration on longevity is consistently positive and statistically significant under various imputation approaches (Table A.X).

*Alternative measures of longevity.* Our main results use the information found by trained genealogists from multiple sources to determine the age at death. However, we can replicate the results using death age retrieved from machine matching our records to the DMF. We continue to estimate a positive and statistically significant coefficient of duration on age at death that is similar in magnitude to our main estimates (Table A.XI).

### B. Impact of CCC duration on lifetime income

We estimate the impact of program duration on lifetime income as proxied by the SSA's Average Indexed Monthly Earnings (AIME), which is the average of an individual's best 35 years of real earnings used by the SSA to calculate pension amounts. 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.[17]

We present graphical results using a binscatter plot in Figure IV (panel B) and OLS regressions in Table III Panel B. Both the binscatter plot and the OLS results show that, controlling for birth cohort, there is a significant relationship between training duration and lifetime earnings (column 2). The estimates imply an increase of $67 additional monthly earnings for those who participated for one year, an increase of 7% relative to average earnings.[18] As we add more controls across the columns, the estimated coefficient declines to

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[17] We do have another, slightly noisier, proxy of earnings: the Primary Insurance Amount (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 Table A.XII for results.

[18] Note that when we exclude controls for birth cohort, there is no relationship between training duration and lifetime earnings. This is because more recently born cohorts had shorter durations due to the end of the program but higher incomes, due to secular trends in earnings, generating a spurious negative correlation.

$50, or 5.2% of average monthly earnings (column 6). This coefficient is less stable than the one for longevity. As a result, the Oster bounds are also wider (23-143) but still greater than zero. When we perform the previous sensitivity exercise, varying the number and types of controls (Figure V panel B), the estimates appear to reach stability if at least three of the eight sets of controls are included, after which the variance in estimates also declines. Again, the minimum values do not include 0 for any number of control groups. The IV estimates (Table IV) suggest a positive impact that is greater than the OLS estimates and statistically significant at the 10% level. The IV estimates suggest increases of about $290 (a roughly 20% increase relative to the mean), though these estimates are not statistically different from the OLS.

These results do not appear to be driven by sample selection or attrition in the SSA data. Duration is uncorrelated with whether we match an enrollee to MBR. Nor does the effect of duration on longevity, an outcome available for enrollees not matched to MBR, change when we limit to the sample matched to the MBR (Table A.IX Panels B and C).

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

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

We estimate the impact of CCC duration on the age at which individuals first claim benefits from the SSA (either disability or pensions), presenting the binscatter plots in Figure IV Panel C and OLS results in Table III Panel C. We find that one year of CCC enrollment increases the age at claiming benefits by almost half a year, with a mean age at claiming of 60 years, suggesting CCC men were in better health, retired later and lived longer. Oster bounds are tight, ranging from 0.220 to 0.462 and the estimates are very stable regardless of control groups included (Figure V). This is consistent with existing work showing that early retirement is associated with worse health as proxied by death at younger ages (Waldron 2001, Fitzpatrick and Moore 2018). The IV estimates are positive, statistically insignificant and larger than OLS estimates at 1.2 instead of 0.5 (Table IV). But again, they are not statistically significantly different from OLS estimates.

We can examine this health channel directly by looking at how duration affects SSDI claiming, another measure of health in Panel D of Figure IV (binscatter), Table III (OLS), and Figure V (sensitivity analysis). When the full set of controls is included, we find that one year in the CCC reduces claiming by 2.1 percentage points, or 10 percent relative to the mean claiming rate in the sample of 21 percent. Oster bounds range from -0.03 to -0.018 and again the estimates are stable to the controls we include (Figure V). The IV estimates are however positive though again they are not statistically significant or different from OLS. The OLS estimate is comparable to the 20-year follow-up of the Job Corps RCT that finds a 2.4 percentage point decrease in SSDI claiming for older participants (Schochet 2021).

Overall, we find that CCC participation improves income and health in the long-run as measured by delayed benefit claiming, reduced SSDI claiming and greater longevity. In Table A.XIII we explore heterogeneity. The only consistent finding along all long-term outcomes is that the young benefitted more from the program, contrary to the Card et al. (2018)'s finding that youths tend to display few gains.

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

We estimate the short-run effect of CCC enrollment to compare our estimates with existing work on the short-run impact of more recent job training programs and to better understand the mechanisms behind the estimated long run impacts of the CCC. We first investigate the effects of CCC duration on employment and wages in 1940, the short run outcomes usually assessed in evaluations of 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 the 9,518 men who participated in CCC before January 1st, 1940, 43% of whom we find in the 1940 census. Duration is unrelated to whether we locate an enrollee's census record.[19]

[19] Duration does not predict whether we find an enrollee in the 1940 census once we include birth cohort and county-quarter fixed effects (Table V column 1).

CCC training duration has little effect on the short-run labor market outcomes of CCC participants (Table V and Table A.XIV). Most men (91%) are in the labor force, and longer CCC training had at best a very small effect on this outcome: a 2.0% increase relative to the mean. We observe no effect on employment (conditional on labor force participation) as reported in the 1940 Census. There is a small, negative, and imprecise effect of duration on earnings in 1940. Our results suggest that the labor market effects of job training are much greater in the long run than in the short run, consistent with recent reviews.

### 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 percentage point increase in the probability we find the individual in the WWII enlistment records, a 12% increase relative to the mean (Table V Column 6 and Table A.XV). 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 to census and SSA databases, 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 and better health that increased the likelihood of success in the military. Indeed, the final report of the CCC noted that “enrollees made splendid soldier material” (McEntee 1942).

We find that one more year of training translated into roughly one 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 we believe that the program increased heights of CCC enrollees. First, national reports of the CCC program show that the average height gain in the CCC was half an inch (McEntee 1942). Second, 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). Indeed, estimates suggest that at the turn of the 20th century men continued growing until age 26 (Sullivan, 1971). 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. Our estimates are likely larger than the CCC average gain in height because our population is more disadvantaged than the average CCC enrollee. Finally, we find that the gains mostly come from the youngest in our sample, as one would expect. For enrollees 20 years or older, the gains are about 0.822 inches and statistically insignificant compared to 1.143 inches for the whole sample (Table A.XV). Moreover, 9% of our sample of CCC enrollees were likely younger than they reported.

Consistent with this, we also observe a 4.7% increase in BMI, a common indicator of short-term nutrition (Table V, Column 8). The final CCC report documents an average weight gain of enrollees during the program of 11 pounds, translating to an average 8% increase in BMI (McEntee 1942). Our results suggest that half of these gains persisted. These results showing improved health in the short run are consistent with our finding that CCC service lowered SSDI claiming and increased longevity in the long run. We conclude that the CCC improved overall health among participants in the short and the long run.

### C. Effects on Education and Geographic Mobility

We also estimate the impact of CCC duration on 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. We control for the time since discharge (or equivalently the year of observation) to account for the fact that the outcomes are not observed at the same time for all enrollees.

We estimate a positive and statistically significant effect of duration on years of schooling of about 0.17 years, controlling for education at baseline (Table V Column 9).[20] 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, during the early 20th century (see Lleras-Muney 2002 or Goldin and Katz 2008). This effect likely represents a combination of additional schooling completed as part of the CCC and schooling obtained after discharge from the CCC.

[20] 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 (Table A.XV Column 7).

CCC reports indicate that 8% of men obtained additional schooling during the program. 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 account 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 three percent of participants moved in the short-term (by 1940 Census or WWII enrollment) and one more year of training increased this probability of moving by 5.7 percentage points, or 17% relative to the mean (Table VI Column 2 and Table A.XVI for all specifications). This is substantial, particularly during a period characterized by historically low migration nationwide.[21] 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 median county level mortality from 1950 to 1968 (Table VI Columns 3-4). Over the long run, however, most individuals moved and the effect of duration on mobility fades (Table VI Columns 5-7).

In sum, in the short run, job training had no impact on labor market outcomes. It did, however, improve health and increase schooling and geographic mobility. These short run impacts on health and human capital help explain the long run/lifetime impacts on earnings, disability, age at retirement and longevity. Overall, these findings are in line with the evidence from developing countries reported in Kluve et al. (2019)'s review: comprehensive programs like the CCC work better than programs that narrowly focus on a single skill, perhaps because they have benefits along many margins.

