



# Health Profession Opportunity Grants (HPOG 2.0) Short-Term Impact Report

National and Tribal Evaluation of the  
2<sup>nd</sup> Generation of Health Profession  
Opportunity Grants (HPOG 2.0)

OPRE Report 2022-37



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Jacob Alex Klerman, David Ross Judkins, Sarah Prenovitz, and Gretchen Locke, Abt Associates

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Nicole Constance and Lisa Zingman, Contracting Officer's Representatives  
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Project Director: Gretchen Locke

Principal Investigators: Jacob Alex Klerman and Thomas Konrad

Abt Associates  
6130 Executive Boulevard  
Rockville, MD 20852

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*Note:* At the request of OPRE, some of the text in this report is taken verbatim or with only minor edits from earlier reports for OPRE—for the Pathways for Advancing Careers and Education evaluation, and for the national evaluations of two rounds of Health Profession Opportunity Grants (HPOG 1.0 and HPOG 2.0).

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## Overview

### Introduction

Following on a first round of Health Profession Opportunity Grants (HPOG) Program awards in 2010 (“HPOG 1.0”), the Office of Family Assistance (OFA) of the Administration for Children and Families (ACF), within the U.S. Department of Health and Human Services, awarded a second round of 32 five-year grants (“HPOG 2.0”) in 2015. Local HPOG 2.0 programs provided education, training, and support services (including financial and other assistance) to Temporary Assistance for Needy Families (TANF) recipients and other low-income adults for occupations in the healthcare field that pay well and were expected to either experience labor shortages or be in high demand.

ACF’s Office of Planning, Research, and Evaluation oversaw an evaluation to assess the success of the HPOG 2.0 Program across the diversity of 38 local HPOG 2.0 programs implemented by the 27 non-Tribal grantees. (Programs of five Tribal grantees were evaluated separately.) This *HPOG 2.0 Short-Term Impact Report* documents impacts on enrollees admitted to local programs during the first 25 months of program operations. The primary focus is on short-term impacts at about 15 months after study entry on training, credential receipt, and earnings. The report also includes some intermediate-term impacts (through two and a half years) on select education and labor market outcomes for enrollees admitted during the first 13 months of program operations.

Findings describe both overall *average impact* across all program participants across all 27 non-Tribal grantees nationwide and impacts for selected *subgroups* of participants. Later reports will share complete intermediate impacts, as well as longer-term impacts.

### Research Questions

- What is the average impact of access to a local HPOG 2.0 program on receipt of training (in general and for healthcare professions) and support services, on employment and earnings (in general and in healthcare professions), and on broader measures of well-being?
- How does the impact of HPOG 2.0 programs vary with study members’ baseline characteristics (e.g., age, education, TANF receipt)?

### Purpose

The HPOG 2.0 Impact Evaluation is making an important contribution to the field’s collective knowledge about sector-based and career pathways programs. This research helps to assess whether local programs responding to the HPOG 2.0 funding opportunity were both individually and collectively effective in achieving its training and labor market goals for participants offered access to their services.

## Key Findings

Compared with study members who were interested in local HPOG 2.0 programs but denied access, through 15 months after random assignment:

- Those study members given access to a local HPOG 2.0 program are more likely (by 16 percentage points) to have made *educational progress*—defined as having completed training by earning a credential or having been continuously enrolled in training. This favorable impact on the evaluation’s short-term confirmatory outcome indicates that, overall, HPOG 2.0 is on track to achieve its goals.
- Collectively, HPOG 2.0 programs have a significant impact on each of the four secondary outcomes in the educational progress domain, including an increase (by 17 points) in the percentage *earning any new credential* from either a training provider or some other authority such as a state board); an increase (9 points) in the percentage *earning exam-based certifications or licenses* from authorities other than training providers; an increase (10 points) in the percentage *completing six or more months of training*, and an increase (1.4 months) in *months of training*.
- Collectively, HPOG 2.0 programs increase (by 3 percentage points) *career connectedness*—defined as being employed full-time, in training full-time, or at least part-time in both employment and training. Programs also increase self-assessed *career progress* (by 0.2 points on a four-point scale; an impact equivalent to an effect size of 0.22 standard deviations, which implies that 59 percent of the treatment group has a higher level of career progress than the median value in the control group).
- Collectively, HPOG 2.0 programs have no detectable impacts on *earnings*, on *any employment*, or on *employment with health insurance benefits*.
- Collectively, HPOG 2.0 programs increase *employment in healthcare* (by 4 percentage points).

## Methods

The HPOG 2.0 Impact Evaluation is large, randomizing more than 52,000 low-income adults by its end in 2021. The evaluation is using an experimental design in which eligible applicants to grantees’ local HPOG 2.0 programs are assigned at random either to a treatment group that can access program services or to a control group that cannot. To estimate the programs’ collective impact, the evaluation combines data across sites, then compares average outcomes for the two groups. The impact analysis uses data from a Short-Term Follow-Up Survey administered about 15 months after randomization to a subset of study members. The analysis also uses administrative data from the National Directory of New Hires and the National Student Clearinghouse through two and half years after randomization for the earliest enrollees. The analyses in this report were pre-specified in an [Analysis Plan](#) and registered with the Open Science Framework and the Registry of Efficacy and Effectiveness Studies.

## Executive Summary

The statutory purpose of the Health Profession Opportunity Grants (HPOG) Program is to provide education and training to Temporary Assistance for Needy Families (TANF) recipients and other adults with low incomes for occupations in the healthcare field that pay well and were expected to either experience labor shortages or be in high demand. Following on a first round of HPOG awards in 2010 (“HPOG 1.0”), the Office of Family Assistance of the Administration for Children and Families (ACF), within the U.S. Department of Health and Human Services, in 2015 awarded a second round of 32 five-year grants (“HPOG 2.0”).

Each HPOG 2.0 grantee designed and implemented one or more local programs to meet these goals by providing education, occupational training, and support and employment services. The resulting programs were diverse. Compared to HPOG 1.0, ACF increased the emphasis in HPOG 2.0 on articulated career pathways, serving those with low basic skills, and employer engagement.

HPOG was authorized as a demonstration program with a mandated federal evaluation. ACF’s Office of Planning, Research, and Evaluation (OPRE) oversees a multipronged research and evaluation strategy to assess the effectiveness of the HPOG Program. OPRE contracted with Abt Associates and its partners the Urban Institute, MEF Associates, NORC at the University of Chicago, and Insight Policy Research to conduct such an evaluation of HPOG 2.0. The evaluation is an opportunity to learn more about job training in general and job training for the healthcare professions in particular—overall, and for adults with low incomes and TANF populations.

Programs of five Tribal grantees were evaluated separately from 27 non-Tribal grantees.<sup>1</sup> The “National” Evaluation of the non-Tribal grantees includes an Implementation Study, a Systems Study, an Outcomes Study, an Impact Evaluation, and a Cost-Benefit Analysis. This is the first report of the Impact Evaluation.

### Evaluation Design

The Impact Evaluation focuses on the collective impact of the 38 local programs designed and implemented by the non-Tribal grantees—each relative to other services available in the local community. From this perspective, HPOG 2.0 is successful as a funding stream and program model if its grants go to organizations that can design and implement successful local programs. When we refer to the impact of “HPOG 2.0” or “HPOG 2.0 programs,” we are referring to this collective impact. Descriptions of impacts are written in the present tense because the testing procedures in this report only flag impacts as statistically significant if they are consistently large enough across the local programs to appear replicable for future cohorts of students in

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<sup>1</sup> For details of the Tribal Evaluation, see <https://www.acf.hhs.gov/opre/research/project/tribal-evaluation-of-the-2nd-generation-of-health-profession-opportunity-grants-tribal-hpog-20-evaluation>.

programs like those created under HPOG 2.0. Future reports will also investigate whether there are any patterns in the impacts of local programs.<sup>2</sup>

The Impact Evaluation uses an experimental design in which eligible program applicants are assigned at random to either a “treatment” group that can access the local HPOG 2.0 program or a “control” group that cannot, and then compares the two groups’ average outcomes. By comparing outcomes for randomized applicants, the evaluation provides strong evidence on the impact of the offer of HPOG 2.0.

This design was selected because it provides strong estimates of the evaluation’s primary research question: What is the impact of *being offered* access to HPOG 2.0 training and services, as opposed to the impact of *receiving* HPOG 2.0 training and services. Such an “intent-to-treat” design assesses whether the treatment group members obtained better outcomes from having access to the HPOG 2.0 local program (including all its components) than the outcomes they could have obtained without the program, but with access to other training and services in their local communities.

We chose this focus on the impact of the offer of access rather than the impact of receipt for several reasons. Beyond the local HPOG 2.0 program, there are other training options and the data clearly show that those not offered HPOG 2.0 often enrolled in those other training options. When there are other options, the relevant policy issue concerns the incremental effect of this program—above and beyond the other programs. This is what intent-to-treat estimates.

This *HPOG 2.0 Short-Term Impact Report* documents short-term impacts (about 15 months after randomization) on training, credential receipt, and labor market outcomes for study members pooled across all 38 non-Tribal grantee programs. It also shares intermediate-term impacts (through two and a half years) for early cohorts of those members. Findings describe both overall *average impact* (across local programs) and impacts for selected *subgroups* of study members. These follow-up periods may be too short to detect the impact of longer or follow-on training. Later reports will describe fuller intermediate-term as well as longer-term impacts.

## Research Questions

The study’s research questions are:

- What is the average impact of access to a local HPOG 2.0 program on receipt of training (in general and for healthcare professions) and support services, on employment and earnings (in general and in healthcare professions), and on broader measures of well-being?
- How does the impact of HPOG 2.0 programs vary with study members’ baseline characteristics (e.g., age, education, TANF receipt)?

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<sup>2</sup> This report does not consider the efficacy of particular training components or services; nor does it estimate impacts of each grantee’s HPOG 2.0 program. A separate appendix volume (Judkins et al. 2022) provides estimated impacts on a small set of outcomes for each of the 38 programs designed and implemented by the 27 non-Tribal grantees.

## Data Sources

Most outcomes were measured in the evaluation's Short-Term Follow-Up Survey, with the typical survey interview occurring about 17 months after randomization. The survey attempted to interview everyone in the "survey cohort"—those study members randomized in months 14 to 25 of program operations. Interviews were completed with 9,620 study members, yielding a response rate of 74 percent.

Other outcomes are measured through two and a half years after randomization with administrative data on the "pre-survey cohort"—the 10,117 study members randomized in the first 13 months of program operations and therefore excluded from the Short-Term Follow-Up Survey.<sup>3</sup> Sources of these administrative data are the National Directory of New Hires (NDNH) for employment and earnings outcomes, and the National Student Clearinghouse (NSC) for education outcomes at degree-granting institutions.

## Major Finding

- **In the short term, HPOG 2.0 programs substantially increase educational progress—defined as having completed training by earning a credential or having been continuously enrolled in training.**

This measure—educational progress—was pre-selected as the *confirmatory outcome* for the short-term impact analysis; that is, as the outcome for which a favorable impact indicates that HPOG 2.0 is on track to achieve its goals.<sup>4</sup> About 15 months after randomization, 63 percent of the treatment group are making educational progress, compared to 46 percent of the control group (see **Exhibit ES-1**, first column). This impact of 16 percentage points is an increase of more than a third relative to the control group level.

## Other Key Impact Findings

This section presents the estimated collective impact of access to local HPOG 2.0 programs for other key outcomes and notes some differential impacts for subgroups. Unless otherwise noted, all outcomes are measured in the Short-Term Follow-Up Survey.

### Impacts on Education and Training Outcomes

- **In addition to improving educational progress in the short-term, HPOG 2.0 programs lead to large relative increases across a range of other training measures.**

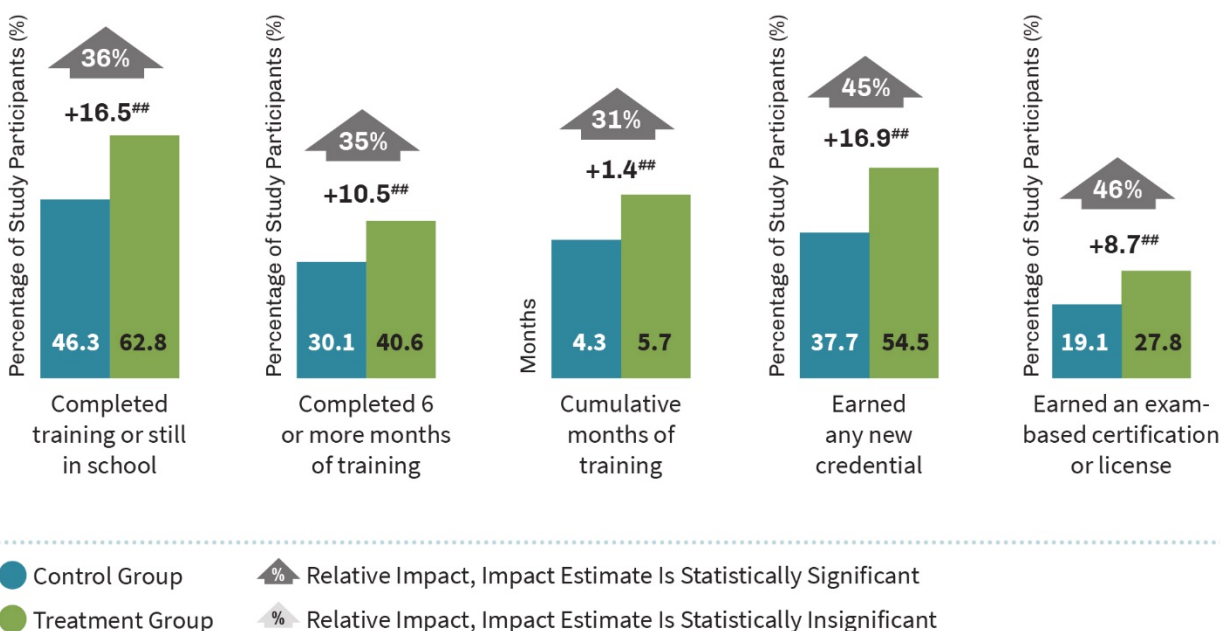
As shown in **Exhibit ES-1**, HPOG 2.0 programs improve completing at least six months of training, cumulative months of training, receipt of a credential, and receipt of an exam-based

<sup>3</sup> Project funds did not allow follow-up surveys with all study members, of whom there were over 52,000 by the end of the grants. Study members randomized before March of 2017 were not eligible for the survey. Neither were those randomized after February of 2018.

<sup>4</sup> This outcome was designated as confirmatory in both the evaluation's *Analysis Plan* (Judkins, Klerman, and Locke 2020) and the evaluation's registration pages (<https://osf.io/nv2fz> and <https://sreereg.icpsr.umich.edu/sreereg/subEntry/2576/pdf?section=all&action=download>).

certification or license. The increases are large: a third or more relative to control group levels for each outcome.

**Exhibit ES-1 Short-Term Education and Training Impacts, by Outcome**



Source: Short-Term Follow-Up Survey

Note: The numbers above the bars indicate impact estimates. Scaling designed so that difference in height between blue and green bars in fractions of an inch is proportional to the effect size, which is calculated as the absolute impact divided by the population standard deviation. This is a common method in meta-analyses to compare impacts measured on different scales.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

Percentage point impacts on training outcomes in healthcare specifically (not shown in Exhibit ES-1) are slightly larger than the impacts on training overall as shown. This is because, in addition to increasing overall training, HPOG 2.0 causes some people who otherwise would have gotten training for non-healthcare occupations to instead get training for healthcare occupations.

As of the Short-Term Follow-Up Survey about 15 months after random assignment, 25 percent of the treatment group is in training, as is 19 percent of the control group. This suggests both that training is likely to continue to increase for both groups past 15 months and that the impact of HPOG 2.0 on training will likely continue to increase.

- **For the pre-survey cohort, HPOG 2.0 programs increase college enrollment through Q6; from Q7 through Q10 there is no detected impact.**

Follow-up through about two and a half years (10 quarters) for the pre-survey cohort using NSC administrative data suggests that the share of the treatment group in college in a given quarter peaks in the quarter after randomization (Q1), at 31 percent, which is 7 percentage points more than in the control group. Thereafter, both the level of enrollment and the impact of HPOG 2.0 decline. HPOG 2.0 programs increase college enrollment through, but not past, a year and a

half after randomization (Q6). Two and a half years after randomization (Q10), about 16 percent of both the treatment group and the control group are in training. This suggests that impact on college enrollment is likely to decline for the survey cohort past 15 months.

NSC data capture only training provided at colleges. Because much short training is not delivered at colleges, the NSC's coverage of training early in the period after randomization is likely weak. In contrast, because longer (more than six months) training is more likely to be delivered at colleges, the NSC's coverage of later training is likely stronger. That many treatment group members are enrolled in college even 10 quarters after randomization suggests that eventually they will earn more long-term certificates and degrees.

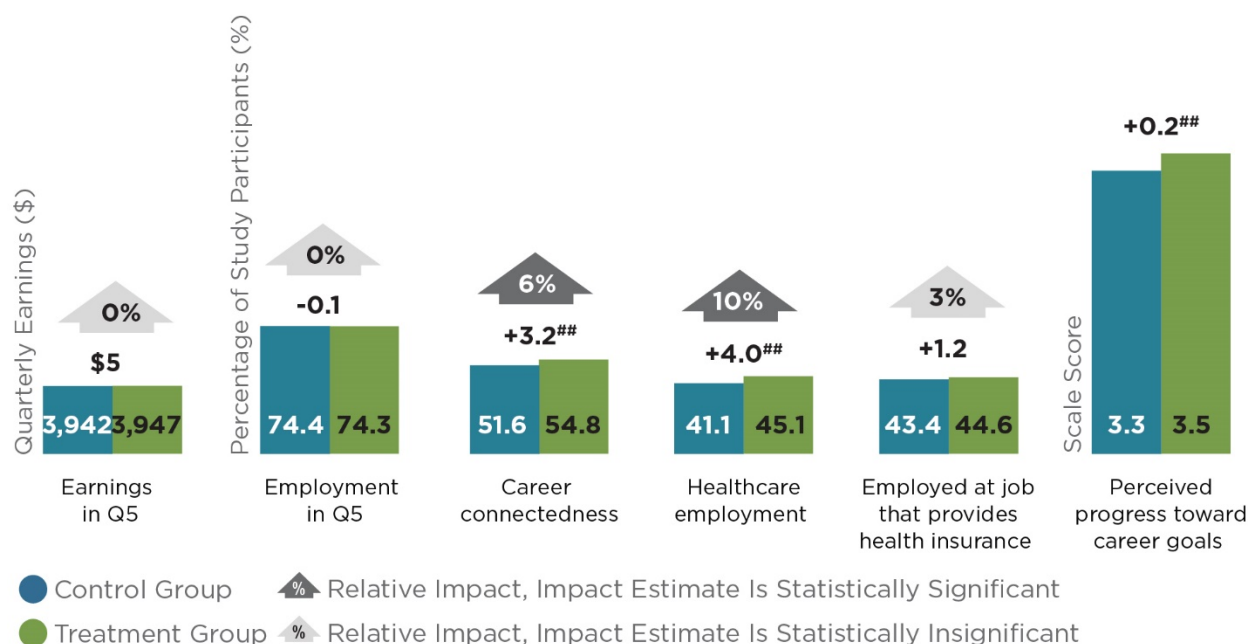
However, the lack of a significant treatment-control contrast on enrollment at this point in time is not encouraging for HPOG 2.0 impacts on long-term certificates and degrees.

- **In the short term, HPOG 2.0 programs improve training outcomes for all subgroups, though impacts are larger for some subgroups.**

Relative to study members not in these subgroups, the impact of HPOG 2.0 programs on educational progress is larger for older study members; those with dependent children; those not already enrolled in training at baseline; those with no life challenges very often interfering with work, school, or family responsibilities; and those not receiving TANF benefits at baseline. The impact was largest for those older than age 30 (20 percentage points; 11 points for those younger than age 25).

### **Impacts on Labor Market Outcomes**

Based on the theory of change, training through HPOG 2.0 programs should, over the long term, (1) improve labor market outcomes and broader measures of well-being; and (2) expand the healthcare workforce. It is unclear whether one should expect to detect impact on these outcomes as of this short-term report (**Exhibit ES-2**).

**Exhibit ES-2 Short-Term Labor Market Impacts, by Outcome**

Source: Short-Term Follow-Up Survey; National Directory of New Hires

Note: The numbers above the bars indicate impact estimates. "Perceived progress towards career goals" is a continuous scale score based on three survey items about (1) progress towards long-range educational goals, (2) progress towards long-range employment goals, and (3) whether the respondent sees themselves on a career path. Scaling designed so the difference in height between blue and green bars in fractions of an inch is proportional to the effect size, which is calculated as the absolute impact divided by the population standard deviation. This is a common method in meta-analyses to compare impacts measured on different scales. The relative impact on perceived progress towards career goals is not displayed because relative impact is not meaningful in the absence of a natural scale.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

- **In the short term, HPOG 2.0 programs do not improve earnings or most other labor market outcomes.**

On the first goal of increasing earnings, NDNH administrative data show that in the fifth quarter after randomization, study members' earnings are approximately \$4,000 per quarter, and about three-quarters are employed. Short-Term Follow-Up Survey data show that about two-fifths of the sample are employed at jobs that offer health insurance (a measure of job quality).

For none of these three outcomes does the evaluation detect an impact of HPOG 2.0 programs. Furthermore, through two and a half years after randomization, the NDNH data for the pre-survey cohort show no evidence of impact on either earnings or employment. Finally, for earnings, there is little evidence of HPOG 2.0 programs having a differential impact across subgroups or a positive impact in any subgroup.

- **In the short term, HPOG 2.0 programs increase employment in healthcare occupations.**

On the second goal of expanding the healthcare workforce, HPOG 2.0 programs shift employment into healthcare occupations (by 4 percentage points): 45 percent in the treatment group versus 41 percent in the control group. Consistent with this positive impact on healthcare

employment, counting hours and earnings outside of health care as zero hours and zero dollars, HPOG 2.0 programs also increase healthcare hours and earnings. For all three outcomes, the increase is about 10 percent relative to the control group level.

In the short term, HPOG 2.0 programs improve career connectedness and perception of progress towards career goals.

Though HPOG 2.0 programs do not increase short-term earnings, they do improve two potential precursors for future earnings. As shown in **Exhibit ES-2**, they increase *career connectedness*—defined as being engaged full-time in work or school or at least part-time in both: 55 percent in the treatment group versus 52 percent in the control group. HPOG 2.0 programs also increase scores on a scale measuring self-assessed career progress, indicating that even if members of the treatment group do not yet have higher earnings, they see themselves as moving closer to their goals than do their counterparts in the control group.

## Conclusion and Implications

In summary, these short-term estimates show large collective impacts of HPOG 2.0 programs on healthcare training completed and moderate impact on employment in healthcare occupations. Thus, the estimates imply progress towards the second of the HPOG Program's dual policy goals—expanding the skilled healthcare workforce.

With respect to increasing earnings, the results are mixed. The program's logic model posits three steps: (1) that HPOG 2.0's training and support services lead to more training, (2) which leads to receipt of more credentials, (3) which in turn leads to higher earnings. On the first two, the evaluation finds large short-term impacts on completing or remaining in training and on credential receipt—increases in the treatment group of a third or more relative to the control group levels. Nevertheless, the evaluation does not find impacts on labor market outcomes (the third step) in the short or (for the pre-survey cohort) in the intermediate term.

This report's follow-up period may be too early to expect results on earnings. Later reports, which will include longer follow-up on all cohorts and larger samples at all follow-up intervals, will provide additional estimates that will show whether significant impacts on earnings arise later. Future reports will also investigate whether there are any patterns in the impacts of local programs.

## Important Terms for This Report

### Terms Related to HPOG

**career pathways:** a framework for occupational training that combines education, training, and support services; aligns with the skill demands of the local economy; and helps participants to enter and/or advance in a career within a specific occupation sector or occupational cluster.

**HPOG or HPOG Program:** the national Health Profession Opportunity Grants initiative, including all grantees and their local programs.

**HPOG grantee:** the organization that was awarded the HPOG grant and funding by the U.S. Department of Health and Human Services.

**[local] HPOG program:** a unique set of local education and training courses, services, and personnel within a career pathways framework; a single HPOG grantee may fund one or more HPOG programs.

**HPOG [program] partners:** organizations other than the grantee or program operator directly involved in operating a local HPOG program.

**HPOG program participants:** enrollees in a local HPOG program's education and training courses and related support services.

**credential:** a broad term for any formal recognition either of educational attainment (certificate, degree, diploma) typically through credit accumulation, or of occupational skills mastery (certification, license) typically through an exam.

### Terms Related to the HPOG 2.0 Impact Evaluation

**study members:** all individuals who consented to be part of the HPOG 2.0 evaluation, regardless of which experimental group (treatment or control) they were randomly assigned to, and regardless of whether they ever enrolled in, attended, or completed courses or used support services.

**treatment group members:** those study members who were randomly assigned to the group offered access to the local HPOG program.

**control group members:** those study members who were randomly assigned to the group not offered access to the local HPOG program, but who could access any other education, training, or services available in the community.

**pre-survey cohort:** study members randomized between the beginning of enrollment in February 2016 and February 2017, inclusive (first 13 months of program operation); represented in administrative data from the National Student Clearinghouse and the

National Directory of New Hires, providing about two and a half years of follow-up.  $N=10,117$ .

**survey cohort:** study members randomized between March 2017 and February 2018, inclusive (approximately the second year of program operations); respondents to the Short-Term Follow-Up Survey at about 15 months after randomization.  $N=9,620$ .

**outcomes:** the specific measures of interest that HPOG aims to influence. The HPOG 2.0 Impact Evaluation defines three types of outcomes:

- **confirmatory outcomes:** main indicators of the extent to which an intervention/program is making progress towards its goals. If no predicted confirmatory impacts are detected, then the tested intervention is not considered successful.
  - For HPOG 2.0, the confirmatory outcome in this first impact report is **educational progress**, defined as having completed training by earning a credential or having been continuously enrolled in training as of the Short-Term Follow-Up Survey (approximately 15 months after randomization).
- **secondary outcomes:** additional important outcomes identified in the intervention/program's logic model.
- **exploratory outcomes:** measured program effects that may help improve our understanding of findings from the confirmatory and secondary analyses.

**outcome domain:** a category of related outcomes; e.g., educational progress, labor market, or well-being.

**college:** per the Integrated Postsecondary Education Data System, a degree-granting postsecondary institution eligible to participate in federal Title IV financial aid programs.

## The HPOG Research and Evaluation Portfolio

ACF's Office of Planning, Research, and Evaluation (OPRE) is using a multipronged research and evaluation strategy to assess the implementation, outcomes, and impacts of two rounds of HPOG awards.