### D. Falsification Tests

We conduct one final 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, but we can also test if duration predicts these pre-intervention outcomes. The only pre-CCC outcome that duration predicts is education. Training duration does not predict height, weight or labor market activity prior to enrollment

[21] In the 1940 census, 12% of people report living in a different county than in 1935. https://www.census.gov/dataviz/visualizations/010/

(Table A.XVII). These results suggest that by and 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 using an instrumental variable and a novel control function approach that incorporates evidence from the JC RCT to address any remaining selection.

## VIII. Control Function Approaches: Incorporating Evidence from the Job Corp RCT to Address Selection Bias in the CCC

To further corroborate our findings, in this next section, we implement a recently developed approach to address selection that relies on a different set of assumptions and leverages information from existing randomized experiments.

### A. Overview

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. We apply and extend methods developed by Athey, Chetty, and Imbens (2020) that enable researchers to exploit data from RCTs to address potential bias in results based on observational data in a control function 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).[22] We address potential endogeneity of our CCC estimates using two approaches that make use of the JC RCT.

The two approaches require the JC to be an externally valid experiment for the CCC, either for the treatment effect directly or, more realistically, for the degree of selection bias. We first present comparisons of the CCC and JC (limiting the JC sample to men only). Then we proceed to describe our two approaches to correct for bias in the CCC estimates in greater detail and present our bias-corrected estimates. Finally, we quantify how relaxing the assumptions changes our estimates and discuss other limitations of this approach.

[22] 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."

*Comparing CCC and JC Enrollees.* Overall, JC and CCC participants are similar along important dimensions (Table A.XIX). Both are young (19 years old on average) and have relatively few years of schooling. JC participants have completed 10.2 years of schooling, compared with 8.6 for the CCC enrollees, and 19% have graduated from high school compared with 12% of the CCC enrollees. The CCC sample has considerably more Hispanic enrollees, due to the fact we concentrate on CO and NM, whereas the JC data is national.

Participants in the JC and CCC 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 at all. Conditional on training, the duration among the treated group in JC is 7.8 months, similar to the CCC. Reasons for leaving are also similar across the two programs (Figure II).[23] Finally, and perhaps most importantly, when we try to predict duration in the JC, we also find evidence of both positive and negative selection into duration. Specifically, we find that on average in both samples: 1) individuals who have ever worked previously serve shorter durations, 2) Hispanic enrollees serve longer, and 3) individuals with more years of schooling in CCC and with high school or GED degree in JC serve longer (Figure III).

Moreover, the JC and CCC enrollees are both disadvantaged *relative to* the general population at the time. For example, comparing the average education of the JC participants to men aged 16-24 in the 1990 census, JC participants have 1.8 fewer years of schooling, or 15 percent lower than the men 16-24 in the census (Table II). Comparing CCC enrollees to men in the 1940 census, they have completed 1.2 fewer years of schooling (13 percent lower). This pattern is true for other measures such as household income and employment. In the JC sample, 81% come from households with income below the 41st percentile, in the CCC, 63% do. JC and CCC men are also significantly more likely to be unemployed than the population of young men at the time—for the JC, they are six times more likely to be unemployed, and for the CCC, they are nearly five times more likely.

Even though CCC and JC samples are both relatively disadvantaged, to make the samples even more comparable, we construct a set of weights using the entropy balance approach in

[23] About 30% of JC enrollees complete the program, compared with 38% of the CCC. 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.

Hainmueller (2012) using the relative characteristics calculated for each sample. The results based on the reweighted JC sample using the calculated weights make the relative disadvantage between JC and CCC data even more similar, resulting in almost identical standard deviation differences in education, unemployment, and imputed Hispanic share (Table A.XX). We use both the unweighted and reweighted JC sample in our subsequent analyses.

*Comparing short-term effects.* We reproduce the short-run JC randomized evaluation results in Schochet et al. (2008) using the sample of males and examine additional outcomes. The results are very similar (Table VII). In the first column, we present estimates that compare the outcomes of those assigned to treatment to those of the control (the reduced form or ITT effect). 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. The 2SLS estimates represent a rescaling of the ITT estimates in the first column, obtained by dividing the ITT by the mean effect of the program on duration (0.487 years from Table A.XVIII). In the third column, we present 2SLS estimates using the reweighted JC sample to be more comparable to the CCC sample.

These estimates represent the causal effect of duration (under standard assumptions).[24] The columns 4-6 show OLS estimates for duration for JC (unweighted), duration for JC (reweighted) and for CCC, respectively. Overall, short term labor market outcomes in JC were more positive than in CCC where the impacts were insignificant. In both the CCC and the JC data, education and mobility increased significantly. The table also shows that the JC increased self-reported health, consistent with our findings that CCC improved health, measured by height and weight.

*Comparing longer-term effects.* The estimated longer-run effects of the CCC on income and disability are also similar to estimates obtained from the RCT of JC. Given that enrollees participated for 0.83 years on average, the effect of CCC on lifetime earnings is 4.6%. The latest evaluation of JC, which tracks individual tax records 20 years after the program (Schochet 2021), finds that participation in JC had a statistically insignificant increase in wages of 2.3%, with the CCC effects (4.6) well within their 95% confidence interval [-4.1%; 8.8%]. The evaluation also

[24] 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. Table A.XX presents balance tests of baseline characteristics of the JC applicants.

reports a 40% reduction in SSDI benefits among older JC participants, though not in the overall sample.[25]

In sum, participants in JC are similar in many dimensions to CCC participants and they experienced qualitatively similar long-run (20-year) 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 short-term labor market outcomes. We now formally describe our method of using short-run, experimental estimates from JC to adjust lifetime, 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 the long-run impact of the CCC. The first approach assumes that the true short-run causal treatment effect of the JC and the CCC is the same. The second assumes that selection into longer duration in the JC and the CCC is the same, an assumption for which we present supporting evidence.

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:

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

Where $y_{iS}^{ST}$ is the short-term outcome for individual $i$ in sample $S \in \{CCC, JC\}$; $W_{iS}$ is duration of training in program (either CCC or JC); $\tau_S^{ST}$ is the short-term treatment effect; $X_{iS}$ includes other controls; and $\alpha_{iS}^{ST}$ 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 $\alpha_{iS}^{ST}$ given the experimental nature of the data but correlated with training duration $W_{iJC}$. This allows us to correct for the endogeneity of duration 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:

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[25] We calculate the numbers using Treatment-on-the-treated (TOT) effect of W2 earnings at 2015 (Schochet 2021, Tables 2 and A2). 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.

*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:*

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

Assumption 2 is the key assumption. It requires that the source of the endogeneity *on duration* for the short-term and long-term outcomes in the CCC sample comes from a common, unobserved, component. If this component can be recovered, we could simply control for it in our analysis of the long-term outcome and obtain unbiased estimates of the treatment effect. Although it is not observed directly, we can recover it from the short-term outcomes if we can obtain an unbiased estimate of the short-term treatment effect, $\tau_{CCC}^{ST}$. The two approaches differ in how we arrive at the unbiased estimate of $\tau_{CCC}^{ST}$.

### 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 $\tau_{CCC}^{ST} = \tau_{JC}^{ST}$: the ST treatment effect obtained from the RCT of JC is an unbiased estimate of the ST effect in the CCC.

We make additional assumptions in this paper to account for the differences in settings. First, in Athey et al. (2020) the treatment is binary, whereas in our case it is continuous. This makes the assumption of exogeneity of treatment in the ST sample stronger here than in the binary treatment case. Secondly the experiment does not yield direct estimates of the effect of duration, so we employ 2SLS to obtain them. As a result, we need to make additional assumptions. In particular, the standard IV assumptions (relevance and exclusion restriction) must hold. See Supplementary Appendix C for details.

After obtaining an unbiased estimate of the JC's short-term effects ($\hat{\tau}_{JC}^{ST}$) using IV methods, we calculate the short-term CCC residuals, $\hat{\alpha}_{iCCC}^{ST} = Y_{iCCC}^{ST} - \hat{\gamma}X_{iCCC} - \hat{\tau}_{JC}^{ST}W_{iCCC}$ and include the residuals as control functions in the long-term regressions. If Assumption 2 holds and JC's short-term effect is an unbiased estimate of the CCC's short-term effect (so that $\hat{\alpha}_{iCCC}^{ST}$ consistently estimates $\alpha_{iCCC}^{ST}$), then the coefficient on duration estimated including the control functions gives us the LT causal effect of the CCC. Note that these residuals can only be computed for the sample of participants for whom we observe both short-term and lifetime outcomes.

The control function estimates for our long-run outcomes are presented in Table VIII.[26] For comparison, we present OLS results for the same sample in Panel A—reassuringly, despite the sample size reductions, these estimates are very similar to our main estimates. We report the control function results using both the unweighted and reweighted JC sample in Panel B. The LT estimates of the effect of duration on our long-term outcomes are unaffected by the inclusion of control functions for all samples and specifications, including the specification using the reweighted JC sample to compute the control function. This again suggests any bias in the OLS estimates is small.

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 and the CCC, not on the treatment effect in JC alone.[27] Instead, these results can reflect that 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. However, the results are similar if we use other ST outcomes to compute the control functions or include different sets of controls making c) unlikely (Table A.XXI).

We consider these results encouraging, but they 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 less so for labor market outcomes. The returns to training, like the returns to other human capital investments, are 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). Labor demand for low-skilled laborers in the post WWII period was stronger than it was in the economy of the early 2000s, which had stagnant wages for low-income groups (Piketty,

[26] We present the specification based on education as our ST outcome, which maximizes our sample size, in the main tables. However, we consider multiple short-run residuals (since we have multiple short-run outcomes) and our results are robust to their inclusion.

[27] 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 can still generate a change in the estimated coefficients.

Saez and Zucman 2018) and low-skill workers (Autor, Katz and Kearney 2008). This suggests that the assumption of constant treatment effects over time and place might be too restrictive for labor market outcomes. In the next section we consider a second approach that relies on what we argue is a more realistic assumption: that selection is similar across settings.

### D. Second Approach: Assuming Equivalent Selection into Longer Duration in 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. Instead, we assume that the selection bias into longer training duration is the same in JC and CCC. Our starting point is the assumption that the two samples share key features, most importantly that the choice in the observational CCC sample to participate in the program depends on the same unobserved components as the decision to comply with the assignment to the program in the JC experiment. If we view this decision in both cases as motivated by individual specific preferences, it is plausible that these unobserved components are closely related.

In our case and based on economic theory we can hypothesize that the more educated will have greater outside options (more offers in the labor market), which would lead them to stay in the training program for shorter periods of time. Similarly, poor individuals will likely benefit more from the income that the program provides, leading them to remain in the program for longer. Both of these considerations would apply in the CCC and in the RCT.

Stated another way, the key assumption is that the bias in the OLS estimates in the JC sample, as estimated by the difference between the JC's OLS and 2SLS estimates, is the same as the bias in the OLS estimates in the CCC sample. Formally, this requires an additional linearity assumption, namely that $\hat{\mu} = \hat{\tau}^{ST}_{JC,OLS} - \hat{\tau}^{ST}_{JC,2SLS}$ is a consistent estimator of the selection bias in both the CCC and JC.

We argue that those who stayed in the program for extended periods of time likely had similar motives. The following set of facts support this assumption: 1) the mean duration and the distribution of reasons for dismissal are similar, 2) the observables predict duration in a similar fashion, and 3) the enrollees in the JC and CCC samples exhibit similar patterns in observable characteristics and relative disadvantage compared to their contemporary peers.

We estimate the selection bias by taking the difference between the OLS and IV estimates from JC: $\hat{\mu} = \hat{\tau}_{JC,OLS}^{ST} - \hat{\tau}_{JC,2SLS}^{ST}$. The estimate represents the omitted variable or selection bias in JC, which in turn is an estimate of the CCC selection bias under the above assumption. Then, we use our adjusted OLS estimate of CCC short-term outcomes ($\hat{\tau}_{CCC}^{ST} = \hat{\tau}_{CCC,OLS}^{ST} - \hat{\mu}$) 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. See Supplementary Appendix C for the detailed procedure.

We present the results for the long-run outcomes using the control function calculated from our short-run education outcome (Table VIII Panel C). Again, we find that regardless of the control functions that we include and whether we reweight to make samples comparable in terms of relative disadvantage, the estimates are similar to the original OLS estimates, suggesting little bias in our LT estimates of the effects of training (Table A.XXII).

### E. Limitations

Athey et al. (2020) present nonparametric identification results, with key assumptions including a restriction on the dimension of the unobserved selection component and monotonicity of the outcome in these unobserved components. In our application, we extend this by estimating effects of interest using instrumental variables, and in doing so we augment their assumptions with conventional IV assumptions, which include some parametric components. In addition, in the second approach, we assume that the bias resulting from selection is additively linear, a standard formulation of the omitted variable bias.[28]

We can quantify how violations of some of the assumptions in each approach change the estimated coefficient of the long-run effect. 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 the case of the first approach, $\phi$ is the difference between JC and CCC short-run treatment effects. In the second approach, $\phi$ stems from the difference in the

[28] Although making such assumptions is unavoidable, we can change some of the assumptions we make in the implementation of the approach: for example, instead of selection bias being additive, we could assume it is proportional.

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

We present the "bias" terms as symmetric bounds around the estimate in the last rows of Table VIII Panels B and C. These bias terms can be multiplied by a desired amount of the relevant percentage difference in either 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.

Finally, we have so far assumed that the long-run treatment effects are homogenous. However, we can allow for some forms of treatment effect heterogeneity (see Supplementary Appendix C for more details).[29] To investigate the presence of heterogeneity in the long-term outcomes, we estimated models where the control function is interacted with the duration. We report the average treatment effect (ATE) from these specifications in Table A.XXIII and Table A.XXIV. We find that the ATEs that allow for heterogeneity are in fact very similar to our main effects. We can also account for unobserved heterogeneity in the short-term outcomes by exploiting the presence of multiple short-term outcomes so that we can recover two (or more) residuals that affect the primary outcome. We can then control for and/or interact these multiple residuals with the duration in the long-term equation. These results are shown in panels C and D of Table A.XXI and Table A.XXII (not interacted) and Table A.XXIII and Table A.XXIV (interacted) and again our estimate of the ATE remains unchanged. We conclude that our estimates of the long term effects of the program are largely unbiased.

[29] We cannot allow for completely unrestricted heterogeneity at the individual level. For example, if we allow for an $i$ subscript on $\tau_S^{ST}$, treatment effect heterogeneity will lead the residual from a projection of $Y_{iS}^{ST}$ on $W_{iS}$ to exhibit a variance that depends positively on $(W_{iS} - \overline{W}_S)^2$. In this case the estimator would no longer be consistent for the average effect. Endogenous heteroskedasticity of this nature is a common problem in many empirical applications. If the heterogeneity is limited one would expect the biases to be modest.

## IX. Discussion

The CCC was a comprehensive paid employment and training program for unemployed youth during the deepest economic recession in US history. While existing work has examined the impact of New Deal programs on aggregate economic outcomes, this represents the first effort to estimate lifetime impacts on program participants. We find that the program benefitted participants over their entire lifetimes.

What broader lessons does this historical evaluation offer? We conclude by discussing two ways of thinking about the value of this program and other lessons that are generalizable to the present.

First, we consider whether the program fully compensated the participants for the losses they suffered as a result of the Great Depression. Similar to other New Deal Programs, the CCC was intended to help recipients during a severe economic crisis. Did the program succeed? To answer this, we make use of the findings from Schwandt and von Wachter (2020) who investigated the impact of the 1982 recession on longevity and earnings to estimate the cost of recessions over the lifetime. They report that a change in the unemployment rate of 3.9 percentage points lowered longevity of young adults entering the labor market by 6-9 months and find that a 1% increase in the unemployment rate at the time of entry into the labor market lowers middle age incomes by 1.3%.