### *HPOG First Round (HPOG 1.0)*

ACF awarded 32 five-year grants in 2010, with 18 grantees receiving extensions into 2016. A research team oversaw development and operation of a management information system called the Performance Reporting System (PRS) used by all grantees.

**HPOG Implementation and Outcomes Research.** The team also conducted implementation and outcomes research for the 27 non-Tribal grants:

- The descriptive implementation and outcomes report is available here: <https://www.acf.hhs.gov/opre/report/descriptive-implementation-and-outcome-study-report-national-implementation-evaluation>.
- The systems change analysis is available here: <https://www.acf.hhs.gov/opre/resource/systems-change-under-the-health-profession-opportunity-grants-program>.
- The final report on the implementation research is available here: <https://www.acf.hhs.gov/opre/resource/final-report-national-implementation-evaluation-of-the-first-round-health-profession-opportunity-grants-hpog-10>.

OPRE also sponsored the **Evaluation of Tribal HPOG**, an implementation and outcomes study of the five Tribal grants. The final report is available here: <https://www.acf.hhs.gov/opre/resource/tribal-health-profession-opportunity-grants-hpog-program-evaluation-final-report>.

**HPOG 1.0 Impact Study.** For 23 of the 27 first-round non-Tribal grants, the research team conducted an experimental study—the HPOG 1.0 Impact Study—to assess the impacts of the HPOG intervention. Local HPOG programs randomly assigned eligible applicants to a “treatment” group that could access HPOG or a “control” group that could not. Three of the 23 HPOG grantees are also participating in another OPRE-sponsored evaluation of career pathways programs begun in 2007, Pathways for Advancing Careers and Education (PACE).

- The *Health Profession Opportunity Grants (HPOG 1.0) Impact Study Interim Report* assesses short-term outcomes for the treatment and control groups based on follow-up surveys initiated about **15 months** after random assignment and on administrative data on employment and earnings. It also draws on the implementation research results for the 23 grantees and site visits conducted specifically for the Impact Study. The report is available here: <https://www.acf.hhs.gov/opre/resource/health-profession-opportunity-grants-hpog-10-impact-study-interim-report-implementation-short-term-impacts>.
- The *Health Profession Opportunity Grants (HPOG 1.0) Impact Study Three-Year Impacts Report* shares impacts from administrative data and follow-up surveys initiated approximately **three years** after randomization. The report was produced as part of the Career Pathways Intermediate Outcomes Study, which is continuing to follow HPOG Impact Study and PACE project participants. The report is available here: <https://www.acf.hhs.gov/opre/resource/health-profession-opportunity-grants-hpog-10-impact-study-three-year-impacts-report>.
- The research team is continuing to document longer-term outcomes for HPOG Impact Study and PACE project participants and will describe outcomes approximately **6 and 10 years** (pending additional funding) after randomization for HPOG 1.0 and PACE programs. More information is available here: <https://www.acf.hhs.gov/opre/research/project/career-pathways-long-term-outcomes-study>.

- Program-level reports on the implementation and early impacts of each of the nine programs in the PACE project are available here: <https://www.acf.hhs.gov/opre/research/project/pathways-for-advancing-careers-and-education>.

### **HPOG Second Round (HPOG 2.0)**

In 2015, ACF awarded a second round of five-year HPOG grants (HPOG 2.0) to 32 organizations in 21 states; five were Tribal organizations and 27 non-Tribal. HPOG 2.0 was extended an additional 12 months, ending September 2021. ACF awarded an evaluation contract for **The National and Tribal Evaluation of the 2nd Generation of Health Profession Opportunity Grants (HPOG 2.0)**.

Like the HPOG 1.0 evaluation, the research team oversaw development of a management information system used by all grantees. The HPOG 2.0 system was known as the Participant Accomplishment and Grant Evaluation System (PAGES). The system was used for program management and performance monitoring, and to record grantee and participant data for use in HPOG 2.0 evaluations.

The **HPOG 2.0 National Evaluation of the non-Tribal grantees** uses follow-up survey data, PAGES data, and other administrative data to assess outcomes for new study members who apply to the second-round programs.

- **HPOG 2.0 Impact Evaluation.** The 27 non-Tribal HPOG 2.0 grantees, operating 38 local programs, participated in an experimental study to assess the impacts of HPOG 2.0. Local HPOG programs randomly assigned eligible applicants to a “treatment” group that could access HPOG 2.0 services or a “control” group that could not. The study randomized more than 52,000 study members by the end of the program in 2021. All study members completed a baseline survey upon entering the study. The evaluation is assessing short-term impacts (about 15 months after random assignment), intermediate-term impacts (about 36 months after random assignment), and longer-term impacts (about 66 months after random assignment).

In addition to the impact evaluation, OPRE also is sponsoring a descriptive evaluation and cost-benefit analysis of the 27 non-Tribal HPOG 2.0 grants:

- **HPOG 2.0 Descriptive Evaluation.** The research team is conducting implementation, outcomes, and systems studies. The evaluation is exploring how the HPOG 2.0 local programs are implemented across grantees (Implementation Study), what individual-level outcomes and outputs occur (Outcomes Study), and how HPOG influences service delivery systems (Systems Study).
- **HPOG 2.0 Cost-Benefit Analysis.** The non-Tribal HPOG 2.0 grantees participated in a cost-benefit analysis that will compare the estimated costs of operating the average HPOG 2.0 program to the monetized value of benefits produced.

The **HPOG 2.0 Tribal Evaluation** includes a separate implementation and outcomes evaluation of the five Tribal grants.

- The final report of the evaluation is available here: <https://www.acf.hhs.gov/opre/report/tribal-health-profession-opportunity-grants-hpog-20-evaluation-final-report>.

### **For More Information on All of These Research Activities**

<https://www.acf.hhs.gov/opre/research/project/evaluation-portfolio-for-the-health-profession-opportunity-grants-hpog>

## 1. Introduction

Many Americans have low hourly wages and low earnings. Because individuals with higher educational attainment tend to have lower unemployment and higher earnings than those with less education, policymakers frequently turn to job skills training and other postsecondary education as a strategy for increasing earnings by preparing people for higher-skilled, better-paying occupations.<sup>5</sup>

The **Health Profession Opportunity Grants (HPOG) Program** was one effort implementing that strategy. Following on a first round of awards in 2010 (“HPOG 1.0”), the Office of Family Assistance (OFA), within the Administration for Children and Families (ACF) in the U.S. Department of Health and Human Services, awarded a second round of 32 grants (“HPOG 2.0”) in 2015 to 27 non-Tribal (“National”) grantees and 5 Tribal grantees. The HPOG Program funded grantees to provide education, training, and support services to Temporary Assistance for Needy Families (TANF) recipients and other low-income adults for occupations in the healthcare field that pay well and were expected to either experience labor shortages or be in high demand. Altogether, HPOG 2.0 served more than 35,000 low-income adults.

HPOG was authorized as a demonstration program with a mandated federal evaluation. ACF’s Office of Planning, Research, and Evaluation (OPRE) oversees a multipronged research and evaluation strategy to assess the effectiveness of the HPOG Program. In 2015, OPRE awarded a contract to Abt Associates and its partners the Urban Institute, MEF Associates, NORC at the University of Chicago, and Insight Policy Research to conduct the **National and Tribal Evaluation of the 2<sup>nd</sup> Generation of Health Profession Opportunity Grants**.<sup>6</sup>

The National Evaluation’s Impact Evaluation uses an experimental design in which eligible applicants to grantees’ 38 local HPOG 2.0 programs are assigned at random either to a treatment group, which can access program services, or to a control group, which cannot. After an initial planning period, random assignment began with the start of grantees’ local program operations in February 2016 and ended in 2021.

This *HPOG 2.0 Short-Term Impact Report* documents short-term impacts (about 15 months after randomization) on training, credential receipt, and labor market outcomes for all 27 non-Tribal grantees. This report focuses on impacts for the 9,620 study members<sup>7</sup> who were randomly assigned in the second 12 months of HPOG 2.0 local program operations (between March 2017 and February 2018) and who responded to the evaluation’s Short-Term Follow-Up

<sup>5</sup> “Unemployment Rates and Earnings by Educational Attainment,” Employment Projections, U.S. Bureau of Labor Statistics, last modified September 4, 2019, <https://www.bls.gov/emp/chart-unemployment-earnings-education.htm>.

<sup>6</sup> This Impact Evaluation is part of OPRE’s diverse research portfolio to assess the success of HPOG on participant educational attainment, employment, and earnings. For an overview, see the text box **The HPOG Research and Evaluation Portfolio** just before Chapter 1. Programs of the five Tribal grantees are being evaluated separately.

<sup>7</sup> This report uses the term “study members” to refer collectively to members of the treatment and control groups. Moreover, the term applies whether or not those study members actively engaged with any HPOG 2.0 activity (treatment group) or similar activity available in the community (control group) beyond getting far enough into the application process to be randomly assigned.

Survey. **Impacts reported in this document focus on job training—including this report’s confirmatory outcome, educational progress—and support services received.** Impacts on labor market outcomes and broader measures of well-being are also reported. Because about a quarter of study members were still in training at the time data for this report were collected, it may be too early to see impacts on earnings and other employment-related outcomes.

The balance of this opening chapter is organized as follows. Section 1.1 presents a high-level description of the HPOG Program. Section 1.2 gives an overview of this HPOG 2.0 Impact Evaluation. Section 1.3 considers findings from the broader literature on the impact of job training programs and from the evaluation of HPOG 1.0. Section 1.4 describes the organization of the balance of the report; and Section 1.5 describes planned future HPOG 2.0 reports.

## 1.1 The HPOG Program

This section describes HPOG’s authorizing legislation, the program model and theory of action, the local HPOG programs, the enrollees, and training received.

### 1.1.1 The Authorizing Legislation

As part of the Affordable Care Act of 2010, Congress authorized funds for the Health Profession Opportunity Grants (HPOG) Program “to conduct demonstration projects that provide eligible individuals with the opportunity to obtain education and training for occupations in the healthcare field that pay well and are expected to either experience labor shortages or be in high demand.”<sup>8</sup> The legislation also stressed the importance of serving recipients of Temporary Assistance for Needy Families and other individuals with low incomes.

It also required the HPOG 2.0 program to be evaluated:

*Such evaluation shall include identification of successful activities for creating opportunities for developing and sustaining, particularly with respect to low-income individuals and other entry-level workers, a health professions workforce that has accessible entry points, that meets high standards for education, training, certification, and professional development, and that provides increased wages and affordable benefits, including health care coverage, that are responsive to the workforce’s needs.*

From this, the evaluation draws its two focal outcomes: building a healthcare workforce and increasing wages and benefits.

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<sup>8</sup> HPOG was authorized by the Affordable Care Act (ACA), Public Law 111-148, 124 Stat. 119, March 23, 2010, sect. 5507(a), “Demonstration Projects to Provide Low-Income Individuals with Opportunities for Education, Training, and Career Advancement to Address Health Professions Workforce Needs,” adding sect. 2008(a) to the Social Security Act, 42 U.S.C. 1397g(a). The second round of grant awards were extended until September 29, 2021.

### 1.1.2 Program Model and Theory of Action

For each round of funding, OFA issued a Funding Opportunity Announcement (FOA) soliciting applications from eligible entities. Those FOAs described a model for the HPOG Program whose theory of action is depicted in **Exhibit 1-1**.<sup>9</sup>

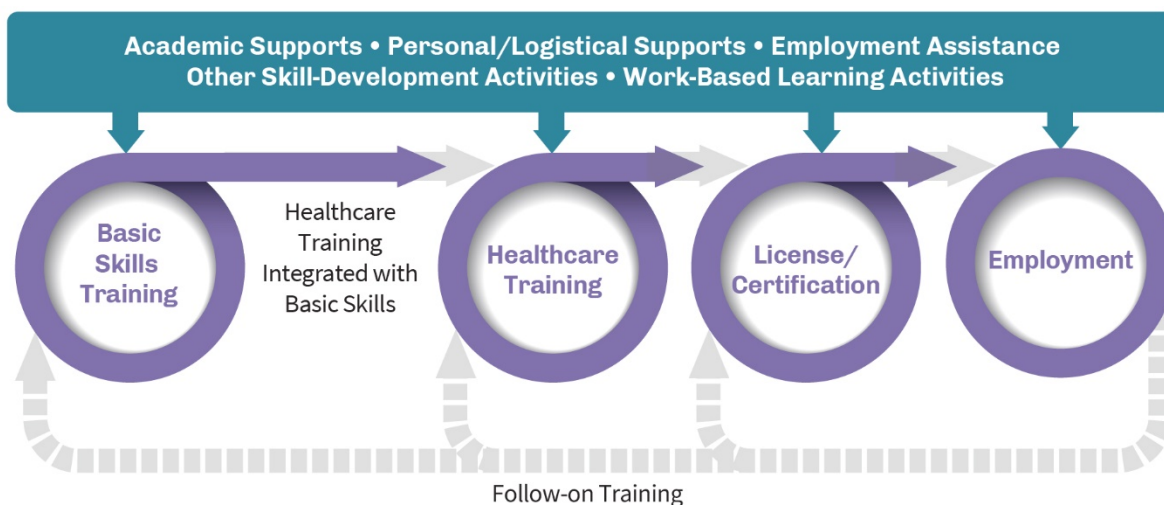
Specifically, consistent with principles of the “career pathways framework” (see box to the right), local HPOG programs were to provide **basic skills education**, as needed. This basic skills education was intended to allow program participants to enroll in and complete **occupational healthcare training** for which they would otherwise not have been qualified. That healthcare training would, in turn, lead to better labor market outcomes—in particular, higher earnings. Then, perhaps after some time in the labor market, HPOG program participants would return for additional basic skills, as needed, and healthcare training. That additional healthcare training would lead to higher-level licenses and certifications and even better labor market outcomes.

#### Career Pathways Framework Components

To effectively engage, retain, and facilitate learning of a diverse population, career pathways programs involve training opportunities that

- award clearly defined and industry-recognized credentials;
- build to add higher competencies in a defined career path;
- are flexibly delivered to accommodate nontraditional students;
- are integrated with work-based learning opportunities (such as internships, externships, clinical placements); and
- integrate varied supports aimed to ensure students’ program persistence, program completion, and subsequent workplace success. (Fein 2012)

**Exhibit 1-1 A Theory of Action for the HPOG Program**



<sup>9</sup> See Appendix Section A.1 for more discussion of the HPOG 2.0 FOA and Section A.4 for a conventional logic model characterization of an HPOG 2.0 program. All mentions in this report of appendices or appendix exhibits refer to the separate accompanying technical appendix volume (Judkins et al. 2022).

Also consistent with the career pathways framework's principles, HPOG grantee programs were to provide **academic supports, personal and logistical supports, and employment supports**. Together those services were intended to help program participants complete their education and training programs and find employment.

There was considerable continuity between the FOAs for HPOG 1.0 and HPOG 2.0. Nevertheless, based on descriptive evaluation findings about program implementation, outcomes, and participant characteristics and performance data available at the time,<sup>10</sup> OFA adjusted the FOA language for HPOG 2.0 (see OFA 2019) to more strongly emphasize

- clearly articulated pathways;
- the provision of adult basic education; and
- stronger employer engagement.

In addition, the HPOG 2.0 FOA required grantees to identify and describe their recruitment, referral, and eligibility criteria to ensure that study members were, among other requirements, TANF recipients or otherwise had low incomes. The FOA explicitly discouraged recruitment of anyone currently enrolled in similar education or training programs, but also made clear it was not discouraging grantees from re-enrolling past HPOG 1.0 program participants so they could advance along a career pathway.

### 1.1.3 Local HPOG Programs

This report focuses on short-term impacts of the local programs implemented by the 27 non-Tribal HPOG 2.0 grantees (see Appendix Exhibit A-2 for the full list). That group of grantees was diverse. They included institutions of higher education (10 grantees), workforce system agencies (7 grantees), community-based organizations (6 grantees), and state agencies (4 grantees). About half (14) had been funded under HPOG 1.0; about half (13) had not. They were located in 21 different states, operating 38 different local programs.

Reflecting that diversity, the local programs were also diverse within the requirements of the FOA and the principles of the career pathways framework. For the most part, the local HPOG 2.0 programs did not provide HPOG-specific training or create new training courses with the HPOG funds. Instead, HPOG programs provided navigation services for enrollment in existing education and job training courses, financial aid counseling, support services, and assistance with paying the costs of training. The support services included case management, personal and logistical supports, academic support, and employment assistance. Programs provided training and support services themselves, had partners that provided them, or referred participants to other providers in the community.

### 1.1.4 The Study Members

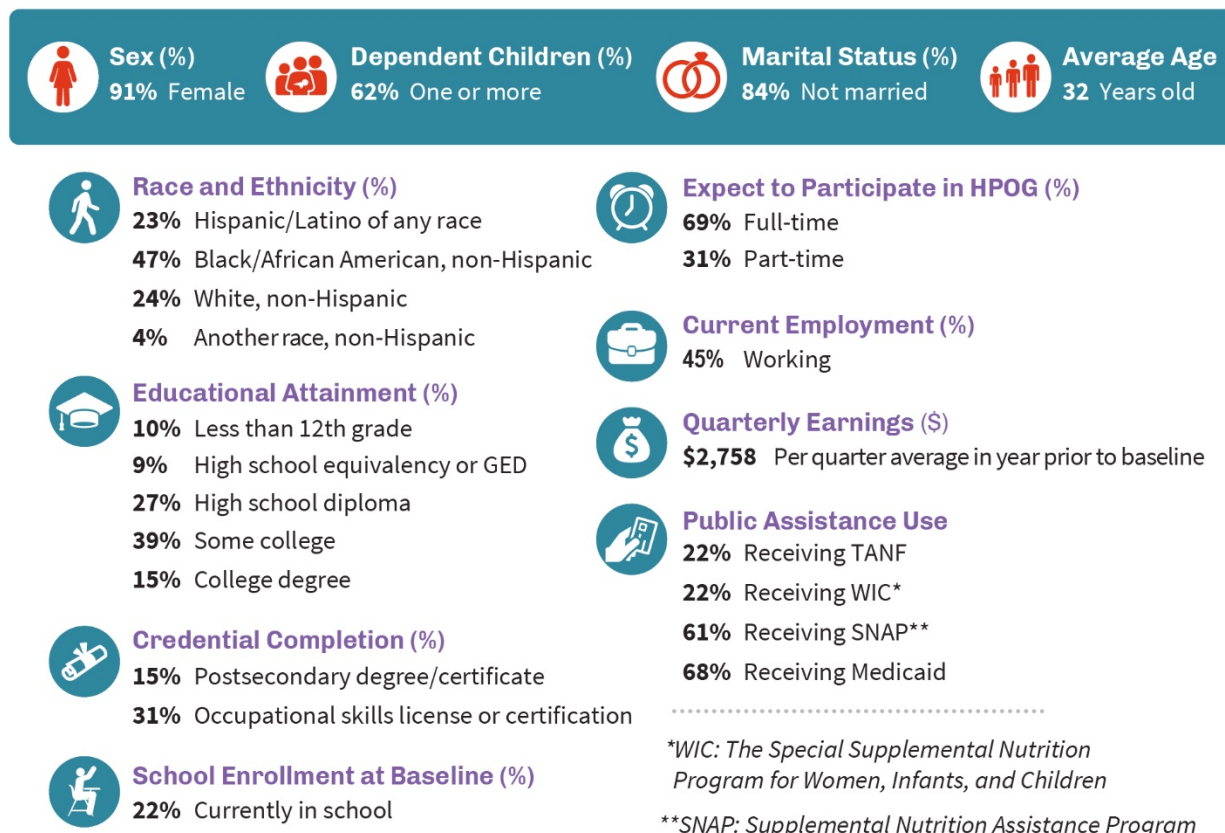
The HPOG 2.0 FOA established some eligibility criteria that grantees were to apply to all program applicants, including the criterion that applicants must be receiving TANF assistance or

<sup>10</sup> At that time, though outcomes for HPOG 1.0 were available, findings from the impact evaluation of HPOG 1.0 were not yet available.

meet other locally defined criteria for being “low income.” Grantees had broad discretion to set other eligibility criteria. For example, they could establish minimum grade-level standards for literacy and numeracy and could assess applicants’ suitability for a career in healthcare.

**Exhibit 1-2** shows the characteristics of those study members who were randomized into the study between February 2016 and February 2018, as of their entry into the study (at “baseline”).

**Exhibit 1-2 Selected Characteristics of HPOG 2.0 Study Members at Baseline**



Source: Participant Accomplishment and Grant Evaluation System

Note: The participant data immediately before randomization was pulled from the Participant Accomplishment and Grant Evaluation System on 3/24/2020. Those data are current through November 2019. The sample includes members of both treatment group and control group with random assignment dates between February 2016 and February 2018 (N=23,096). The quarterly earnings data at baseline were pulled from the National Directory of New Hires on 3/27/2020 on a slightly smaller sample of 22,443 study members with good social security numbers..

In summary, the HPOG 2.0 study members

- are overwhelmingly women;
- are older than students entering college immediately after high school;
- are more likely to have dependent children than are younger college students;
- have low earnings in the year prior to randomization (equivalent to about 20 hours per week at the minimum wage); and

- (most of them) are receiving public benefits through the Supplemental Nutrition Assistance Program (SNAP) and Medicaid programs; only about a fifth are receiving benefits from TANF or the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).

These tabulations suggest a nontraditional population for postsecondary education and training. Such a nontraditional population is consistent with the FOAs (OFA 2010, 2015): “The intent of the HPOG program is to provide education and training to individuals who would otherwise not have access to it” (2015, p. 3).

However, the HPOG 2.0 study members:

- already attended college prior to study enrollment (more than half have completed some college, compared to 38 percent of all low-income women older than age 25).<sup>11</sup>

With a few important exceptions, HPOG 2.0 study members have similar characteristics to HPOG 1.0 study members. Consistent with OFA’s intent when it crafted the HPOG 2.0 FOA, between HPOG 1.0 and HPOG 2.0 the number of TANF recipients enrolled in the program increased (from 13 percent to 22 percent) and the number of individuals currently enrolled in similar education or training programs at time of randomization decreased (26 percent to 22 percent).<sup>12</sup>

### 1.1.5 Training Received

HPOG 2.0 programs provided occupational healthcare training and, as appropriate, basic skills education. Chapters 2 and 3 of this report present estimates of the collective impact of HPOG 2.0 local programs on training received—from all sources—by combining data across programs then comparing the average outcomes of the treatment group versus the average outcomes of the control group.

To provide context for the discussion of the impact of HPOG 2.0, this section provides descriptive information on training provided to the treatment group by HPOG 2.0 local programs. This analysis is based on HPOG 2.0 program administrative data from the Participant Accomplishment and Grant Evaluation System (PAGES)<sup>13</sup> for those treatment group members randomized in the first 12 months of HPOG 2.0 (the sample with the longest follow-up period).

<sup>11</sup> “POV-29. Years of School Completed by Poverty Status, Sex, Age, Nativity and Citizenship,” Current Population Survey Detailed Tables for Poverty, United States Census Bureau. Accessed on September 17, 2020, [https://www.census.gov/data/tables/time-series/demo/income-poverty/cps-pov/pov-29.html#par\\_textimage\\_10](https://www.census.gov/data/tables/time-series/demo/income-poverty/cps-pov/pov-29.html#par_textimage_10).

<sup>12</sup> Here and unless otherwise stated throughout this report, HPOG 1.0 results are from the *Three-Year Impacts Report* (Peck et al. 2019) and refer to everyone randomly assigned under HPOG 1.0.

<sup>13</sup> PAGES is the system used for program management and performance monitoring, and to record grantee and participant data for use in HPOG 2.0 evaluations.

Through three years after randomization, among treatment group members (those offered HPOG 2.0) randomized in the first 12 months:

- Most commonly train for entry-level occupations;<sup>14</sup> that is, for a job likely to pay less than \$15 an hour (e.g., Nursing Assistant, Phlebotomist). About half have completed such entry-level training by three years after randomization (47 percent), and another 10 percent have started but not completed.
- Few train for above-entry-level training; that is, for a job likely to pay more than \$15 per hour (e.g., Licensed Practical Nurse, Licensed Vocational Nurse). About 16 percent have completed above-entry-level training, and another 9 percent have started but not completed.
- About a third have not yet completed any occupational training. Some of them—19 percent of those randomized in the first 12 months of HPOG 2.0—have started an occupational training and might complete it in the future.
- Some have completed multiple entry-level trainings; however, at least through three years after randomization, rarely have they followed entry-level training with above-entry-level training: only 3 percent have completed an above-entry-level training after completing an entry-level training, and another 4 percent have started but not completed an above-entry-level training after completing an entry-level training.

These results are little changed from HPOG 1.0 (see analysis by Klerman, Litwok, and Morris [forthcoming]).<sup>15</sup>

## 1.2 Overview of the HPOG 2.0 Impact Evaluation

This is the first report of the HPOG 2.0 National Evaluation's Impact Evaluation.<sup>16</sup> This section provides an overview of the evaluation; its *Design Plan* (Klerman, Judkins, and Locke 2019) and *Analysis Plan* (Judkins, Klerman, and Locke et al. 2020) provide more detail, as does Appendix C.

<sup>14</sup> PAGES defined a scheme of three Career Pathway Levels—entry-level, intermediate-level, high-level—based on the hourly wages. The discussion here combines the second and third levels into “above-entry-level.” This analysis assigns trainings to a Career Pathway Level using data from the HPOG 1.0 Three-Year Follow-Up Survey. Specifically, the analysis tabulates average hourly wage in the survey by last completed training, as reported in the HPOG 1.0 Performance Reporting System (the management information and reporting system for HPOG 1.0 grants, similar to PAGES). These average hourly wages by training are used to assign training to a Career Pathways Level. See the report, *Occupational Training for “Jobs That Pay Well”: Patterns from the Health Profession Opportunity Grants (HPOG)* (Klerman et al., forthcoming) for more on the assignment of Career Pathway Level and more analysis of the changes between HPOG 1.0 and HPOG 2.0.

<sup>15</sup> The analysis by Klerman, Litwok, and Morris (forthcoming) deliberately compares follow-up periods for HPOG 2.0 before the COVID-19 pandemic versus a similar follow-up period for HPOG 1.0. Specifically, for both HPOG 1.0 and HPOG 2.0, the analysis considers those entering in the respective program's first year and follows them for three years.

<sup>16</sup> See the text box **The HPOG Research and Evaluation Portfolio** just before Chapter 1 for an overview of the full HPOG 2.0 evaluation portfolio and the broader OPRE Career Pathways evaluation portfolio of which HPOG 2.0 is a part.