Our results suggest that those who participated in the program for one more year lived 0.96 years longer [CI 0.5-1.4] and their earnings were 5.2% higher [CI 2.3-8.1]. While these benefits are substantial, they suggest that the program may not in fact have fully compensated affected youth. Unemployment rates during the Great Depression have been estimated to have risen to a high of 25 percentage points, from a pre-depression baseline of around 5 or less.[30] If the Great Depression increased unemployment by 20 percentage points, we would predict an average loss of lifetime income of 26% and a decline in longevity of 3.4 years.[31] Assuming linearity and using the upper bounds for our estimates, only those that participated in CCC for at least 3.5 years, far greater than what was allowed, would have been fully compensated.

[30] Unemployment rates are not well measured prior to 1940, see Card (2011).
[31] The computation for income assumes that middle age income is a good proxy for lifetime income. Prior literature finds support for this assumption (Chetty et al. 2014).

However, modern recessions have resulted in much smaller increases in unemployment, suggesting that such a program would compensate enrollees. For example, these gains would in fact completely offset the losses of youth entering the labor market in the 1982 recession. Thus, paid employment programs may serve as a way to protect youth who do not qualify for countercyclical programs like Unemployment Insurance.

An alternative way to assess the value of this program is to assess if it was worth the cost, and in particular, to determine how our ability to estimate long-term gains changes our assessment of a program's value. To answer this question, we calculate the Marginal Value of Public Funds (MVPF) following the approach by Hendren and Sprung-Keyser (2020).[32] It's worth noting that the MVPF we compute based on our results alone likely misses additional benefits and certain costs. The program likely benefited not just enrollees, but also their families—parents, siblings, and potentially their extended family (Kugler et al. 2015) and children—and the communities and the landscape where the CCC operated. Our calculations also miss potential negative spillover and general equilibrium effects of such a large labor mobilization program. Though we think spillovers are likely minimal in the context of the CCC because of the unusually high unemployment at the time and the placement of CCC camps far from existing labor markets, one should still consider them (Crépon et al. 2013).

We estimate an MVPF of 6.0 when we include the willingness-to-pay for increases in longevity, disability reductions, and increases in claiming ages, well above 1. If we only include earnings gains and exclude mortality reductions, the MVPF is 2.5, still significantly greater than 1, but below 6.[33] If we used the short-term impacts only, the MVPF would be below 1. This

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[32] The main approach consists of estimating the present discounted value of cost and benefits of the program, including their effects on government revenues (for details see Supplementary Appendix D). The CCC program costs include 1) the upfront cost of the program estimated by Levine (2010) to be $1,004 per enrollee per year in 1939 dollars ($14,384 in 2017 dollars); and 2) increases in social security payouts from enrollees both living longer and having increased PIA. These costs are mitigated by: 1) the tax increases of $6,965, resulting from the increased earnings, which we calculate by applying a tax rate of 33.6% to the AIME; 2) a decrease in SSDI payout of $911 resulting from lower claiming rates and the increase in claiming age; and 3) a decrease in Social Security payout of $733 resulting from the increase in retirement age. Additionally, the program benefits include 4) the willingness to pay (WTP) for the increase in survival rates, which we value using a statistical value of a year of life of $150,000, a middle range estimate across studies (Keller et al. 2021), 5) an increase in after-tax earnings of $13,642, which is simply the post-tax portion of pre-tax income gains; 6) the monthly real wage of $66.25 recipients obtained while enrolled (BLS 1941), which includes benefits such as room and board, multiplied by average duration and computed in 2017 dollars, $11,390; and finally, 7) the decrease in benefit from loss of SSDI income of $911.

[33] 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 2020)—we ignore these. Families received transfers, which could have benefitted them but also potentially distorted their behaviors.

contrasts with the MVPF of less than one calculated for JC participants in Hendren and Sprung-Keyser (2020).[34] The key difference is due to our ability to look at *lifetime* effects on *multiple* outcomes, particularly health and longevity.

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. In our case, our ability to look at multiple outcomes over the long run, and in particular our ability to consider health impacts, vastly increases the assessed value of the program. However, these benefits are hard to assess in the short-term—we do not find any economically or statistically significant impacts on survival until after age 55 and only find effects on income over the long term. Overall, our findings confirm Krueger's view in Heckman and Krueger (2005) that the benefits of training are in fact similar to the benefits of education, and like education, the resulting benefits may only manifest over the long term. An important question for future research is whether there exist short-term markers that can be used to predict these long-term gains. If so, this would provide policy makers, who are often constrained by short-term budget and other considerations, with better evidence regarding likely lifetime benefits.

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[34] 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.

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Table I

Summary Statistics of the Civilian Conservation Corps (CCC) Participants

| | Mortality Sample | | | Social Security Sample | | |
|---|---|---|---|---|---|---|
| | N | Mean | Std Dev | N | Mean | Std Dev |
| **Service Characteristics** | | | | | | |
| Duration of service (yrs) | 17,639 | 0.826 | 0.708 | 12,455 | 0.816 | 0.701 |
| First allottee amount (dollars per month) | 17,088 | 21.67 | 3.721 | 12,097 | 21.70 | 3.683 |
| **Characteristics in Enrollment Application** | | | | | | |
| Age at enrollment | 17,449 | 18.73 | 2.170 | 12,330 | 18.74 | 2.242 |
| Enrollment year | 17,639 | 1,939 | 1.894 | 12,455 | 1939 | 1.889 |
| Hispanic (imputed using hispanic index) | 17,639 | 0.451 | 0.498 | 12,455 | 0.432 | 0.495 |
| ***Additional information in CO records*** | | | | | | |
| Highest grade completed | 11,235 | 8.674 | 2.081 | 8,225 | 8.700 | 2.055 |
| Household size excluding applicant | 6,283 | 4.763 | 2.591 | 4,730 | 4.725 | 2.575 |
| Live on farm? | 6,460 | 0.253 | 0.435 | 4,846 | 0.252 | 0.434 |
| Height (Inches) | 6,475 | 67.88 | 3.083 | 4,860 | 67.92 | 3.053 |
| Weight (100 pounds) | 6,561 | 1.390 | 0.172 | 4,922 | 1.391 | 0.171 |
| Underweight | 6,461 | 0.0689 | 0.253 | 4,849 | 0.0685 | 0.253 |
| Father Living | 6,339 | 0.803 | 0.398 | 4,765 | 0.806 | 0.396 |
| Mother Living | 6,391 | 0.855 | 0.352 | 4,808 | 0.855 | 0.352 |
| **Match Rates^** | | | | | | |
| Matched to 1940 Census | 23,722 | 0.449 | 0.497 | 12,455 | 0.487 | 0.500 |
| Matched to 1940 Census Among Serving Before 1940 | 9,890 | 0.433 | 0.496 | 5,151 | 0.483 | 0.500 |
| Matched to WWII records | 23,722 | 0.306 | 0.461 | 12,455 | 0.347 | 0.476 |

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. Social Security Sample are those we can match to the Master Beneficiary Records (MBR). 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. ^ Match Rates are calculated over the full analytical sample.

Table II

Comparison of Jobs Corps (JC) and Civilian Conservation Corps (CCC) Samples to Contemporary Censuses

| | JC | | CCC | | |
|---|---|---|---|---|---|
| Sample | 1990 census | JC participants | 1940 census, national | 1940 census, NM+CO | CCC participants |
| Education | | | | | |
| Mean | 11.97 | 10.16 | 9.84 | 9.89 | 8.60 |
| Standard Deviation | 2.23 | 1.54 | 2.66 | 2.62 | 1.65 |
| Non-White Share | | | | | |
| Mean | 0.30 | 0.75 | 0.12 | 0.20 | 0.52 |
| Standard Deviation | 0.46 | 0.43 | 0.32 | 0.40 | 0.50 |
| Unemployed | | | | | |
| Mean | 0.13 | 0.79 | 0.17 | 0.19 | 0.91 |
| Standard Deviation | 0.33 | 0.41 | 0.38 | 0.39 | 0.17 |
| Farm | | | | | |
| Mean | 0.01 | 0.03 | 0.27 | 0.30 | 0.25 |
| Standard Deviation | 0.12 | 0.16 | 0.44 | 0.46 | 0.25 |
| Household Size | | | | | |
| Mean | 3.34 | 4.47 | 4.80 | 4.72 | 4.74 |
| Standard Deviation | 1.92 | 2.04 | 2.56 | 2.71 | 2.60 |
| Personal Income (Conditional on Working)* | | | | | |
| Mean | 9711.78 | 2078.08 | 582.84 | 522.58 | 189.74 |
| Standard Deviation | 9074.64 | 2469.42 | 499.99 | 482.96 | 220.66 |
| Household Income Share Below National 41th Percentile** | | | | | |
| Mean | 0.41 | 0.81 | 0.41 | 0.49 | 0.63 |
| Standard Deviation | 0.49 | 0.39 | 0.49 | 0.50 | 0.48 |