### 1.2.1 Research Questions

The Impact Evaluation’s major research questions are:

- What is the average impact of access to a local HPOG 2.0 program on receipt of training (in general and for healthcare professions) and support services, on employment and earnings (in general and in healthcare professions), and on broader measures of well-being?
- How does the impact of HPOG 2.0 programs vary with study members’ baseline characteristics (e.g., age, education, TANF receipt)?

These research questions consider *impact*; that is, outcomes for those study members offered access to their local HPOG 2.0 program relative to otherwise identical study members who were not offered that access.

### 1.2.2 Design

All 27 non-Tribal grantees randomly assigned eligible applicants by a lottery-like process to either to have access to HPOG 2.0 training and other services (treatment group) or not to have that access (control group). For every two applicants that grantees assigned to the treatment group, they assigned one applicant to the control group.<sup>17</sup> The control group could not access HPOG training and services but could access other education, training, and services in the community during the study period. Random assignment ensured that the only systematic difference between treatment group members and control group members was the offer of HPOG services.

This design was selected because it provides strong estimates of the evaluation’s primary research question: What is the impact of *being offered* access to HPOG 2.0 training and services, as opposed to the impact of *receiving* HPOG 2.0 training and services? Such an “intent-to-treat” design assesses whether the treatment group members obtained better outcomes from having access to the HPOG 2.0 local program (including all its components) than the outcomes they could have obtained without the program, but with access to other training and services in their local communities.

We chose this focus on the impact of the offer of access rather than the impact of receipt for several reasons. Beyond the local HPOG 2.0 program, there are other training options and the data clearly show that those not offered HPOG 2.0 often enroll in those other training options. When there are other options, the relevant policy issue concerns the incremental effect of this program—above and beyond the other programs. This is what intent-to-treat estimates. The alternative, treatment-on-the-treated, would estimate the impact of getting HPOG 2.0 training relative to getting no training. For most purposes, that comparison is less interesting. To see

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<sup>17</sup> In early 2020, the COVID-19 pandemic began severely affecting grant activities. Study member recruitment became much more difficult due to lockdowns and social distancing requirements, and training opportunities became more limited as training providers shifted to remote learning. In response, the research team worked with ACF and grantees to modify the treatment-to-control random assignment ratio from 2:1 to 3:1. This change allowed more people to receive HPOG services. In addition, during this period, ACF extended the end date on the grants from September 2020 to September 2021. The random assignment ratio change was fully implemented in December 2020 and remained in effect through the end of the grant period in September 2021.

that, consider the case where getting training—from HPOG 2.0 or from other training options—substantially increases earnings, but being offered HPOG 2.0 does not increase training at all. In that case, training is clearly good, but the HPOG 2.0 program does not increase training or earnings.

Our design and methods estimate the impact of some future HPOG 2.0-like grant program; not merely the impact of these 27 HPOG 2.0 local programs. As such, the estimates do not only apply to the past. Consistent with that perspective, we describe the estimates using the present tense.

Evaluating the separate impact of components of HPOG programs was not a primary goal of the evaluation. Strong estimates of the impact of separate components would have required a different research design, likely involving random assignment of components—either to programs or to individuals within program (as was done in HPOG 1.0). Later evaluation reports will include exploratory analyses of the impact of some components.

### 1.2.3 Analysis

This section provides a brief discussion of several aspects of analysis procedures. Most of the procedures are common in the analysis of randomized trials of social interventions. Additional details can be found in the separate appendix volume (Judkins et al. 2022), particularly Appendix Sections C.3 (nonresponse adjustment), C.6 (imputation), C.7 (variance estimation), and C.8 (regression adjustment).

#### *Intent-to-Treat*

Sometimes an evaluation attempts to directly evaluate training experiences such as earning a degree, where the question is whether earning a degree is better than not earning it. This HPOG 2.0 Impact Evaluation adopts the approach more conventional in experimental studies, focusing on the impact of being *offered* a new training opportunity.<sup>18</sup>

More specifically, this report’s impact estimates report the effect of the *offer* to receive a collection of training services (navigation, instruction, and support) from the local HPOG 2.0 program, not the impact of the services themselves. Such an impact is known as the “intent-to-treat” effect. Some treatment group members offered the opportunity to receive training services from the local HPOG 2.0 program choose not to participate in any of the services; likewise, some control group members (not offered the opportunity to receive HPOG 2.0 services) end up getting training and related support services outside of HPOG, sometimes quite similar to those offered by the local HPOG 2.0 program.<sup>19</sup> In fact, because many times the courses the HPOG 2.0 programs offer are those already taught at local community colleges, members of the control group can and sometimes do take the same courses as treatment group members, lacking only

<sup>18</sup> The treatment/control random assignment strategy prioritizes estimating the impact of the offer of the program versus not being offered. A design focused on “identification of successful activities” might have incorporated multi-armed random assignment in which the evaluation created alternative program models and who was assigned to those alternative program models was randomly assigned.

<sup>19</sup> HPOG 2.0 programs provided a list of alternative services—and sometimes informal referrals—to those randomly assigned to the control group.

access to the navigation and support services organized by the local HPOG 2.0 program and provided to treatment group members only.

Even if HPOG training helps program participants reach goals such as increased earnings relative to what they would experience without training, the impacts in this report reflect only how much *more* beneficial HPOG-supported training and services are than existing training and services in the community that anyone can access. Depending on the HPOG 2.0 local program, these extra benefits could accrue from the program expanding access to training, providing better training, providing better training supports, or any combination thereof.

There are a variety of alternatives to intent-to-treat analysis that are often referred to as “Treatment on the Treated” (TOT) analyses, in which the evaluator attempts to estimate the impact of treatment on those who actually receive treatment. Typically, such analyses involve discarding everyone from the treatment group who did not receive a meaningful dose of treatment and often also involve discarding everyone from the control group who received something similar to the treatment through an outside mechanism. This report does not contain any such analyses. There are several reasons why this evaluation chose not to perform any TOT analyses. Most important of these is the lack of relevance of TOT analyses to public policy.

The leaders of this evaluation believe that the impact of training relative to no training does not address the crucial policy issues. HPOG 2.0 control group members receive considerable training and services. HPOG 2.0 was established to get them more training and more earnings than they would have otherwise.

Beyond this lack of relevance to policy, TOT estimates are technically very difficult to prepare well and to justify. The researcher must first decide what level of treatment dosage is meaningful. For example, in the case of HPOG 2.0, would showing up for a pre-training group advising session be adequate? What about a single day of training? Second, the researcher must decide what constitutes equivalent treatment in the control group. For example, in the case of HPOG 2.0, would showing up for a single day of any type of training at any local training provider count or would it need to be healthcare training specifically? Third, since it cannot be assumed that those study participants dropped from the study (dropped from the treatment group because of lack of treatment or dropped from the control group because of experiencing something too similar to treatment) are random samples of those recruited for the study, non-experimental analysis methods must be employed that make strong, untestable assumptions about causal pathways.

In summary, this evaluation provides only intent-to-treat analyses because they are relevant to policy, conceptually clear, and can be justified without implausible assumptions about causal pathways.

### ***Multiple Comparisons***

This document reports estimates of impact for a large number of outcomes. In any impact evaluation, as the number of outcomes estimated increases, the likelihood grows sharply of at least one *false positive* result—meaning reporting the detection of an impact even when the program has no real effect whatsoever. With as few as 10 outcomes estimated, this likelihood of

one false positive result is already 40 percent. This is known as the “multiple comparisons problem.” For this reason, testing for program impacts on too many outcomes weakens the rigor of an evaluation. Authors and readers unaware of this multiple comparisons problem will often incorrectly treat a positive result among many negative or neutral results as evidence of program success, when in fact the positive result could be false.

We addressed the multiple comparisons problem by pre-specifying, prior to analysis, how we will interpret findings based on categorized hypotheses and (as discussed in detail in the next section) by limiting the number of key outcomes examined within each domain.

### ***Categories of Hypotheses***

An evaluation can respond to the problem of multiple comparisons while still preserving flexibility to explore a wide variety of outcomes. To do this, the research team can structure the analysis by establishing and specifying beforehand three categories of hypotheses: confirmatory, secondary, and exploratory. See Judkins, Klerman, and Locke (2020) for the outcomes pre-specified for this evaluation in each category.

As defined in the section **Important Terms for This Report** (just before this introductory chapter),

- A **confirmatory** outcome is a main indicator of the extent to which a program is making progress towards its goals. Accordingly, it is the focal outcome in text and in summary documents. In this short-term report, the confirmatory outcome is *educational progress*.
- **Secondary** outcomes are additional important outcomes identified in a program’s logic model (see the HPOG 2.0 logic model in Appendix Exhibit A-4). In this report, a favorable effect on any particular secondary outcome is only weak evidence of program success, however, because there are 10 secondary outcomes. Even if HPOG 2.0 were completely ineffective, the probability of falsely detecting a significant impact on at least one of those 10 secondary outcomes would be 40 percent, much too high a probability to serve as a sound basis for public policy. Secondary outcomes are also discussed in summary documents.
- All other outcomes are **exploratory**; any favorable effects on them should not be interpreted as evidence of program success. Because of this, exploratory outcomes are not discussed in summary documents where casual readers might overinterpret them. Despite these limitations, exploratory outcomes can be useful for further exploring why confirmatory- or secondary-level results arose.

When analyzing potential impacts on exploratory outcomes, this report considers not only the result of single tests but also the pattern of results across the entire block of related outcomes. For example, if there are tests for impacts on 10 related outcomes and only one is statistically different from zero, then the reader is advised to interpret its meaning with caution.

### ***Statistical Testing***

As is standard in evaluations of public programs, this report’s formal statistical analysis starts from a default assumption that the HPOG 2.0 model is ineffective. Formally, this default

assumption is referred to as the “null hypothesis.” *Statistical testing* is the process of assessing the evidence against that null hypothesis of no impact. In this evaluation, all estimated impacts and most other explicit comparisons have been formally tested for no impact (overall) or no difference in impact (for subgroups). If we find strong evidence to reject the null hypothesis, we conclude that the program is effective. Unless explicitly noted otherwise, the text discusses only impacts with strong evidence against the null hypothesis.

For confirmatory and secondary outcomes of HPOG 2.0, these statistical tests are “one-sided,” meaning that the null hypothesis excludes the possibility that the program might be not only merely ineffective but actually harmful. This allows the tests to be more sensitive to small improvements caused by HPOG 2.0. One-sided tests are appropriate, however, only when the theory of action strongly supports the alternative hypothesis that HPOG 2.0 will improve the outcome. For example, we test for an increase in training receipt but not for a decrease.

“Two-sided” tests can detect both beneficial and harmful effects of HPOG 2.0. They are the default for most scientific investigations, but two-sided tests are less sensitive to beneficial effects than are one-sided tests. For exploratory outcomes of HPOG 2.0, this evaluation uses two-sided tests.

### ***p-Values***

Each test is driven by a statistic that is large when the null hypothesis is *false* (i.e., when the program has an impact on that outcome) and is small when the null hypothesis is *true* (i.e., when the program has no impact on that outcome). Most commonly, this statistic is the absolute value of the ratio of the impact to its estimated *standard error*, a measure of the precision of the estimated impact. An impact estimated from a large sample size will generally have a small standard error, whereas an impact estimated from a small sample size will generally have a large standard error.

The takeaway from each test is called a “*p*-value.” It is the probability of observing (by chance) a value of the test statistic as large or larger than the observed value when the null hypothesis is true (i.e., when the program has no impact on that outcome). That is, a small *p*-value is strong evidence for an HPOG 2.0 impact on the outcome, whereas a large *p*-value signals that evidence for an HPOG 2.0 impact is weak.

For the most part, this report does not show *p*-values in the impact exhibits. Rather, asterisks (\*) and hashtags (#) are used to flag estimated impacts with small *p*-values—that is, values where the evidence is strong enough to reject the null (no impact) hypothesis.

Asterisks distinguish results where a two-sided test reveals a statistically significant impact:

- \* at the 5 percent level ( $p < .05$ )
- \*\* at the 1 percent level ( $p < .01$ )

Hashtags distinguish results where the one-sided null hypothesis is rejected:

- # at the 5 percent level ( $p < .05$ )
- ## at the 1 percent level ( $p < .01$ )

Any impacts flagged with asterisks or hashtags are said to be “statistically significant.” Furthermore, unless explicitly noted otherwise, only results statistically significant at the 5 percent level ( $p < .05$ ) are discussed in the text.<sup>20</sup>

### ***Prospective Inference***

An important step in the calculation of a test statistic and its associated  $p$ -value is the calculation of the standard errors on estimated impacts. In evaluations of the impact of collections of programs (as in the impact evaluations of HPOG 1.0 and 2.0), there is substantial controversy in the research literature about how to calculate the standard errors. The two main alternatives are “prospective” and “retrospective.” In a prospective analysis, the researcher uses information about the consistency of impacts across programs to strengthen inferences about the likely experiences of *future* cohorts of study subjects. In contrast, in a retrospective analysis, the researcher ignores information about the consistency of impacts across programs and instead tries to maximize power to detect any impacts on the pooled set of study subjects who had been *actually recruited* for this evaluation. The two approaches are equivalent if the programs of interest have identical true impacts. Otherwise, the prospective standard errors are larger because they include uncertainty generated by the selection of sites.

In the case of heterogeneous effects across programs, the use of prospective standard errors will result in fewer impacts being deemed statistically significant. Nonetheless, these larger standard errors are the appropriate measures of uncertainty for extrapolating results from evaluation of decentralized programs to a similar potential future program. This likely impact of such a program at some new site is usually the key prospective policy choice. Accordingly, this report’s analysis uses prospective standard errors.<sup>21</sup>

### ***Statistical Significance versus Substantive Relevance***

This study’s large sample sizes imply that some estimated impacts are statistically significant but are not large enough to be of substantive relevance. Based on comparisons to other evaluations and models projecting future earnings gains based on additional training, the report notes when statistically significant results can plausibly be viewed as substantively irrelevant.

In addition, sample sizes are large enough that most outcomes with true impacts large enough to be substantively relevant should be statistically significant. This is not true for subgroup analyses, where smaller sample sizes mean that substantively relevant impacts can easily go undetected.

The text box **How to Read This Report’s Impact Exhibits** (at the end of this chapter) provides additional discussion of how to understand the results presented in this report.

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<sup>20</sup> Appendix Section C.7 provides further discussion of testing procedures.

<sup>21</sup> For readers who prefer retrospective standard errors, tests of confirmatory and secondary outcomes using them appear in Appendix Section C.7. HPOG 2.0 program impact estimates based on retrospective analytic methods are, as expected, very similar to those based on prospective analytic methods. The HPOG 2.0 retrospective variance estimates, however, are much smaller than prospective variance estimates for education outcomes. Somewhat surprisingly, variance estimates for HPOG 2.0 program impacts on earnings and other life outcomes did not vary much by prospective versus retrospective.

### 1.2.4 Outcomes

For this report, the HPOG 2.0 Impact Evaluation pre-specified a confirmatory outcome in its *Design Plan* (Klerman, Judkins, and Locke 2019):

- **educational progress in the five quarters after randomization** (where “progress” means having completed training by earning a credential or having been continuously enrolled in training).

The evaluation also estimates impacts on a range of other outcomes: *training received, support services received, training costs, earnings, healthcare employment, career progress, skills, and well-being*. The evaluation pre-specified 10 of these outcomes as secondary (see Judkins, Klerman, and Locke [2020] for the list).<sup>22</sup> These secondary outcomes receive more prominent attention in the analysis than other outcomes and all subgroup analyses that are deemed exploratory. Consistent with the ACF Evaluation Policy’s principle of transparency,<sup>23</sup> both the *Impact Evaluation Design Plan* and *Analysis Plan* were published and the designated confirmatory and secondary outcomes were registered prior to conducting any analyses.<sup>24</sup>

### 1.2.5 Sources of Variation in Impacts

This report includes explorations of variation in impacts across subgroups defined by participant characteristics at baseline (Sections 3.5 and 4.5). Following the *Analysis Plan* (Judkins, Klerman, and Locke 2020), these explorations involve only a small number of outcomes and a modest number of dimensions of participant characteristics, such as demographics, baseline skills, experiences, advantages, and challenges. Discussion focuses on differences in impacts for different values of the subgroup (e.g., impact for men versus impact for women). This report also discusses the level of variation in impacts across local HPOG 2.0 programs in Section 3.6.<sup>25</sup>

### 1.2.6 Data Sources

Data from PAGES about the history of study members prior to study enrollment were used to define subgroups for the study of variation in impacts mentioned in Section 1.2.5, and were used in technical adjustments to the estimated overall impacts (Appendix Section C.8). PAGES also contains data on training, services, and employment outcomes for the period following study enrollment. However, this report uses none of these data because they are missing for members of the control group.

<sup>22</sup> Future reports, with longer follow-up periods, will give higher priority to labor market outcomes—both total earnings and employment in healthcare. Future reports will also give more attention to pathways through which the program achieves its impact.

<sup>23</sup> <https://www.acf.hhs.gov/opre/report/acf-evaluation-policy>

<sup>24</sup> Analysis plans for this report were registered with both the Open Science Framework (OSF) and the Registry of Efficacy and Effectiveness Studies (REES). REES is jointly run by the Inter-university Consortium for Political and Social Research and the Society for Research on Educational Effectiveness. The OSF registration is at <https://osf.io/nv2fz>. For the REES registration, search for “HPOG” at <https://sreereg.icpsr.umich.edu/sreereg/search/search>.

<sup>25</sup> We report impacts for individual programs in Appendix H.

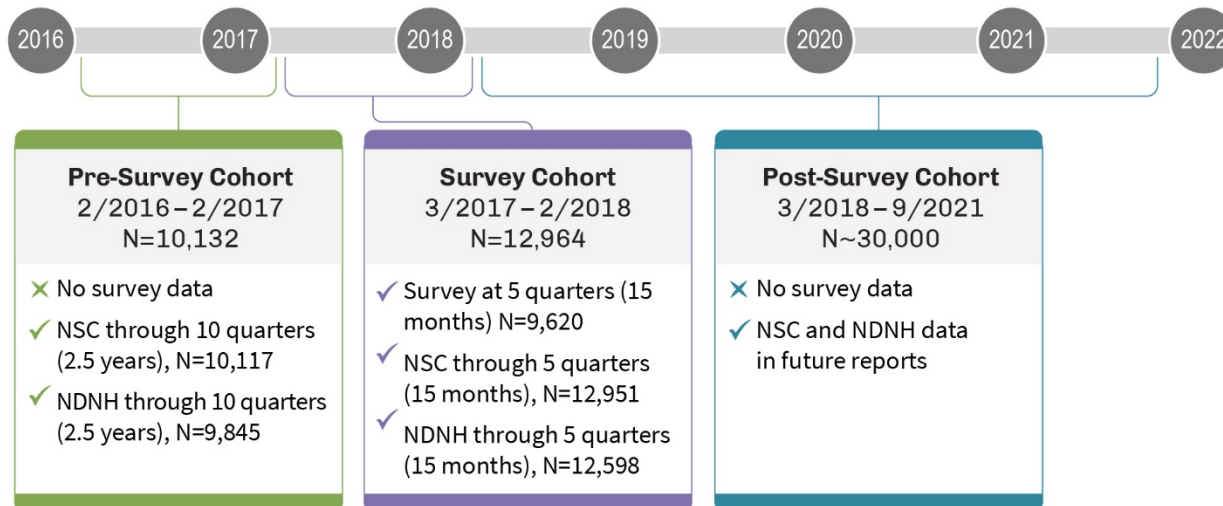
The body of this report provides analyses of outcomes measured with three data sources (see **Exhibit 1-3**):

- The **Short-Term Follow-Up Survey** of study members provided information on a range of outcomes including training and support services received, employment history, earnings, and participation in means-tested public benefit programs. Survey interviewing began 14 months after randomization. Most of the survey interviews were completed 15 to 17 months after randomization; some were completed as late as 29 months after randomization. Median time to interview was 17 months; mean time was 17.8 months. The survey attempted to interview everyone in the “survey cohort” (defined below). Interviews were completed with 9,620 study members, yielding an average response rate of 74 percent.<sup>26</sup>
- The **National Student Clearinghouse (NSC)** provides information on enrollment in and degree receipt from most colleges and universities, including public community colleges, the most frequently attended type of postsecondary institution for students in this study. With rare exceptions, NSC excludes both Title IV schools that are not degree granting and schools that do not meet federal requirements to be Title IV schools. This study’s primary use of NSC data was to measure full-time-equivalent months of study at colleges and degree receipt. Although the NSC does not capture the sorts of short-term training facilitated by many HPOG programs, its data are useful for measuring the extent to which HPOG trainees “ladder up” to more advanced training opportunities over longer follow-up periods. See Section 2.5 and Appendix Section C.4 for more on NSC data.
- The **National Directory of New Hires (NDNH)** provides information on quarterly employment and earnings. The data are based on state Unemployment Insurance filings, augmented by federal payroll information. As such, NDNH misses self-employment income and informal sector earnings not reported to state Unemployment Insurance programs. See Section 4.5 and Appendix Section C.5 for more on NDNH data.

Contributions of these three sources to the study dataset vary by “cohort,” as shown in Exhibit 1-3.

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<sup>26</sup> The response rate for the HPOG 2.0 Short-Term Follow-Up Survey was substantially higher in the treatment group (77 percent) than in the control group (70 percent).

**Exhibit 1-3 Short-Term Impact Study Analysis Cohorts, Follow-Up Periods, Sample Sizes, and Data Sources**

Survey data are available only for the “**survey cohort**”—study members randomized between March 2017 and February 2018, inclusive. This report refers to survey results as “short-term” findings; regardless of the timing of the survey interview, most of the outcomes are measured as of 15 months. The text notes when outcomes instead are measured as of the time of interview. See Appendix Section C.2 for more on these data.

Analyses of administrative data primarily report on the “**pre-survey cohort**”—study members randomized between February 2016 and February 2017, a sample of 10,132 study members. NSC data are available for almost the entire pre-survey cohort (10,117), but NDNH data are available only for 9,845 study members due to issues with reported Social Security numbers. For this pre-survey cohort, administrative data provide quarterly outcomes through two and a half years of follow-up, more than a year beyond the measurement point for most Short-Term Follow-Up Survey outcomes.<sup>27</sup> This report refers to these administrative data results for the pre-survey cohort as “**intermediate-term**” findings. Analyses of the pre-survey cohort with administrative data provide insights into the extent to which short-term findings (first 15 months or so after randomization) are likely to continue into the intermediate term (first two and a half years or so after randomization).

The “**post-survey cohort**” will be analyzed in future reports. At the end of September 2021, a new contract was awarded to Abt Associates to conduct a second, Short-Term Follow-Up Survey, restricted to those study members enrolled in the study after the outbreak of the COVID19 pandemic.<sup>28</sup> An analysis plan for this survey has not yet been prepared.

<sup>27</sup> Exhibits in Chapters 2, 3, and 4 report results for this entire pre-survey cohort. Additional administrative data are available for survey cohorts at shorter follow-up periods and for those randomized towards the start of the pre-survey period at longer follow-up periods. Comparing pre-survey cohort and survey cohort impacts for Q5 and Q6 after randomization reveals no evidence of cross-cohort differences. Throughout the report, footnotes discuss outcomes for which considering other samples suggests different results.

<sup>28</sup> Specifically, all 6,848 study members randomized after May 10, 2020, will be eligible for the survey.

In this Short-Term Impact Report, unless otherwise noted, all impact analyses are for the survey cohort, and outcomes are measured as they were measured in the Short-Term Follow-Up Survey.

### 1.3 Findings from the Broader Training Literature and from HPOG 1.0

This section considers past evidence on the impacts of job training programs in general and career pathways programs in particular.<sup>29</sup> The literature consistently shows that job training programs substantially and statistically significantly improve training outcomes (e.g., receipt of any training, months of training, and receipt of credentials).

In contrast, the extent to which and the conditions under which job training programs increase earnings are areas of ongoing debate and analysis in the policy literature. For some job training programs, experimental evaluations provide clear evidence of increases in earnings, often of more than \$1,000 per quarter, roughly a 20 percent increase relative to the control group. In contrast, many other experimental evaluations of job training programs in general and career pathways in particular do not detect any impact of training on earnings.

The evaluation of a job training program with most relevance to HPOG 2.0 is, of course, HPOG 1.0. Its *Three-Year Impacts Report* (Peck et al. 2019) found that HPOG 1.0 increased completion of training by 13 percentage points (from 63 to 75 percent) but did not increase average earnings. The logic model for job training posits that more job training induces higher earnings. These three-year findings of more training but not higher earnings—findings not isolated to HPOG 1.0—raise questions about the logic model, at least as implicitly interpreted by that report’s confirmatory outcome.<sup>30</sup>

There are plausible reasons to expect HPOG 2.0’s impact on earnings to be larger than HPOG 1.0’s. As described earlier, applicants for HPOG 2.0 awards responded to a new FOA that was specifically modified to encourage longer and follow-on training. Furthermore, HPOG 1.0 grantees then applying for HPOG 2.0 funds (14 of the 27 were such “returning” grantees) could build on their HPOG 1.0 experience to refine their program models.

Another reason why the impact of HPOG 2.0 on earnings might be larger than that of HPOG 1.0 concerns timing. HPOG 1.0 local programs did not begin random assignment until Year 3 of the five-year grants. As a result, on average, the HPOG 1.0 impact sample had only about two years until the HPOG 1.0 grant—and the support it provided to program participants—ended.<sup>31</sup> Many of those program participants might not have had enough time before HPOG 1.0 ended to

<sup>29</sup> Appendix B provides a tabular summary of the studies supporting the statements in this section.

<sup>30</sup> The *PACE Three-Year Cross-Site Report* (Juras and Buron 2021) and the final chapter of the HPOG 1.0 *Three-Year Impacts Report* (Peck et al. 2019) consider several possible approaches to reconciling impacts on training, but not on earnings.

<sup>31</sup> HPOG 1.0 study members on average were randomized *late in Year 4*. They would have had part of Year 4, all of Year 5, and—given that many grantees received no-cost extensions—part of the next year to get HPOG 1.0–supported training. Those randomized *earlier* would have had slightly more time, up to three years. Those randomized *later* would have had less time; some only a few months. Those randomized towards the end of the grant period could have completed only a short training and often did not have time to finish it with program support.

finish longer training programs or to return for follow-on training. In contrast, HPOG 2.0 local programs began randomization as soon as they began enrolling trainees in February, 2016. When HPOG 2.0 funding ended in 2021, some treatment group members had more than five years of access to HPOG 2.0.

Having more time could support higher rates of enrollment in and completion of longer trainings and more follow-on training than is reported in this short-term report. Higher rates of longer trainings and more follow-on training might lead to larger impacts on earnings, but perhaps not until the coming *Intermediate-Term Impact Report* with another two years of follow-up.