Notes: We compare characteristics of JC and CCC participants to men aged 16-24 in their contemporary Censuses (1990 for JC and 1940 for CCC). Normalized Difference is (Mean Characteristic of JC or CCC - Mean of contemporary Census) / SD of Census. * For CCC, personal income are sourced from enrollees matched to the 1940 Census and calculated for those who serve after 1940. ** Household income is calculated by summing the wage income of all household members. This is done ensure consistency across 1990 and 1940 Censuses. We use 41th Percentile as a meaningful comparison cutoff point since, in the JC Baseline Survey, around 81% responded that their household income was less than $18,000, which is the 41th percentile of household income in 1990 calculated using our measure of household income from the Census.

Table III

Effect of CCC Service Duration on Longevity and Lifetime Labor Market Outcomes

| | (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 Death Age | 73.62 | 73.62 | 73.62 | 73.62 | 73.62 | 73.62 | 73.30 |
| Oster Bounds | [0.0127, 0.0136] | | | | | | |
| **Panel B: Average Indexed Monthly Earnings (MBR sample claimed 1979 and later)** | | | | | | | |
| Duration of service (yrs) | -0.083 | 67.048*** | 62.791*** | 62.450*** | 56.717*** | 50.134*** | 48.707*** |
| | (10.181) | (12.186) | (12.501) | (12.616) | (13.723) | (14.690) | (17.236) |
| 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 AIME | 963.62 | 963.62 | 963.62 | 963.62 | 963.62 | 963.62 | 1010.70 |
| Oster Bounds | [23.30; 143.34] | | | | | | |
| **Panel C: Retirement or Social Security Disability Insurance (SSDI) claiming age** | | | | | | | |
| Duration of service (yrs) | 0.506*** | 0.509*** | 0.452*** | 0.462*** | 0.427*** | 0.401*** | 0.554*** |
| | (0.065) | (0.086) | (0.089) | (0.089) | (0.097) | (0.107) | (0.124) |
| 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 Claiming Age | 60.27 | 60.27 | 60.27 | 60.27 | 60.27 | 60.27 | 60.43 |
| Oster Bounds | [0.220; 0.462] | | | | | | |
| **Panel D: SSDI Claiming (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) | (0.009) | (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 SSDI | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 | 0.21 | 0.20 |
| Oster Bounds | [-0.0304; -0.0181] | | | | | | |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment 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 V. MBR = Master Beneficiary Records.

Table IV

Instrumental Variable Estimates of the Effect of CCC on Longevity and Lifetime Labor Market

| Outcome: | Log age at death | Average Indexed Monthly Earnings (AIME) | Retirement or disability claiming age | Social Security Disability Insurance |
| --- | --- | --- | --- | --- |
| IV | 0.013 | 287.937* | 1.189 | 0.018 |
| | (0.038) | (169.308) | (1.574) | (0.114) |
| OLS | 0.014*** | 42.574** | 0.511*** | -0.023* |
| | (0.004) | (20.402) | (0.143) | (0.013) |
| First Stage | -0.165*** | -0.167*** | -0.162*** | -0.170*** |
| | (0.022) | (0.024) | (0.025) | (0.024) |
| F-stat | 55.11 | 47.42 | 42.08 | 48.67 |
| Observations | 9,049 | 5,529 | 6,169 | 5,474 |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample is restricted only to those that died after age >= 45 and those who were dismissed after end of term or for the convenience of the government. Our instrumental variable (IV) is whether the enrollee was dismissed for convenience of the government. We present the 2-stage least squares (2SLS) intsrumental variable regression's coefficient on duration, OLS regression coefficient on duration, first stage coefficient on our instrument from regression of duration on the instrument, and F-statistic on the instrument from the first stage. Controls include birth and County-Year-Quarter of Enrollment fixed effects, individual controls, camp characteristics, such as distance from nearest city and average temperature, peer characteristics, where peers are defined as other enrollees serving in the same camp at the same time. We do not include camp fixed effects because they are highly predictive of camp closures -- the results are similar with camp fixed effects but the standard errors are even larger. For complete list of controls, refer to text or Appendix Table V.

Table V

Effect of CCC Service Duration on Labor Market Outcomes Observed in the 1940 Census and WWII Records

| | (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) | Body Mass Index | Education (yrs) |
| Duration of service (yrs) | 0.012 | 0.018* | 0.316 | -14.977 | -0.015 | 0.038*** | 1.143*** | 1.017*** | 0.169*** |
| | (0.012) | (0.010) | (1.194) | (26.394) | (0.062) | (0.007) | (0.221) | (0.204) | (0.040) |
| Observations | 9,518 | 4,052 | 2,361 | 2,149 | 1,750 | 22,964 | 5,770 | 5,287 | 9,586 |
| Mean Dependent Variable | 0.43 | 0.91 | 27.88 | 383.71 | 471.25 | 0.31 | 67.55 | 21.53 | 9.23 |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment in parentheses, *** p<0.01, ** p<0.05, * p<0.1. This table only displays specification in Column (6) of Table III 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 XIV and XV.

Table VI
Effect of CCC 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*** | 0.045** | -0.043* | -0.029** | 0.003 | 0.006 |
| | (0.007) | (0.011) | (0.020) | (0.023) | (0.015) | (0.012) | (0.015) |
| Observations | 9,568 | 9,568 | 3,165 | 3,175 | 7,235 | 7,079 | 5,313 |
| Mean Dependent Variable | 0.09 | 0.33 | 0.65 | 0.38 | 0.5 | 0.79 | 0.25 |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment in parentheses, *** p<0.01, ** p<0.05, * p<0.1. This table only displays specification in Column (6) of Table III 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 XVI.

Table VII
Comparing the Effects of the CCC to the Effects of Job Corps

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Jobs Corps Data | | | | | CCC |
| | Randomized Control Trial | | | OLS | OLS, Weighted | OLS |
| | Coefficient on Treatment Dummy (ITT) | 2SLS Instrument Duration with Treatment | 2SLS, Weighted | Coefficient on Duration (years)+ | Coefficient on Duration (years)+ | Coefficient on Duration (years) |
| **Years of schooling** | 0.184*** | 0.393*** | 0.320** | 0.360*** | 0.333*** | 0.169*** |
| | (0.039) | (0.084) | (0.130) | (0.041) | (0.075) | (0.040) |
| Observations | 6,280 | 6,280 | 6,013 | 3,407 | 3,341 | 9,620 |
| **Employment (in week of the survey)^** | 0.026** | 0.056** | 0.088** | 0.060*** | 0.085*** | -0.015 |
| | (0.013) | (0.027) | (0.040) | (0.015) | (0.020) | (0.022) |
| Observations | 6,022 | 6,022 | 5,782 | 3,285 | 3,227 | 3,684 |
| **Weeks worked in previous year** | 1.615*** | 3.443*** | 3.635** | 2.629*** | 3.025*** | 0.265 |
| | (0.536) | (1.142) | (1.741) | (0.610) | (0.718) | (1.199) |
| Observations | 6,235 | 6,235 | 5,971 | 3,382 | 3,316 | 2,360 |
| **Total Annual Earnings in previous year** | 969.765*** | 2,083.466*** | 1,999.766* | 1,055.435*** | 509.052 | -14.497 |
| | (280.804) | (603.598) | (1,041.844) | (336.311) | (576.113) | (26.389) |
| Observations | 6,081 | 6,081 | 5,835 | 3,317 | 3,257 | 2,148 |
| **ln(Earnings) \| weeks worked>0** | 0.038 | 0.080 | 0.126 | 0.078** | 0.027 | -0.014 |
| | (0.027) | (0.057) | (0.103) | (0.031) | (0.055) | (0.062) |
| Observations | 5,009 | 5,009 | 4,805 | 2,753 | 2,698 | 1,749 |
| **Moved^^** | 0.018* | 0.038* | -0.017 | 0.060*** | 0.028 | 0.057*** |
| | (0.011) | (0.023) | (0.036) | (0.014) | (0.021) | (0.011) |
| Observations | 6,301 | 6,301 | 6,032 | 3,419 | 3,348 | 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 | 7.8 | | | | | |
| Individual controls? | No | No | | Yes | Yes | Yes |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment for CCC and site-by-year-quarter for Jobs Corps in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Jobs Corps sample is males only. + Sample includes all treated, including those with zero duration. Controls include year and quarter of baseline, year and quarter of 48-month 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 VIII