## 1.4 Organization of This Report

The balance of this report proceeds as follows:

- **Chapter 2**—impacts on starting training, receipt of support services, and financing of training.
- **Chapter 3**—impacts on months of training persistence, credential receipt, and this report's confirmatory outcome, *educational progress*.
- **Chapter 4**—impacts on labor market outcomes (employment, earnings), perceptions of career progress, and household well-being.
- **Chapter 5**—summary of findings and discussion of open issues for the next impact report.

A separate **technical appendix volume** (Judkins et al. 2022) includes additional material on context (Appendix A); on the literature (Appendix B); on data and methods (Appendix C); supplemental results (Appendices D through F, corresponding to Chapters 2 through 4; an attempt to separate the effects of HPOG post-enrollment supports from the effects of HPOG on enrollment (Appendix G); and local-program-specific estimates (Appendix H).

## 1.5 Future HPOG 2.0 Reporting

Future HPOG 2.0 reports will provide impact estimates for longer follow-up periods:

- *Intermediate-Term Impact Report*—planned for 2023, to include analyses of the Intermediate-Term Follow-Up Survey data (three years after randomization) and administrative data through approximately four years after randomization.
- *Longer-Term Impact Report*—planned for 2025, to include analyses of the Long-Term Follow-Up Survey and administrative data through approximately six years after randomization.

In particular, note that all outcomes and impacts included in this *Short-Term Impact Report* predate the onset of the COVID-19 pandemic in 2020. Later reports will include outcomes and impacts long after its onset. Sources of information about impacts during the pandemic and its aftermath include the NSC, NDNH, and a new round of short-term follow-up. This new round will use a slightly modified version of the survey instrument used for this report and will be administered to study members randomized between spring 2020 and the end of the HPOG 2.0

grants in September 2021. The Short-Term Follow-Up Survey interviews for this “COVID cohort” are scheduled to start in late fall 2021 and continue through April 2023. A report on this extra round of short-term follow-up will be published in 2025, most likely separate from the Longer-Term Impact Report also planned for 2025.

Each of these future reports will include analyses that search for patterns in the effects of the 38 local programs.

### How to Read This Report's Impact Exhibits

Most exhibits presenting impacts in Chapters 2 through 4 use the format shown below. The first column on the left (**Outcome**) identifies the outcomes whose findings appear in the rows, as well as the units of that outcome (e.g., percent, dollars, months). To the right are the following data columns:

1. **Treatment Group Mean** presents the mean outcome for the treatment group, adjusted to correct for random baseline differences between the treatment group and control group (as explained in Appendix Section C.8).
2. **Control Group Mean** presents the mean outcome for the control group.
3. **Impact (Difference)** is the difference between the treatment group and control group means—that is, the impact on that outcome of the treatment group members being offered the intervention.
4. **Standard Error** presents a measure of uncertainty in the impact. It reflects chance variation due to randomization, measurement error in the outcome, and variability of the effect across HPOG 2.0 local programs.
5. **Relative Impact (%)** presents the impact as a percentage change from the control group mean. For outcomes where no natural unit of measurement exists (e.g., psychological well-being), this column reports **Effect Size** instead of relative impact. The effect size is the ratio of the impact to the standard deviation of the pooled sample (i.e., “pooled” meaning across the treatment and control groups). Both relative impact and effect size offer a sense of how “big” or “small” the impact of the intervention is on the treatment group relative to the control group.

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
Employment rate in Q1 (%)	66.9	69.9	-2.9**	1.0	-4.2
Sample size	580	468			

Source: Short-Term Follow-Up Survey

Note: “Relative impact” represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

In the example exhibit above:

- **Impact (Difference).** Members of the treatment group had an adjusted employment rate in the first quarter following the quarter of randomization of 66.9 percent. The employment rate in the control group was 69.9 percent. So HPOG 2.0 reduced employment in this quarter by 2.9 percentage points, as shown in this cell. That the impact is flagged with two asterisks (\*\*) means that the null hypothesis of no HPOG 2.0 impact on this outcome is rejected with a  $p$ -value smaller than .01, as discussed in Section 1.2.3 (“Analysis”).
- **Standard Error.** The standard error on this impact is 1.0 percentage points. This standard error can be used to construct a *confidence interval*—meaning the reader can have strong confidence

that the true impact is within roughly two standard errors of the reported impact. In this example, then, a strong confidence interval runs from an impact (reduction) of -4.9 percentage points to -0.9 percentage points.

- **Relative Impact (%)**. The relative impact is (before rounding the numbers in the computation) a 4.2 percent reduction, calculated as  $100 \times [-2.9 / 69.9]$ .

### ***Highlighting of Confirmatory and Secondary Outcomes***

Exhibits presenting impacts indicate confirmatory outcomes using ***bolded and italicized text*** and secondary outcomes using **bolded text**. This is done both to signal that these outcomes were pre-specified as focal and to alert the reader that these are one-sided tests. Outcomes that are neither bolded nor italicized represent exploratory outcomes. In the example exhibit above, that “Employment rate in Q1 (%)” is not bolded or italicized indicates that it is an exploratory outcome, and therefore the formal test for statistical significance of the estimated impact was two-sided, also explained in Section 1.2.3 (“Analysis”).

### ***Sample Size***

Sample sizes for the treatment group and control group typically appear in the final row of the exhibit, as in the shown example. Where the sample size varies strongly from row to row (outcome to outcome), the treatment and control sample sizes will be shown in two additional columns at the far right of the exhibit.

For survey outcomes with unimputed missing data, these sample sizes reflect the number of study members eligible to answer the question, rather than the number with non-missing responses.

## 2. Impacts on Starting Training, Supports, and Costs

### Chapter 2 Key Findings

Analyses of the **Short-Term Follow-Up Survey** responses at about 15 months after randomization find that HPOG 2.0 programs:

- Increase starting training (by 19 percentage points; 34% higher than the control group).
- Increase starting training for all subgroups (except those already in training at baseline), with larger effects for study members who are older, employed at baseline, and not receiving means-tested public benefits.
- Decrease out-of-pocket expenditures by trainees and their families, despite more training.

Analyses of the **Short-Term Follow-Up Survey** responses at about 15 months after randomization find that, among respondents who start training and compared with the control group, members of the HPOG 2.0 treatment group:

- Have lower educational expenses and less difficulty financing their education.
- Are no more or less satisfied with training.

Analyses of **National Student Clearinghouse** data find that HPOG 2.0 programs:

- Increase college enrollment for the pre-survey cohort, though the increase is smaller than in the survey data.

Grantees' HPOG 2.0 programs offered the treatment group training, support services for that training, and assistance with tuition and other costs. This chapter reports estimates of the impact on those outcomes of being offered access to the programs.

The first four sections of this chapter present estimates based on the Short-Term Follow-Up Survey. Section 2.1 considers starting training by the fifth quarter following randomization. Section 2.2 presents impacts on receipt of basic skills education. Section 2.3 presents impacts on the use of cognitive skills in everyday life. Section 2.4 considers the student experiences for those who enroll in training, including support services, training costs, and satisfaction with training. Section 2.5 uses NSC data to estimate impacts of HPOG 2.0 programs on starting education or training for the pre-survey cohort, through more than three years after randomization. Finally, Section 2.6 summarizes the chapter's findings and considers their implications.<sup>32</sup>

### 2.1 Starting Training

Using information from the Short-Term Follow-Up Survey through five quarters after randomization, this section considers impacts on starting training—that is, any training from any source (not just through an HPOG 2.0 program) since randomization. Many of those participants

<sup>32</sup> Section 3.5 of Chapter 3 presents explorations of how the impact of grantees' HPOG 2.0 programs on enrollment and other education outcomes varies across subgroups.

who get any training since randomization had also gotten some training before randomization and some already were enrolled in training at the time of randomization (see **Exhibit 2-1**).

▪ **HPOG 2.0 programs moderately increase starting training.**

More than three-quarters (76 percent) of the treatment group start training (broadly defined to include both basic skills education and occupational healthcare training). The rate is considerably lower in the control group (56 percent), indicating an impact of 19 percentage points—an increase of more than a third relative to the control group level.

HPOG 2.0's impact on this outcome of 19 percentage points is twice as large as the corresponding impact of HPOG 1.0 was on this outcome (9 percentage points). This is both because the share in the HPOG 2.0 treatment group who start training is higher (76 percent versus 71 percent in HPOG 1.0) and because the share in the HPOG 2.0 control group who start training is lower (56 percent versus 61 percent in HPOG 1.0).

**Exhibit 2-1 Impact of HPOG 2.0 on Starting Training, by Source and Type of Training**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Cumulatively through 15 Months:</b>					
Started training at any training provider (%)	75.5	56.4	+19.1**	2.7	33.9
Started training at a college (%)	54.7	39.6	+15.1**	2.7	38.2
Started training at some other Title IV postsecondary school (%)	8.8	7.2	+1.6	0.9	22.2
Started training at some other training provider (%)	16.9	12.0	+4.9**	1.5	40.8
<b>Cumulatively as of the Survey:</b>					
Started any training to prepare for a healthcare occupation <sup>a</sup> (%)	71.8	51.2	+20.6**	2.5	40.2
Started any training to prepare for a non-healthcare occupation (%)	2.1	4.3	-2.2**	0.6	-50.3
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

<sup>a</sup> Healthcare occupation defined here by direct self-classification. See Appendix Exhibit D-1 for impacts based on alternate classification methods.

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests. "Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Two complementary perspectives are useful for thinking about the implications of the high level of starting training in the control group (56 percent). From one perspective, it is clear that applicants to the grantees' HPOG programs are motivated and situated in a training-rich environment. For HPOG 2.0 to have any impact, starting training in the treatment group needed to be above that high control group level of training. HPOG 2.0 succeeded at significantly increasing starting training despite this high bar.

The HPOG 2.0 FOA suggests another perspective. The FOA instructed grantees to enroll "TANF recipients or other low-income individuals [who] would not otherwise have access to the education and training activities proposed" (p. 24). Compared with HPOG 1.0 grantees, HPOG

2.0 grantees succeeded in recruiting program participants who were significantly less likely to have enrolled in training absent the program, and especially less likely to already be in any training at the time of HPOG program application. Nevertheless, comparing rates of starting training in the treatment group versus the control group suggests that three-quarters of those who received training with HPOG 2.0 would have found other routes to training in the absence of the program.<sup>33</sup>

- **Across programs, most of HPOG 2.0's impact on training comes from training provided by colleges.**

Training can be conducted in a variety of settings. The Short-Term Follow-Up Survey captures training that includes class-taking and lasts at least one week. This excludes much on-the-job training, which generally does not include a substantial classroom component. The first panel of **Exhibit 2-1** presents information on starting training by the source of training. Training providers are categorized as colleges (defined as degree-granting postsecondary schools), other Title IV postsecondary schools (those whose students can receive federal financial aid), and other training providers (including employers, unions, social service agencies, and schools that do not participate in Title IV programs, among others).

HPOG 2.0 programs increase starting training at a college by 15 percentage points. HPOG 2.0 programs also increase training at other training providers by 5 percentage points. There is no impact on training at other Title IV postsecondary schools.

- **HPOG 2.0 programs increase enrollment in healthcare training.**

HPOG 2.0 programs facilitated training only for healthcare professions. By applying, study members showed an interest in that training. The second panel of **Exhibit 2-1** shows that consistent with this interest, almost all of the training received by both the treatment and control groups is for healthcare. Moreover, HPOG 2.0 programs boost receipt of any healthcare training by 21 percentage points, from 51 to 72 percent.<sup>34</sup>

The increase in receipt is almost all the result of more training, rather than a shift away from non-healthcare training. Non-healthcare training decreases by only 2 percentage points, whereas healthcare training increases by 21 percentage points.

## 2.2 Basic Skills Education

The HPOG 2.0 FOA specified that grantees were to provide basic skills education to “individuals who would not otherwise have access to education and training, including those with low reading and math skills” (OFA 2015). In this report, basic skills education primarily means

<sup>33</sup> The three-quarters is the ratio of starting training in the control group to starting training in the treatment group; that is,  $75\% = 56\% / 76\%$ .

<sup>34</sup> Not all of this training was strictly in healthcare occupations. Some was in allied occupations that are commonly employed in healthcare institutions, such as Medical Office Secretary or Medical Insurance Coder. Respondents were simply asked whether any of their training was to prepare for work in an occupation in the “field of healthcare.”

reading and math skills that are required for training at the postsecondary level and includes both adult basic skills education and college developmental education. (See **Exhibit 2-2**.)

▪ **HPOG 2.0 programs increase enrollment in basic skills education.**

HPOG 2.0 increases by 6 percentage points the likelihood of ever enrolling in basic skills or developmental education, a relative impact of 46 percent. The relative increase in the likelihood of having either completed basic skills education or still being enrolled is similarly large, as is the increase in the likelihood of being allowed to enroll in regular postsecondary courses after completing basic skills education.

**Exhibit 2-2 Impacts on Basic Skills Education Course-Taking**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>As of the Survey</b>					
Ever enrolled in basic skills education course (%)	20.1	13.8	+6.3**	1.7	45.6
Completed or still enrolled in basic skills education course (%)	17.6	12.1	+5.5**	1.5	45.2
Allowed to enroll in regular courses after completing basic skills education course (%)	12.6	8.8	+3.7**	1.2	42.2
Sample size	888	404			

Source: Short-Term Follow-Up Survey

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests. "Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

## 2.3 Use of Cognitive Skills and Self-Directed Learning

In addition to skills and credentials specific to a particular job, occupational and basic skills education can develop in students' cognitive skills and learning strategies that apply to a wide variety of careers and life situations. HPOG 2.0 programs might affect the use of these skills/strategies either by providing training that develops them directly or by enabling treatment group members to enter occupations that provide more opportunities for learning them. Short-Term Follow-Up Survey respondents provided information on their use of such skills in everyday life, self-directed online learning, and research they conducted about schools and careers. Results from these questions discussed below are provided in Appendix Exhibit D-5.

▪ **HPOG 2.0 programs increase the use of reading skills, but not computer or math skills.**

HPOG 2.0 leads to a small improvement in the use of reading skills: by 3.4 points on a scale score. The scale ranges from 100 to 500 and is constructed based on responses to five questions about the frequency with which sample members engage in various reading activities

in everyday life (e.g., reading directions or instructions). This 3.4-point impact is equivalent to an effect size of 0.05, commonly considered a “small” effect (Cohen 1988).<sup>35</sup>

HPOG 2.0 programs do not significantly increase computer or math skill use, based on similarly constructed scales.

- **HPOG 2.0 programs increase all types of self-directed online learning.**

HPOG 2.0 increases the use of all kinds of self-directed learning asked about on the survey, including watching an online video to try to solve a math problem, taking an online course to prepare for admission exams, and taking an online course to improve English language skills or vocabulary. Some 63 percent of the treatment group uses one or more of these strategies, an increase of 7 percentage points over the control group.

- **HPOG 2.0 programs increase in-person career research, but not school research.**

HPOG 2.0 increases by 3 percentage points the likelihood that sample members visit a local business to learn more about a career. HPOG 2.0 does not affect online research about careers, or online research about schools that offer courses to prepare for those careers. Among both the treatment and control groups, such online research was very common, however.

## 2.4 Student Experiences

This section compares the experiences—receipt of support services, costs, and training satisfaction—of the subgroup of treatment group members who enroll in any training versus the experiences of the subgroup of control group members who enroll in other training available in the community. This section refers to these groups as treatment group and control group “trainees,” although some enroll only in basic skills education. Study members who did not enroll in any education or training were excluded from the analyses.<sup>36</sup>

These contrasts between treatment and control group trainees are *not* randomization-based estimates of the impact of HPOG 2.0 programs on post-enrollment experiences. The contrast could be due either to an HPOG-induced change in who starts training or to HPOG-induced changes in the training experience. Despite the inability to tell which path is active, these contrasts are interesting because they show how the training environment under HPOG 2.0 services and support is different from what control group trainees experience on their own. These compositional effects could be of particular interest to training providers. For example,

<sup>35</sup> The concept of an effect size was developed to facilitate comparisons across related studies where outcomes are measured on varying scales. It is particularly useful for scales such as career progress for which no natural scale such as dollars or percent agreement is available. Effect size is defined as the ratio of the impact to the standard deviation of the outcome. An alternative way to compare studies with different scales is to state what percentage of the treatment group has a higher score than the median score for the control group. On that metric, an effect size of 0.05 means that 52 percent of the treatment group has a higher level of career progress than the median response in the control group.

<sup>36</sup> Put differently, these analyses are conditional on any education or training.

the contrasts indicate the volumes of support services providers need to be prepared to deliver in order to match the HPOG 2.0 experience.

### 2.4.1 Support Services

OFA's FOA encouraged grantees to use HPOG funding for several types of services, including academic supports, case management, child care, transportation assistance, tuition assistance, other training-related financial support, and instruction in workplace behaviors. These support services are an important part of the HPOG 2.0 logic model. They are one of the main ways a local HPOG program can influence outcomes after their students enroll in training. (See **Exhibit 2-3**.)

**Exhibit 2-3 Impact on Receipt of Educational Supports, Conditional on Any Training since Randomization**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>As of 15 Months after Randomization, Ever Received:</b>					
Tutoring (%)	31.3	29.7	+1.6	1.7	5.5
Academic advising (%)	49.1	47.0	+2.1	2.3	4.5
Financial aid advising (%)	39.5	42.7	-3.2	2.1	-7.5
Career counseling (%)	38.8	33.3	+5.5**	1.8	16.4
Job search or placement assistance (%)	39.5	32.6	+6.9**	1.7	21.0
Either career counseling or job search/placement assistance (%)	52.6	46.3	+6.3**	1.6	13.7
Caseworker assistance (%)	23.3	15.2	+8.1**	1.5	53.5
Any support services (%)	77.8	75.2	+2.6	1.6	3.4
<b>As of 15 Months after Randomization, Hours of Support Services in:</b>					
All sessions	15.0	13.1	+2.0	1.0	15.1
One-on-one sessions	8.6	7.9	+0.7	0.5	8.4
Group sessions	6.5	5.2	+1.3	0.7	25.4
<b>As of the Survey:</b>					
Had any training that included guidance on workplace behaviors (%)	43.4	39.0	+4.4*	1.7	11.3
Sample size	5,068	1,689			

Source: Short-Term Follow-Up Survey

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests. "Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

- **Trainees in the HPOG 2.0 treatment group receive more career-related and caseworker services than do control group trainees.**

Among HPOG 2.0 treatment group members who enroll in training, the proportion receiving two types of supports was higher than the corresponding portion of the control group trainees: 6 percentage points higher for career-related supports (career counseling, job search assistance, placement assistance) and 8 percentage points higher for caseworker assistance. These impacts are large. The study did not detect any difference in other supports (tutoring, academic advising, or financial aid advising). Additionally, the average hours of support services

received is relatively low. These results suggest that HPOG 2.0 programs did not deliver support services at the levels seen in some counseling-intensive programs.<sup>37</sup>

- **HPOG 2.0 treatment group trainees do not receive more training in workplace behaviors than do control group trainees.**

The Short-Term Follow-Up Survey asked study members whether they “attended classes or counseling sessions where [they] learned about messages that [they might] be unintentionally sending with [their] dress, body language, or manner of speaking when [they] apply for a job.” The share of HPOG 2.0 treatment group trainees who report receiving this training in workplace behaviors is not significantly higher than the share of control group trainees who report it.<sup>38</sup>

#### 2.4.2 Costs to Study Members

HPOG 2.0 grantees often used funds to pay the treatment group trainees’ tuition and other direct costs of training. In addition, grantees sometimes used HPOG 2.0 funds to provide emergency assistance (e.g., help with rent, utilities, food, or car repairs). By statute, HPOG was prevented from providing stipends. (See **Exhibit 2-4.**)

- **HPOG 2.0 programs substantially decrease the total out-of-pocket training expenditures for treatment group trainees and their families.**

In the 15 months following random assignment, treatment group trainees spend about \$358 less of their own resources than do control group trainees, a 42 percent decrease. Amounts of training expenses paid from family resources and loans are also lower for treatment group trainees, by a similar proportion. Treatment group trainees are also 9 percentage points less likely to borrow money in their own name, or to borrow money in either their own or others’ names.

HPOG 2.0 programs provided assistance with child care expenses, but there is no statistical difference in receipt of such assistance between trainees in the treatment and control groups.

Treatment group trainees are less likely to report difficulty obtaining financial support, with 65 percent reporting that finding support is “not very difficult,” compared with 49 percent in the control group. The fraction reporting that obtaining financial support was “very difficult” is about half as large in the treatment group compared to the control group.

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<sup>37</sup> HPOG 2.0 unconditional impacts, shown in Appendix D, are broadly similar to unconditional impacts in HPOG 1.0. Appendix Section D.4 also has some discussion of intensity of support services, noting that some programs have much more intensive support services.

<sup>38</sup> Fein and Hamadyk (2018) note training in “psycho-social skills” as a component of the highly successful Year Up career pathways program.

**Exhibit 2-4 Impacts on Educational Costs and Sources of Support, Conditional on Any Training since Randomization**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Amount of Training Expenses Paid From:</b>					
Own resources (\$)	450	808	-358**	71	-44.3
Own or family resources or loans (\$)	2,486	4,106	-1,620**	313	-39.5
Own resources plus child care expenses paid by any source (\$)	2,168	2,247	-79	460	-3.5
Own or family resources or loans plus child care expenses paid by any source (\$)	4,204	5,545	-1,341*	592	-24.2
<b>Financing Strategies:</b>					
Third-party assistance with child care expenses (conditional on reporting children) (%)	4.0	3.3	+0.8	0.7	22.9
Borrowed money in own name (%)	20.7	29.1	-8.4**	1.9	-28.8
Borrowed money in own or others' names (%)	21.4	30.2	-8.8**	2.0	-29.1
<b>Difficulty Obtaining Financial Support:</b>					
Very difficult (%)	12.3	22.5	-10.1**	1.6	-45.1
Somewhat difficult (%)	29.8	34.3	-4.5**	1.3	-13.0
Not very difficult (%)	64.5	49.1	+15.4**	2.0	31.5
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests. "Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

### 2.4.3 Training Satisfaction

The Short-Term Follow-Up Survey also asked study members with any formal training during the reference period about their subjective satisfaction with the training. Satisfaction might measure intangible elements of training quality, which could affect its value. Satisfaction might also influence program participants' interest in returning for more training or persisting in a career related to that training (see Appendix Exhibit D.4-3).

- **Among those who enroll in training, treatment and control group trainees do not differ on most elements of training satisfaction in the short term.**

Treatment group trainees are 3 percentage points less likely to report that their courses are not relevant to other things in their lives and 3 percentage points more likely to report receiving assistance with course selection, compared to control group trainees. Considered alone, these results might suggest that training offered through HPOG 2.0 programs was more focused and relevant than training provided elsewhere, perhaps because treatment group trainees are better able to select appropriate courses. However, given the large number of outcomes examined, the small number that are statistically significant, and the lack of a formal correction for multiple comparisons, this result should be viewed with some caution.

## 2.5 Intermediate-Term Impacts on Starting Training for Pre-survey Cohort from NSC Data

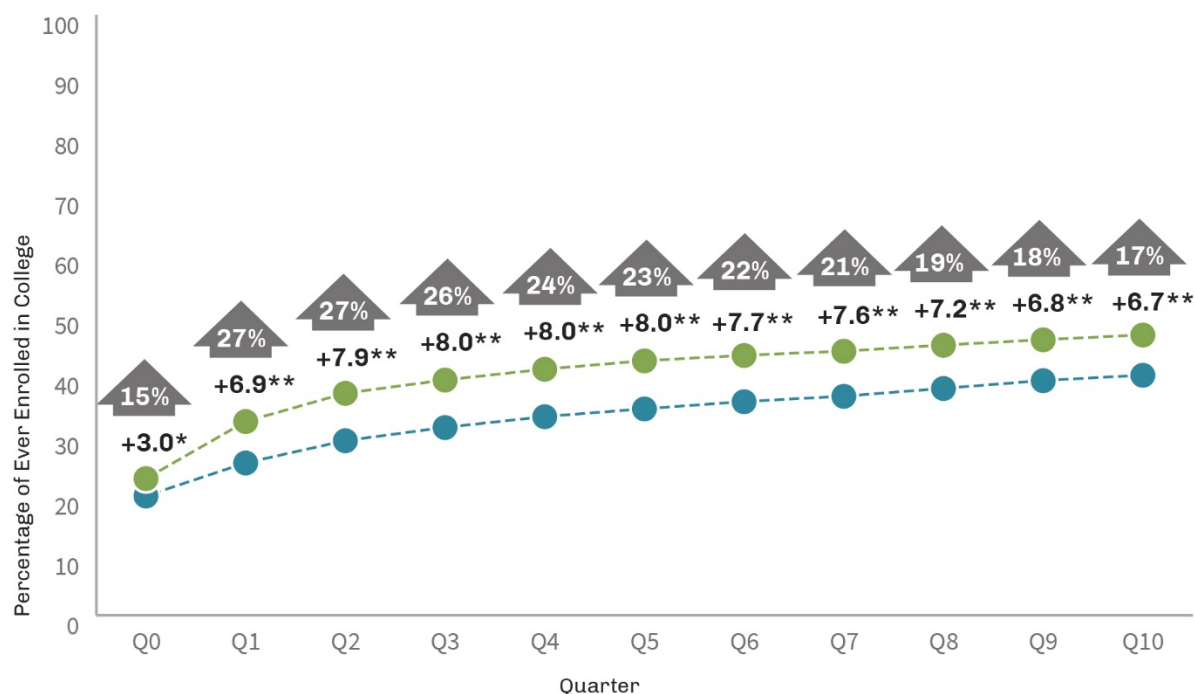
Section 2.1 of this chapter presented results from the Short-Term Follow-Up Survey at about 15 months after randomization for the survey cohort on the measure *ever enrolled in college*. Using data from NSC, this section presents results on college enrollment for the pre-survey cohort, through two and a half years after randomization (where the quarter of randomization is Q0).<sup>39</sup>

- **For the pre-survey cohort, from Q5 to Q10, more study members start college, but the impact of HPOG 2.0 programs does not change much over that longer follow-up period.**

From Q5 (about the date of the survey) through Q10, rates of ever enrolled in college increase gradually over time, from 34 percent in Q5 to 40 percent in Q10 for the control group, and from 42 percent in Q5 to 47 percent in Q10 for the treatment group (see **Exhibit 2-5**). However, HPOG 2.0 programs' impact on ever enrolled drifts down from 8.0 percentage points at Q5 to 6.7 percentage points in Q10.<sup>40</sup> Thus, rather than the impact on ever enrolled in college growing over time, the control group is catching up and the impact of HPOG 2.0 programs is shrinking.

<sup>39</sup> The two data sources are complementary, but do not align perfectly. As of the Short-Term Follow-Up Survey, about 55 percent of the treatment group and 40 percent of the control group has ever enrolled in college. As of Q5, roughly the time of the survey, NSC shows lower rates of *ever enrolled in college* since randomization for the survey cohort: 40 percent in the treatment group and 34 percent in the control group. Possible reasons for this discrepancy are discussed in Appendix Section C.4.

<sup>40</sup> Appendix Section D.5 provides analyses for the full sample, showing larger impacts at durations past Q10. Sensitivity analyses suggest that those larger impacts are unlikely to continue as later cohorts reach those durations.

**Exhibit 2-5** *Impact on Ever Enrolled in College since Random Assignment, by Quarter after Random Assignment (pre-survey cohort)*

● Control Group    ● Treatment Group    ▲ % Relative Impact, Impact Estimate Is Statistically Significant

Source: National Student Clearinghouse

Note: The numbers above the circles indicate impact estimates. Includes all members of the pre-survey cohort (i.e., through February 2017). N=10,117. All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests. "Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ). Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

## 2.6 Discussion

Starting training is the first step in the theory of action for HPOG 2.0 programs: Without engagement in training, program participants will not accumulate the skills and credentials that can prepare them for healthcare occupations and for higher earnings. This chapter has presented findings on training enrollment at about 15 months after randomization from the Short-Term Follow-Up Survey and findings at about two and a half years after randomization from NSC data for the earliest enrollees in HPOG 2.0 programs.

With respect to starting training, HPOG 2.0 has two goals: (1) increase the fraction of program enrollees who start any training (including basic skills education) and (2) increase the fraction who start healthcare occupational training. In the short term, HPOG 2.0 programs have had success with both goals. Relative to the control group, a third more of the treatment group starts any training; a larger share starts occupational healthcare training.

However, rates of starting training are also high in the control group. Based on the level of training in the control group relative to the treatment group, three-quarters of the treatment

group would have started training even without their local HPOG 2.0 program. In addition, intermediate-term impacts on starting training for the pre-survey cohort are no larger than short-term impacts for that cohort. Put differently, there is no evidence so far that the impact across HPOG 2.0 programs on starting training increases past the short term.

Finally, the HPOG 2.0 treatment group spent less of their personal and family resources on training expenses in the short term. Comparing members of the treatment and control groups who took up training shows that treatment group trainees and their families spend less on training, have less difficulty finding financial support, receive more career-related support services, and are about as satisfied with training as are control group trainees.

The next chapter explores the extent to which HPOG 2.0 programs improved *educational progress* (the confirmatory outcome) as well as the length of training and credentials received.

### 3. Impacts on Educational Progress

#### Chapter 3 Key Findings

Analyses of the **Short-Term Follow-Up Survey** responses at about 15 months after randomization find that HPOG 2.0 programs:

- Improve the study's pre-specified confirmatory outcome *educational progress*—defined as having completed training by earning a credential or having been continuously enrolled in training as of 15 months after randomization—by 16 percentage points.
- Increase training duration by 1.4 months, a relative increase of nearly a third.
- Increase performance on most other measures of training persistence and success, usually by a third or more.

Analyses of **National Student Clearinghouse** data for the pre-survey cohort through about two and a half years after randomization find that HPOG 2.0 programs:

- Modestly increase college enrollment through Q6, but not thereafter.
- Increase cumulative months of full-time-equivalent college enrollment by 0.7 months.
- Slightly increase receipt of college credentials, by 2 percentage points.
- Have no detectable impact on receipt of degrees.

Chapter 2 reported that HPOG 2.0 programs substantially increase the share of the treatment group who start training. The HPOG 2.0 logic model posits that once enrolled in training, HPOG 2.0 programs' support services promote trainees' persistence in that training and then receipt of credentials from the training institution and from other organizations (e.g., government or professional organizations). This chapter explores to what extent these posited impacts occur.

The chapter is organized as follows. The first four sections present impact estimates based on the Short-Term Follow-Up Survey for educational progress (Section 3.1), training duration (Section 3.2), credential attainment (Section 3.3), and share in training at the time of the survey (Section 3.4). Section 3.5 considers variation in impacts for survey outcomes by participant characteristics and grantee type. Section 3.6 reports how much impacts vary across local programs. Then Section 3.7 presents impacts for the pre-survey cohort on college persistence about two and a half years after randomization, based on National Student Clearinghouse (NSC) data. Finally, Section 3.8 summarizes and discusses the findings.

### 3.1 Educational Progress

Like HPOG 1.0, the HPOG 2.0 Impact Evaluation’s pre-specified confirmatory outcome is **educational progress**—defined as having completed training by earning a credential or having been continuously enrolled in training 15 months after randomization.<sup>41</sup> Selection of this outcome was based on the HPOG 2.0 logic model (see Appendix Exhibit A-4), which anticipated that many successful program participants might still be in training at 15 months after randomization.

- **HPOG 2.0 programs substantially increase educational progress, the study’s confirmatory outcome.**

In the short term, 63 percent of the treatment group are making educational progress, compared to 46 percent of the control group. This 17 percentage point difference is a third higher than the control group level (**Exhibit 3-1**). More than 80 percent of this educational progress is from earning a new credential—whether from a training provider or from a third party (54 percent of the 63 percent in the treatment group; 38 percent of the 46 percent in the control group). The rest of the trainees have not yet earned a new credential but are still in training 15 months after randomization without having ever dropped out.<sup>42</sup>

The impacts on earned credentials arose from strong impacts on both exam-based credentials from professional, state, and industry authorities (9 percentage points) and credentials issued by training providers (16 percentage points).<sup>43</sup>

One perspective suggests that these large relative impacts on training and credentials are not surprising. Unlike for HPOG 1.0, HPOG 2.0 induces a large increase in training enrollment—by about a third (see Section 2.1). Given this large relative increase in the share of study members starting training, it would be surprising if there were not also a large relative increase in months

#### Defining “Educational Progress”

As of 15 months after randomization, the respondent:

- Completes training by earning any credential, including
  - a professional, state, or industry certification or license (typically by passing a third-party exam); or
  - a diploma, certificate, or degree awarded by a college or other training provider
- Or started training and has not dropped out at any time since then

<sup>41</sup> The operationalization of this outcome is slightly different for HPOG 2.0 than for HPOG 1.0 because of differences in the design of the early surveys for the two evaluations. Only HPOG 2.0 captured sub-degree credentials issued by colleges and other training providers. Otherwise, the two evaluations measure this outcome the same way.

<sup>42</sup> **Exhibit 3-1** shows the impact of HPOG 2.0 programs on enrollment as of 15 months after randomization, including some program participants who have already earned credentials as well as some who dropped out and then returned to school. Following the concepts defined in the evaluation of HPOG 1.0, program participants who dropped out without earning a credential are not counted as having made educational progress even if they re-enrolled by the time of the survey.

<sup>43</sup> The two impacts sum to more than 17 percentage points because some study members earn both types of credentials.

of training and credentials. Appendix G considers this argument, noting that large relative impacts on months of training and credentials can arise from two pathways. First, some of the increase likely follows directly from higher rates of starting training. Second, some of the increase likely follows from more training and credentials among those who would have gotten some training even without access to an HPOG 2.0 program. That appendix notes that principal stratification ideas (Frangakis and Rubin 2002) can be used to explore the relative importance of these two pathways. Doing so is beyond the scope of this report.

**Exhibit 3-1 Impacts on Educational Progress and Related Outcomes**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>As of 15 Months after Randomization</b>					
<b><i>Educational progress (%)</i></b>	62.8	46.3	+16.5##	2.2	35.7
<b>Earned any new credential (%)</b>	54.5	37.7	+16.9##	2.2	44.7
<b>Earned an exam-based professional, state, or industry certification or license (%)</b>	27.8	19.1	+8.7##	1.4	45.6
Earned a credential from any training provider (%)	40.0	24.4	+15.6**	1.9	63.8
Enrolled in training (%)	29.1	23.2	+5.9**	1.8	25.5
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: The confirmatory outcome is bolded and italicized and statistical significance is based on one-sided hypothesis tests; secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded or italicized and statistical significance is based on two-sided hypothesis tests.

“Relative impact” represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

## 3.2 Training Duration

This section considers evidence on training duration as of the Short-Term Follow-Up Survey and its implications for earnings impacts. These results cover training at all types of training providers. (Section 3.7 uses NSC data to consider persistence over a longer time frame but only at colleges.) Training duration is defined broadly to include not just occupational training but also basic skills education and pre-requisites for occupational training (e.g., high school biology).

- **In the short term, HPOG 2.0 programs increase months of training.**

In the short term, members of the treatment group (whether or not they start training) have received an average of 5.7 months of training, whereas control group members have received an average of 4.3 months (**Exhibit 3-2**). This impact of 1.4 months represents an increase of

nearly a third relative to the control group.<sup>44</sup> Put differently, three-quarters of the months of training received by those offered HPOG 2.0 would have been received even without access to an HPOG 2.0 program (that is 4.3 of the 5.7 months).

### Exhibit 3-2 Training Persistence

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Cumulative Months of Training through 15 Months after Randomization:</b>					
<b>Including part-time and partial months</b>	5.7	4.3	+1.4##	0.3	31.4
<b>Completed At Least 6 Months of Training by 15 Months after Randomization (%):</b>					
<b>Including part-time and partial months</b>	40.6	30.1	+10.5##	2.2	34.7
<b>Enrolled in Training at 15 Months after Randomization:</b>					
Any enrollment during month 15 (%)	24.9	19.4	+5.5**	1.3	28.5
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded and statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

HPOG 2.0 programs continued for more than two years past the 15 month follow-up for those in the Short-Term Follow-Up Survey sample. There is therefore scope for continued growth in the impact on months of training (which would be captured in future reports). If the impact on months of training grows substantially, it is plausible that an impact on earnings would as well. Sections 3.4 and 3.7 provide some insights into the likely future path of impacts on months of training.

There is some reason to think that training at "colleges"<sup>45</sup> may be intrinsically more valuable than training at other postsecondary institutions (see Abt Associates 2015). This is because college systems often offer to build and transfer stackable credentials—including potential for progressing from non-credit training to higher levels of credit-based instruction and credentials. For both the treatment group and control group, about three-quarters of training months are at colleges (see Appendix Section E.2). The other quarter is split evenly between other Title IV postsecondary schools and other training providers. This large share of training at colleges is encouraging for the utility of the analyses of NSC data presented in Section 3.7.

<sup>44</sup> These are estimates of months in which there is any training. An ideal measure would probably be full-time-equivalent months; that is, adjusting for partial months at the start and end of training spells and for less than full-time training during those spells. It is not possible to create an ideal measure from the survey, but Appendix Section C.2.5 considers some possible adjustments. Those adjustments suggest that using the ideal measure would likely cut the level of training by roughly 1 month; slightly more in the treatment group than in the control group. In net, those adjustments suggest that the impact on full-time-equivalent months of training is probably about 1 month (rather than the 1.4 months in **Exhibit 3-2**).

<sup>45</sup> The term "college" has no universal definition and there is no legal authority governing its usage. Our preferred definition is that embodied in the Integrated Postsecondary Education Data System—namely degree-granting postsecondary institutions eligible to participate in federal Title IV financial aid programs. Analysis for this report matched student-reported school names to the System to determine which training providers were colleges.

- **HPOG 2.0 programs increase the percentage of the treatment group with at least six months of training.**

Some non-experimental evidence indicates that longer trainings are more likely to induce increased earnings (e.g., Dadgar and Trimble 2015; Litwok, Peck, and Walton 2020). Although the magnitude of the increase in total months of training over the 15 months after randomization is small (1.4 months), it is enough to increase the percentage of trainees with at least six months of training by 11 percentage points, from 30 to 41 percent.

### 3.3 Credits and Credential Attainment

As discussed in Section 3.1, HPOG 2.0 has large impacts on two secondary outcomes focused on the earning of credentials: a 17 percentage point impact on earning a credential from any type of training provider and a 9 point impact on earning exam-based certifications and licenses from authorities other than schools (as defined by the survey item in the textbox at right).

The evaluation emphasizes the earning of the exam-based credential from the state or other non-provider authority (e.g., professional association, licensing agency) because many healthcare professions require such credentials.<sup>46</sup> Most HPOG 2.0 programs prepare students for healthcare professions requiring such exam-based credentials, and many of the programs provide assistance with exam preparation and exam fees.

This section contextualizes the results from Section 3.1 and reports on additional short-term impacts of HPOG 2.0 programs on credit accumulation and credential attainment (**Exhibit**

#### Short-Term Follow-Up Survey Item Detail

Q. Obtained a professional, state, or industry certification or license:

- A professional certification or license shows you are qualified to perform a specific job like Licensed Realtor, Certified Medical Assistant, Certified Construction Manager, or an IT certification.
- A professional certification is awarded by an organization; a state or industry license is awarded by a licensing agency. Both professional certifications and state or industry licenses need to be renewed periodically.

Since [the date of randomization], have you received any professional certifications, or state or industry licenses? Please do not include any commercial licenses such as a liquor license or vending license.

<sup>46</sup> Consistent with this characteristic of healthcare labor markets, OFA's FOA (2015, p. 2) explicitly required local HPOG programs to offer trainings that "result in an employer or industry recognized credential (which can include a license, third-party certification, postsecondary educational certificate or degree, as well as a Registered Apprenticeship certificate)."

Another reason for focusing on this exam-based credential outcome is the issue of blinding. The authorities that grant these credentials have no access to the randomization status of study members. Though there is no evidence of this, a college HPOG 2.0 grantee could theoretically develop new credentials that are essentially available only to members of the treatment group and/or are of questionable value. A college grantee could also be more generous in the award of established credentials to members of the treatment group. So an increase in credentials awarded to its HPOG 2.0 program participants by other than the grantee is less susceptible to the bias that may occur in open-blind studies (meaning those in which both study members and administrators are aware of the treatment status of study members). Because of this issue, an effect on such external (non-grantee) credentials may be a stronger signal for the potential for future earnings increases.

**3-3).** Section 3.7 considers intermediate-term impacts on credentials issued by colleges using data from NSC.

In the short term, HPOG 2.0 substantially increases attainment of healthcare credentials. Almost all of the short-term impact of HPOG 2.0 programs on new credentials are on healthcare credentials.

- **In the short term, HPOG 2.0 programs have no impacts on longer-term certificates and diplomas or on degrees.**

All the impact on credentials issued by training providers is in the form of certificates and licenses that take less than a year of study. The analysis did not detect any effect on degrees or on other credit-based, school-issued credentials that require a year or more of study. In fact, about half the impact on new credentials arises from study members who study for less than six months.

About a quarter of the impact on credentials arises from study at training providers that are not eligible to participate in Title IV programs. Such credentials can be less stackable, and any credits earned less transferable if the participant decides to return to school later on.

- **HPOG 2.0 programs nearly double the incidence of earning a credential and then continuing with further training.**

One important change in the FOA from HPOG 1.0 to HPOG 2.0 was stronger emphasis on consistency with the career pathways framework, in particular stacking trainings. In response to that change, the HPOG 2.0 Impact Evaluation added the outcome of earning a credential and then continuing training. Only 6 percent of the control group achieves this goal; the treatment group achieves more than double that rate at 13 percent (**Exhibit 3-3**).<sup>47</sup> Data directly comparing HPOG 2.0 versus HPOG 1.0 on this outcome are not yet ready.<sup>48</sup>

- **HPOG 2.0 increases the percentage of study members earning any credits, but not the average number of credits earned.**

HPOG 2.0 had an impact of 5 percentage points on earning at least one college credit. However, there is no net increase in the average number of credits earned (**Exhibit 3-3**).<sup>49</sup>

<sup>47</sup> Using administrative data on HPOG 2.0 program participants, Loprest and Sick (2018) determined that the most common initial training was Certified Nursing Assistant (CNA) and the most common follow-on training was for a “CNA-plus credential” such as Certified Medication Aide or Patient Care Technician.

<sup>48</sup> Surveys used in the HPOG 1.0 impact analysis did not collect data on this outcome. However, an analysis of PRS data found that 16 percent of HPOG 1.0 participants had completed a training and started a second as of 18 months after randomization (Werner et al. 2018). This suggests that HPOG 2.0 may actually be less encouraging of this pathway than HPOG 1.0 was, but it is not clear that the measurements are comparable.

<sup>49</sup> Directly estimating impact on behavior for those induced to earn at least one credit requires additional assumptions and some version of the principal stratification methods discussed in Appendix G.

**Exhibit 3-3 Additional Impacts on Credits and Earned Credentials**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>As of 15 Months after Randomization</b>					
<b>Earned any new credential (%)</b>	54.5	37.7	+16.9##	2.2	44.7
Earned a new healthcare credential (%)	51.9	34.2	+17.7**	2.4	51.9
<b>Earned an exam-based professional, state, or industry certificate or license (%)</b>	27.8	19.1	+8.7##	1.4	45.6
Earned a certificate or diploma from a training provider (college or other) that requires:					
Less than a year of study (%)	26.9	14.7	+12.2**	1.7	83.0
A year or more of study (%)	5.8	5.7	+0.1	0.4	2.3
Earned a new credential and completed: (%)					
>6 months of training	24.1	15.4	+8.7**	1.9	56.7
<6 months of training	30.4	22.3	+8.1**	1.6	36.4
Earned a degree (%)	1.9	2.7	-0.8	0.7	-30.4
Earned any new credential and continued/resumed training (%)	13.1	6.2	+6.9**	1.1	110.0
Earned any new credential from:					
Any training provider (%)	40.0	24.4	+15.6**	1.9	63.8
College (%)	26.1	15.5	+10.7**	1.9	68.9
Other Title IV postsecondary school (%)	4.8	3.6	+1.3	0.7	35.3
Other training provider (%)	9.9	5.6	+4.3**	1.1	76.5
<b>As of the Survey</b>					
Earned any credits (%)	31.2	25.6	+5.5**	1.8	21.6
Number of credits earned	10.8	10.7	+0.1	1.0	1.0
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded and statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

### 3.4 Enrollment at Follow-Up and Implications for Future Impacts

The career pathways framework developed by Fein (2012) suggests an important role for returning to training after some period of work. However, available evidence for HPOG 1.0 and for HPOG 2.0 from PAGES suggests that such returns to training are not common.<sup>50</sup> Therefore, the impact of HPOG 2.0 programs on enrollment at 15 months is informative about increases in their impact on educational progress by the time of the next follow-up at 36 months after

<sup>50</sup> Klerman et al. (forthcoming) report analyses of PAGES data on such return to training leading to additional credentials. These are additional credentials for the treatment group through HPOG 2.0 programs. Additional credentials for the treatment group not through HPOG 2.0 programs will increase these rates. On the other side, for impacts on earnings, the crucial issue is the impact on credentials. Because the control group will also receive additional credentials, any increase in the impact on credentials is likely to be considerably smaller than the increase in credentials as measured in PAGES.

randomization.<sup>51</sup> The larger the differences in enrollment at 15 months, the larger the plausible increases in impact on cumulative training past 15 months.

- **HPOG 2.0 programs increase the prevalence of being in training at 15 months after randomization, but the impact is modest.**

At about 15 months after randomization, 23 percent of the control group is enrolled in training; HPOG 2.0 programs increase this prevalence by 6 percentage points (see **Exhibit 3-1**). This suggests that the 17 percentage point impact on credential attainment observed at 15 months is likely to grow by no more than a few percentage points by the time of the Intermediate-Term Follow-Up Survey, around 36 months after randomization. The discussion coming in Section 3.8 suggests that this projection has implications for the likely growth of impacts on labor market outcomes.

In Section 3.7, analysis of NSC data for the pre-survey cohort provides complementary insights on future training for the survey cohort. Analyses based on that future survey and presented in the *Intermediate-Term Impact Report* (anticipated to be published in 2023) will provide direct evidence on training through 21 months after the 15-month Short-Term Follow-Up Survey.

### 3.5 Variation in Impacts by Study Member Baseline and Grantee Characteristics

This section explores sources of variation in the impact of HPOG 2.0 programs on survey outcomes from this chapter and Chapter 2 in terms of 10 measures of baseline characteristics of study members and one grantee characteristic. The section provides graphical figures that illustrate this variation for educational progress (the study's confirmatory outcome) and six other outcomes in the education domain. Some details such as standard errors *p*-values were left out of these figures but can be found in Appendices D and E.

The impact on educational progress varies by age; the presence of dependent children; school enrollment at baseline; life challenges interfering with work, school, or family responsibilities; English proficiency; and receipt of public assistance.

As shown in **Exhibit 3-4** (page 422), relative to study members not in these subgroups, the impact of HPOG 2.0 programs on educational progress is larger for older study members; those with dependent children; those not already enrolled in training at baseline; those with no life challenges very often interfering with work, school, or family responsibilities;<sup>52</sup> those who are proficient in English; and those not receiving TANF benefits at baseline. The impact is largest

<sup>51</sup> The intuition underlying the analysis and discussion in Section 3.4 is that study members in training at follow-up will almost certainly get more training. The formal analogy is to the relation between censored durations (i.e., durations through the interview date) and completed durations (including time past the interview date).

Also and conversely, study members not in training at follow-up are much less likely to get more training. Consistent with career pathways principles, some of those not in training at follow-up will return to training. That said, the available evidence suggests that there will not be a lot of such return to training (see Klerman et al. [forthcoming] and the NSC results in Section 3.7 below).

<sup>52</sup> Queried life challenges include child care arrangements, transportation, and an illness/health condition.

for those older than age 30 (20 percentage points versus 11 points for those younger than age 25). Further study is warranted, but the smaller impact for TANF recipients could be related to smaller impacts for those with more life challenges. Analysis detected no significant variation in impact by type of grantee (college versus non-college).

**Exhibits 3-5 through 3-10** (pages 433-488) show parallel results for six additional survey outcomes in the education domain.

### How to Read Exhibits 3-5 through 3-10 on Subgroup Comparisons

- Left-hand side lists subgroups and their prevalence in the sample
- The top, blue bars give average values for treatment group
- The bottom, green bars give average values for control group
- Numbers to right of the bar show impacts for each subgroup
- Numbers on far right show differences in impacts
- Asterisks indicate statistical significance as noted below each figure
- For splits of the sample into three or more subgroups (such as race/ethnicity), only selected comparisons appear, all with reference to the same subgroup (such as non-Hispanic White)

These six exhibits are summarized in **Exhibit 3-11** (page 499). In that summary table, a single dagger (†) indicates that the test for differential impacts across the subgroups yielded a  $p$ -value less than .05. A double dagger (††) indicates that the  $p$ -value is less than .01.

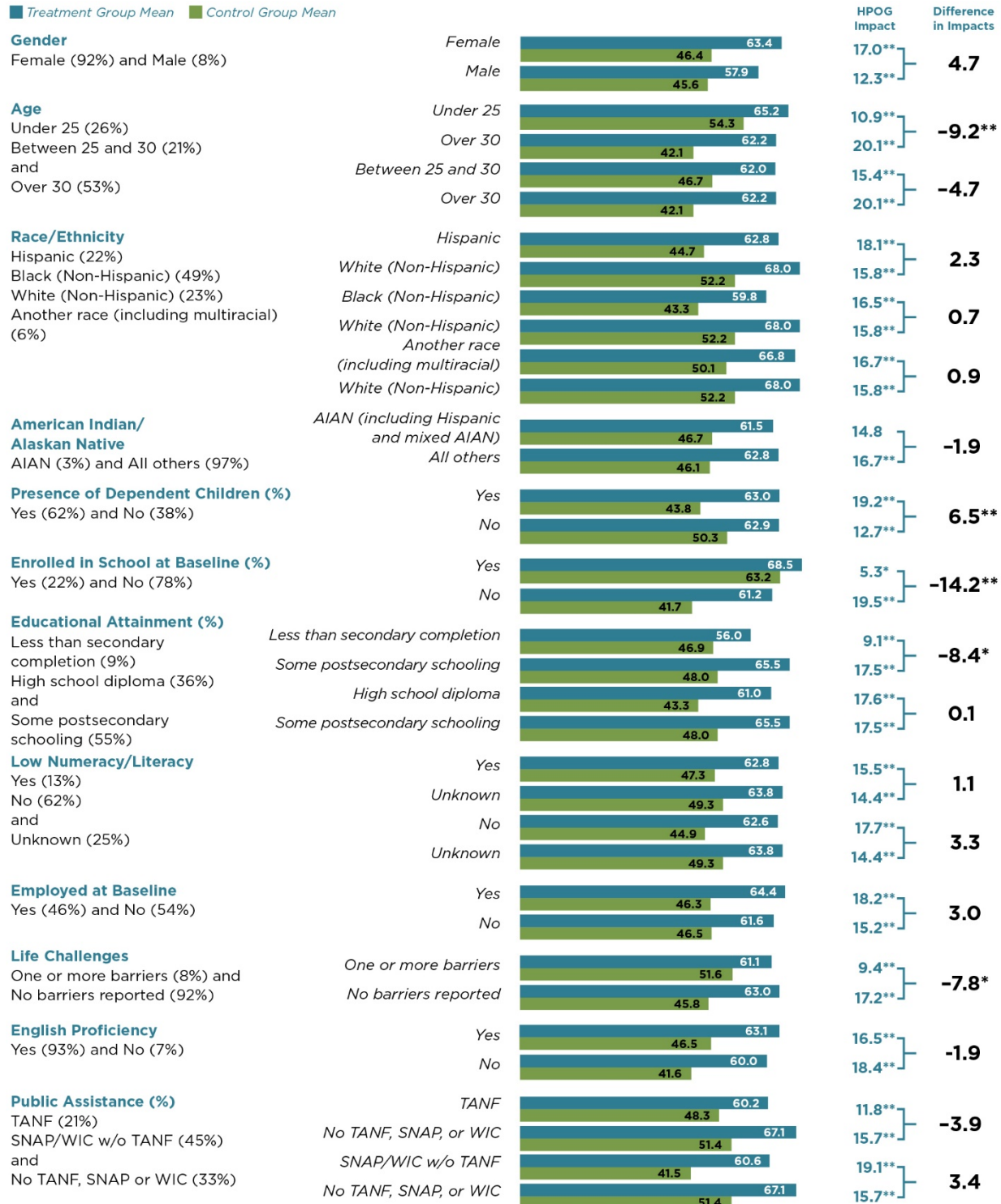
Across outcomes in the education domain, the subgroup dimensions associated most consistently with differential impacts are age, school enrollment at baseline, and receipt of public assistance at baseline. **Exhibit 3-12** (page 50) shows the details for these three dimensions with consistent evidence of differential impacts:<sup>53</sup>

- Impacts are smaller for study members younger than age 25.
- Study members not already enrolled in school at baseline experience larger impacts.
- Those receiving TANF benefits at baseline experience smaller impacts.
- Impacts on months of training and on being in training at 15 months after randomization are greater at college-led HPOG 2.0 programs than at other HPOG 2.0 programs. In fact, it appears that programs not led by colleges have almost no impact on their participants being in training at 15 months.