Long-term Estimates of the Effects of CCC Using Education Control Functions for Identification

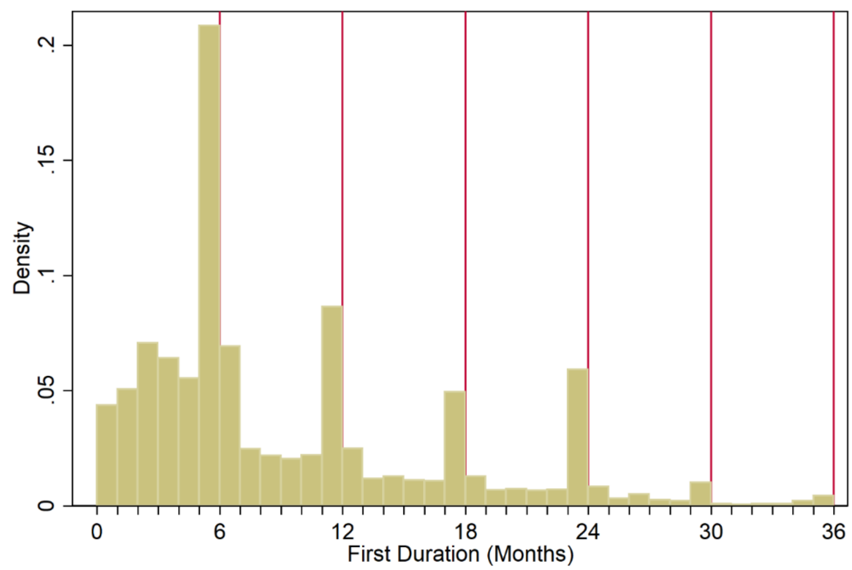
| | (1) | (3) | (4) | (5) |
|---|---|---|---|---|
| *Dependent Variable:* | *Log Death Age* | *Average Indexed Monthly Earnings* | *Retirement or Disability Claiming Age* | *Social Security Disability Insurance* |
| **Panel A: OLS Without Control Functions** | | | | |
| Duration of service (yrs) | 0.013*** | 47.882** | 0.509*** | -0.02 |
| | (0.004) | (21.416) | (0.189) | (0.014) |
| **Panel B: Control Function Approach 1 (Athey et al 2020)** | | | | |
| ***Unweighted*** | | | | |
| Duration of service (yrs) | 0.013*** | 52.363** | 0.418*** | -0.022 |
| | (0.004) | (21.640) | (0.142) | (0.014) |
| Bounds to account for assumption violations^ | ±7.22E-05 | ±1.152 | ±6.14E-03 | ±5.78E-04 |
| ***Reweighted*** | | | | |
| Duration of service (yrs) | 0.013*** | 50.230** | 0.407*** | -0.021 |
| | (0.004) | (21.512) | (0.141) | (0.014) |
| Bounds to account for assumption violations^ | ±1.34E-04 | ±2.13E+00 | ±1.14E-02 | ±1.07E-03 |
| **Panel C: Control Function Approach 2 (This Paper)** | | | | |
| ***Unweighted*** | | | | |
| Duration of service (yrs) | 0.013*** | 46.809** | 0.388*** | -0.019 |
| | (0.004) | (21.391) | (0.142) | (0.014) |
| Bounds to account for assumption violations^ | ±5.75E-07 | ±0.000 | ±3.66E-07 | ±5.96E-07 |
| ***Reweighted*** | | | | |
| Duration of service (yrs) | 0.013*** | 45.816** | 0.382*** | -0.019 |
| | (0.004) | (21.377) | (0.142) | (0.014) |
| Bounds to account for assumption violations^ | ±9.31E-05 | ±1.39E+00 | ±8.34E-03 | ±7.32E-04 |
| Observations | 7,722 | 4,613 | 4,575 | 4,575 |

Notes: Standard errors clustered at the level of county-by-year-quarter of enlistment 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, Average Indexed Monthly Earnings (AIME), retirement or disability claiming age, and Social Security Disability Insurnace (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.

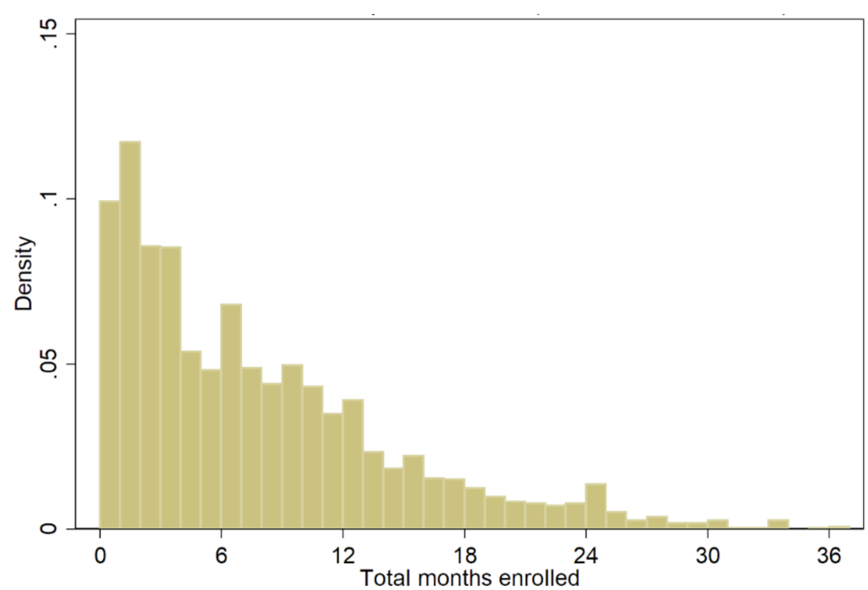
Figure I

Distribution of Service Duration in the Civilian Conservation Corps (CCC) Records and Jobs Corps

Panel A: CCC




Panel B: Jobs Corps

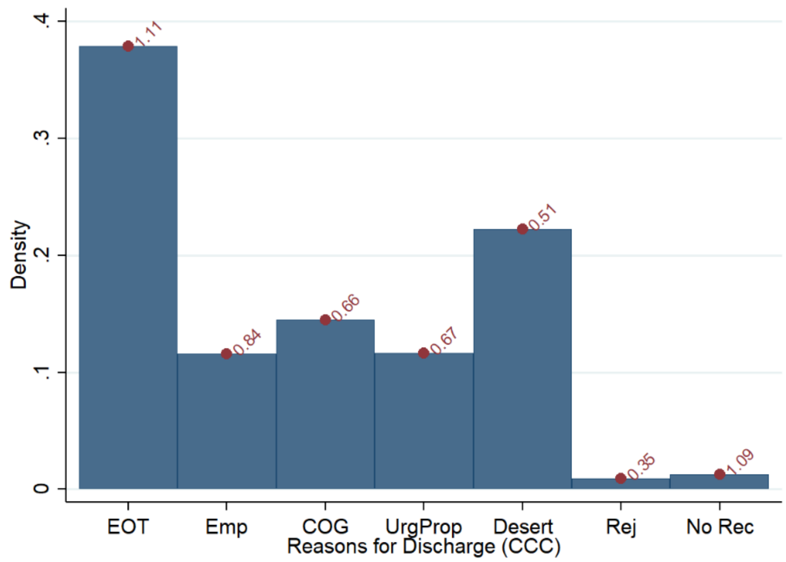



Notes: We exclude durations greater than 3 years (less than 1% of the observations) in this figure. Mean duration in this sample is 9.44 months (s.d. 7.47) for CCC and 5.8 months (s.d. 6.6) for Jobs Corps. In Panel B, we exclude individuals who were not assigned to treatment and therefore have 0 duration.