<sup>53</sup> The impacts on receiving at least six months of training are not statistically different at college-led programs than at other programs. Given the magnitude of the estimated difference and the significant impact on months of training, this comparison is also shown. Detail on subgroup impacts for other outcomes can be found in Appendices D, E, and F.

**Exhibit 3-4 Impact on Educational Progress, by Subgroup**

■ Treatment Group Mean ■ Control Group Mean

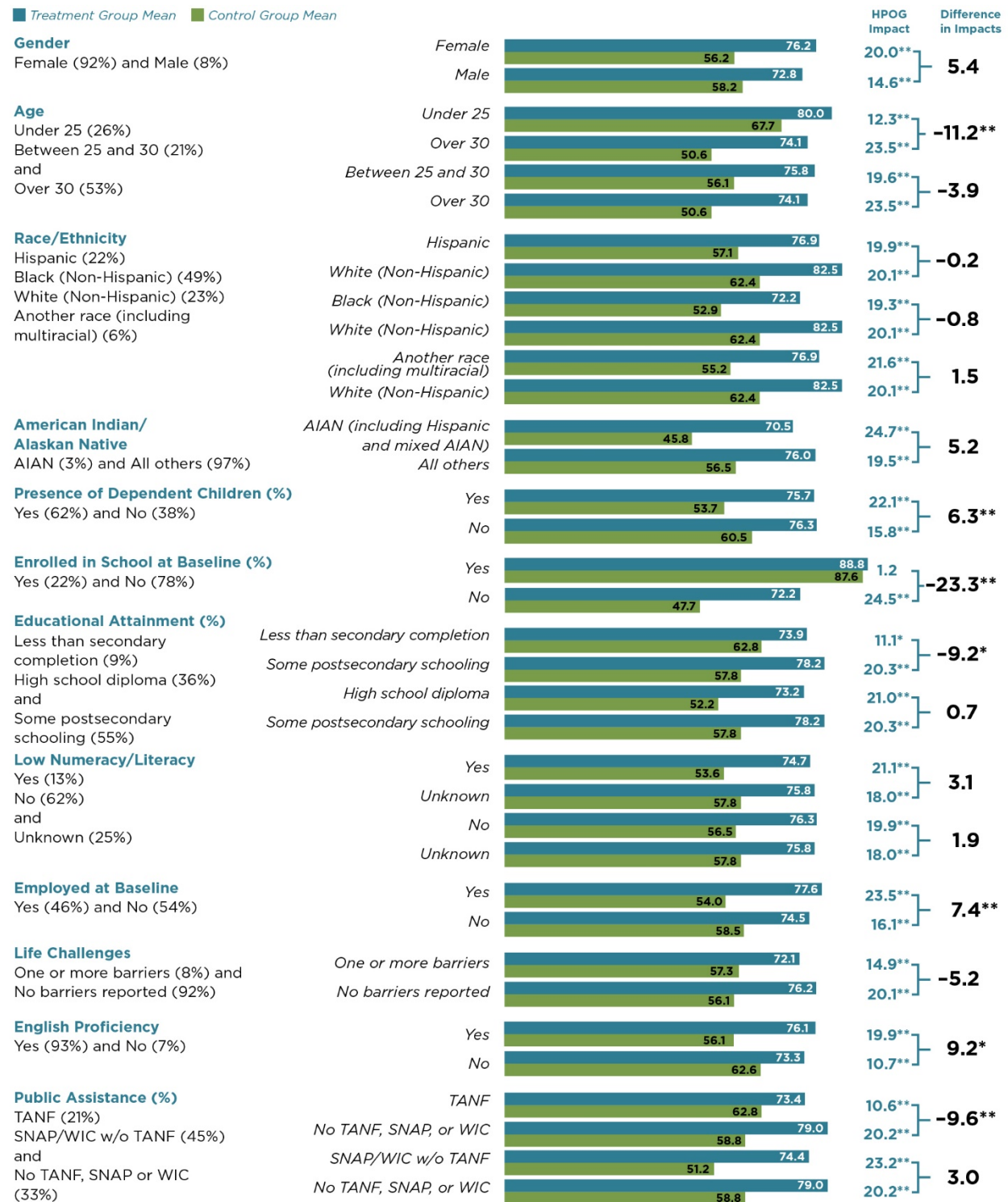


Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 3-5 Impact on Any Training as of 15 Months after Randomization, by Subgroup**

Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

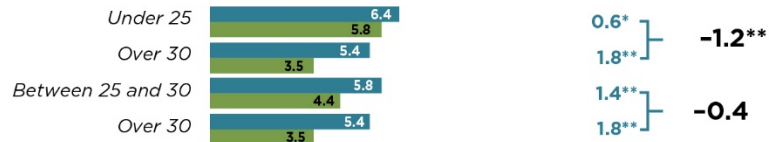
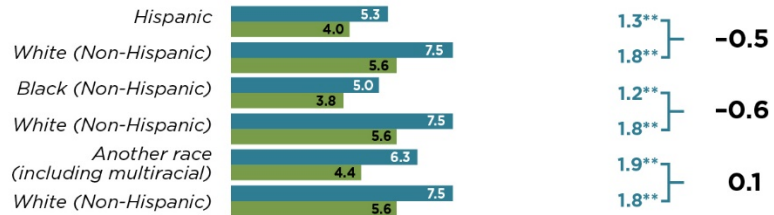
Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 3-6 Impact on Months of Training as of 15 Months after Randomization, by Subgroup**

■ Treatment Group Mean ■ Control Group Mean

**Gender**

Female (92%) and Male (8%)

**Age**Under 25 (26%)  
Between 25 and 30 (21%)  
and  
Over 30 (53%)**Race/Ethnicity**Hispanic (22%)  
Black (Non-Hispanic) (49%)  
White (Non-Hispanic) (23%)  
Another race (including  
multiracial) (6%)**American Indian/  
Alaskan Native**

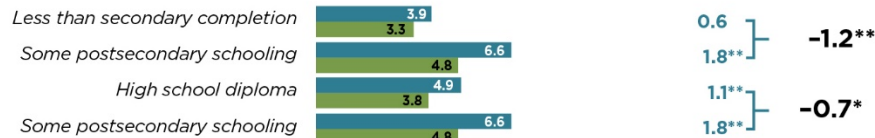
AIAN (3%) and All others (97%)

**Presence of Dependent Children (%)**

Yes (62%) and No (38%)

**Enrolled in School at Baseline (%)**

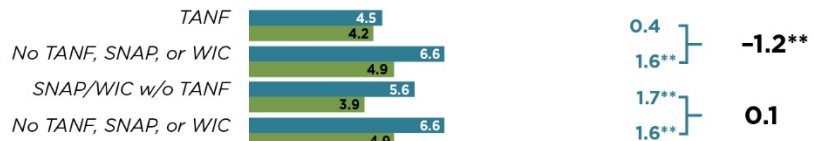
Yes (22%) and No (78%)

**Educational Attainment (%)**Less than secondary  
completion (9%)  
High school diploma (36%)  
and  
Some postsecondary  
schooling (55%)**Low Numeracy/Literacy**Yes (13%)  
No (62%)  
and  
Unknown (25%)**Employed at Baseline**

Yes (46%) and No (54%)

**Life Challenges**One or more barriers (8%) and  
No barriers reported (92%)**English Proficiency**

Yes (93%) and No (7%)

**Public Assistance (%)**TANF (21%)  
SNAP/WIC w/o TANF (45%)  
and  
No TANF, SNAP or WIC (33%)

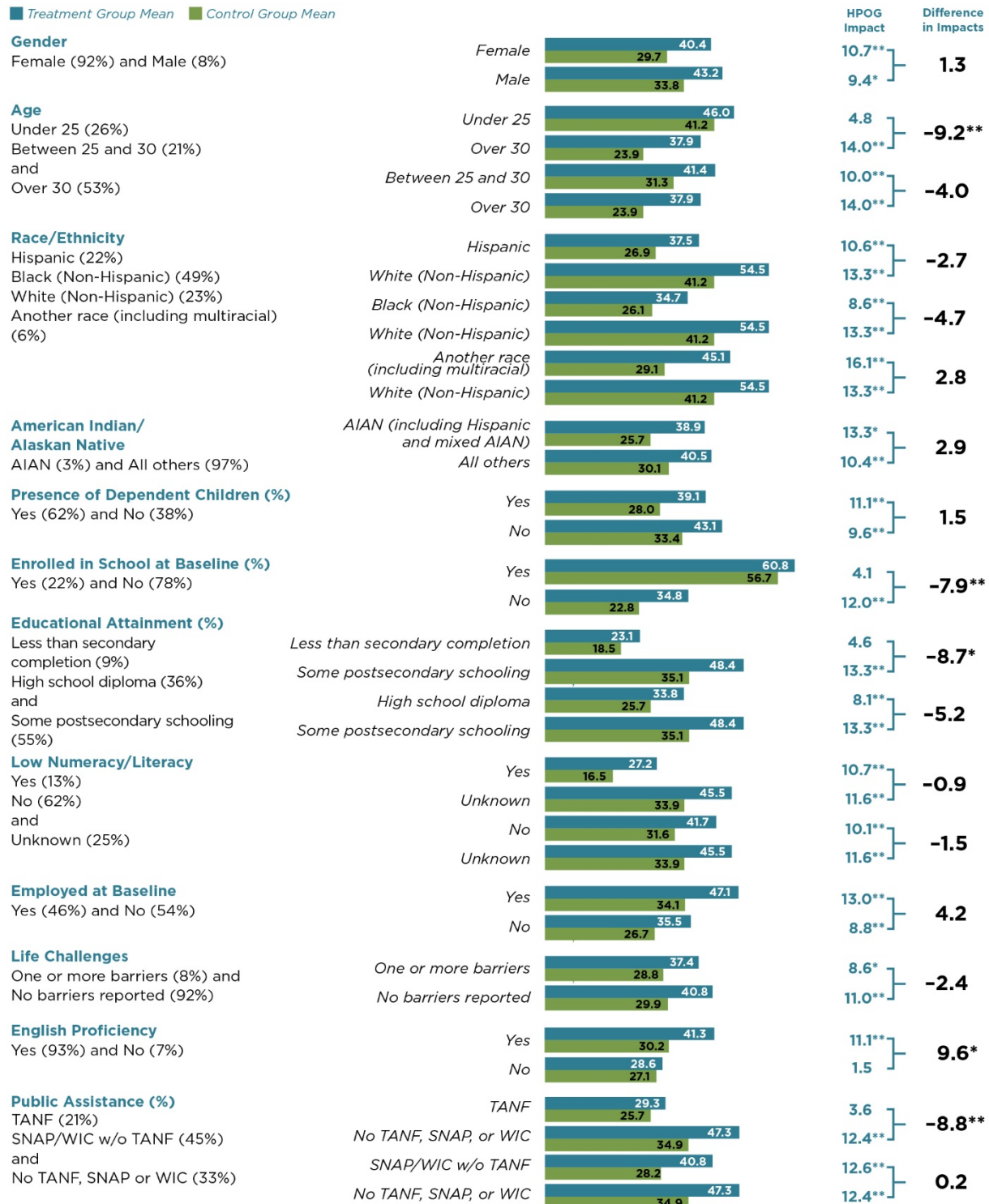
Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

### Exhibit 3-7 Impact on Having Completed Six or More Months of Training as of 15 Months after Randomization, by Subgroup

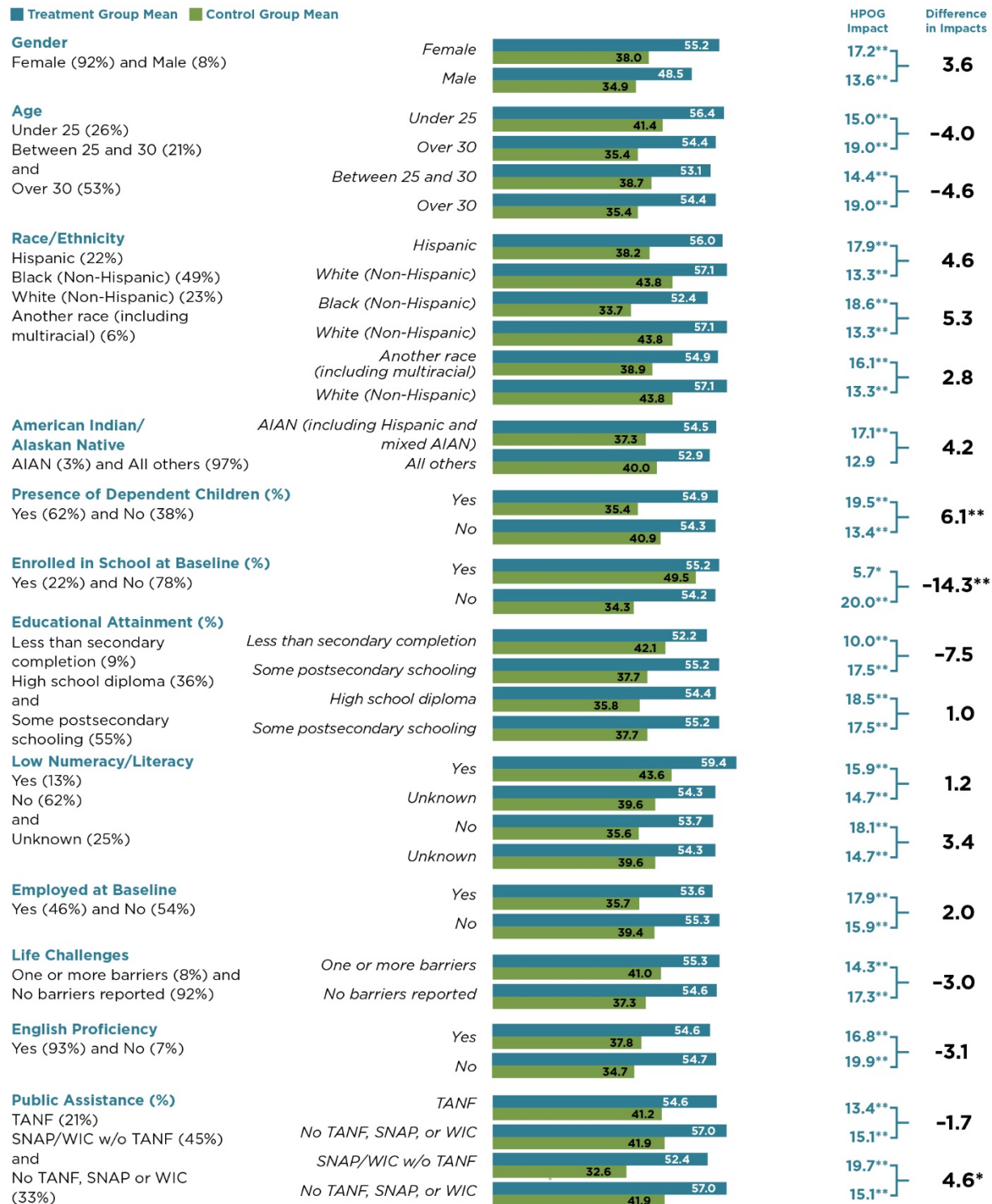


Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 3-8 Impact on Earning Any Credential as of 15 Months after Randomization, by Subgroup**

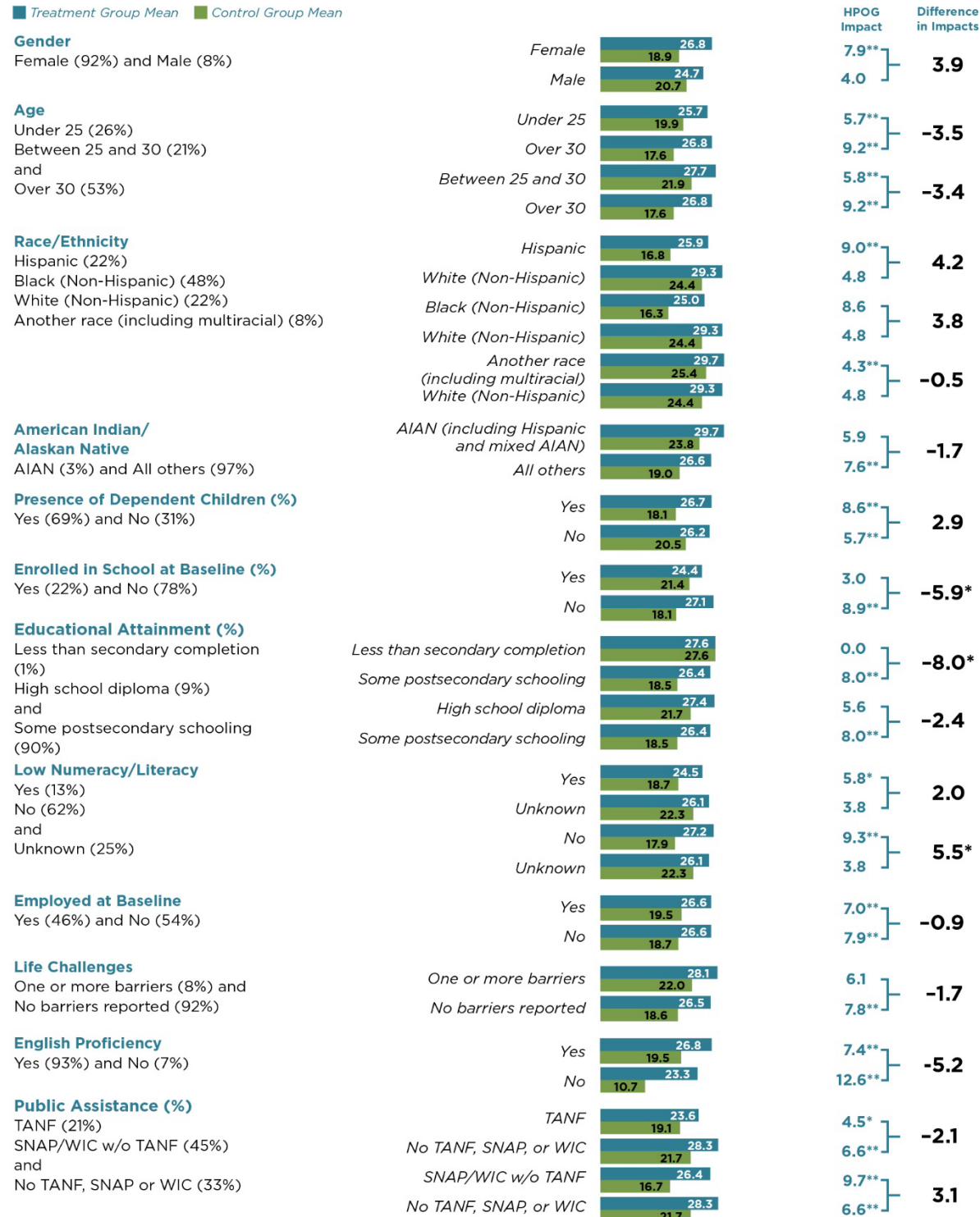
Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

### Exhibit 3-9 Impact on Earning Any Exam-Based Certification or License as of 15 Months after Randomization, by Subgroup

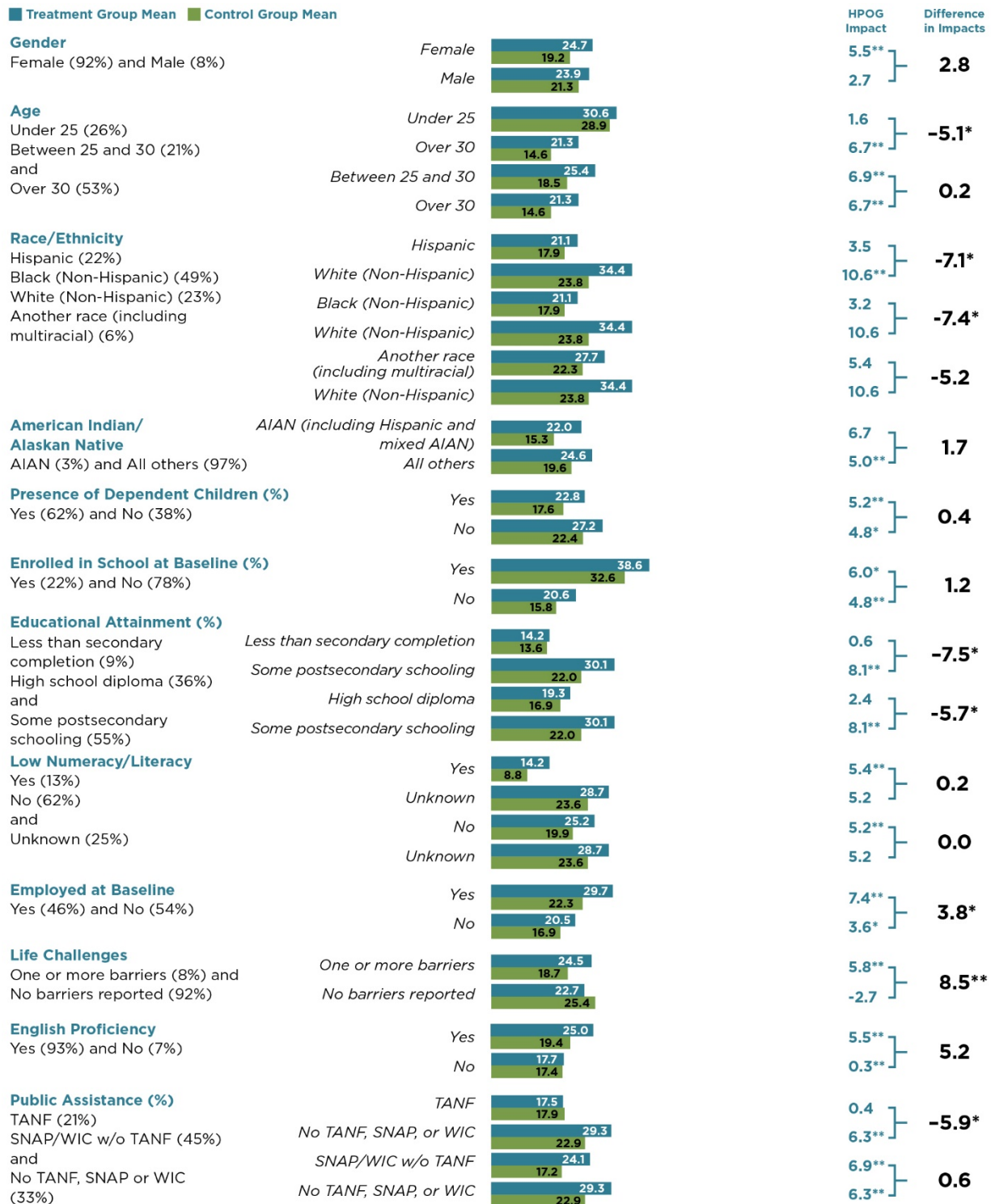


Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 3-10 Impact on Being in Training as of the Follow-Up Interview, by Subgroup**

SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 3-11 Variation in Impacts on Education Outcomes with Participant Baseline and Grantee Characteristics**

Source	Any Training	Months of Training	At Least 6 Months of Training	Earning Any Credential	Any Exam-Based Certification or License	Still in Training	Educational Progress
<b>Participant Characteristic at Baseline</b>							
Gender							
Age	†	††	†				††
Race/ethnicity							
Presence of dependent children				†			†
School enrollment	††	††	††	††	†		††
Educational attainment							
Literacy and numeracy skills					†		
Employment status	††	†					
Life challenges						†	†
English proficiency	†	†	†				
Receipt of public assistance	††	††	†	†		††	†
<b>Grantee Characteristic</b>							
Type of organization		†				†	

Source: Abt analysis of HPOG 2.0 Short-Term Follow-Up Survey data

Note: Educational progress is the confirmatory outcome for this report. A study member demonstrates educational progress either by earning a credential (any type) or by still being in training with no dropout since starting training. Statistical significance levels for tests of heterogeneity of effects across subgroups are indicated with daggers, as follows: †††=1 percent, ††=5 percent, †=10 percent. No dagger means that no significant evidence of heterogeneity was detected.

**Exhibit 3-12 Impacts on Education Outcomes for Subgroups with Most Consistent Differential Impacts**

Source	Any Training	Months of Training	At Least 6 Months of Training	Educational Progress	Earning Any Credential	Any Exam-Based Certification or License	Still in Training
<b>Participant at Baseline</b>							
Age							
Under 25	+12.3**	+0.6*	+4.8	+10.9**			
Between 25 and 30	+19.6**	+1.4**	+10.0**	+15.4**			
Over 30	+23.5**	+1.8**	+14.0**	+20.1**			
School enrollment							
Yes	+1.2	+0.6	+4.1	+5.3*	+5.7*	+3.0	
No	+24.5**	+1.6**	+12.0**	+19.5**	+20.0**	+8.9**	
Proficient in English?							
Yes	+19.9**	+1.5**	+11.1**				
No	+10.7**	+0.1	+1.5				
Receipt of public assistance							
TANF receipt	+10.6**	+0.4	+3.6	+11.8**	+13.4**		-0.4
SNAP/WIC receipt (without TANF)	+23.2**	+1.7**	+12.6**	+19.1**	+19.7**		+6.9**
No TANF, SNAP, or WIC receipt	+20.2**	+1.6**	+12.4**	+15.7**	+15.1**		+6.3**
<b>Grantee</b>							
Type of organization							
College-led HPOG 2.0 program		+2.1**	+15.8**				+10.5**
Other HPOG 2.0 program		+0.9**	+7.2**				+3.3

Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: Abt analysis of HPOG 2.0 Short-Term Follow-Up Survey data

Note: All of the subgroup analysis is exploratory.

Blank cells in table indicate that there is not statistically significant difference across the subgroups for that outcome. Only those subgroups with the most consistent differential impacts whose cells have a corresponding dagger in Exhibit 3-11 are populated here. Note, however, that this does not mean that all pairwise comparisons are statistically significant when there is statistically significant variation across a set of three subgroups. For example, the impacts of HPOG 2.0 programs on months of training vary significantly by TANF receipt, but not by receipt of SNAP or WIC among study members not receiving TANF.

Statistical significance is based on two-sided hypothesis tests as follows: \*\*=1 percent, \*=5 percent.

### 3.6 Variation in Impacts across Local Programs

This section reports how much the impact of HPOG 2.0 programs varies locally across the 38 grantee programs. **Exhibit 3-13** shows estimated 25th and 75th percentiles of impacts for the confirmatory outcome (*educational progress*) and for secondary education outcomes.

By definition, the 25<sup>th</sup> percentile of a distribution is any value  $x$  such that 25 percent of the sample has values smaller than or equal to  $x$  and 75 percent of the sample has values larger than or equal to  $x$ . In this application, this means that the 25<sup>th</sup> percentile is midway between the impacts for the 9<sup>th</sup> and 10<sup>th</sup> least-effective programs, and the 75<sup>th</sup> percentile is midway between the impacts for the 9<sup>th</sup> and 10<sup>th</sup> most-effective programs.<sup>54</sup> These two percentiles are often presented in the literature as a measure of spread in a distribution. Together, they define the “interquartile range.” Fifty (50) percent of programs are estimated to have impacts within this range.

The exhibit also shows the ratio of the cross-program impact variance to the squared population standard deviation for each outcome. This is a useful summary measure because it is unitless. The impacts are measured on a variety of different scales, such as percentage points, months, and dollars. If we were to show the cross-program impact variance on these original scales, it would be impossible to meaningfully compare the figures for outcomes such as earnings and educational progress. Although the methodology used for the body of this report does not generate estimated impacts for specific local programs, the research team did prepare these using an alternate methodology. The results are in Appendix H.

- **The impact of HPOG 2.0 programs on education outcomes varies substantially across them, particularly for length of training.**

As the exhibit shows, there is strong variation in impacts across the programs for these outcomes. The typical local HPOG 2.0 program has an impact on educational progress (defined as completed training or continuously in school, the confirmatory outcome) of between 9 and 24 percentage points. Relative to the squared population standard deviation, cross-program impact variance is largest for months of training. A quarter of local programs have impacts of 0.4 months or fewer, whereas another quarter of programs have impacts of 2.3 months or more.

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<sup>54</sup> Note, however, that the percentiles shown in the exhibit are not based on estimating impacts for the individual programs. Instead, percentiles are based on the assumptions that impacts are normally distributed across programs and that the cross-program impact variance is perfectly estimated. The cross-program impact variance is the variance in true impacts across programs.

**Exhibit 3-13 Cross-Program Percentiles of Local Impacts for Confirmatory and Secondary Education Outcomes**

Outcome (15 months after randomization)	Cross-Program Impact Percentile			Ratio of Cross-Program Impact Variance to Squared Population Standard Deviation
	25th	50th	75th	
Educational Outcome				
Educational progress (%)	9.0	16.5	24.1	.05
Earned any new credential (%)	8.9	16.9	24.8	.06
Earned an exam-based professional, state, or industry certificate or license (%)	4.6	8.7	12.8	.02
Cumulative months of training	0.4	1.4	2.3	.07
Completed 6 or more months of training (%)	2.6	10.5	18.3	.06

Source: HPOG 2.0 Short-Term Follow-Up Survey

Note: Assumes that local impacts are normally distributed.

### 3.7 Intermediate-Term Impacts on Training Progress for Pre-survey Cohort from NSC Data

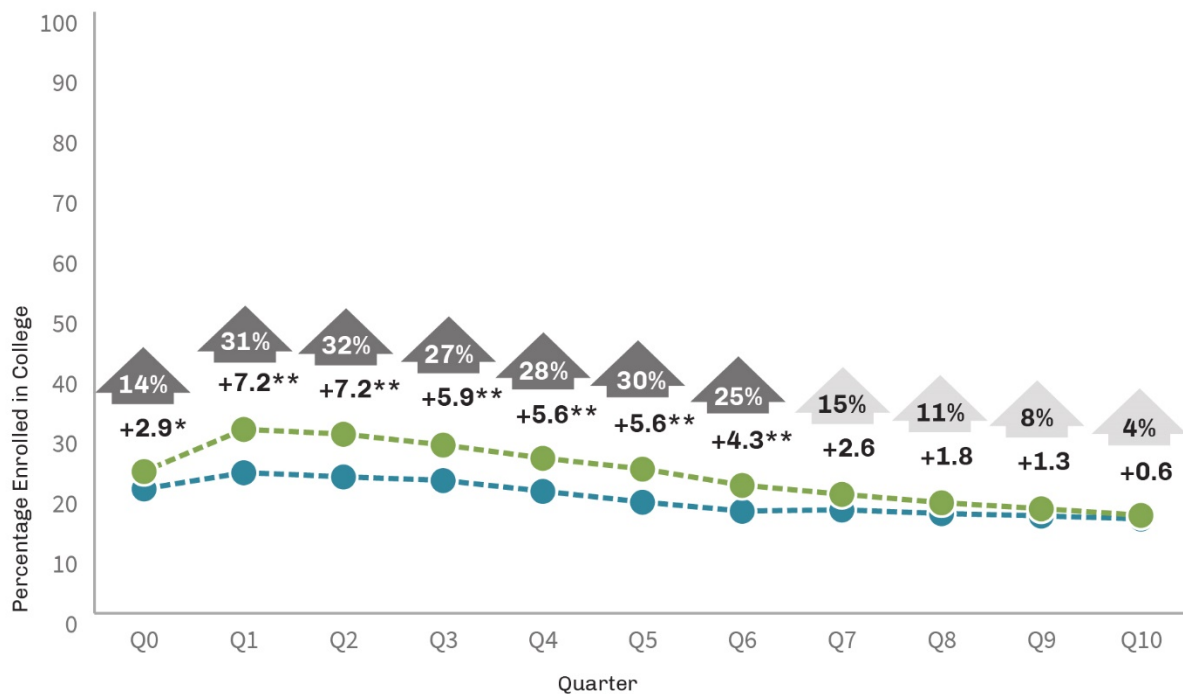
The previous six sections of this chapter use Short-Term Follow-Up Survey data to present estimated impacts on months of training, credential receipt, and educational progress (this report's confirmatory outcome) through 15 months after randomization for the survey cohort. Using data from NSC, this section presents estimated impacts on equivalent training at colleges for the pre-survey cohort, through two and a half years after randomization (where the quarter of randomization is Q0).<sup>55</sup>

- **For the pre-survey cohort, HPOG 2.0 programs increase college enrollment through Q6; from Q7 through Q10, there is no detected impact.**

In Q0 through Q6, HPOG 2.0 programs increase college enrollment (**Exhibit 3-14**). The impact peaks in Q1, at 7 percentage points. Then, beginning in Q3, the impact declines, such that no impact is detected past Q6.<sup>56</sup> As more of the sample reaches the Q10 time point, the size of the sample will increase and precision will improve, making it plausible that small impacts will be detected in future reports. However, based on the current data, a 95 percent confidence interval on the impact at Q10 runs from -1.0 to +2.2 percentage points. Thus, we can rule out even moderate impacts on college enrollment in the intermediate term.

<sup>55</sup> The two data sources are complementary, but do not align perfectly. Possible reasons for imperfect alignment are discussed in Appendix Section C.4.

<sup>56</sup> All analyses of the pre-survey cohort in the body of the report include only those study members observed through at least Q10. Thus, some who are observed past Q5 but not through Q10 are not included in these exhibits. The evaluation adopts this strategy so that changes with time since randomization are for a fixed set of people (and thus changes do not represent changes in the sample). For most analyses, this choice does not appear to be consequential. This analysis is an exception. Including everyone observed in every quarter, there is impact through Q8, but not thereafter—up to Q13 (Appendix Section E.6). Future reports will include longer follow-up and, as appropriate, discussion of inter-cohort differences.

**Exhibit 3-14 Impacts on College Enrollment (pre-survey cohort)**

● Control Group      ▲ Relative Impact, Impact Estimate Is Statistically Significant  
 ● Treatment Group      ▲ Relative Impact, Impact Estimate Is Statistically Insignificant

Source: National Student Clearinghouse

Note: The numbers above the circles indicate impact estimates. Includes all members of the pre-survey cohort (i.e., through February 2017). N=10,117.

All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

- **For the pre-survey cohort, HPOG 2.0 programs have a small impact of 0.7 months on cumulative full-time-equivalent months of college enrollment through two and a half years after randomization.**

Cumulative full-time-equivalent months of college enrollment increase steadily in the control group (to 3.8 months) and in the treatment group (to 4.5 months). This is an impact of 0.7 months as of two and a half years after randomization (see Appendix Section E.6). However, from Q7 forward, the impact on cumulative full-time-equivalent college enrollment is stable and well under one month. Furthermore, the lack of an impact on college enrollment in Q10 suggests that the impact of HPOG 2.0 programs on cumulative full-time-equivalent months of college enrollment is unlikely to increase substantially with longer follow-up.

- **For the pre-survey cohort, HPOG 2.0 programs increase receipt of college credentials through two and a half years after randomization, but not receipt of degrees (associate's and above).**

Two and a half years after randomization, 12 percent of the control group and 14 percent of the treatment group have received a college credential. This is a small impact of 2 percentage

points; that is, even if the impact of an additional credential on earnings is large, this impact of HPOG 2.0 does not lead to even a moderate impact on earnings.<sup>57</sup>

Through two and a half years after randomization, 6 percent of both the treatment group and the control group have received a degree. HPOG 2.0 programs have not increased that rate for the pre-survey cohort (see Appendix Section E.6). Two and a half years might be too early to observe impacts on degrees, but the lack of an impact on college enrollment in Q10 suggests that the impact of HPOG programs on credentials and degrees is unlikely to increase substantially.

### 3.8 Discussion

There are two complementary approaches to thinking about this chapter's finding of the impact of HPOG 2.0 programs on measures of educational progress.

The first approach follows from the specification in the *Design Plan* and *Analysis Plan* for this report (Klerman, Judkins, and Locke 2019; Judkins, Klerman, and Locke 2020). Before seeing the evaluation's results, the *Analysis Plan* specified and the research team registered *educational progress*—defined as having completed training by earning a credential or having been continuously enrolled in training—as the sole confirmatory outcome for this *Short-Term Impact Report*. This choice was consistent with the following:

1. OFA's FOA (2015) and its emphasis on *any credential* as a key outcome for HPOG 2.0 programs.
2. HPOG 1.0's choice of *educational progress (any credential)* as the confirmatory outcome in its *Design Report* (Peck et al. 2014) for its 15-month follow-up report (Peck et al. 2018).
3. The HPOG 2.0 evaluation's logic model. That logic model (see **Appendix Exhibit A-4**) posits that the offer of access to an HPOG program will lead to more training (than in the absence of HPOG), which will lead to more credentials, which will in turn lead to higher earnings and radiating impacts on other measures of well-being. This report's short-term follow-up point of 15 months was posited to be too early to expect to see impacts on earnings. As such, *average Q5 earnings* was specified as a secondary, but not confirmatory, outcome. Instead, timing considerations urged selecting as the confirmatory outcome for this short-term report an outcome that occurred by 15 months after random assignment that could lead to later impacts on earnings. It was plausible that short-term (i.e., 15-month) impact on educational progress implied that longer-term (36 months or later) impact on earnings was likely.

With respect to this confirmatory outcome, the findings are clear. In the control group, 46 percent of members show educational progress, whereas in the treatment group, 63 percent

<sup>57</sup> Suppose that a credential doubled quarterly earnings, from approximately \$5,000 per quarter to approximately \$10,000 per quarter. Then a 2 percentage point increase in credentials would imply an impact of access to an HPOG 2.0 program on quarterly earnings of \$100, or 2 percent.

show educational progress. This is an impact of 17 percentage points and a relative increase of more than a third. The estimate is unambiguously positive.

The conventional multiple comparisons decision framework (see Schochet 2008) suggests that a favorable impact on a confirmatory outcome should be taken as a favorable finding for the intervention under evaluation, at least through the available follow-up period. Indeed, this unambiguously favorable impact on the HPOG 2.0 evaluation's short-term confirmatory outcome indicates that programs are on track to achieve the HPOG 2.0 demonstration's goals.

The second approach to thinking about these results questions the assumption that impacts on educational progress will likely lead to impacts on earnings. As defined, educational progress is nearly dosage-blind; that is, educational progress counts earning any credential as a success, no matter how short the corresponding training. The second approach focuses, instead, on dosage-sensitive outcomes.

The natural dosage-sensitive outcome is *months of training*. Though HPOG 2.0 increases months of training by a third,<sup>58</sup> the absolute impact is small: 1.4 months.<sup>59</sup> One could imagine this impact growing if there was a strong impact on *being in training as of 15 months*. However, the impact of HPOG programs on that outcome is also small: 6 percentage points. A small impact on training persistence at 15 months suggests that after 15 months, the impact on months of training should not be expected to grow by much.<sup>60</sup>

NSC-based estimates—for the early (pre-survey) cohort and through two and a half years (30 months)—reinforce these findings. Those estimates imply that HPOG 2.0 increases full-time-equivalent months of college by 0.6 months at a 15-month follow-up.<sup>61</sup> Between 15 and 30 months, that impact only grows to 0.7 months. Furthermore, given that there is no impact on being enrolled in training beyond Q6, the impact on full-time-equivalent months beyond two and a half years is unlikely to grow much.<sup>62</sup>

Despite the large relative impacts on educational progress, these impacts on months of training seem unlikely to lead to detectable and substantively important impacts on earnings. Pending

<sup>58</sup> That is impact as a fraction of the control group level.

<sup>59</sup> This is the survey-based estimate, through 15 months after random assignment.

<sup>60</sup> Though 19 percent of the control group is in training as of 15 months, 25 percent of the treatment group is, an impact of 6 percentage points. If study members still in training at 15 months got, on average, another year of training, the impact on months of training would rise by half: from 1.4 months to 2.1 months (0.72 months = 12 months x 6 percentage point impact on still in training at 15 months). As is argued below, an impact of 2.1 months would still imply only a small impact on earnings.

<sup>61</sup> This NSC-based estimate is smaller than the survey-based estimate for at least two reasons. First, the NSC measure does not include non-college training (see Appendix Section C.4 for more on measurement in the NSC), whereas the survey captures both college-based and non-college training. Second, the survey-based measure is months with any training. In contrast, the NSC-based measure is full-time-equivalent months of training—and many months of training appear to be part-time. Note also that for the computations that follow, full-time-equivalent months are the more appropriate concept.

<sup>62</sup> Beyond 18 months (Q6) there is no detected treatment/control difference in college enrollment. Thus, computations for the NSC equivalent to those for the survey in the previous footnote imply no growth in impact at all.

future estimates of the longer-term impact of HPOG 2.0 on earnings, non-experimental estimates of the return to an additional month of training/college suggest that an impact on training of about a month would likely have an impact on quarterly earnings of less than \$100.<sup>63</sup> An impact of that magnitude would be small; about a 10th of the \$1,000 per quarter impact of job training programs that have detected impacts and are often pointed to as models (e.g., Per Scholas, Project QUEST, Year Up; see **Appendix Exhibit B.2**). Furthermore, even with its very large samples, the HPOG 2.0 evaluation could not detect an impact that small.

These computations are only suggestive. It is possible that an incremental month of HPOG 2.0 program training is much more valuable than is implied by the econometric literature. Specifically, it is possible that HPOG 2.0 program support services—in particular, academic advising—lead to more efficient schooling; that is, more degrees per month of training. It is also possible that longer follow-up will find much larger impacts on months of training.

Nevertheless, in as much as large relative impacts on educational progress do not in fact lead to detected, substantively important longer-term impacts on earnings, this second approach has implications for evaluation design. Evaluators may want to rethink focusing on dosage-blind outcomes (e.g., *any credential*, and *any credential or still in training*). Measuring dosage-sensitive outcomes is harder but seems worthy of serious consideration. More methods work is needed to explore which dosage-sensitive outcome(s) would best balance feasibility and usefulness.<sup>64</sup>

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<sup>63</sup> Two approaches are both consistent with this conclusion. First, Card's (2001) survey suggests that another year of generic education increases earnings by about 10 percent. For HPOG 2.0, this would imply an impact of less than \$100 per quarter. Specifically, earnings for this population are roughly \$5,000 per quarter (see Exhibit 4-7, at ten quarters). If we take a full year of schooling as 10 months, then a linear extrapolation would imply that an impact of 1.4 months would increase earnings by \$70 (= \$5,000 per quarter x 1 percent per additional month of training x 1.4 additional months). If the impact on months of training would grow to 2.1 months, the implied impact on earnings would be \$105. Note, however, that the appropriate concept is impact on full-time-equivalent months of training, and that impact is likely smaller than the impact on months of training.

Second, more recent analyses of community college data (Stevens, et al. 2019), using weaker non-experimental methods, suggest returns to an additional month of college that are about twice as large—especially for women training in healthcare professions. So an impact of an additional full-time-equivalent month of training of 2 percent would suggest an impact on earnings of \$70 (= \$5,000 x 2 percent per additional month of training x 0.7 additional months; where 0.7 additional months is the NSC estimate of the impact on FTE months of college, see Appendix Section E.6).

<sup>64</sup> See Appendix Section A.6's discussion of a particular dosage-sensitive outcome—*months of training* (rather than *any credential*).

## 4. Impacts on Labor Market Outcomes, Healthcare Employment, Well-Being, and Public Assistance Receipt

### Chapter 4 Key Findings

Analyses of the **Short-Term Follow-Up Survey** responses at about 15 months after randomization find that HPOG 2.0 programs:

- Increase employment in the healthcare sector.
- Have no detected impact on having a job that offers health insurance, an indication of job quality.
- Increase “career connectedness” (being occupied full-time with some combination of work and/or school).
- Increase self-perceived career progress.

Analyses of **National Directory of New Hires** data find:

- Pooling the pre-survey and survey cohorts, at five quarters (about 15 months) after randomization, HPOG 2.0 programs have no detected impact on employment or earnings—both pre-specified secondary outcomes.
- For the pre-survey cohort, HPOG 2.0 programs have no detected positive impact on earnings through two and a half years after randomization.

This chapter reports estimates of the impact of HPOG 2.0 on labor market outcomes, well-being, and public assistance receipt. The first three sections present short-term impacts from the Short-Term Follow-Up Survey on labor market outcomes (Section 4.1), healthcare-specific labor market outcomes (Section 4.2), possible precursors of career success (Section 4.3), and measures of income, well-being, and public assistance receipt (Section 4.4).

Most survey outcomes are reported 15 months after random assignment. For those in training towards the end of this interval, this may be too early to expect to see impacts on earnings, well-being, and public assistance receipt.

Then Section 4.5 uses National Directory of New Hires (NDNH) data to present short-term impacts (about 15 months after randomization) and intermediate-term impacts (about two and a half years after randomization) on earnings and employment outcomes. Finally, Section 4.6 provides some discussion of the findings.

### 4.1 Labor Market Outcomes

This section presents evidence of the short-term impact of HPOG 2.0 on labor market outcomes as measured in the Short-Term Follow-Up Survey.

▪ **In the short term, HPOG 2.0 programs do not detectably increase employment.**

Consistent with higher rates of training and the findings of almost all evaluations of job training programs, survey data show that employment in the quarter after randomization<sup>65</sup> is lower for the HPOG 2.0 treatment group, compared with the control group. From Q2 to Q5, HPOG 2.0 programs do not increase or decrease employment (**Exhibit 4-1**). The estimates are quite precise, meaning that we can rule out an increase in employment greater than 2.5 percentage points.

**Exhibit 4-1 Impacts on Employment, Earnings, and Wages**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Any Employment (%)</b>					
Q1	61.0	64.7	-3.7**	1.3	-5.7
Q2	66.3	68.2	-1.9	1.2	-2.7
Q3	68.2	69.2	-0.9	0.9	-1.3
Q4	69.5	69.4	+0.1	0.8	0.1
Q5	71.6	71.3	+0.3	1.1	0.5
<b>Employment Status at Survey</b>					
Employed (%)	66.4	66.3	+0.1	1.2	0.2
Hours per week	23.6	23.4	+0.2	0.5	0.7
Quarterly earnings (\$)	3,313	3,250	+62	73	1.9
Employed with above average wage <sup>a</sup> (%)	33.9	33.0	+0.9	1.1	2.9
Hourly wage if employed (conditional) (\$)	14.09	13.94	+0.15	0.13	1.1
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

<sup>a</sup> "Above average wage" means a wage higher than the median wage among employed members of the control group.

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e., 100 x [impact / control group mean]).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

▪ **In the short term, HPOG 2.0 programs do not detectably increase most labor market outcomes, including employment or earnings.**

As of the survey interview, HPOG 2.0 programs have no detectable impact on employment (about three-quarters of treatment and control group members are employed), hours employed per week (unconditional on employment), quarterly earnings (about \$3,300 per quarter), hourly wages above the control group median (of \$14), or hourly wages (conditional on employment).

<sup>65</sup> For survey data, quarters are relative to the day of randomization. Thus, Q1 is weeks 1 to 13 after randomization, Q2 is weeks 14 to 26 after randomization, etc. In contrast, for NDNH data, quarters are full calendar quarters after randomization. Thus, for example, for a participant randomized in week 10 of a calendar quarter (i.e., with three weeks left in the calendar quarter), Q1 is weeks 4 to 16 after randomization, Q2 is weeks 17 to 29 after randomization, etc.

- **In the short term, HPOG 2.0 programs do not detectably improve access to employer-offered health insurance or other measures of job quality.**

As of the survey interview, slightly less than half of members of both the treatment and control groups are in jobs that offer health insurance<sup>66</sup> (**Exhibit 4-2**). Similarly, as of the interview, there is no detectable difference between the treatment and control groups for having a job with a regular schedule, paid sick leave, or family-friendly policies.<sup>67</sup>

- **In the short term, HPOG 2.0 increases career connectedness.**

The survey follow-up period might be too early to expect an impact on earnings, so the evaluation pre-specified *career connectedness*—defined as being engaged full-time in work or school or at least part-time in both—as a possible precursor of future earnings impacts.<sup>68</sup> HPOG 2.0 programs have a small impact of 3 percentage points (52 percent versus 55 percent) on career connectedness, a relative increase of 6 percent.<sup>69</sup>

**Exhibit 4-2 Impacts on Employment with Benefits and Career Connectedness**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Has a Job as of the Survey and That Job: (%)</b>					
<b>Offers health insurance</b>	44.6	43.4	+1.2	1.2	2.7
Has regular schedule	50.6	50.3	+0.3	1.3	0.5
Offers full set of standard benefits	27.5	26.9	+0.5	1.0	2.0
Has paid sick days	38.6	37.8	+0.9	1.2	2.3
Has family-friendly policies	34.4	34.5	-0.1	1.2	-0.3
Has generous support for future formal education or training	26.4	26.0	+0.4	1.1	1.4
<b>Full-Time Engagement with Some Combination of Work and/or School as of 15 Months after Randomization (%)</b>					
<b>Career connectedness</b>	54.8	51.6	+3.2 <sup>##</sup>	1.1	6.3
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded and statistical significance is based on two-sided hypothesis tests.

“Relative impact” represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

<sup>66</sup> This result diverges from that for HPOG 1.0, which found that HPOG programs increased the likelihood of having a job that offers health insurance. However, the estimated impacts are not statistically different from each other ( $p=.52$ ; see Gelman and Stern 2006).

<sup>67</sup> Outcomes in this paragraph are unconditional; that is, the entire sample is included in the analysis regardless of whether they are employed at the time of the interview. For any attribute (e.g., offers health insurance), both study members who do have a job but it lacks the attribute and study members who do not have a job code as “No.”

<sup>68</sup> Full-time employment is defined as working 35 or more hours per week. Full-time enrollment is defined as spending 12 or more hours per week in class. For additional details, see Appendix Section C.2.2.

<sup>69</sup> As discussed in Appendix Section C.6.1, there was an error in the survey design that resulted in extensive missing data on hours engaged in school or work when a respondent was doing both at the same time. To address this error, two planned secondary outcomes involving full-time-equivalent months of training were replaced with alternate measures that do not adjust for part-time enrollment. Additionally, though exact hours were not collected, there is sufficient information to infer that the respondent was both in school and working at least part-time, so the missing data on hours do not cause any problem for measurement of career connectedness.

## 4.2 Healthcare Labor Market Outcomes

As noted in Chapter 1, HPOG has dual policy goals: (1) increasing earnings of recipients of TANF and other low-income adults; and (2) providing a skilled workforce to meet the needs of the healthcare sector. This section focuses on healthcare-specific labor market outcomes.

- **In the short term, HPOG 2.0 programs increase hours worked and earnings in healthcare jobs.**

One of HPOG 2.0's dual policy goals was to increase the skilled healthcare workforce. HPOG 2.0 programs increase healthcare employment by 4 percentage points, which is 10 percent higher relative to the control group (**Exhibit 4-3**).<sup>70</sup> As with employment in healthcare, HPOG 2.0 programs also increase weekly earnings and hours worked in healthcare by 10 percent relative to the control group.<sup>71</sup>

**Exhibit 4-3 Impacts on Healthcare Labor Market Outcomes**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Current Employment in the "Healthcare Field" (direct self-classification)</b>					
<b>Currently employed in healthcare (%)</b>	45.1	41.1	+4.0##	0.9	9.7
Typical weekly hours	14.9	13.6	+1.4**	0.3	10.3
Typical weekly earnings (\$)	218	197	+21**	6	10.6
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded or italicized, and statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

<sup>70</sup> This impact on healthcare employment is robust to other definitions of "healthcare job." For some definitions, impacts are larger (see Appendix Section F.2). For example, defining a healthcare job based on open-ended questions about the respondent's job and associated typical duties yields an increase of 13 to 15 percent, depending on whether the definition includes allied work in the healthcare industry (e.g., Orderly).

<sup>71</sup> This calculation counts hours and earnings from employment outside of healthcare as zero hours and zero earnings. The increase in hours and earnings is what might be expected from the increase in healthcare employment (e.g., if those induced to enter healthcare employment work similar hours and earnings to those who otherwise worked in healthcare).

### 4.3 Precursors of Career Success

This section considers some subjective measures of career success as of the Short-Term Follow-Up Survey that might be precursors of future impacts on earnings (**Exhibit 4-4**).

- **In the short term, HPOG 2.0 programs increase self-reported progress towards career goals.**

As of the survey interview, HPOG 2.0 programs increase the treatment group's belief that they are making progress towards their educational and career goals and they are on a career path. The treatment group's score (3.46) is 0.18 points greater than the control group's score (3.28), which implies an effect size of 0.22, commonly considered a "small" effect (Cohen 1988).<sup>72</sup> (A value of 4 indicates strong agreement that they are making progress on each of the three dimensions. A value of 3 indicates that they "somewhat agree" with the statement that they are making progress. So HPOG 2.0 programs push many participants from mostly weak agreement to strong agreement on the statement that they are making progress toward their goals.)

- **In the short term, HPOG 2.0 programs improve some non-monetary aspects of jobs.**

As of the survey interview, HPOG 2.0 programs increase the likelihood that study members get jobs that they perceive as having opportunities for advancement and being closely related to their training. However, HPOG 2.0 programs do not detectably help the treatment group get jobs in the short term that they perceive as very satisfying or highly desirable.<sup>73</sup>

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<sup>72</sup> The concept of an effect size was developed to facilitate comparisons across related studies where outcomes were measured on different scales. It is particularly useful for scales such as career progress for which no natural scale such as dollars or percent agreement is available. Effect size is defined as the ratio of the impact to the standard deviation of the outcome.