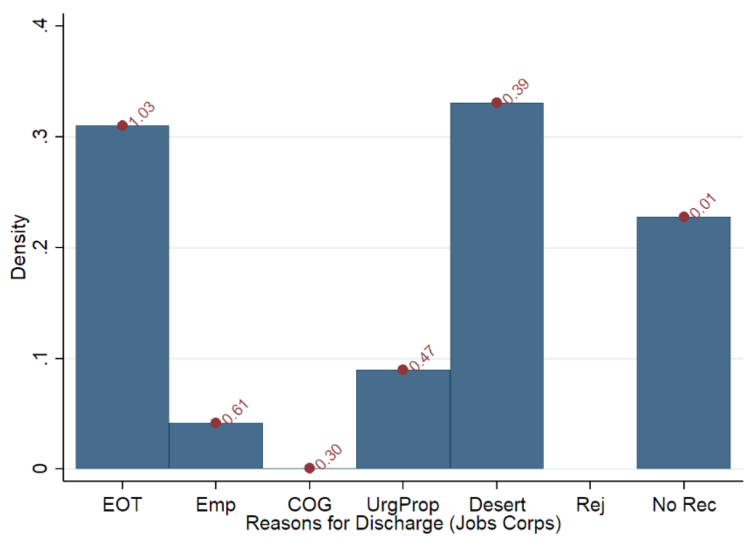
Figure II

Distribution of Reason for Discharge

Panel A: Civilian Conservation Corps (CCC)




Panel B: Jobs Corps

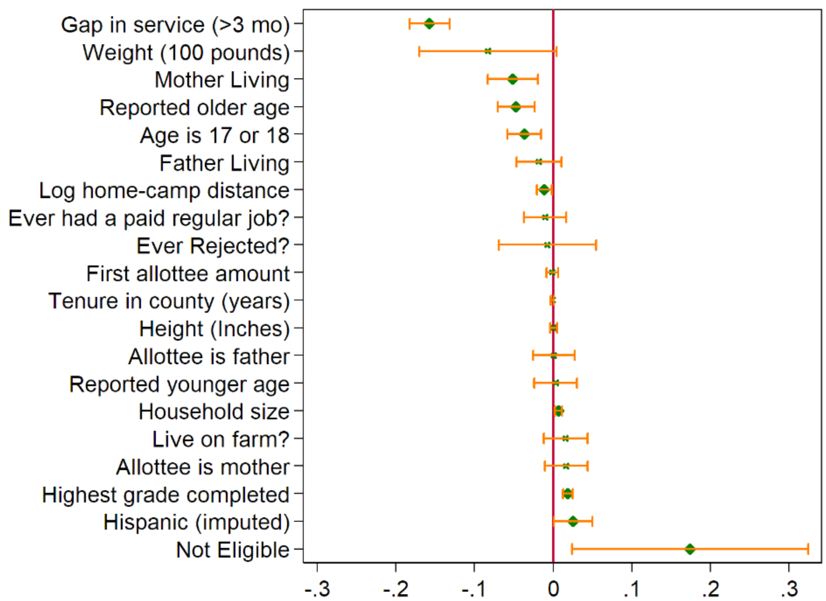



Notes: Values on top of the bar graph are mean duration (in years) for each category: EOT (End of Term), Emp (employment outside the program), COG (Convenience of the Government), UrgProp (Urgent and Proper Call), Desert, Rej (Rejected), No Rec (No record). Reasons for Jobs Corps was harmonized to match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

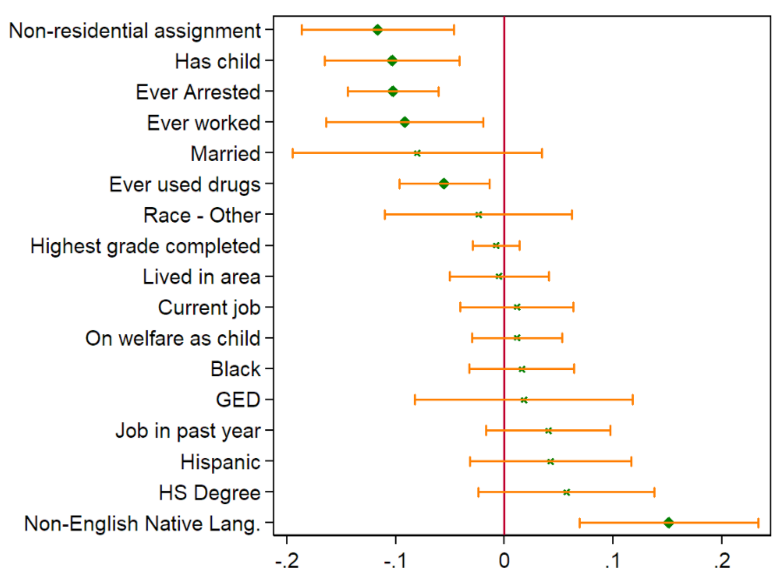
Figure III

Determinants of Duration

Panel A: Civilian Conservation Corps (CCC)




Panel B: Jobs Corps

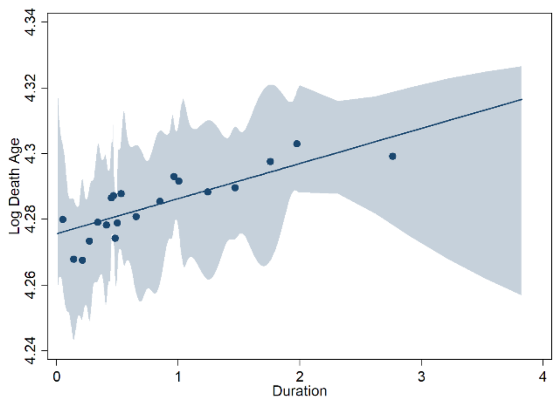



Notes: Estimates and 95% confidence intervals plotted for coefficient estimates on selected variables from regressing duration on various individual, camp, and peer characteristics. Coefficients in diamond are statistically significant at the 95% level. Mean duration for the estimation sample is 0.84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