An alternative way to compare studies with different scales is to state what percentage of the treatment group has a higher score than the median score for the control group. On that metric, an effect size of 0.22 means that 59 percent of the treatment group has a higher level of career progress than the median response in the control group.

<sup>73</sup> "Highly desirable" means the job provides defined benefits (such as paid sick leave), family-friendly policies, personal satisfaction, opportunities for advancement, and employer support for further training. All reported jobs are grouped into five clusters. Jobs in one of these five clusters are identified as highly desirable. Jobs in the highly desirable category (31 percent of all reported jobs) have an above-average score on each cluster-analysis-defined dimension. Jobs in each of the other four clusters have a below-average score on at least one dimension. That is not to say each *individual* job in such a cluster has an above-average score on every dimension. For example, a job with very high scores on four dimensions might be "highly desirable" despite a low score on one dimension. See Appendix Section C.2 for details of construction.

**Exhibit 4-4 Impacts on Leading Precursors of Career Progress**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>As of the Survey</b>					
<b>Perception of progress towards career goals (scale score)</b>	3.46	3.28	+0.17##	0.03	5.3
Has a job and that job: (%)					
Is very satisfying	25.9	25.7	+0.1	1.2	0.5
Has opportunities for career advancement	42.6	39.8	+2.8*	1.1	7.0
Is closely related to training	33.8	29.1	+4.7**	1.3	16.3
Is highly desirable	20.7	20.6	+0.0	1.1	0.2
Sample size	6,646	2,974			

Source: Short-Term Follow-Up Survey

Note: Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded or italicized, and statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

"Progress towards career goals" is a continuous scale score based on three items about (1) progress towards long-range educational goals, (2) progress towards long-range employment goals, and (3) whether the individual respondent sees themselves on a career path.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

## 4.4 Income, Well-Being, and Public Assistance Outcomes

**Exhibit 4-5** presents estimates of the short-term impact of HPOG 2.0 programs on income, measures of well-being, and public assistance receipt.<sup>74</sup>

- **In the short term, HPOG 2.0 programs do not detectably increase personal or household income or most other measures of well-being.**

In general, the treatment group's average income and well-being are not statistically different from that of the control group at 15 months after randomization. The one exception is that a slightly smaller share of the treatment group reported "trouble making ends meet." Because HPOG 2.0 programs have not increased earnings or income in the short term, perhaps this reported hardship reduction is due to provision by the programs of financial assistance in paying for training or from emergency assistance (see Section 2.4.2).<sup>75</sup>

<sup>74</sup> See Appendix Section F.4 for an analysis of impacts on family structure.

<sup>75</sup> The evaluation of HPOG 1.0 did not investigate this outcome at 15 months but found a similar impact of 3.2 percentage points at three years after randomization.

**Exhibit 4-5 Impacts on Income, Other Measures of Well-Being, and Public Assistance Receipt**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Income (\$)</b>					
Monthly personal income	1,359	1,368	-9	21	-0.6
Monthly household income	1,901	1,920	-19	46	-1.0
<b>Other Well-Being (%)</b>					
Has ready funds to cover \$400 emergency	13.2	13.4	-0.2	0.9	-1.2
Covered by health insurance	78.8	77.8	+1.0	0.8	1.3
In excellent health	18.2	18.2	-0.1	0.7	-0.3
Food security	84.9	84.1	+0.8	0.7	1.0
Any signs of financial distress	63.0	63.8	-0.9	0.9	-1.4
Trouble making ends meet	31.4	33.7	-2.2**	0.9	-6.6
Housing stability	84.7	83.1	+1.5	1.0	1.8
<b>Any Household Member Participating in: (%)</b>					
TANF	9.1	8.6	+0.5	0.6	5.8
SNAP	46.7	45.4	+1.2	1.1	2.7
Either TANF or SNAP	47.2	46.3	+0.9	1.1	2.0
TANF, SNAP, or Medicaid	67.0	66.5	+0.5	0.9	0.8
Any means-tested benefits	75.1	74.3	+0.8	0.7	1.1
Sample size	6,646	2,974			

Key: SNAP= Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families.

Source: Short-Term Follow-Up Survey

Note: All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

- **In the short term, HPOG 2.0 programs do not detectably change participation in means-tested public assistance.**

Similar levels of treatment and control group members participate in TANF, SNAP, Medicaid, or other means-tested benefit programs as of the Short-Term Follow-Up Survey. Overall, about three-quarters of treatment and control group members are in a household receiving at least one means-tested benefit.

## 4.5 Earnings and Employment from NDNH

Section 4.1 presented labor market results from the Short-Term Follow-Up Survey. This section presents results from administrative data on employment and earnings from the National Directory of New Hires. The section begins with short-term (through Q5) estimates of impacts for the pooled pre-survey and survey cohorts ( $N=22,443$ ). The section then presents intermediate-term (through Q10) estimates of impact for the large pre-survey cohort receiving services from less mature program implementations ( $N=9,845$ ).

- **For the pooled pre-survey and survey cohorts, HPOG 2.0 programs do not detectably increase employment or earnings in Q5.**

**Exhibit 4-6** presents employment and earnings from the quarter of randomization (Q0) through the fifth full calendar quarter (Q5, or about 15 months) after randomization for the pooled pre-survey and survey cohorts. HPOG 2.0 programs depress employment and earnings in Q0, Q1, and Q2. Such lower employment and earnings during training are expected and are a near-universal finding in studies of job training.<sup>76</sup> Time spent in training is time not available for working and earning.

Nevertheless, the treatment and control groups have similar employment rates and earnings in Q5 (both secondary outcomes). On average, employment rates are slightly more than 75 percent and quarterly earnings are slightly less than \$4,000. These NDNH results are qualitatively similar to the Short-Term Follow-Up Survey results (see Section 4.1).

**Exhibit 4-6 Impacts on Earnings and Employment through Q5 (combined pre-survey and survey cohorts)**

Outcome	Treatment Group Mean	Control Group Mean	Impact (Difference)	Standard Error	Relative Impact (%)
<b>Any Employment during the Quarter (%)</b>					
Q0	63.3	64.7	-1.4*	0.6	-2.1
Q1	68.1	70.4	-2.3**	0.7	-3.2
Q2	71.4	72.8	-1.4	0.7	-1.9
Q3	73.2	73.1	+0.0	0.7	0.0
Q4	74.5	74.0	+0.6	0.8	0.8
Q5	76.0	75.1	+0.8	0.8	1.1
<b>Earnings during the Quarter (\$)</b>					
Q0	2,242	2,236	+6	33	0.3
Q1	2,560	2,731	-171**	44	-6.3
Q2	3,011	3,163	-151**	48	-4.8
Q3	3,374	3,440	-66	58	-1.9
Q4	3,626	3,670	-45	59	-1.2
Q5	3,947	3,942	+5	64	0.1
Sample size	15,063	7,380			

Source: National Directory of New Hires

Note: Pooled survey and pre-survey cohorts (N=22,423).

Secondary outcomes are bolded and statistical significance is based on one-sided hypothesis tests; exploratory outcomes are not bolded and statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

Statistical significance levels for one-sided hypothesis tests are indicated with hashtags, as follows: ##=1 percent, #=5 percent.

<sup>76</sup> Sample size likely explains why the Short-Term Follow-Up Survey does not find a negative impact on Q2 employment but NDNH does. In total, the pre-survey NDNH sample is more than twice the size of the survey sample (23,096 versus 9,620) and the standard error of the estimate is half as large (0.6 percentage points versus 1.2 percentage points).

- **For the pre-survey cohort, HPOG 2.0 programs do not detectably increase earnings through two and a half years after randomization.**

As shown in **Exhibit 4-7** (below), for the pre-survey cohort, the study provides no evidence through two and a half years after randomization that HPOG 2.0 programs increase earnings.<sup>77</sup>

These results are broadly consistent with the HPOG 1.0 results. Although the HPOG 1.0 evaluation detected a positive impact on earnings of \$137 in Q5, the HPOG 1.0 and HPOG 2.0 estimated impacts are not statistically different from each other. Furthermore, in the broader literature, neither impact would be considered large or economically meaningful. Like the HPOG 2.0 results through Q10 shown here, HPOG 1.0 did not detect an impact on earnings in Q6 through Q10.

As shown in **Exhibit 4-8** (page 677), there is little evidence that impacts on earnings vary by participant characteristics, either in the short term for the pooled pre-survey and survey cohorts or in the intermediate term for the pre-survey cohort only. (For details on associated standard errors and *p*-values, see Appendix Section F.5.) There is also little evidence that the impact on earnings varies across local programs (see Appendix Section C.7.4 and Appendix H).

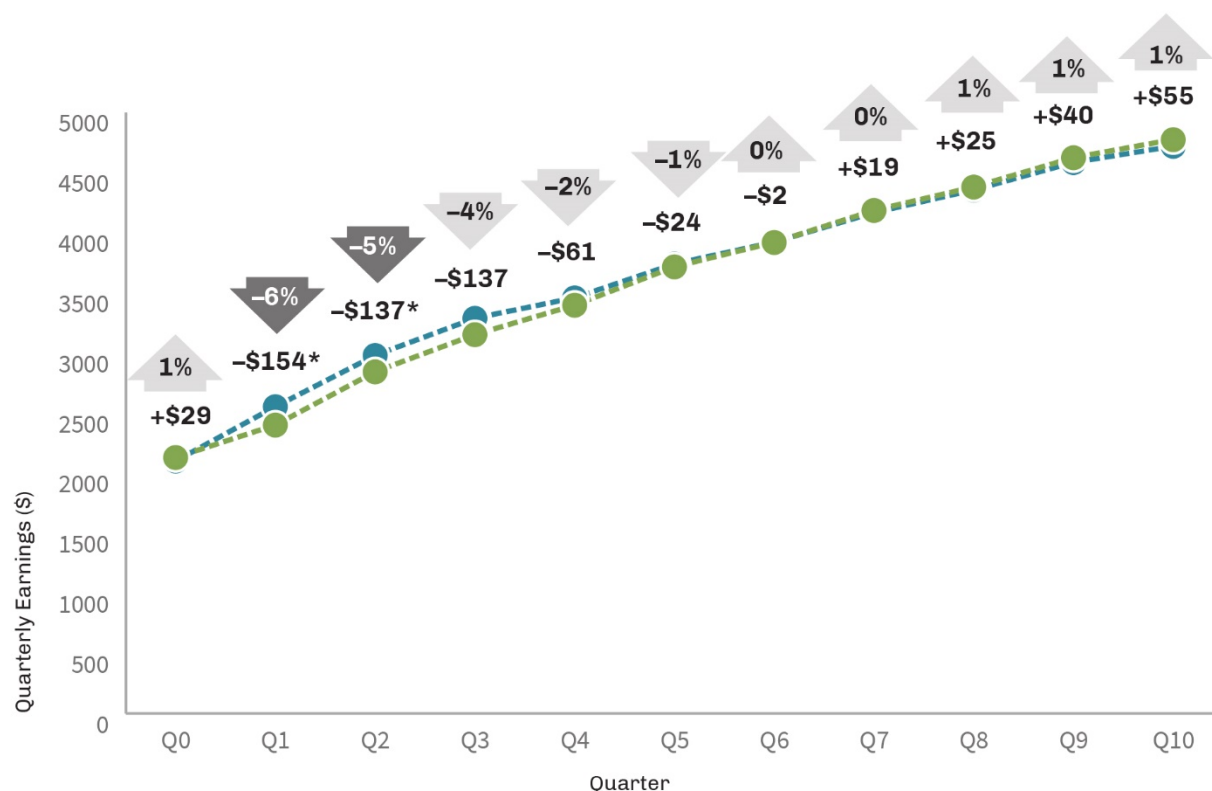
- **For the pre-survey cohort, HPOG 2.0 programs do not detectably increase employment, through two and a half years after randomization.**

As with earnings, in no quarter does the study detect that HPOG 2.0 programs increase employment (see Appendix Section F.5).

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<sup>77</sup> For those study members randomized early in the pre-survey cohort, NDNH provides even longer follow-up (out to Q13 after randomization). Appendix Section F.5 provides analyses of that longer follow-up. Those analyses find positive impacts on earnings in Q11. The reason for this impact to arise 11 quarters out is unclear. The *Intermediate-Term Impact Report* will explore whether this pattern continues.

**Exhibit 4-7 Impacts on Earnings, by Quarter (pre-survey cohort only)**



● Control Group      ▲ % Relative Impact, Impact Estimate Is Statistically Significant  
● Treatment Group      ▲ % Relative Impact, Impact Estimate Is Statistically Insignificant

Source: National Directory of New Hires

Note: The numbers above the circles indicate impact estimates. Includes all members of the pre-survey cohort (i.e., through February 2017). N=9,845.

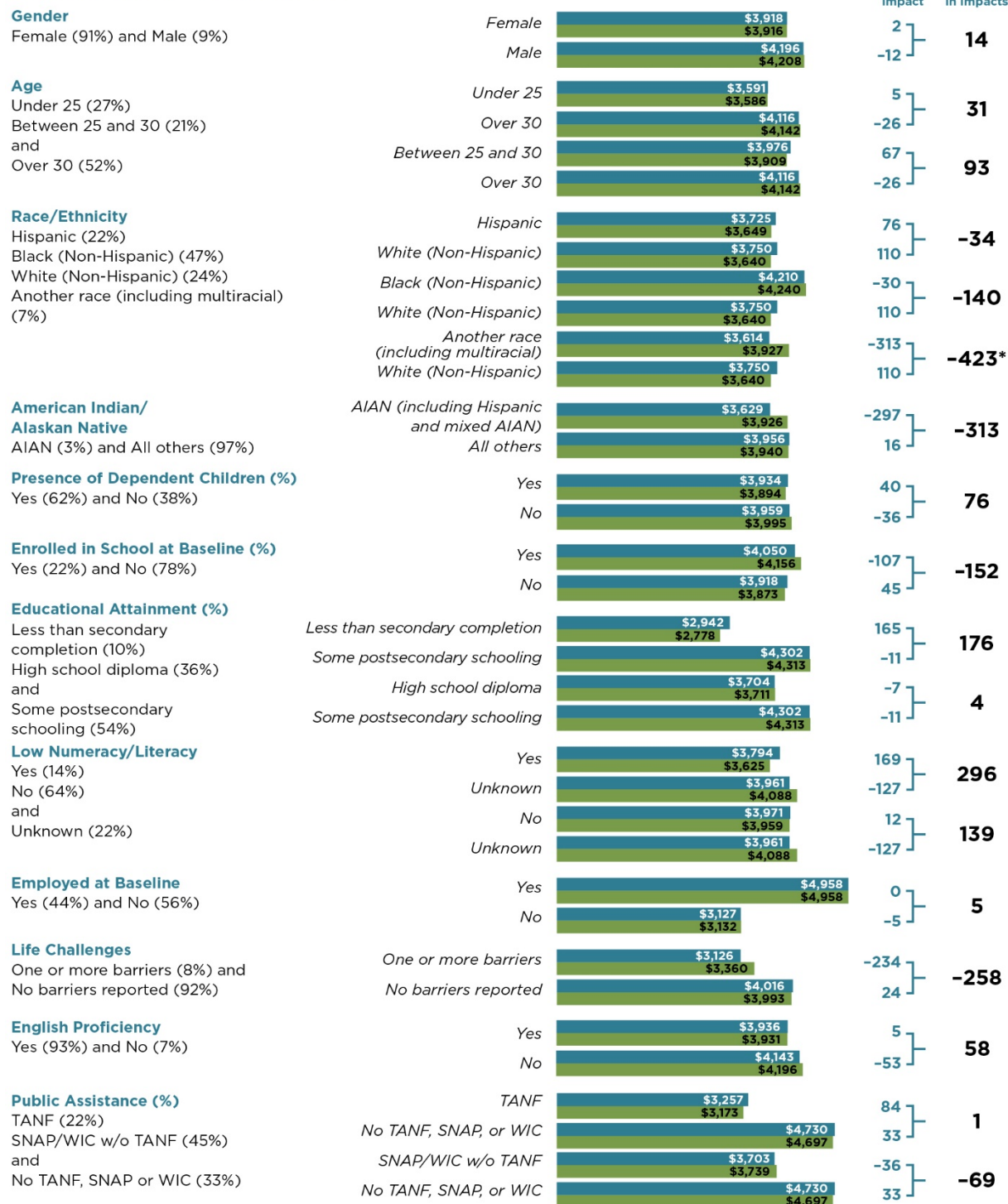
All outcomes in this exhibit are exploratory. Statistical significance is based on two-sided hypothesis tests.

"Relative impact" represents impacts as a percentage of the corresponding control group mean (i.e.,  $100 \times [\text{impact} / \text{control group mean}]$ ).

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

**Exhibit 4-8 Impact on Q5 Earnings, by Subgroup**

■ Treatment Group Mean ■ Control Group Mean



Key: SNAP=Supplemental Nutrition Assistance Program. TANF=Temporary Assistance for Needy Families. WIC=Special Supplemental Nutrition Program for Women, Infants, and Children.

Source: National Directory of New Hires

Note: All of the subgroup analysis is exploratory and statistical significance is based on two-sided hypothesis tests.

Statistical significance levels for two-sided hypothesis tests are indicated with asterisks, as follows: \*\*=1 percent, \*=5 percent.

## 4.6 Discussion

While the statutory goals of HPOG are to provide healthcare training and to expand the healthcare workforce, participant motivation is more forward looking. They forgo earnings and leisure to complete the training because they expect the training to lead to higher earnings. This chapter therefore focuses on HPOG 2.0's impacts on two outcomes: (1) earnings and (2) the size of the healthcare workforce.

With respect to *earnings*, this chapter provides no evidence that HPOG 2.0 increases earnings in the short term. Because the short term (through five quarters after randomization) might be too early to detect impacts on earnings, no labor market outcome was specified as confirmatory. However, for the pre-survey cohort, Section 4.5 reports no evidence of impact for intermediate-term follow-up periods—that is, no detected impact on earnings through two and a half years (10 quarters) after randomization. These results are like those in the HPOG 1.0 three-year impact report: no evidence of an increase in Q10 earnings (Peck et al. 2019).

For most prior studies of similar programs, small sample sizes plausibly might explain the similar lack of a detectable impact on earnings. This critique does not apply to the HPOG 2.0 short-term results for the survey cohort, to the HPOG 2.0 intermediate-term results for the pre-survey cohort, or to the HPOG 1.0 results that they follow. Each of these samples includes more than 7,000 study members and could therefore reliably detect impacts of about \$200 per quarter.<sup>78</sup>

In addition to this lack of impact on earnings, this *Short-Term Impact Report* found no impact on most other labor market outcomes: employment with health insurance, other fringe benefits, or other objective measures of working conditions. Furthermore, given the lack of impact on earnings, it is not surprising that there are few impacts on income or broader measures of well-being or receipt of public assistance.

In contrast, with respect to the size of *the healthcare workforce*, this chapter reported a modest shift into healthcare employment (about 4 percentage points). HPOG 2.0 is helping program participants find jobs in the short term that are more closely related to their training and that give them the feeling of making progress on their career goals. Thus, despite the lack of material gains, on average, program participants are more hopeful about the future. Finally, these are adults who wanted to work in healthcare; HPOG 2.0 has allowed more of them to do that.

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<sup>78</sup> Following practice in HPOG 1.0 and HPOG 2.0, these computations use *prospective* standard errors. They thus imply moderately larger samples than would be implied by the *retrospective* standard errors used in the previous paragraph and footnote and in most other studies. Nonetheless, the evaluation of HPOG 1.0 published a standard error of just \$80 on the estimated impact of those programs on Q5 earnings (Peck et al. 2019), and this HPOG 2.0 report has a corresponding standard error of just \$69.

## 5. Discussion

The HPOG 2.0 National Evaluation's Impact Evaluation is making an important contribution to the field's collective knowledge about sector-based and career pathways programs. The evaluation is large, randomly assigning more than 50,000 study members over the grant period (2015-2021). Furthermore, it encompasses all 38 of the non-Tribal local HPOG 2.0 programs, each operating in its own way but under broad ACF guidelines. This research approach allows the evaluation to assess whether programs responding to the HPOG 2.0 Funding Opportunity Announcement—across their many implementations—shifted a range of outcomes.

This *Short-Term Impact Report* is the first report for the HPOG 2.0 Impact Evaluation. It has presented estimates of impact for:

- a range of education/training, labor market, and well-being outcomes—in particular, this report's confirmatory outcome, *educational progress*—from the Short-Term Follow-Up Survey at about 15 months after randomization, for those study members randomized during months 14 to 25 of program operations (the “survey cohort”);
- education outcomes at colleges (but not other types of training providers) from the National Student Clearinghouse through two and half years after randomization for those randomized during the first 13 months of program operations (the “pre-survey cohort”); and
- labor market outcomes from the National Directory of New Hires through five quarters for the survey cohort and through about two and half years after randomization for the pre-survey cohort.

Because many study members are still in training, this *Short-Term Impact Report* may be too early to see impacts on earnings—a key outcome for a job training program such as HPOG 2.0. Consistent with that perspective, this final chapter does not try to explain the results to this time point or discuss policy implications. Instead, it proceeds as follows: Section 5.1 notes the program's goals and logic model, summarizes the results relative to those program goals and logic model, and compares those results to findings in the broader literature on job training programs. Then Section 5.2 identifies key issues for the *Intermediate-Term Impact Report*, measuring outcomes through two and a half years after randomization for the entire sample, anticipated to be released in 2023.

### 5.1 Findings to Date in Context

The HPOG 2.0 logic model posits the following steps:

- (1) HPOG will increase access to training in healthcare professions and to various support services (including academic supports, personal and logistical supports, and employment supports), which will induce more training and more completion of training.
- (2) This training will lead to more receipt of healthcare credentials (certificates, certifications, and licenses, as well as degrees) both from training providers and from third parties such as state boards and industry associations.

- (3) The receipt of healthcare credentials will lead to more employment in healthcare occupations; better labor market outcomes (in particular, higher earnings); less use of public assistance; and improvements in well-being.

Relative to the steps of the logic model, the results thus far are mixed.

Analyses of the Short-Term Follow-Up Survey data find that the offer of HPOG 2.0 leads to large relative increases in receipt of support services, enrollment in training, training duration, and credentials. HPOG 2.0 also leads to a favorable and large relative impact on the evaluation's pre-specified confirmatory outcome: *educational progress*, defined as having completed training by earning a credential or being continuously in training at 15 months after randomization. This impact is *clear progress on the initial steps of the HPOG 2.0 logic model—improving access to training and boosting receipt of credentials*.

However, this increase in credentials is accomplished with only a small impact on months of training: 1.4 months. Complementary analyses of NSC data on outcomes at colleges through two and half years after randomization find that HPOG 2.0 has positive but small impacts on training at these institutions (0.7 full-time-equivalent months; 2 percentage points more credentials).

Also, as of the Short-Term Follow-Up Survey, HPOG 2.0 leads to increases in healthcare training, healthcare credential receipt, and healthcare employment. This is *clear progress on HPOG 2.0's policy goal of expanding the healthcare workforce*.

Finally, neither in the short term for the survey cohort nor in the intermediate term for the pre-survey cohort does the study detect impacts on earnings. Thus, as of this *Short-Term Impact Report*, there is *no progress on the policy goal of increased earnings*.

This broad pattern of results is consistent with most, but not all, other recent evaluations of job training programs (see Appendix B for details). Some high-quality experimental evaluations find evidence of impacts on both training and earnings in the short and intermediate terms. In contrast, however, most high-quality experimental evaluations—including HPOG 1.0—find evidence of impacts on training, but not on earnings—neither in the short term nor in the intermediate term.

In net, the literature suggests that increasing earnings for TANF recipients and other adults with low incomes is hard. Many have not graduated high school; they are older than conventional students; most identify as Black or Hispanic/Latino and therefore face ongoing discrimination and other employment barriers (e.g., from residential segregation).

## 5.2 Looking Ahead to the Intermediate-Term Impact Report

The ultimate goal of job training programs, including HPOG 2.0, is to raise earnings. The findings presented in this *Short-Term Impact Report* do not detect impacts in Q5 for the pre-survey and survey cohorts, nor through Q10 for the pre-survey cohort only. Results to be presented in the next impact report at 30 months—the *Intermediate-Term Impact Report*—will

be informative about the importance of several possible explanations for this lack of detected earnings impacts at 15 months after randomization:

- Perhaps the pattern is due to *insufficient sample*; that is, perhaps HPOG’s supports and training *do* cause more earnings, even in the short and intermediate terms—but the impact is too small to be detected with sample sizes available for this first impact report. This seems most plausible for subgroups of trainees. The larger samples available in the next impact report will allow detection of smaller impacts, at all follow-up periods, if present.
- Perhaps the pattern is due to the *relatively short follow-up periods* of available data; that is, perhaps more training *does* cause higher earnings but only past the follow-up periods of this report (15 months, or even two and a half years after randomization for the pre-survey cohort). In particular, perhaps only the combination of longer initial training plus follow-on training causes higher earnings. If true, the short-term (15 months) and intermediate-term (two and a half years for the pre-survey sample) follow-up periods in this report might be too short for such longer trainings and follow-on trainings to have been completed and therefore too short to cause higher earnings. The longer follow-up periods for the full sample available in the next impact report will allow detection of such later emerging impacts of training, if present.
- Perhaps the pattern is due to *weak early implementation* of HPOG 2.0 in local programs; that is, perhaps more training from mature programs *does* cause higher earnings, but the training from the early implementations captured in this first report does *not* cause higher earnings. Measures of outcomes for trainees who experience more mature program implementations available in the next impact report will allow detection of such impacts of mature programs, if present.
- Or perhaps, in fact, *the HPOG program model needs to be revisited*; that is, something about the training, supports, and personnel induced by the HPOG 2.0 FOA and the credentials its program completers receive is insufficient to generate meaningful earnings impacts at any follow-up period for the target population—TANF recipients and other adults with low incomes. In particular, local HPOG programs appear to have focused on getting trainees *some* credential, when perhaps longer duration credentials are needed to substantially increase earnings.

HPOG 2.0 appears to have caused large relative impacts on earning any credential, but those credentials are overwhelmingly for very short trainings. Further, HPOG 2.0’s impact on months of training is in relative terms large, but in absolute terms small, about a month. Impact on longer credentials also appears to be small. This pattern of results—large impacts on receipt of any credential, small impacts on months of training, and no impacts on earnings—could suggest that “receipt of any credential” is insufficient to yield detectable earnings impacts. Instead, it could be that detectable impacts on earnings require some combination of larger impacts on months of training and longer credentials. If the next impact report finds no impacts on earnings—with a larger sample, more mature programs, and a longer follow-up—that would be evidence for this conjecture.

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