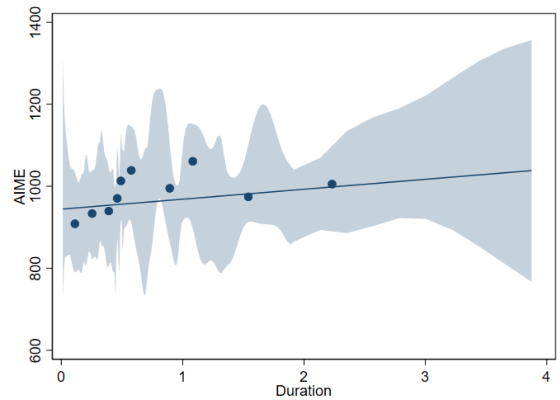
Figure IV

Binscatter Plots of Long-Term Outcomes

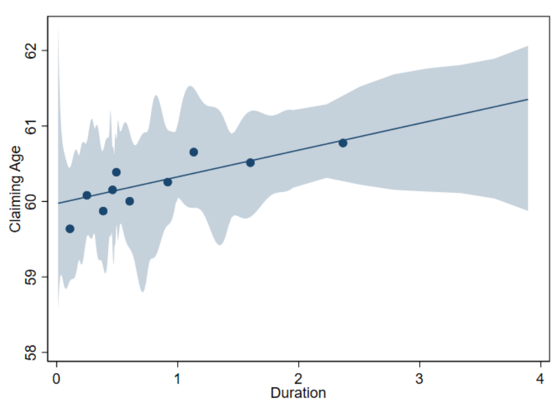
Panel A: Log Death Age



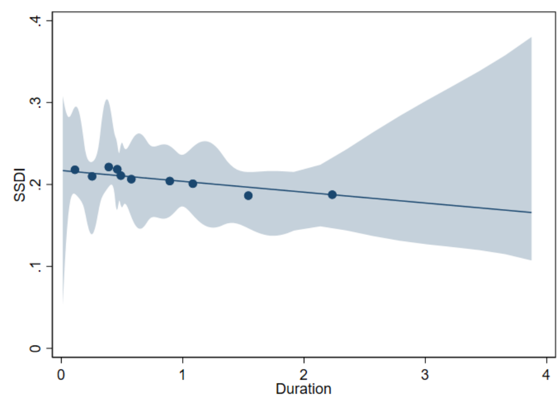

Panel B: Average Indexed Monthly Earnings




Panel C: Retirement or Disability Claiming Age




Panel D: Social Security Disability Insurance Claiming

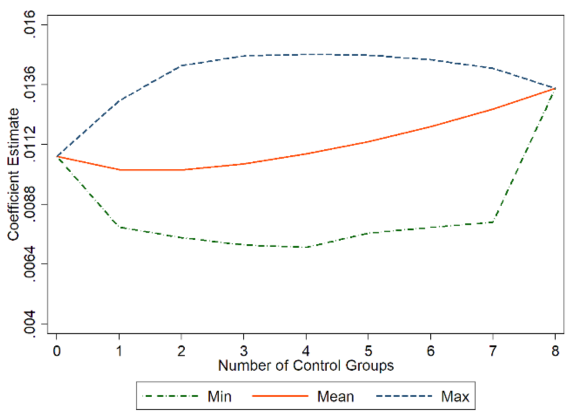



Notes: Authors computation based on death records (Panel A) and/or administrative program data matched to the Master Beneficiary Records (Panels B-D) and using the binscatter methodology of Cattaneo et al. 2023. It plots each variable controlling for birth year. We pick the number of bins (20 for Panel A, 10 for Panels B-D) and implement direct-plug-in data-driven choice of the optimal degree of polynomial and smoothness constraints for both confidence band and bin means.

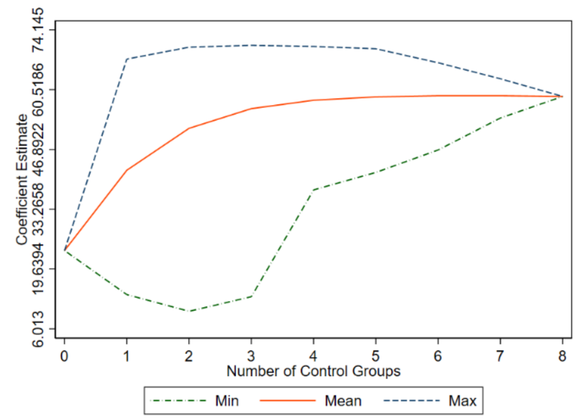
Figure V

Visualizing Sensitivity to Controls

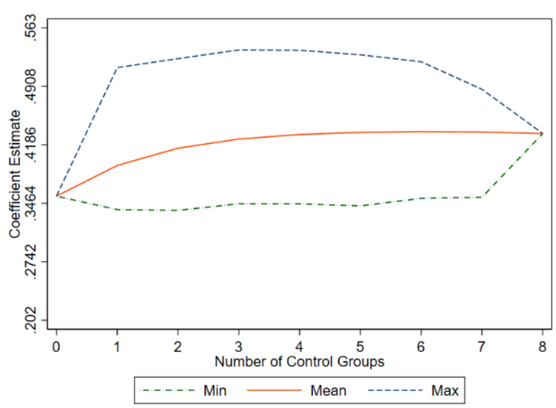
Panel A Log Death Age



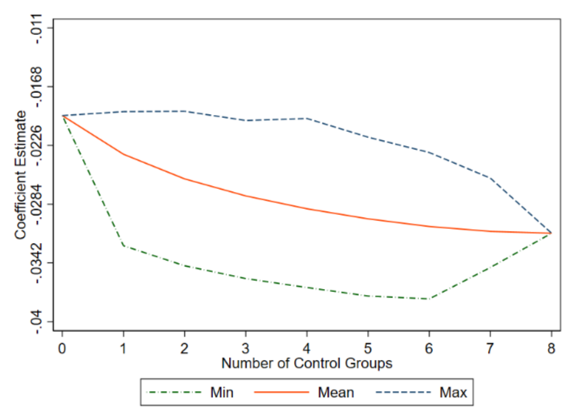

Panel B Average Indexed Monthly Earnings




Panel C Retirement or Disability Claiming Age



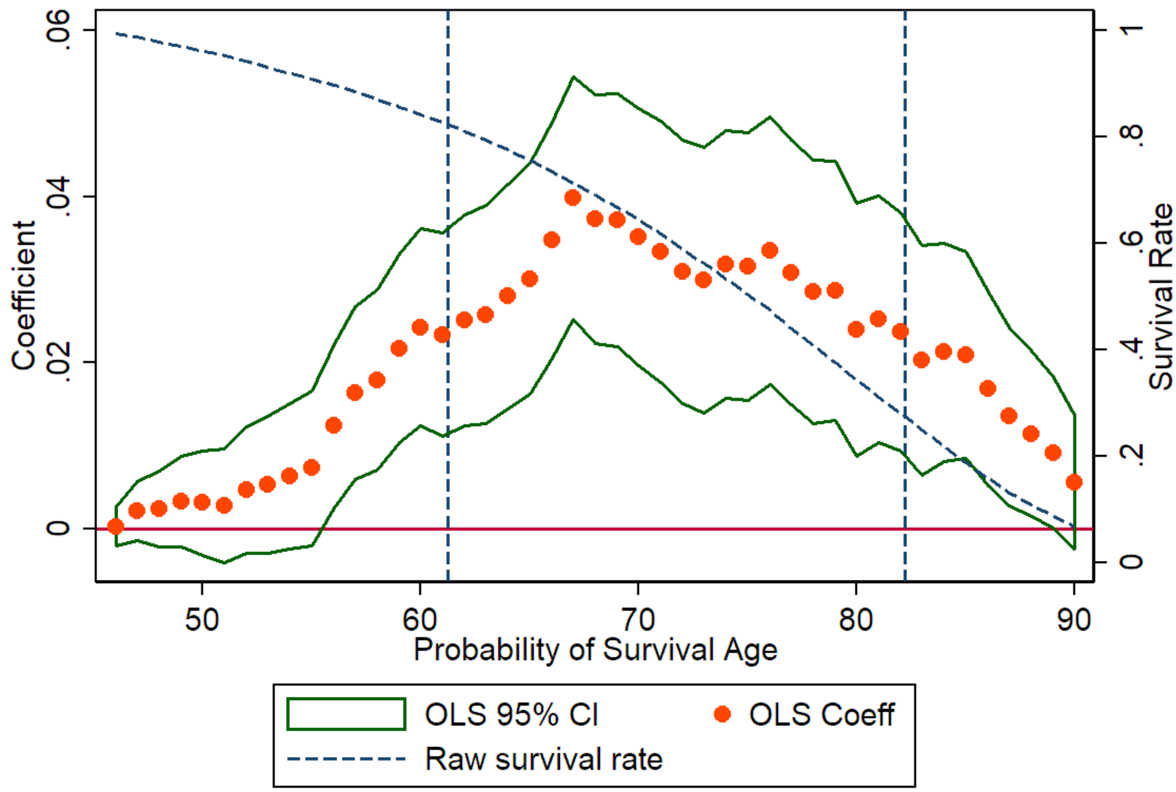

Panel D Social Security Disability Insurance Claiming




Note: We follow the methodology of Chan, Gentzkow, and Yu (2022) to visualize the sensitivity of our coefficient estimates on the choice and order of covariate inclusion. We split our covariates into 8 groups and run the OLS regression using every combination of covariate groups, varying the number of groups that are included in total. We exclude cohort (birth year) fixed effects from the 8 groups and include them in all specifications. The x-axis represents the number of covariate groups to be included, with 0 only including cohort fixed effects and 8 including all covariate groups. For example, for 4 control groups, the number of specifications would be 8 choose 4 = 70. The y-axis shows the average coefficient estimate (solid line) as well as minimum and maximum estimates (dotted/dashed lines).

Figure VI

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 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 III for details). On the right y-axis we plot the survival rate.